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Empirical Analysis of Crime and Law Enforcement

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Prohlášení

Prohlašuji, že jsem habilitační práci s názvem "Empirical Analysis of Crime and Law Enforcement" vypracoval samostatně, pouze na základě uvedených pramenů a literatury.

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Contents

Chapter 1

Introduction

The Economics of Crime emerged as a new field of economics in the 1970s, spawned by Gary Becker's article Crime and Punishment: An Economic Approach (Becker 1968). Its underlying thesis – that crime can be fruitfully studied with the standard toolbox of economic analysis – inspired generations of scholars. The field today spans literally thousands of papers, both theoretical and empirical. Becker's seminal article alone has accumulated over 11000 citations[1]. The theoretical literature developed models of rational criminal behavior (e.g., Block and Heineke 1975), models of optimal law enforcement (e.g., Polinsky and Shavell 1984), positive models of the behavior of enforcement officials (e.g., Landes 1971) or normative models of criminal procedure (e.g., Lando 2005).

The empirical literature has focused predominantly on testing the crucial implication of the rational theory of criminal behavior: do offenders respond to changes in the probability and severity of punishment? The early literature, starting with Ehrlich (1973), was beset with serious data problems and identification issues (see Levitt and Miles 2007 for a discussion). The empirical research was reinvigorated in the 1990s with the diffusion of quasi-experimental econometric techniques (difference-in-differences, instrumental variables, regression discontinuity etc.). Researchers started exploiting legal reforms, political shocks or even terrorist attacks as a source of exogenous variation in the probability and severity of punishment to identify causal effects on crime (e.g., Drago et al 2009, Evans and Owens 2007, Draca, Machin and Witt 2011). Steven Levitt was undoubtedly the most visible trailblazer

[1] Google Scholar, accessed October 4, 2014.

of this approach (e.g., Levitt 1997, Levitt 1998). He also made the Economics of Crime known to the masses with the publication of Freakonomics, a global best-seller (Levitt and Dubner 2005).

Another stream of empirical literature investigated the functioning of the law enforcement process. After all, policemen, prosecutors and judges are rational and self-interested agents as well and there is no reason to expect that they would be immune to changes in incentives and legal rules. Papers in this literature study, for example, the responses of police to explicit monetary incentives (Benson et al 1992), plea bargaining tactics (Bjerk 2005) or the effects of career objectives of enforcers on their decision making (Rasmusen et al 2009).

My research agenda contributes to the empirical study of crime in general and law enforcement in particular. The essays collected in the habilitation thesis investigate the links between law enforcement, procedural rules, crime, and behavior of enforcement officials. Methodologically, all essays employ the quasi-experimental research design (with the exception of chapter 6 which employs a laboratory experiment.) Chapter 2 contributes to the literature by providing new evidence on the effect of deterrence on crime based on a unique quasi-natural experiment, and by representing the first attempt to estimate the economic model of crime using Czech data. Chapters 3 through 6 make novel contributions to the literature by testing hypotheses that had been essentially untested in the previous literature. Below, I briefly summarize the research question and scientific contribution of each chapter. Their main results are presented in the conclusions, where I also discuss more general lessons learned from this research agenda.

Does deterrence cut crime? The economics of crime literature has traditionally attempted to answer this question using cross-sectional or panel data at the level of cities, regions, or states. Many authors regressed the crime rates on empirical measures of the probability and severity of punishment (e.g., Ehrlich 1973, Wolpin 1980, Cornwell and Trumbull 1994). One weakness of this approach is the lack of a clear source of exogenous variation in deterrence; another is the relatively small variation in deterrence that generally exists across geographical units or over time. *Chapter 2* exploits the unprecedented drop in deterrence that followed immediately after the Velvet Revolution in the Czech Republic to estimate the effect of deterrence on crime. For example, in 1988, the chance that someone committing a robbery would be charged was 78%; of those charged with robbery, 82% were convicted. Just four years later, only 36% of robberies translated into charges, and only 53% of charges resulted in convictions. Crime rates also rose very sharply after the Velvet Revolution. I use alternative regression techniques to separate the contribution of deterrence to the growth in crime rates from the contribution of other (observed and unobserved) factors.

Probability and severity – these are the two dimensions of punishment that determine criminal behavior in Becker's (1968) model and most of the models that followed. Yet there is another important dimension of punishment: celerity. If the time elapsed between crime and punishment is shortened, the punishment is perceived as effectively more severe by the offender at the time of committing the offense because it is discounted less heavily. A shorter criminal procedure should therefore deter crime. In addition, shorter criminal procedure may potentially affect the behavior of law enforcers. They would be more willing to pursue offenders if the procedural work that follows arrest is less time consuming for them. In *Chapter 3*, I estimate the effects of a shorter duration of criminal procedure on crime rates, and indirectly also on the behavior of police officers. The Czech criminal procedure reform of 2002 offers a rare quasi-natural experiment to study this question. The reform introduced a much simpler (fast-track) procedure for prosecuting petty crimes such as simple thefts or driving-related offenses. The reform was adopted with varying intensity across districts. This allows me to identify causal effects by instrumental variables: In the first stage, I use the share of cases in a district that are actually prosecuted by the fast-track procedure as an instrument for the duration of criminal cases. In the second stage, I use the instrumented duration to estimate the effect of duration on crime rates.

In *Chapter 4*, I also study the Czech fast-track procedure further down in the criminal justice process. Specifically, I evaluate its effects on the duration of criminal cases and on the probability that the defendant is charged by the prosecutor and convicted by the court. Such evaluation sheds light on two general questions. First, what can policy makers do to speed up the criminal justice process? The obvious solution – hiring more judges, prosecutors, and policemen – is very costly. Simplifying the law enforcement process, on the other hand, may deliver a "technological improvement" – delivering more output with the same inputs. Second, the economic analysis of criminal procedure has been centered around plea bargaining, a distinctly American procedure. Economists have been traditionally rather positive about plea bargaining, arguing that it releases resources that the prosecutors may use to pursue defendants more vigorously. In equilibrium it increases the probability of conviction for defendants who plea guilty (obviously) but also for those whose cases are decided at trial. The introduction of the fast-track procedure had similar underlying economics: by allowing petty crimes to be prosecuted at low cost, it released enforcement resources. I therefore evaluate both its direct effects on the petty offenses as well as spillover effects on other, serious offenses.

Chapter 5 remains in the courtroom but moves geographically across the ocean. In a widely cited paper, Andreoni (1991) derived a theory of judges' responses to increases in the severity of punishment. Judges take into account the social cost of their conviction/acquittal verdicts. Increasing the severity of punishment increases the social cost of wrongful conviction and hence the judges convict with a lower probability. While generally accepted as a theory, Andreoni's hypothesis has been subject to next to zero empirical testing. Fusako Tsuchimoto and I test the hypothesis, again exploiting a quasi-experimental research design. The so-called Truth-in-Sentencing Laws were adopted by various U.S. states throughout the 1990s. They required that convicted offenders have to serve at least 85% of the sentence in prison, compared with the previous practice of releasing offenders typically after serving half of the sentence. Using an individual-level dataset of more than 80,000 criminal cases, we evaluate their effects on the probability of conviction and other case outcomes.

The last *Chapter 6* is devoted to the fact-finding procedures in court trials. Together with Michael K. Block, Jeffrey S. Parker and Olga Vyborna, we compare the efficacy of two stylized procedures: the adversarial ("American") and inquisitorial ("European") procedure. Under the adversarial procedure, the parties are responsible for presenting evidence and questioning the witnesses while the judge has a relatively passive role. Under the inquisitorial procedure, the judge conducts the interrogation of the parties and witnesses. Which of these two procedures does better in revealing the truth? We conducted a series of laboratory experiments in which subject played roles of plaintiffs, defendants and judges in a court trial. One of the parties ("Mr. Wrong") was given private incriminating information that would cause him to lose the case, if it was revealed during the trial. The key outcome of interest was whether the incriminating information is revealed. The subjects, including the judges, faced high-powered monetary incentives, and the rules of the trials were varied to mimic the key features of the adversarial and inquisitorial procedures. While the experimental cases were civil cases, the key differences between adversarial and inquisitorial systems exist also in criminal trials and thus the findings potentially generalize also to criminal procedure.

Chapter 2

Crime, Deterrence, and Democracy[1]

2.1 Introduction

Does deterrence cut crime? The empirical literature testing the basic prediction of the economic model of crime has taken several approaches. The early papers used data aggregated at the level of counties, states, or countries, and regressed the crime rates on empirical measures of the probability and severity of punishment (e.g., Ehrlich (1973), Wolpin (1980), Cornwell and Trumbull (1994)). The weakness of this approach is the lack of a clear source of exogenous variation in deterrence; it is therefore difficult to give the estimated deterrence effects a causal interpretation. Later approaches overcame this weakness by using an explicit identification strategy.

Some authors found instruments for changes in deterrence (e.g., Levitt (1997), Evans and Owens (2007)), exploited variation in legislative changes (e.g., Shepherd (2002)) or, most relevantly for this paper, studied the effects of major short-term shocks. Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005) and Draca, Machin, and Witt (2011) find large reductions in certain crimes during large increases in the deployment of police forces in the wake of terrorist attacks or during terror alerts. These approaches, however, often estimate the effects of policy interventions (e.g., putting more policemen on the street) rather than the behavioral relationship

[1]Published in Dušek, L., Crime, Deterrence and Democracy, German Economic Review, Vol. 13, No. 4 (2012), pp. 447–469. I appreciate comments and suggestions from Bruce Benson, Randall Filer, Štěpán Jurajda, Jan Kmenta, Daniel Münich, William Trumbull,anonymous referees, and participants at the GDN, EEA, and EALE conferences. Tomáš Konečný, Pavel Dvořák and Luboš Dostál provided excellent research assistance. Financial support from Global Development Network grant RRC IV-22/04 is gratefully acknowledged.

postulated by the economic model of crime – the link between the probability and severity of punishment and the crime rates.

This paper combines the last approach with the first. It investigates the effect on crime of a large and sudden drop in deterrence brought about by the collapse of communism in the Czech Republic. It also estimates the conventional deterrence effects of the probability of charges, probability of conviction, and the length of prison sentence. The ability to estimate the effects of the three criminal justice variables is also an improvement in the literature because of the lack of consistent conviction data for the U.S. states or counties.[2]

The abrupt regime change in November 1989 was soon followed by very sharp declines in the probability that an offender is arrested, charged, and convicted. The variation in deterrence generated by this shock is rarely observed within a single jurisdiction. For example, in 1988, the chance that someone committing a robbery would be charged was 78%; of those charged with robbery, 82% were convicted and 85% of those convicted were sentenced to prison. Just four years later, only 36% of robberies translated into charges, only 53% of charges resulted in convictions and 76% of convicts were sentenced to prison. Similar declines occurred for other offenses.[3]

Additionally, an increase in crime has turned out to be an unexpected and unpleasant side effect of democracy. The murder rate increased from 0.93 in 1988 to 2.5 in 1992 and continued to rise until the late 1990's (see Figure 2.1). The rise in property crime was even more pronounced. Just during the first post-revolution year, the number of thefts and burglaries increased five-fold, and later stabilized at around 8 times its level under communism. Crime became one of the major negative aspects of post-1989 development and one of the major concerns of ordinary citizens as well as politicians (Tucek et al. (1999)).

The growth in crime after the fall of communism was by no means unique to the Czech Republic. Table 2.1 shows the crime rates for Hungary, Poland, and East Ger-

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[2]Mustard (2003) is a rare deterrence study using the conviction and sentence county-level data in the U.S.; Mustard succeeded to collect such data for four U.S. states.

[3]To put the numbers in perspective: One of the largest shocks to police deployment studied in the literature (Draca, Machin, and Witt (2011)) was a 34% increase in police hours in London after the July 2005 terrorist attacks. In Wolpin (1980)'s analysis of robberies in California, England and Wales, and Japan, the clearance rate fell from 38.5 to 23.5 percent in California and from 50.7 to 42 percent in England and Wales during the 16-year sample period.

many. Like in the Czech case, homicides, robberies and thefts increased quickly by a factor of two or more in these countries. The table is meant to be only illustrative as I was not able to obtain sufficiently detailed data for other countries that would allow a reliable empirical analysis. I therefore confine the paper to a single country, at a cost of having fewer observations and forgoing the cross-country variation but at a benefit of using regional data that contain detailed deterrence measures and are comparable over time and across geographical units.

The dataset is a panel of the Czech Republic's regions. In addition to the variation over time, there is substantial variation between regions in the change in deterrence from the pre-1989 to the post-1989 period. I estimate the relationship between measures of deterrence and crime rates for six crime categories: murder, robbery, theft (including burglary), failure to support, rape, and intentional injury.[4] The key specification is a seemingly unrelated regressions model with lagged deterrence variables. I find that deterrence has statistically and economically significant effect on robberies, thefts, intentional injuries, and, in some specifications, also on the failure to support. However, I do not find statistically significant deterrence effects for murders and rapes. The estimates of the elasticity of the crime rate with respect to the probability of charge lie between -0.25 and -0.87 for robberies, and between -0.51 and -0.66 for thefts. The elasticities of the crime rate with respect to the conditional probability of conviction and the expected length of prison sentence are smaller in magnitude. I also predict how the crime rates would have evolved if deterrence had stayed at the 1989 levels. One quarter of the growth in robberies and over one half of the growth in thefts and intentional injuries during the 1990's can be explained by the fall in deterrence.

A natural concern is that the estimates do not reflect the effect of weaker deterrence but the effect of unobservable shocks associated with the transition from communism to democracy which also contributed to the growth in crime. For example, replacing central planning with the basic institutions of capitalism inevitably increased the gains from criminal activities: free trade made it easier to sell stolen goods abroad; open borders attracted crowds of tourists that are potential targets of robbers and

[4] These crimes were selected because they are serious or most numerous. To clarify the definitions of less-standard crimes: Failure to support is defined as non-fulfilling one's legal duty to materially support another person, e.g. when a divorced father stops making alimony payments, and is punishable by imprisonment (section 213 of the Czech Criminal Code). Intentional injury comprises only non-serious intentional injuries (section 221 of the Czech Criminal Code).

thieves; higher incomes and household wealth increased the value of goods that can be stolen[5]; and the rise in entrepreneurial activity gave rise to new types of conflicts that potentially may be resolved by violence. Rising unemployment reduced the opportunity costs of criminal activities. New social phenomena such as drugs, human trafficking, and organized crime have altered the nature of certain crimes such as murders and robberies (Cejp 2003).

I do acknowledge that such shocks undoubtedly contributed to the growth in crime. I control for some of them (e.g., changes in unemployment or income inequality) but it is fruitless to hope that all of them could ever be captured as observable variables in any dataset. Still, there are several arguments and specifications supporting the claim that the estimates indeed capture deterrence effects (presented in detail in Section 2.4). They are based on the lack of correlation between the post-1989 changes in deterrence and pre-1989 observables at the regional level, the patterns in the estimated year fixed effects, and the absence of a structural break in the estimated deterrence effects.

2.2 Democracy and deterrence

The transition from communism to democracy brought a large decline in deterrence through several channels. One was a major shakedown of police forces undertaken immediately after the 1989 Velvet Revolution. It involved abolishing the secret police, laying off or degrading many higher-rank officers, and reorganizing the internal structure and procedures. These measures together reduced (at least temporarily) the capability of the police to prevent and investigate crime. The second important channel, I argue, was a wide range of civil rights reforms that eliminated the oppressiveness and abuses in the communist system of (in)justice. They generally restricted the powers of law enforcement authorities, protected citizens against certain practices of those authorities, and made punishment less severe.[6]

[5]This can be documented by the increase in the stock of consumer durables. The number of cars per 100 households rose from 62 (1990) to 70 (2000), and the number of refrigerators/freezers rose from 118 (1990) to 153 (2000). Data on less basic durables are not available until the mid-1990's but for example video recorders were a rarity in the socialist economy while in 1995 there were 29 video recorders per 100 households and in 2008 there were 48. (Source: Czech Republic Yearbooks.)

[6]Vujtech et al. (2001) provide an overview of the reforms.

Specifically, the length of time during which a person can be detained was shortened from 48 to 24 hours, and the limit was being strictly enforced. The investigation procedure was substantially reorganized. The old procedure made it possible to carry out investigation, collect evidence, and only after that inform the suspect about the charges; the new procedure requires all investigations to be carried out against a particular person who has to be informed about them from the beginning.

The rights of defendants, such as the right to remain silent, the right to have consultation with counsel at any time, the right to have counsel present during interrogation, and the right to read all documentation regarding one's case during all stages of the criminal process were newly granted or expanded. Wiretapping of communication between the defendant and his counsel was disallowed without exception. Release on bail was made possible. Decisions regarding arrest and pretrial detention were shifted from the state attorneys to the judges.

Few people would prefer living in a society where the civil rights just described are denied. However, limiting the powers of law enforcement authorities and extending the rights of offenders increases the chance that a guilty offender is not punished. Such institutional changes would be reflected in a reduction in the empirical probability that an offender is charged, convicted, and sentenced to prison, although it is difficult to assess the contribution of specific changes. Anecdotal evidence includes a survey conducted among police officers at the end of 1990 who generally complained that "the 24 hour limit on the detention of suspects is the greatest obstacle in collecting evidence" (Tomin (1991)). According to a conversation with a judge, a high fraction of cases in the early 1990's were dismissed on purely procedural grounds, since the police did not yet adapt to the new rules and were violating some of the new rights of defendants.

The democratic reforms also reduced the severity of punishment by the elimination of the death penalty[7], by improvements in prison conditions, and by a gradual shift away from imprisonment towards alternative forms of punishment such as public works, contractual settlement between offender and victim, and probation.

[7]The occurrence of capital punishment under communism, while rare, was much more frequent than in the U.S. states that currently use it. 1200 murders were committed and 17 offenders were executed for murder during 1980–1989, implying that about one in 70 murders was punished by death. In contrast, one in 300 murders was punished by death in Texas, the U.S. state with the highest execution rate, during 1976–1997 (Katz, Levitt, and Shustorovich 2003, page 319).

Finally, deterrence would have likely declined even in the absence of any policy changes through the "resource saturation" mechanism (Fisher and Nagin (1978, p. 364)). The initial increase in crime immediately following the 1989 revolution could arguably be attributed to factors unrelated to deterrence. Holding resources devoted to enforcement fixed (at least temporarily), such an exogenous increase in crime would reduce the fraction of offenses the police and courts are able to clear. Offenders update their perceptions of the probability of punishment and choose to commit more crime, potentially starting a vicious circle in which more crime breeds more crime.

Several authors investigated the relationship between democracy and crime without addressing deterrence per se. Williams and Serrins (1995) exploit the availability of data on crime in the Soviet Union during perestroika. They observe that crime rates in the USSR are an order of magnitude below those in the USA, and that such a large difference can hardly be explained by differences in incomes, inequality or other economic factors. On the other hand Pridemore (2001) constructs an alternative time series of homicides in Russia from the national vital statistics and finds that during the 1980's the homicide rates in Russia and the United States were roughly equal. Andrienko and Shelley (2005) analyze the determinants of violent crime in post-Soviet Russia. Their focus is on the influence of ethnic and political conflict rather than on more standard deterrence variables. Since their dataset covers the years 1992–2000, they cannot assess how much the determinants of crime changed since the Soviet period.

This paper is directly related to Lin (2007) who regresses crime rates in a world-wide panel of countries (already including some of the post-communist countries) against an index of democracy and finds that democracy is associated with higher rates of minor offenses such as theft but lower rates of serious offenses such as murder. This finding contrasts sharply with the experience of the Czech Republic and other post-communist countries where all crimes, including murders, increased substantially.[8] Lin (2007) also documents that more democratic countries have, on average, weaker deterrence.

[8]However, the relative change in crime rates is consistent with Lin's findings, as the percentage increase in serious offenses was much lower than the percentage increase in minor offenses.

2.3 Data

The panel dataset covers the period from 1980 to 2000. All variables are observed at the level of eight administrative regions ("*kraje*") that constituted the main units of regional police and court administration since 1960. The measures of deterrence are constructed from the *Criminal Statistics Yearbooks* published by the Ministry of Justice since the 1970's. They report the number of cases completed at each step of the criminal process for each offense defined by the criminal code. From that information I construct:

P^A, the probability of charge, measured as the number of defendants charged at court divided by the number of offenses. It is a summary measure of the "productivity of police" – its ability to identify and apprehend offenders and to collect sufficient evidence to bring offenders to court.

P^C, the conditional probability of conviction, measured as the number of defendants convicted of a particular type of offense divided by the number of defendants charged. It captures the "productivity of the courts", as well as the burden of proof required to convict a defendant, and the degree of procedural rights granted to defendants.

F, the expected length of prison sentence faced by a convicted offender. It is constructed as the number of offenders sentenced to prison divided by the number of offenders convicted, times the average length of a prison sentence. The average length of prison sentence is computed from the information on the distribution of prison sentences – the yearbook reports the number of offenders sentenced to a prison term of less than 6 months, 6 to 12 months, 1–2 years, 2–5 years, more than 5 years, life imprisonment or the death penalty.[9]

The remaining criminal justice variables are the number of policemen employed by the Police of the Czech Republic in each region, which was provided by the personnel department at the Ministry of the Interior from their internal records, and the average real wage in the public sector which proxies for the cost of police.

[9] From 1991, the reported intervals of prison sentences are less than 1 year, 5–15 years, more than 15 years, and life imprisonment. To construct the average length of a sentence, I assume that the average length of a sentence within each reported interval is equal to the midpoint of that interval, i.e. I take 3 months for the interval of 0–6 months, and so on. For punishments over 5 years, I assume the average length is 10 years. I imputed 50 years as the equivalent punishment for the death penalty or life imprisonment.

The variable to be explained, the number of offenses, is recorded in the Ministry of Justice yearbook up to 1994. From 1992 onwards, the number of offenses has been recorded in the *Statistics of Crime in the Czech Republic*, an internal report of the Police Directorate, using a slightly different methodology. For the overlapping years 1992–1994, I select the higher of the two values as the number of offenses actually used in the analysis.

The reliability of the data covering the totalitarian period needs to be addressed. One might be naturally concerned that the official statistics intentionally underreported the number of crimes as Pridemore (2001) documents to have been the case of homicides in Russia. Fortunately, such concerns can be minimized in the Czech case. First, the Ministry of Justice yearbook was an internal government document, not a propaganda material. In fact, it had been treated as classified and was made available to the public only after 1991. Second, the data came directly from the police, state attorneys and court administrative records and the computerized collection methodology did change over the sample period. The reported numbers of offenses, charges, etc. were produced as simple counts of forms that the officials filed with each step in the criminal procedure.[10]

The possibility of wrongful convictions raises a different concern. If a non-negligible fraction of persons convicted by the communist judiciary were in fact innocent, the probability of conviction constructed from the data overstates the probability of conviction faced by the true offender. Such a measurement error, however, biases the estimate of the deterrent effect of convictions downwards since the true probability of conviction declined after 1989 by less than the observed probability.

I also use several socioeconomic variables that proxy the supply of potential offenders, the gains from committing crime, and the income opportunities from legitimate activities. The supply of potential offenders largely depends on the age and gender composition of the population as a disproportionate fraction of crimes is committed by young men. In order to save the degrees of freedom I construct a single sum-

[10] For example, when the police determine that a criminal offense was committed, based on their own investigative activities or a report from the victims or witnesses, the responsible officer has to fill in a paper form with detailed information about the offense. For statistical purposes, a shorter version of the form is entered into the electronic database. Even if the higher authorities that produce the aggregate statistics are honest, the measurement error in the aggregated data still may arise if the local officers underreport or overreport cases in the electronic database; however, such sources of error are in no way unique to the communist or transition countries.

marizing measure referred to as the "effective supply of offenders": Denoting s_{ajt} the share of men of age a living in region j in year t, and q_{act} the average (across all regions) fraction of crimes c committed by persons of age a in a given year, the effective supply is then computed as $ESF_{cjt} = \sum_a s_{ajt} q_{act}$. Note that the effective supply is specific for each crime category.[11] The gains from criminal activities are proxied by the average wage in the region. The legal income opportunities are captured by the unemployment rate among males aged 20–29 and by a measure of wage inequality, the ratio of the average wage in the construction industry to the average wage in the financial services industry.[12]

Table 2.2 summarizes the crime rates and deterrence measures before and after the regime change. It also shows a substantial variation across regions both in the level of deterrence, and, more importantly, in the changes in deterrence from communism to democracy. For example, while the conditional probability of conviction for robbery fell by 22.4% on average, there is a region (Central Bohemia) where it fell by a mere 10.3% and a region (North Bohemia) where it fell by as much as 37%. This variation across regions provides an additional source of identification.

Figure 2.6 shows the trends in crime rates for each crime category. The year of the regime change (1989) is highlighted. The murder rate increased from approximately 1 murder per 100,000 to almost 3. The robbery rate more than quadrupled, while the theft/burglary rate increased more than 10 times. Moreover, the short-term drop in thefts after 1994 should most likely be attributed to a change in reporting methodology rather than to any actual decline. Rape appears to be the only crime category for which the number of offenses, after the initial jump, returned back to

[11] The computation of q_{act} is based on the number of offenders in each 5-year age interval who were either investigated (till 1990) or charged (since 1991) for each offense, as reported in the Ministry of Justice Yearbook. The share of men in the overall population by 5-year age intervals was provided by the Czech Statistical Office.

[12] All wage and unemployment data come from "Structure of Earnings Survey" and "Employment and Unemployment in the CR as Measured by the Labour Force Sample Survey" series produced by the Czech Statistical Office. For the years prior to 1990, no unemployment measures were available, and for the years 1990–1992, only the nation-wide unemployment rate was available. The very concept of unemployment was unknown under the centrally-planned economy, so I impute unemployment among men aged 20–29 to be zero in all regions for the years prior to 1990. For the years 1990–1992, I take the assumption that the ratio of the region-level unemployment rate among men aged 20–29 and the nation-wide unemployment rate was the same as in 1993, and impute the values accordingly. The same procedure was adopted for the wage data, where the industry-region observations on average wage are available since 1993 while for the years prior to 1993, only the region-wide average wage is available. Moreover, wage data were available for 1980, 1985, and 1990, but only since then at annual intervals; hence, the missing years during the 1980's were filled in by linear extrapolation. All wage variables were deflated to real 1989 Czech *koruna*.

the pre-1989 levels.

Figure 2.2 shows the evolution of the empirical probability of charge. It substantially decreased in the first years of democracy for all crimes except murder. While on average 79% of robbers were brought to court before 1989, only 54% were afterwards. This decline in police productivity is equally pronounced for thefts, where the probability of charge declined from 33% to 19%. The probability of charge rebounded in the mid-1990's almost to the pre-1989 levels for all crimes except robbery.

Figure 2.3 shows the evolution of the empirical probability of being convicted conditional on being charged. Under the communist judiciary, people charged with crime faced near-certainty of being found guilty – specifically, 96%, 77%, and 83% for murder, robbery, and theft, respectively. After the revolution, these probabilities dropped to 72, 61, and 55 percent, and then rebounded almost to the pre-1989 levels for all crimes except thefts.

Finally, Figure 2.4 demonstrates the courts' proclivity to use prison as a form of punishment. The democratic reforms initiated a gradual decline in the use of prisons (with the obvious exception of murders). For example, 59% of thieves were sentenced to prison in 1988, while only 30% were in 2000.

2.4 Estimates

2.4.1 Static framework

The starting point for estimating the relationship between deterrence and crime is a conventional fixed effects specification:

$$\log Y_{ijt} = \beta_i^A \log P_{ijt}^A + \beta_i^C \log P_{ijt}^C + \beta_i^F \log F_{ijt} + \beta_i^X X_{jt} + \lambda_{ij} + \lambda_{it} + \epsilon_{ijt} \qquad (2.1)$$

The subscripts i, j and t denote the crime category, region, and year, Y is the crime rate, and X is a vector of socioeconomic variables. λ_{ij} and λ_{it} are region and year fixed effects. The year fixed effects for 1989 are normalized to zero; thus the fixed effects for other years have the interpretation of an average percentage change in crime rates compared to 1989 that is unexplained by the observables.

It is likely that the error terms ϵ_{ijt} are correlated across offenses within a region-year, and are also serially correlated within region-offense. To account for the first correlation, equation 2.1 is estimated as a system of seemingly unrelated regressions. To account for the serial correlation, one would ideally cluster by region when computing the standard errors. The conventional clustering corrections have, however, poor properties when the number of clusters is small. I follow the recommendation in Cameron, Gelbach and Miller (2008) and Angrist and Pischke (2008, pp.293–322) and estimate the standard errors by block bootstrapping. In block bootstrapping the entire clustering units (regions) are being re-sampled instead of individual observations.

The estimates are presented in Table 2.3. All coefficients on the probabilities of charge and conviction have the expected negative sign. Also, for all crime categories, the coefficient on at least one of the probabilities is statistically significant, and both of them are statistically significant for robbery, theft, rape and injury.[13]

There are potentially two specification issues with the basic SUR framework. One, the SUR estimates of the coefficients on P^A would be biased towards -1 if the number of offenses is measured with error, which is likely due to underreporting.[14] Second, measuring P^A_{ijt}, P^C_{ijt}, and F_{ijt} by their contemporaneous values implicitly assumes that offenders have rational expectations about deterrence. However, several studies documented that individuals have highly different perceptions about the probability of punishment and they base them mostly on their own and their peers' past experiences.[15] I address both of these issues by replacing P^A_{ijt}, P^C_{ijt}, and F_{ijt} with their one-year lags. Given the turbulent social changes of the early 1990's, it may be more plausible to assume that offenders behave "as if" they had adaptive

[13]The statistic for the Breusch-Pagan test for independent equations is 69.9, therefore we reject the hypothesis of no correlation of error terms across equations.

[14]The degree of underreporting can be inferred from the International Crime Victimization Survey conducted in the city of Prague in 2000. According to the survey, 96% of car thefts, 73% of bicycle thefts, 68% of burglaries, 46% of robberies and 41% of small thefts of personal property were reported to police. Since there were no victimization surveys in the Czech Republic prior to 1989, one is left to speculate about how the degree of underreporting changed under democracy. For example, if people report a theft because reporting may increase the chances of getting the stolen object back, the incentive to report weakens when the probability of arrest and conviction falls.

[15]Sah (1991) summarizes surveys on this topic. Lochner (2007) and Rincke and Traxler (2011) provide recent empirical evidence.

expectations, i.e. they base their decision to commit crime on deterrence observed last year. This specification also removes the division bias as the lagged value of P^A does not contain the contemporaneous number of offenses in the denominator.

The results are reported in Table 2.4. Compared to the previous specification, the estimates of β_i^A and β_i^C are smaller in absolute values for all crime categories. The fact that the estimate of β_i^A is smaller could be explained by the removal of the division bias. The fact that the estimates of β_i^C are also smaller opens up a possible explanation that the offenders do in fact have rational expectations and therefore the number of offenses is correlated more strongly with the current rather than the lagged level of deterrence. On the other hand, the estimates of the deterrent effect of expected punishment have the expected negative sign in all but one crime category, they are larger in absolute value in the specification with the lagged values and they are statistically significant for theft, failure to support, and injury.[16]

The estimated deterrence effects have similar magnitude to those found in studies using U.S. state-level or county-level data, at least for robbery and theft. The estimated elasticities of the crime rate with respect to the probability of charge are -0.25 (robbery) and -0.51 (theft). The elasticities with respect to the conditional probability of conviction are -0.15 (robbery) and -0.14 (theft). Somewhat surprisingly, the elasticities with respect to the severity of punishment have higher magnitude and/or higher statistical significance for several crimes, namely theft, failure to support, and injury.[17] For comparison, Eide (2000) reports that the median estimate (out of 118 studies surveyed) of the elasticity of crime rate with respect to various measures of the probability of punishment was -0.7. The study that is probably closest to mine in terms of the choice of explanatory variables and estimation techniques (Cornwell and Trumbull (1994)) finds elasticities of -0.36 (with respect to the probability of arrest) and -0.28 (with respect to the probability of conviction).

[16]I also attempted to eliminate the division bias by instrumenting the probability of being charged with the the ratio of the number of defendants charged to the number of defendants investigated by the police (i.e., persons whom the police identifies as suspects and who would later be charged provided the case against them is strong enough). The instrument is obviously correlated with the regressor since they have a common denominator. If the probability that a person already investigated for a crime is eventually charged is uncorrelated with the measurement error in the number of offenses, $P^{A|I}$ is a valid instrument. Instrumenting for P^A does reduce the estimates of β^A (from -0.70 to -0.41 for robberies, from from -0.66 to -0.54 for theft, from -0.38 to -0.2 for failure to support etc.), with little effect on the estimates of β^C. (Detailed IV results are available upon request.)

[17]This contradicts Ehrlich's (1973) theoretical ordering of elasticities.

My estimated deterrence effects are larger for robbery, theft, and failure to support than for murder, rape, and injury, which is also in line with the earlier deterrence literature.

2.4.2 Discussion

The fall of communism brought about countless social and economic changes that undoubtedly caused an increase in crime (see the discussion in Section 1). The challenge for my estimates is whether they indeed reflect a causal relationship from deterrence to crime or whether they are entirely driven by unobserved shocks that were correlated with deterrence and also caused an increase in crime. Below I present several arguments and specification checks that, considered together, "by the preponderance of the evidence" support the claim that the estimates indeed capture genuine deterrence effects.

To begin with, the estimates are not solely driven by the fall in deterrence in the first transition year, but also by its rebound (although not to the pre-democracy levels) by the mid-1990's. At that time, the crime rates dropped somewhat from their peaks, and the unobserved shocks were presumably evolving less rapidly than in the early 1990's. Also, the estimates are already conservative; the year fixed effects capture unobservable shocks to crime that were common to all regions. The year fixed effects for 1990 are indeed very large for robbery and theft (1.39 and 1.62, respectively). Hence, a very large part of the discontinuous jump in robberies and thefts is attributed to factors other than deterrence.

With the region and year fixed effects included, the coefficients are identified out of between-region variation in changes in deterrence. The identifying assumption behind equation 2.1 is that the changes in deterrence were uncorrelated with the unobservable shocks to crime rates. The estimates would be biased downward if the regions experiencing the largest declines in deterrence also experienced the largest unobservable shocks to crime rates. To address the concern, I run simple regressions explaining the change in deterrence measures during the post-1989 period as a function of the change in either the crime rates or deterrence during the 5 years preceding 1989. If the post-1989 changes were systematically related to pre-1989 changes, one would be worried that deterrence was falling primarily in the regions that were already destined to experience an increase in crime. However, no clear pattern emerges. The partial correlations are significant only for robbery; for other crimes they are insignificant and with varying signs.

I also re-estimated the model in Table 2.4 with the year of the largest shock – 1990 – excluded. The estimated deterrence effects are actually slightly greater in magnitude.

Additional evidence that the estimates indeed capture deterrence – at least for robbery and theft – can be inferred from the evolution of the estimated year fixed effects. They are plotted in Figure 2.5 for the SUR specification with lagged deterrence variables. For robbery and theft, the year fixed effects jump up sharply in 1990 and then do not change significantly until the late 1990's. However, the robbery and theft rates were substantially higher in all years after 1990 than in 1990. My interpretation is that unobservable factors associated with the sudden shift to democracy did cause an abrupt increase in robberies and thefts but they fully materialized already in 1990 while the subsequent growth can be explained by changes in deterrence and socioeconomic variables.

The last check of the lack of correlation between changes in deterrence and unobservables is based on the structural break in the relationship between deterrence and crime rates. I estimate the same models as in Tables 2.3 and 2.4 with each variable also interacted with a democracy dummy (post-1989 years) and test the null hypothesis that the coefficients on the interaction variables are jointly equal to zero.[18] The data may exhibit the structural break for two reasons. It could be a genuine structural break brought about by the regime change. Alternatively, if the changes in unobservables were correlated with the changes in deterrence, they would appear as negative coefficients on the interaction terms even in the absence of a genuine structural break. Table 2.5 shows the results for the specification with contemporaneous deterrence variables.[19] There is very little evidence suggesting a structural break in deterrence. The null hypothesis is rejected only for failure to support and rape, although in the case of rape there is a rather strange structural

[18]The F-test is performed separately for the deterrence and social-economic variables. There is no interaction on the unemployment variable because measured unemployment was zero until 1989.

[19]The results for the specification with lagged deterrence variables are not reported here because they are very similar and show even weaker evidence of a structural break. They are available upon request.

break when the elasticity of crime rate with respect to P^A is positive and significant (+0.375), and democracy significantly reduces this elasticity by 0.616. For all the other crimes, none of the interaction terms on deterrence variables are statistically significant, individually or jointly. Among the social-economic variables, the results show a significant structural break only in the average wage for failure to support (with the expected sign, higher real wages associated with a lower rate of failure to support).

2.4.3 Dynamic framework

The dynamic version of the model incorporates the notion that "more crime breeds more crime" by endogenizing the probability of punishment and the size of the police force. Holding the enforcement resources fixed, an exogenous increase in the number of offenses reduces the probability of punishment. Observing this, offenders will choose to commit even more crimes this year. The enforcement resources will adjust too, since an increase in crime this year will trigger the public's demand for higher enforcement resources next year. This process can be described by three equations:

$$\begin{aligned} \log Y_{ijt} &= \beta_i^A \log P_{ijt-1}^A + \beta_i^C \log P_{ijt-1}^C + \beta_i^F \log F_{ijt-1} + \beta_i^X \log X_{jt} + && (2.2) \\ &\quad + \lambda_{ij}^Y + \lambda_{it}^Y + \epsilon_{ijt}^Y \ , \ \forall i \\ \log P_{ijt-1}^A &= \gamma_i^E \log E_{jt-1} + \gamma_i^Y \log Y_{ijt-1} + \gamma_i^Z \log Z_{jt-1}^P + && (2.3) \\ &\quad + \lambda_{ij}^P + \lambda_{it-1}^P + \epsilon_{ijt-1}^P \ , \ \forall i \\ \log E_{jt-1} &= \sum \delta_i^Y \log Y_{ijt-2} + \delta^E \log E_{jt-2} + \delta^Z \log Z_{jt-1}^E + && (2.4) \\ &\quad + \lambda_{ij}^E + \lambda_{it-1}^E + \epsilon_{ijt-1}^E \end{aligned}$$

Equation 2.2 is the supply-of-offenses equation analogous to equation 2.1 with lagged deterrence variables. Equation 2.3 endogenizes P^A and can be interpreted as the production function of police. The output of police is the probability of charge and the inputs are enforcement resources E_j (measured by the number of police officers per 100,000 inhabitants), number of offenses Y_{ij}, and socioeconomic variables Z^P.

The predicted sign of γ^E is positive and of γ^Y negative.[20] Last, the size of the police force is endogenized in the demand for police equation (2.4) with the lagged crime rates Y_{ijt-2}, lagged size of the police force E_{jt-2}, and socioeconomic variables Z^E_{jt-1} as the explanatory variables.[21]

The system of 13 equations (2.2)-(2.4) is estimated by three-stage least squares and the results are shown in Table 2.6.[22] Even though the probability of charge is endogenized, the model shows strong deterrence effects. The coefficients on the probability of charge for robbery, theft, failure to support, and intentional injury are greater in absolute value than the static SUR estimates with lagged deterrence variables (Table 2.4). The expected length of a prison sentence has a statistically significant effect on failure to support and intentional injury.[23]

2.5 Conclusions

The collapse of communism in the Czech Republic and the very sharp decline in deterrence that immediately followed provided an opportunity to gain new evidence on the old question: whether deterrence cuts crime. I found statistically and economically significant deterrence effects for robbery and theft, somewhat weaker effects for intentional injury and failures to support but insignificant effects for murder and rape. The results are generally robust to alternative specifications. I presented evidence supporting the claim that the results reflect a causal relationship between

[20] I do not model the production function of courts since I do not have appropriate measures of the courts' inputs. Therefore P^C_{ijt} is treated as exogenous. The effective supply of offenders was included as the Z^P variable in the police output equation, with the justification that the police has to spread its effort over a larger group in order to identify a particular offender as the number of potential offenders increases.

[21] The specification for the demand for police equation goes back to Ehrlich (1973). The socioeconomic variable included in the demand for police equation is the average real wage in the public sector - fewer policemen will be demanded if the government has to pay them a higher wage.

[22] The right-hand side of the supply of police equation contains a lagged dependent variable and hence the strict exogeneity assumption is violated. The consistent estimation method would first remove the fixed effects by first differencing and then use lagged first differences in lagged right-hand side variables as instruments (Wooldridge (2002), pp. 299–307). I did try this approach; however, it produced implausible estimates (negative estimate of δ^E, very large standard errors), presumably because of a rather small sample size.

[23] The estimated parameters of the police productivity equations are also plausible. A one-percent increase in the number of police officers increases the probability of charge by 0.64 percent for robberies and .38 percent for thefts. As expected, the number of offenses negatively affects the probability of charge for all crimes except murders. Finally, the demand for police equation shows a strong persistence in the size of police force but no significant adjustment of police to the previous year's crime rates.

deterrence and crime rather than unobservable factors through which democracy led to higher crime, even though such factors were empirically important.

With these results at hand, a natural question to ask is: What would the crime rates be if the democratic government somehow managed to keep deterrence at the communist levels? I use the coefficients from Table 2.4 to predict the crime rates under the assumption that P^A_{ijt}, P^C_{ijt}, and F_{ijt} would stay at the same level as in 1989 for all the following years while the socioeconomic variables and the year fixed effects would evolve as they did. The predicted and actual crime rates aggregated at the national level are plotted in Figure 2.6. Since the estimates do not show a strong deterrence effect on murders and rapes, it is not surprising that stronger deterrence would not change the number of these offenses. The model predicts, however, that the number of robberies, thefts, and intentional injuries would be substantially lower if deterrence did not fall. For example, the robbery rate was 16.6 in 1989 and 48.4 in 2000. Had deterrence stayed the same, the model predicts that the robbery rate would have been only 40.5.

Lin (2007) calibrates the extent in which the differences in crime rates between democracies and non-democracies can be attributed to weaker deterrence. He finds that weaker deterrence is responsible for as much as 40–50% of the democracy's contribution to higher crime. I perform an analogous exercise with the estimated deterrence effects. Table 2.7 shows how much of the change in crime rates between 1989 and 2000 is accounted for by the change in deterrence. The estimates imply that deterrence accounts for 25% of the increase in robberies, 52% of the increase in thefts and 65% of the increase in intentional injuries. Overall, these results provide additional evidence that democracies have different patterns of crime, that deterrence explains a large part of the difference for economic crimes such as thefts and robberies, and that deterrence does not explain much of the differences for serious violent crimes such as murders and rapes.

2.6 Figures and tables

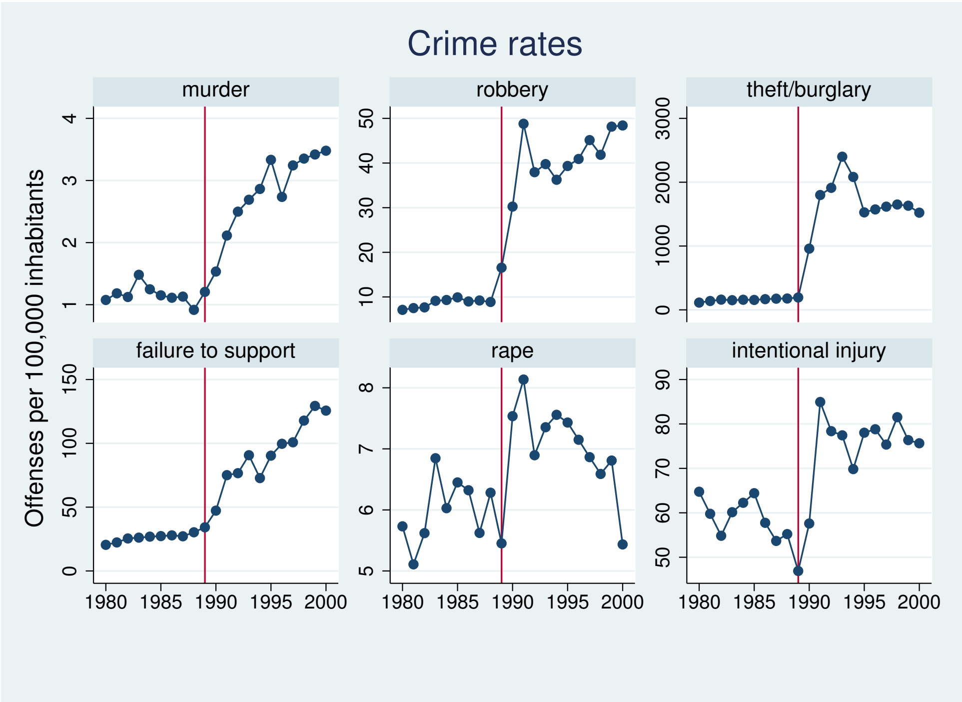

Figure 2.1: Crime rates

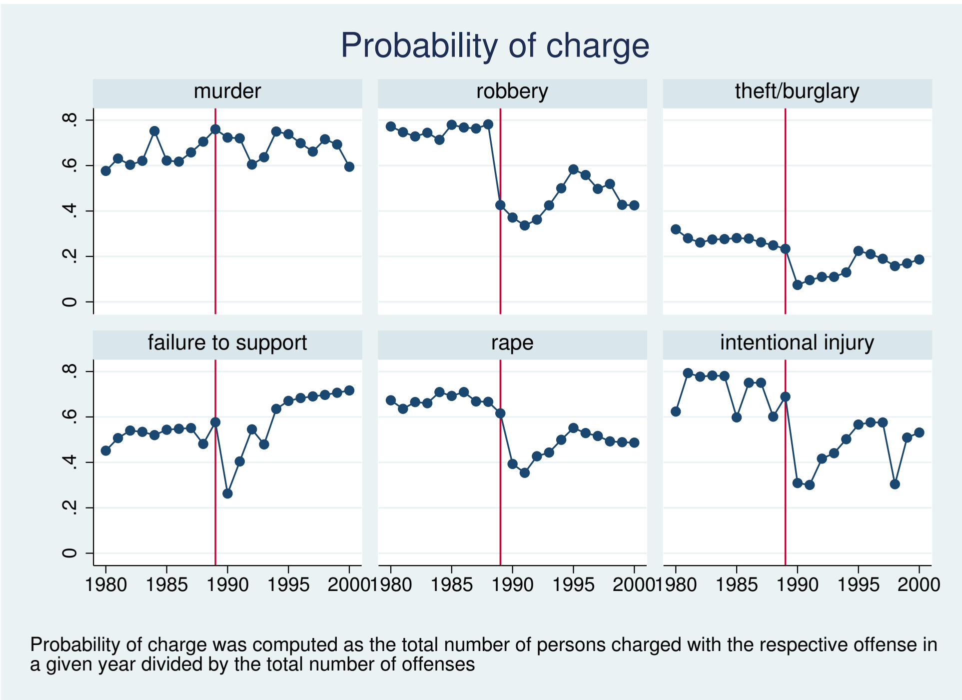

Figure 2.2: Probability of being charged

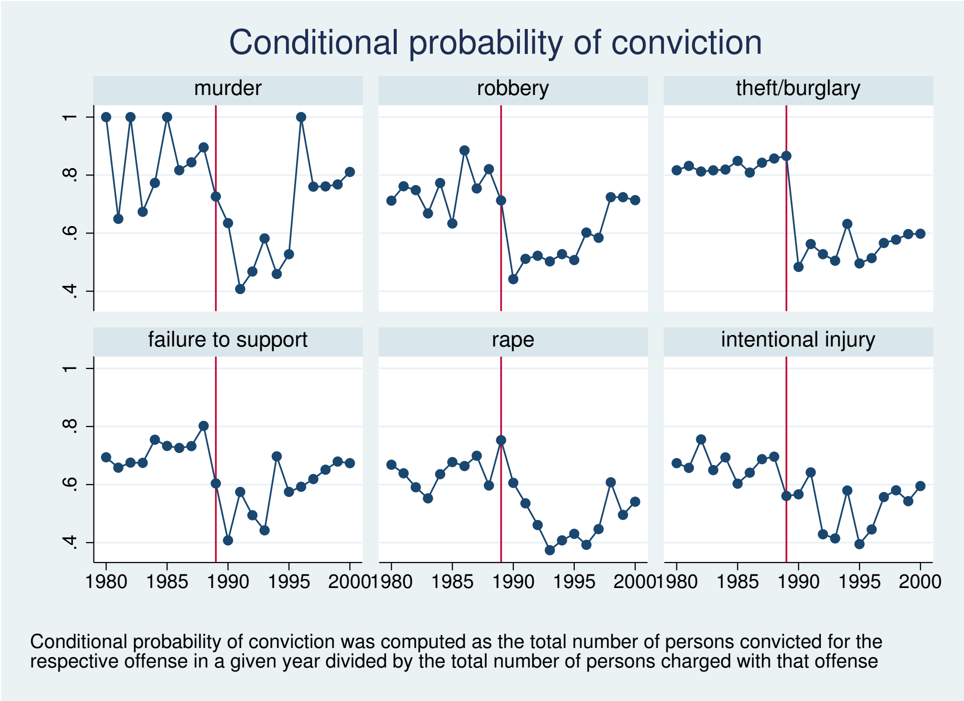

Figure 2.3: Conditional probability of conviction

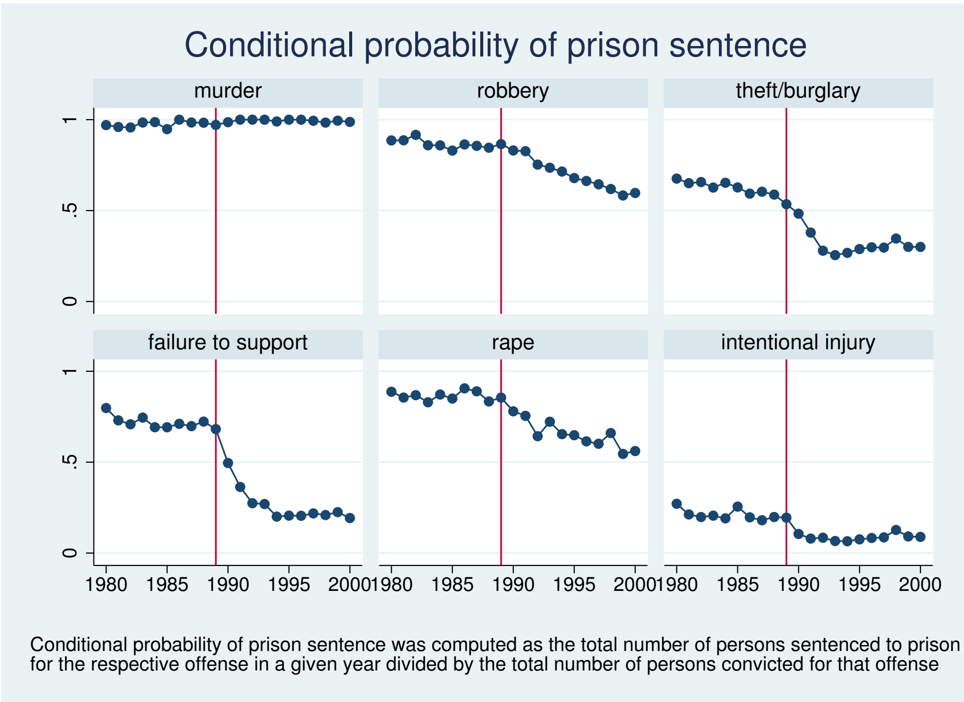

Figure 2.4: Conditional probability of prison sentence

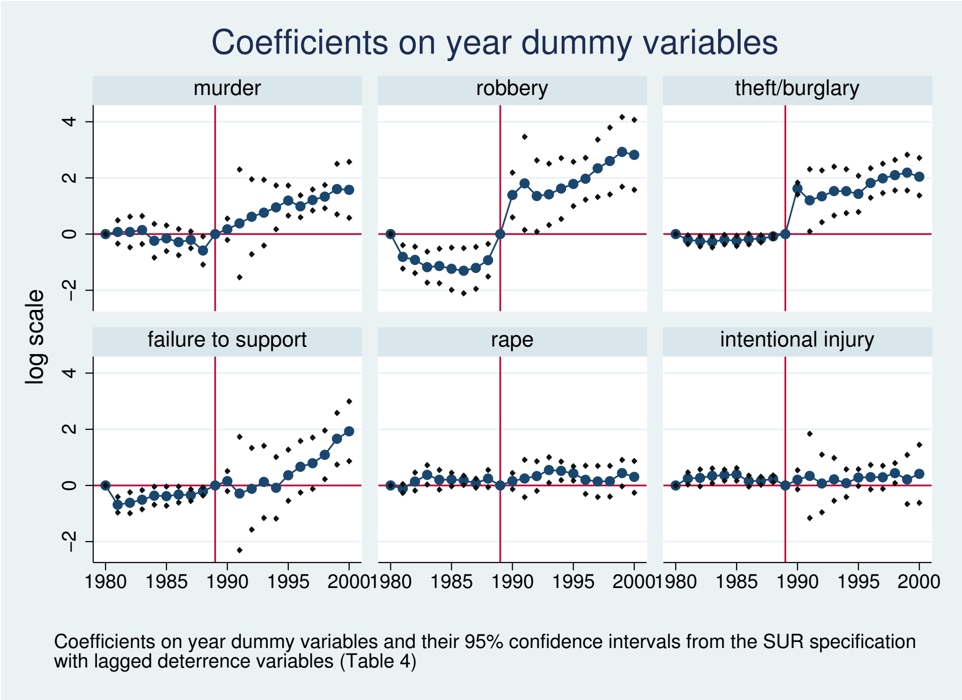

Figure 2.5: Coefficients on year dummy variables

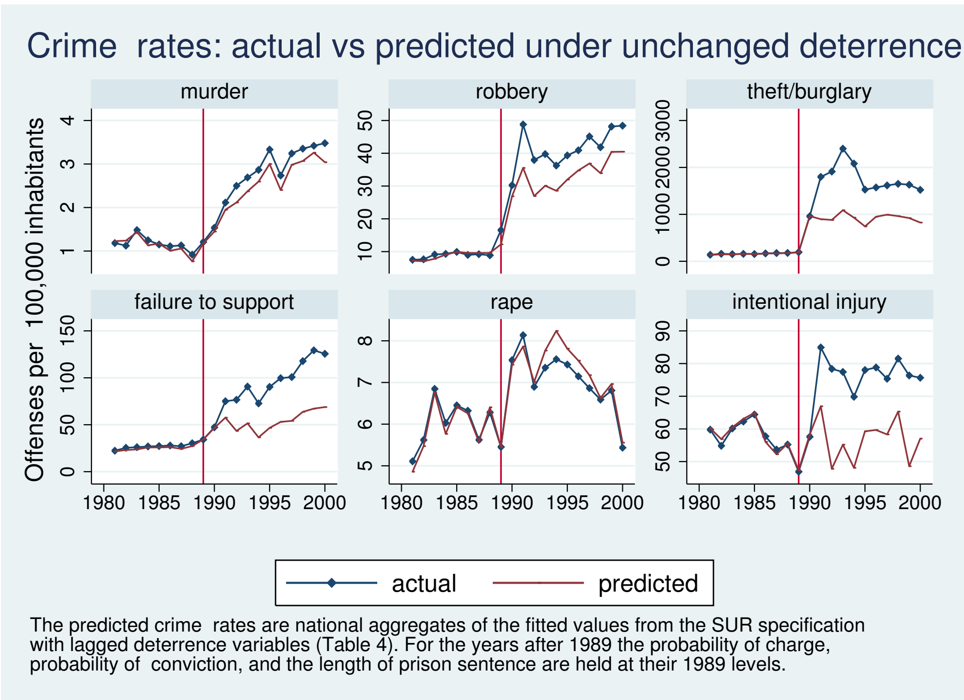

Figure 2.6: Crime rates: actual versus predicted under unchanged deterrence

Table 2.1: International Comparison: Recorded rates of criminal acts per 100,000 inhabitants (averages over 2 years)

	Hungary			Poland			East Germany*		
Year	Intentional homicide	Robbery	Theft	Intentional homicide	Robbery	Theft	Intentional homicide	Robbery	Theft
1986–87	2.1	15	929	1.5	20	306	0.7	5	
1988–89	1.9	16	1 199	1.5	21	340			
1990–91	2.4	33	647	2.4	44	552	1.0	35	529
1992–93	2.9	30	2 415	2.9	50	498	4.1	123	5 854
1994–95	2.9	25	2 217	3.1	65	588	5.5	141	6 400
1996–97	2.7	28	2 751	3.0	73	407	4.7	144	5 956
1998–99	2.6	30	2 715	5.7	102	417			

Source: UN Surveys on Crime Trends and the Operation of Criminal Justice Systems, Polizeische Kriminalstatistik Deutschland
*Notes: Figures for Federal Republic of Germany starting from 1990 onwards and all figures for East Germany were obtained from Polizeische Kriminalstatistik Deutschland. Population data come from World Development Indicators produced by the World Bank, for GDR and FRG from www.populstat.info. Because I was not able to obtain data for all years preceding 1990 for all three countries, some numbers reported in the table represent either 2-year averages or observations for only one of the two years when data for both years were not available.

Table 2.2: Summary statistics

		murder		robbery		theft/burglary		failure to support		rape		intentional injury	
		mean	std. dev.	mean	std. dev.	mean	std. dev.	mean	std. dev.	mean	std. dev.	mean	std. dev.
Crime rate	communism	1.20	0.60	9.2	8.9	161	126	24.3	13.3	5.9	2.0	55.8	21.0
(offenses per	democracy	2.87	1.38	41.8	32.3	1713	1061	91.6	35.2	7.2	2.1	73.0	30.6
100,000 inhab.)	percentage change	148.1	51.7	351.4	95.5	1054.3	157.2	351.3	181.6	22.6	11.3	30.2	17.3
Probability of	communism	0.63	0.18	0.79	0.12	0.33	0.10	0.55	0.10	0.69	0.11	0.73	0.13
charge	democracy	0.68	0.17	0.54	0.17	0.18	0.08	0.59	0.15	0.48	0.11	0.49	0.15
	percentage change	7.5	9.9	−32.3	12.2	−45.4	5.0	7.7	8.9	−29.9	7.2	−33.2	4.7
Probability of	communism	0.98	0.62	0.77	0.18	0.84	0.07	0.70	0.11	0.67	0.16	0.66	0.10
conviction	democracy	0.71	0.38	0.59	0.15	0.55	0.09	0.57	0.12	0.50	0.19	0.51	0.11
	percentage change	−25.0	14.7	−22.4	9.9	−34.1	6.0	−17.7	7.7	−23.7	13.5	−22.0	5.0
Probability of	communism	0.98	0.05	0.87	0.08	0.62	0.06	0.69	0.08	0.86	0.08	0.21	0.05
prison sentence	democracy	0.99	0.03	0.70	0.10	0.31	0.08	0.24	0.10	0.65	0.16	0.09	0.04
	percentage change	1.5	0.9	−19.4	4.3	−49.3	8.1	−64.7	5.3	−24.5	5.6	−58.4	9.0
Length of	communism	132	33	50.3	8.8	20.1	3.3	12.4	1.3	51.2	7.6	9.2	1.9
prison sentence	democracy	127	25	48.9	5.2	15.0	2.6	9.5	1.3	56.2	12.2	12.4	4.4
(months)	percentage change	−2.7	9.7	−2.2	7.9	−24.4	10.0	−23.1	7.8	10.4	13.8	37.5	21.2

Notes: Observations for communism and democracy are averages over 1980-1989 and 1990-2000, respectively.
Percentage change denotes the average (across regions) percentage difference between the communism and democracy averages.

Table 2.3: Static seemingly unrelated regressions

	murder	robbery	theft	failure to support	rape	injury
Probability of charge	−0.281	−0.700***	−0.660***	−0.378***	−0.184*	−0.469***
	(0.241)	(0.154)	(0.050)	(0.135)	(0.107)	(0.112)
Probability of conviction	−0.351***	−0.239***	−0.355***	−0.124	−0.135***	−0.149***
	(0.090)	(0.075)	(0.057)	(0.128)	(0.040)	(0.054)
Expected punishment	0.120	0.048	−0.129	−0.392**	−0.009	−0.090**
	(0.153)	(0.134)	(0.099)	(0.190)	(0.049)	(0.039)
Effective supply of offenders	2.396	6.499**	4.247***	−1.656	−0.517	−0.121
	(2.849)	(2.798)	(1.416)	(2.664)	(1.566)	(3.033)
Average wage	−0.394	−0.507	−0.894	−2.135	−0.088	0.178
	(2.144)	(1.995)	(1.273)	(2.556)	(0.820)	(1.839)
Inequality	−0.034	−1.521	−1.433**	0.500	−0.035	0.221
	(1.695)	(0.982)	(0.648)	(1.908)	(0.744)	(1.149)
Unemployment men 20–29	−0.044	−0.003	−0.010	−0.083**	−0.036**	−0.010
	(0.032)	(0.023)	(0.021)	(0.037)	(0.016)	(0.036)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	167	167	167	167	167	167
"R-squared"	0.83	0.97	0.99	0.97	0.87	0.92

Absolute values of block-cootstrapped standard errors in parentheses.
All variables except unemployment are in logs.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2.4: Static SUR specification with lagged deterrence variables

	murder	robbery	theft	failure to support	rape	injury
Lag probability of charge	−0.100	−0.250***	−0.509***	−0.314	0.027	−0.331*
	(0.103)	(0.058)	(0.080)	(0.219)	(0.102)	(0.173)
Lag probability of conviction	−0.068	−0.147**	−0.140	−0.068	0.054	−0.116
	(0.094)	(0.073)	(0.107)	(0.094)	(0.059)	(0.096)
Lag punishment	−0.086	−0.093	−0.213*	−0.407**	0.022	−0.084***
	(0.153)	(0.228)	(0.129)	(0.170)	(0.034)	(0.031)
Effective supply of offenders	2.249	8.863**	3.483**	−2.538	−3.076	−0.0885
	(2.946)	(3.490)	(1.550)	(3.065)	(3.105)	(3.497)
Average wage	−0.626	−0.366	−1.153	−2.372	−0.086	−0.068
	(2.702)	(1.727)	(1.420)	(2.823)	(0.738)	(2.056)
Inequality	0.0188	−1.198	−1.390*	0.216	−0.831	0.462
	(1.736)	(1.017)	(0.760)	(1.697)	(1.208)	(1.060)
Unemployment	−0.056	−0.022	−0.029	−0.083**	−0.041*	−0.017
	(0.040)	(0.033)	(0.018)	(0.036)	(0.021)	(0.034)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	159	159	159	159	159	159
"R-squared"	0.82	0.96	0.99	0.97	0.84	0.92

Absolute values of block-bootstrapped standard errors in parentheses.
All variables except unemployment are in logs.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2.5: Test for structural break, SUR specification

	murder	robbery	theft	failure to support	rape	injury
Probability of charge	−0.262	−0.869**	−0.600***	−0.468***	0.375*	−0.337
	(0.344)	(0.387)	(0.089)	(0.182)	(0.211)	(0.319)
interacted with democracy	0.0188	0.387	−0.077	0.146	−0.616***	0.010
	(0.287)	(0.499)	(0.151)	(0.282)	(0.194)	(0.406)
Probability of conviction	−0.389***	−0.219	−0.245**	−0.135	−0.094	−0.142
	(0.115)	(0.163)	(0.116)	(0.143)	(0.060)	(0.198)
interacted with democracy	0.061	0.006	−0.053	0.099	−0.040	0.093
	(0.099)	(0.170)	(0.151)	(0.185)	(0.079)	(0.257)
Expected punishment	0.286	0.145	−0.120	0.0379	−0.203	−0.0502
	(0.211)	(0.263)	(0.117)	(0.195)	(0.163)	(0.096)
interacted with democracy	−0.389	−0.218	0.0532	−0.222	0.208	−0.026
	(0.246)	(0.276)	(0.157)	(0.195)	(0.175)	(0.116)
Effective supply of offenders	9.323**	5.666	3.061	−3.613	−0.766	1.702
	(4.700)	(4.424)	(1.958)	(3.649)	(2.134)	(5.594)
interacted with democracy	−7.089	−2.014	1.274	2.210	−0.525	−1.893
	(12.020)	(6.007)	(2.520)	(4.691)	(5.696)	(7.911)
Average wage	−0.437	−0.099	2.462	5.247	1.145	−1.380
	(3.496)	(5.779)	(2.072)	(3.310)	(2.963)	(3.046)
interacted with democracy	−0.552	0.105	−2.448	−5.512*	−1.519	0.986
	(3.360)	(5.097)	(1.891)	(3.159)	(2.466)	(3.079)
Inequality measure	−0.169	−1.930	−2.023*	0.255	−1.191	0.172
	(3.830)	(5.237)	(1.182)	(3.924)	(2.494)	(5.820)
interacted with democracy	0.274	0.398	0.508	−0.099	0.969	0.105
	(3.929)	(4.955)	(1.037)	(3.744)	(2.290)	(5.691)
Unemployment	−0.036	0.001	0.014	−0.039	−0.028	−0.016
	(0.030)	(0.034)	(0.017)	(0.024)	(0.019)	(0.025)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	167	167	167	167	167	167
R-squared	0.83	0.98	0.99	0.98	0.89	0.93
Test for structural break:						
Deterrence variables $\chi^2(3)$	2.65	0.90	0.55	1.31	15.65	0.21
$P > \chi^2$	0.4487	0.8249	0.9074	0.7268	0.0013	0.9761
Socio-econ variables $\chi^2(4)$	2.59	0.13	5.16	9.12	4.82	0.63
$P > \chi^2$	0.6282	0.9979	0.2713	0.0581	0.3065	0.9592

Absolute value of standard errors in parentheses.
All variables except unemployment are in logs.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2.6: Dynamic 3SLS specification with lagged deterrence variables

	murder		robbery		theft/burglary		failure to support		rape		intentional injury		police
Dependent variable:	crime rate	lag prob. of charge	crime rate	lag prob. of charge	crime rate	lag prob. of charge	crime rate	lag prob. of charge	crime rate	lag prob. of charge	crime rate	lag prob. of charge	lag police per 100.000 inhabitants
Lag probability	0.079		−0.399***		−0.663***		−1.066***		0.316*		−0.537**		
of charge	(0.303)		(0.134)		(0.126)		(0.403)		(0.182)		(0.260)		
Lag probability	0.027		−0.135*		−0.117		−0.143		0.069		−0.100		
of conviction	(0.086)		(0.070)		(0.118)		(0.114)		(0.052)		(0.101)		
Lag length of	−0.056		−0.117		−0.205		−0.307*		0.021		−0.070**		
punishment	(0.159)		(0.249)		(0.132)		(0.158)		(0.0387)		(0.0281)		
Effective supply	2.398	−0.906	6.759	−3.915	3.064	2.730	−1.885	−0.263	−4.326	1.529	−0.370	−1.967	
of offenders	(3.319)	(4.794)	(4.385)	(2.652)	(1.932)	(3.405)	(3.017)	(1.225)	(3.468)	(2.766)	(3.566)	(1.716)	
Average wage	−0.620		−0.254		−0.887		−1.605		0.055		−0.019		
	(1.980)		(1.607)		(1.418)		(2.820)		(0.801)		(2.112)		
Inequality	0.177		−1.502		−1.221*		−0.057		−0.672		0.704		
	(1.719)		(0.974)		(0.686)		(1.638)		(1.059)		(0.932)		
Unemployment	−0.0578		−0.022		−0.025		−0.055		−0.041**		−0.014		
men aged 20–29	(0.039)		(0.033)		(0.0182)		(0.038)		(0.021)		(0.031)		
Lag police		−0.110		0.644*		0.384		0.0445		0.148		0.067	
		(0.652)		(0.384)		(0.452)		(0.486)		(0.315)		(0.426)	
Lag crime rate		0.095		−0.364***		−0.637***		−0.293***		−0.191*		−0.215**	
		(0.104)		(0.104)		(0.136)		(0.050)		(0.112)		(0.089)	
2lag police													0.874***
													(0.068)
2lag crime rate -													−0.005
- murder													(0.010)
2lag crime rate -													0.030
- robbery													(0.022)
2lag crime rate -													0.009
- theft													(0.023)
Wage in public													−0.121***
sector													(0.042)

Absolute values of block-bootstrapped standard errors in parentheses.
All variables except unemployment are in logs.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2.7: Contribution of weaker deterrence to the post-1989 growth in crime

		Actual crime rate*	Predicted crime rate**	Fraction of the change accounted for by weaker deterrence
Murder	1989	1.2		
	2000	3.5	3.0	22%
Robbery	1989	16.6		
	2000	48.4	40.5	25%
Theft	1989	193.9		
	2000	1522.7	825.6	52%
Failure to support	1989	34.2		
	2000	125.6	68.8	62%
Rape	1989	5.5		
	2000	5.4	5.6	N/A
Intentional injury	1989	46.9		
	2000	75.7	57.0	65%

*Nation-wide number of offenses per 100,000 inhabitants.
**The predicted crime rates are national aggregates of the fitted values from the SUR specification with lagged deterrence variables of Table 4. For the years after 1989, the probability of charge, probability of conviction, and the length of prison sentence are held at their 1989 levels.

Chapter 3

Time to Punishment: The Effects of a Shorter Criminal Procedure on Crime Rates[1]

3.1 Introduction

The canonical model of criminal sanctions (Becker 1968) tacitly assumes that if an offender is apprehended and convicted, the punishment immediately follows the crime. However, criminal procedure takes time. It involves time-consuming and complicated paperwork on behalf of the investigators, prosecutors, and judges. It typically takes weeks or months until the suspect is identified and arrested, evidence is collected, charges are raised, the case is resolved at trial, the sentence is imposed, the defendant possibly appeals and the appellate trial is held.

The length and complexity of the criminal procedure has implications for the behavior of offenders and law enforcement officials. The offender at the time of committing

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the offense discounts the severity of punishment by the length of time between the offense and the actual imposition of the punishment. Punishment imposed shortly after the offense is effectively more severe and should have a greater deterrent effect on crime. This deterrent effect should be enhanced by the fact that offenders tend to discount the future much more heavily than law-abiding citizens[2] (Herrnstein (1983), Wilson and Herrnstein (1985) and Nagin and Pogarsky (2004)). The economic model of crime therefore predicts a causal relationship between speedier criminal procedure and lower crime rates.[3]

Shorter and simpler procedure may also affect the allocation of enforcement resources by the police or prosecutors. If – as is the case in the procedural reform evaluated in this paper – the shorter procedure applies only to less serious crimes, it generates both endowment and substitution effects. It reduces the time cost of handling less serious cases, and the enforcement officials thus have more time to pursue all cases. However, it also reduces the relative price of pursuing less serious cases. The enforcement officers have an incentive to substitute away from more serious cases and rather pursue less time-intensive but also less serious cases.

Two papers empirically tested for the deterrent effect of shorter criminal procedure. Pellegrina (2008) exploits cross-sectional variation in the length of criminal trials across provinces in Italy to detect a positive and statistically significant relationship between the length of trials and the rate of thefts, robberies, fraud, and racketeering. Soares and Sviatschi (2010) find a similar relationship between the rate at which courts process the criminal caseload (which is indirectly linked to the length of the procedure) and crime rates, in a panel of cantons in Costa Rica. The reallocation of enforcement effort in response to changes in the price of enforcement was investigated

[2]The deterrent effect should exist under both exponential and hyperbolic discounting but its magnitude should depend on the form of discounting. A given reduction in the time from offense to punishment increases the percieved punishment more for an exponential discounter than a hyperbolic discounter if the punishment is still imposed in the relatively distant future. The same reduction increases the perceived punishment more for a hyperbolic discounter if the punishment is imposed very shortly after the offense following the reduction.

[3]The role of discounting in the deterrent effect of punishment has been modeled by Davis (1988) and Lee and McCrary (2005). Listokin (2007) discusses its implications for the design of optimal punishment. A shorter and simpler procedure may also affect crime through conventional deterrence and incapacitation effects because it increases the probability of punishment: The quality of the evidence, once collected, deteriorates over time. A longer procedure makes it more likely that the defendant will turn fugitive. Complex procedure with many procedural steps increases the probability that the defendant will exploit a procedural loophole or that witnesses will modify their initial testimonies.

by Benson, Rasmussen and Sollars (1995) and Baicker and Jacobson (2007). They find that when local police departments in the U.S. were provided with the authority to keep the revenue from assets forfeited in drug enforcement, they shifted their enforcement resources towards drug crimes and away from non-drug crimes.

Estimating the effects of case duration on crime rates is faced with a simultaneity problem: higher crime rates increase the caseload for the police and courts, who then take more time to process the cases. An exogenous variation in case durations is needed to identify the causal effect on crime rates. The Czech criminal procedure reform, adopted in 2002, provides a quasi-natural experiment. It prescribed that certain less serious crimes may be prosecuted via a "fast-track" procedure, with fewer procedural steps, substantially less paperwork, and stricter deadlines. Its stated objectives were to reduce case durations, save resources in the enforcement of less serious crimes, and free up resources for the enforcement of serious crimes.[4] After the reform, the average duration of the procedure (from offense to final adjudication) declined by about a third for offenses that were relatively extensively covered by the fast-track procedure.

The share of cases actually prosecuted via the fast-track procedure differed substantially across districts and offenses. The differential adoption was largely caused by bureaucratic inertia rather than by the desire to cut case durations in districts particularly burdened with crime. Most importantly, it was unrelated to pre-adoption trends in crime rates or case durations. However, the share of fast-track cases in a given district is strongly related to the reduction in case duration following the reform.

The identification strategy is then based on a standard instrumental variable design, where the case duration is instrumented by the share of fast-track cases. The dataset is an annual panel of 79 Czech districts and 24 types of offense covering 1999–2008. It contains information on the number of offenses reported to the police, clearance rates, the share of cases prosecuted via the fast-track procedure, and average case durations. The first-stage regressions estimate (offense-by-offense) the log of average case duration as a function of the share of fast-track cases, socio-economic controls, and district and year fixed effects. The second stage regressions estimate the logarithm of the crime rate as a function of the (instrumented) duration,

[4] Ministry of Justice of the Czech Republic (2001).

clearance rate, socio-economic controls, and the district and year fixed effects.

The outcome variable of interest – the officially recorded crime rate – is a joint product of the underlying true crime rate and police discretion in discovering and recording the crime. The deterrent effect of a shorter procedure should reduce the number of recorded crimes. The enforcement reallocation effect should increase the number of recorded crimes, but only to the extent that the police can influence it. Offenses such as thefts or robberies are typically reported to the police by the victims[5] so the reallocation effect should be relatively weak. I expect the estimated effect of shorter duration on victim-reported offenses to be negative (but still underestimate the true deterrent effect). On the other hand, crimes such as drug offenses or driving offenses are discovered almost exclusively through police enforcement efforts. The police have substantial discretion in influencing the recorded number of such crimes. The reallocation effect may even dominate the deterrent effect. If it does, the estimated effect of shorter duration on police-reported offenses would be positive (and still underestimate the true reallocation effect).

The strongest and most robust result is that the reduction in case duration substantially *increased* the number of two types of police-reported offenses associated with driving: driving under the influence, and obstruction of an official order (a criminal offense that is committed by failing to comply with a court order, and is most frequently committed by drivers who continue to drive with a suspended driving license[6]). The estimates are statistically and economically significant. They imply that in the absence of the reform, the number of recorded driving-under-the-influence cases would have been 20–34 percent below its actual level several years after the reform, and the number of recorded obstruction cases would have been 24–44 percent below. I also find that shorter case duration has a negative effect on two victim-reported crimes (burglaries and embezzlements), but this finding is not

[5]The police have only limited discretion in influencing the recorded number of such crimes. They may attempt to persuade the victim to withdraw the initial report if the amount stolen is small or if it is very unlikely that the offender would ever be found. The police may also record the incident reported by the victim but has discretion in determining whether the incident constitutes a criminal offense. Outright cheating with the records does not seem to be an issue: The police has to initiate a criminal procedure for every offense that a victim reports, and each step of the procedure is entered into a computerized system. The aggregate number of crimes is simply the number of procedures in the computer system that were classified as criminal offenses.

[6]Obstruction of an official order is a fairly frequent offense; it had a crime rate of 51 offenses per 100,000 people in 2008. Other violations under this offense include failing to to obey a restraining order or to show up for a prison sentence. (Sec 171 of the Czech Criminal Code).

robust to regression specification.

The results thus provide only limited evidence of a deterrent effect on victim-reported offenses, but they provide very strong evidence of the reallocation effect: as the police officers were provided with a new means of producing measurable results (prosecutions) at low cost, they responded predictably by exploiting those means and pursuing more extensively precisely those offenses that had reduced enforcement costs.

3.2 Institutional background

Prior to the 2002 reform the Czech Criminal Procedure Code prescribed a unified procedure applicable to all crimes. Practitioners generally agreed that the procedure was unnecessarily burdensome, lengthy and expensive for less serious crimes and for crimes where the evidence clearly indicated guilt (Baxa 2001). The reform introduced a so-called fast-track criminal procedure.[7] Only offenses meeting the following three conditions may be prosecuted via the fast-track procedure:

1) The offense falls into the jurisdiction of the district court (i.e. the lowest court level).

2) The maximum punishment for the offense, as set by the Criminal Code, does not exceed three years of imprisonment.

3) The suspect was apprehended either while committing the crime or immediately after, or the evidence revealed in the early stages of the investigation is sufficient to prosecute the suspect, and there is a reasonable chance that the suspect can be brought to trial in two weeks.

The fast-track procedure cut the duration from arrest to conviction mainly by eliminating duplications of procedural steps at subsequent stages of the criminal procedure and by imposing stricter deadlines. Under the conventional procedure, the police, upon identifying the suspect based on the collected evidence, would raise formal charges. From that point on, the police would essentially repeat the collection of evidence (e.g. interrogating witnesses again) while the suspect has broad

[7]The reform was legislated by Act No. 265/2001. The official Czech title of the fast-track procedure is *"zkrácené přípravné řízení"*.

procedural rights (e.g. to read and comment on the testimonies provided by the witnesses). The case would then be bound over to the state attorney, who would review it and submit the charges to the court. The court could hold a preliminary hearing; then, at the trial, the evidence would be re-presented again and assessed by the judge. The deadlines faced by the law enforcers are fairly flexible.[8]

The fast-track procedure applies to cases where the evidence revealed during investigation is indisputable. The police raise the charges, hand the case over to the state attorney, who reviews the case and submits the prosecution to the court. The text of the prosecution is simpler (it contains the description of the case and the proposed punishment, but not the legal justification or the description of the evidence). The trial is also simplified: with the defendant's consent, the judge may declare certain facts of the case indisputable and hence the evidence need not be presented at trial; there are no closing speeches, etc. The deadlines are far stricter: the police have to hand over the case to the prosecutor in two weeks following the date on which the crime was reported. The prosecutor may, upon request, prolong the deadline by ten days at most; if the deadline is missed, the case reverts to the conventional procedure. The risk of reverting the case to the time-consuming conventional procedure gives the law enforcers a strong incentive to meet the deadlines.

The letter of the legislation prescribes that all eligible cases should be prosecuted via the fast-track. In reality, the law enforcement officials make a discretionary decision whether to run an eligible case through the fast-track or the conventional procedure. The decision rests with the district-level state police officer[9], although the prosecutor may reverse that decision. In practice, the two typically discuss each case informally and reversals of the initial police officer's decisions are rare.

The reform was well received by the police and prosecutors. They report its main advantages to be that the fast-track significantly shortened the procedure, reduced the case backlog, and allowed investigative officers to focus on more complicated and serious cases.[10] It allowed police officers at the very local level to handle far

[8]For example, the police are supposed to hand over less serious cases to the prosecutor within 2 months. However, if they fail to meet the deadline, they merely have to justify that to the prosecutor, who sets a new deadline.

[9]Only state police officers can handle criminal cases. Many cities have a city police, but its authority is limited to minor violations punishable by fines (e.g., traffic violations, loitering). When the city police discover a criminal offense, they pass the case to the state police.

[10]Zeman et al. (2008), my own interviews with police officers.

more criminal cases. These officers emphasized their satisfaction at being able to handle criminal cases from the first contact with the crime all the way through the prosecution; under the conventional procedure they would have to pass the case to a higher-level investigative officer without seeing the final result. There has been no serious proposal to reverse the reform; quite the contrary, a new law that came into force in 2009 expanded the range of offenses that can be prosecuted via the fast-track.

Cases prosecuted via the fast-track procedure are indeed resolved much faster than cases resolved through the conventional procedure. Zeman et al. (2008) report that in 2006, the average length of the procedure from arrest to court decision (not including appeals) was 95 days for fast-track cases but 214 days for conventional cases. Within this, the part of the procedure handled by the police was 7 days in fast-track cases as opposed to 63 days in all cases; the corresponding numbers were 2 and 12 days for state attorneys and 81 and 141 days for the courts.[11]

Figure 3.1 demonstrates that the reform did have an impact on the total duration of the criminal procedure, from offense to final adjudication. The number for each year denotes the average duration of cases for which criminal proceedings started in that year.[12] It divides the offense categories into victim and police reported, and also into "covered" and "other" offenses. Covered offense categories are defined as those with an above-median share of fast-track prosecutions by the end of the sample period.[13] The duration of covered police-reported cases dropped immediately from 350 to 300 days in the first reform year, followed by a gradual reduction to 200 days six years later. For covered victim-reported offenses, the gradual reduction from 450 to 300 days began three years after the reform, but the adoption of the fast-track was also more gradual among victim-reported offenses. The picture is very different for other offenses, where the duration continued on a slowly increasing trend after the reform, and only began to decrease several years after the reform.

[11] According to the conversations with the practitioners, fast-track cases are typically handed over to the court either in a day or two, or at the two-week deadline.

[12] That is, if a crime was committed in 2000 but the police identified and accused the suspect in 2001, the duration of that case enters the observation for 2001.

[13] See Table 3.8. Note that there are many individual cases prosecuted via the conventional procedure in the "covered" offense categories, as well as some cases prosecuted via the fast-track in the "other" offense categories.

3.3 Empirical methodology

3.3.1 Data

The empirical work uses statistical records from the Police of the Czech Republic and the Ministry of Justice, aggregated at the level of district, year, and offense type. There are 79 police districts each with a population of about 125,000 on average.[14] The dataset covers three years before the reform (1999–2001) and seven years afterwards (2002–2008). It contains the number of cases that passed through individual stages of the criminal procedure, starting with the number of offenses reported to the police, the number of cases in which the suspect was identified, the number of prosecutions carried by the conventional and fast-track procedure, etc. The classification of offenses is very detailed. There are between 167 and 175 offense definitions, depending on the year.[15] I aggregate these detailed offenses into 24 broader (and more conventional) offense categories and also drop some obscure or rare offenses.[16] The list of offense categories used in the analysis is given in Table 3.8. The Ministry of Justice records contain procedural information on each criminal case, including the dates of the offense, charges, and final adjudication. I aggregated the records in order to obtain average case durations at the level of the offense, district, and year.[17]

Several socio-demographic characteristics are included in the regressions. These are the population of the district, the share of men in the population by age group[18],

[14]The boundaries of the police districts that circle the capital city (Prague) changed several times during the sample period. I therefore merged those districts into a single district to achieve consistency over time. Likewise, Prague originally had 10 police districts but they were consolidated into 4 districts in 2004. Again, I merge the original smaller Prague districts into 4 new districts to achieve consistency over time. The analysis-ready dataset therefore has 79 districts.

[15]The offense definitions are sometimes narrower than the offenses defined by the Criminal Code and sometimes encompass several offenses defined by the Code. For example, burglaries are divided into 15 categories depending on the object burgled, while the police classification "arson" encompasses three legally different criminal offenses.

[16]For example, military offenses, briberies involving public officials, and also murders because of their very small number and specific procedural rules.

[17]The year denotes the year of the offense, hence observations for year t denote the average duration of cases when the crime was committed in year t (as opposed to cases that were concluded in year t). The offenses in the Ministry of Justice database are classified by the legal definition (the section of the Criminal Code) while the offenses in the Police database are classified by the Police's own coding which rather reflects the factual circumstances of the crime. I aggregate both databases into 23 relatively broader offense categories. As a consequence, there were only few offenses with a conflict between the Police and Ministry of Justice databases such that the offense could not be unambiguously assigned to one of the broader categories.

[18]Specifically, the share of men aged 10–14, 15–19, 20–24, etc. up to 39 and above.

and the share of unemployed men aged 20–29 in the population.

Figure 3.2 plots the raw data on crime rates (number of offenses per 100,000), divided into police/victim reported offenses and covered/other offenses. It previews the key results. The number of covered victim-reported offenses declined gradually throughout most of the sample period (by a third in total). Other victim-reported offenses were also on a on overall downward trend. The rate of covered police-reported offenses was stable before the reform. It jumped up from 127 to 175 in the first post-reform year, and continued to rise at a slower rate thereafter. On the other hand, the police-reported offenses that were generally not covered by the reform were on a declining trend before the reform and declined further in several steps afterwards.

3.3.2 Identifying variation

The identification strategy is based on the fact that the actual adoption of the fast-track procedure varied widely across offenses and districts. The adoption is measured by the share of cases that are prosecuted via the fast-track procedure out of all prosecuted cases. Figures 3.3 and 3.4 show the share of fast-track prosecutions for each offense, aggregated at the national level. The fast-track procedure became used relatively heavily in prosecuting aggravated assault, trespass, burglary, thefts, other property crimes[19], embezzlement, illegal possession of a banking card[20], obstruction of an official order, vandalism, and driving under the influence. The police-reported offenses exhibit a higher share of the fast-track cases because such offenses are typically discovered and recorded when the offender is captured, therefore the identity of the offender is immediately known. Obstruction of an official order has had by far

[19]Damaging someone else's property, unauthorized use of a vehicle, among others.

[20]Unauthorized possession of a banking card (Sec 249b of the Czech Criminal Code 140/1964) is committed by malevolently possessing an ATM card or similar payment instrument that belongs to someone else, without necessarily spending money from it. While admittedly narrow, it is treated here as a separate category among the police-reported offenses. It typically appears in police statistics when a thief is caught with a wallet, and the wallet also contains an ATM card. Depending on the amount of money in the wallet, the police may drop the charges, or charge with theft only, or charge with unauthorized possession of the banking card, or both. The unauthorized possession of a banking card can therefore be used as a substitute charge againts a thief who would have otherwise escaped punishment, or as an add-on charge to punish a thief more harshly. There is some legal ambiguity over which uses constitute unauthorized possession, which further enhances the police's discretion. (It is also a relatively frequent offense, with a crime rate of 75 offenses per 100,000 in 2008.)

the highest share of fast-track cases from the beginning. It is an administratively simple offense, and the evidence is usually straightforward.

The variation across districts is presented in Table 3.1. It shows the mean, standard deviation, and the 5th and 95th percentiles of the share of fast-track prosecutions for the covered offenses in 2002 (the first post-reform year) and in 2008 (the last year in our data) at the district level. The fast-track procedure immediately became the prevalent method for prosecuting obstructions of an official order, with 55 percent on average, and 27 percent in the 5th percentile district. For theft, the initial share the fast-track prosecutions was 21 percent, varying from 7 percent in the 5th percentile to 39 percent in the 95th percentile. Six years later, there is an overall increase in adoption of the fast-track procedure for all offenses, but this occurs mainly through an even higher usage in the districts at the top of the distribution. E.g. the share of fast-track theft cases increased by 13 percentage points both on average and at the 95th percentile, but only by 8 percentage points at the 5th percentile. The share of fast-track prosecutions was still zero for many offenses in the districts at the 5th percentile six years after the reform.

Endogeneity of adoption presents a concern. The law enforcers choose whether to prosecute via the fast-track or the conventional procedure. Naturally, one may suspect that districts experiencing higher crime levels or rising crime trends may adopt the fast-track procedure more intensively as a measure to cut crime. They may also adopt other crime-cutting measures, introducing an omitted variable bias. Likewise, districts with unduly long case durations may adopt the fast-track procedure more intensively in order to cut case durations. The share fast-track prosecutions is also in part determined by the distribution of case characteristics which determine whether the case is eligible for the fast-track. Those characteristics may also be correlated with trends in crime rates or duration.[21]

I interviewed several Ministry of Interior, Police, and State Attorney officials to collect anecdotal evidence about the causes of the large variation across districts. In their view, the differences between districts were driven first and foremost by bureaucratic inertia – some police officers and prosecutors were more willing to experiment

[21]For example, an increase in the crime rate may imply that a smaller fraction of offenders is identified within two weeks, or the increase may come disproportionately from more complicated offenses that cannot be prosecuted via the fast-track. This may imply a mechanical negative correlation between the crime rate and the share of fast-track prosecutions.

with new methods than others. To a secondary degree, they were a by-product of the division of labor between patrol and investigative police units. Internal police guidelines divide the workload between these units, and such guidelines are issued by central, regional, and district chiefs, with an increasing level of detail. The investigative units generally disdain the fast-track procedure as a matter of their professional culture. In districts where the guidelines allocate more petty crimes to the investigative units, the share of fast-track prosecutions is lower. Many factors determine the allocation of labor in the guidelines, other than concerns about the use of the fast-track procedure; the resulting share of fast-track prosecutions is ancillary to those factors. There was also no political pressure from the central government or the regional governments to adopt the fast-track procedure intensively in specific districts.[22]

Last, the practitioners indicated that differences in the adoption of the fast-track procedure could be caused by the relative overload of the police officers and prosecutors. Police officers in districts with higher case loads tended to adopt the fast-track procedure more intensively in order to put more cases "off the table". In districts with low case loads, the officers reported that there was essentially no pressure to spend time and effort on learning and adopting the new procedure. This last explanation posits a relationship between adoption intensity, (relative) staffing of the police, and crime *levels*. Importantly for the identification strategy, none of the anecdotal explanations posits a relationship between adoption intensity and the *trends* in crime rates. Long case durations were never mentioned as a factor influencing adoption.

I check whether the intensity of adoption is correlated with crime rates prior to adoption. The adoption intensity in a district is measured by the share of fast-track cases among covered offenses in the first post-adoption year (2002).[23] Figure 3.5

[22]The boundaries of the police districts did not correspond with the political districts during the period covered in this study. The police force was organized into 8 regions further subdivied into 79 districts, while the local governments are organized into 14 regions and approx. 6200 municipalities. The police chiefs did not have counterparts in elected political offices covering the same jurisdiction. (After the sample period the police force was reorganized into 14 regions that match the political regions, but the police districts still do not have a counterpart in district-level government.)

[23]I experimented with other plausible measures, such as the share of fast-track cases among covered offenses during the entire post-reform period, or the share of fast-track cases in the offense with the highest fast-track adoption (obstruction of an official order). They did not exhibit any stronger correlation with pre-adoption durations or crime rates than the measure presented here.

plots the violent and property crime rates in each district in the last pre-reform year (2001) against adoption intensity. There is a positive correlation for violent crimes. For property crimes, the visible positive correlation is driven by the four Prague districts (AI-AIV) that have the highest property crime rates, as two of them were also among the most fervent adopters.[24]

Figure 3.6 plots an equivalent picture for the percentage changes in crime rates during the pre-reform (1999–2001) period. Adoption intensity is unrelated to pre-reform trends in crime rates.

In a similar manner, intensity of adoption is plotted against case durations and caseload (crimes per police officer) in the last pre-adoption year (Figure 3.7). It indicates that adoption is positively but very weakly related to the duration of the court phase of the procedure and to the caseload per police officer. The relationship with load is driven by five outliers (four Prague districts and Pilsen) that have very high caseloads and were above-average (but not the highest) adopters. Figure 3.8 shows that fast-track adoption was not related to the percentage change in durations and caseloads during the three years preceding the adoption.[25]

3.3.3 Estimation

I estimate an instrumental variable (IV) model in which the share of fast-track cases serves as an instrument for the duration of the criminal procedure. The unit of analysis is the offense category (o), district (i), and year (t). I control for permanent differences between districts and for common shocks by including district and year fixed effects. The estimates are identified from (i) comparing the change in case duration in high-adoption districts with the change in case duration in low-adoption districts and (ii) comparing the change in crime rates in districts with a large predicted reduction in duration with the change in crime rates in districts with small a

[24] Outside the capital city the correlation between property crime levels and adoption intensity disappears.

[25] To check for endogeneity rigorously, I estimated regressions explaining the adoption intensity as a function of pre-reform crime level, police case load, case duration, pre-reform trends in these variables, and socio-economic controls. None of these variables was a statistically significant predictor of adoption intensity. The only exception was total population, which had a negative effect on adoption after controlling for other factors. Because of the lack of statistical significance for the key variables, the results of these regressions are not presented here but are available upon request.

predicted reduction in duration.

The first-stage regression is of the form:

$$log\, d_{oit} = \beta_{1o} s_{oit} + \delta_{1o} log X_{it} + \lambda_{1oi} + \lambda_{1ot} + \epsilon_{1oit} \tag{3.1}$$

where d_{oit} is the average case duration in days (from offense to final adjudication), s_{oit} is the share of fast-track cases out of all prosecuted cases, X_{it} denotes a vector of socio-economic characteristics, λ_{1oi} and λ_{1ot} are the district and year fixed effects, and ϵ_{1oit} is the error term.

The main regression of interest (2nd stage) is of the form:

$$log\, y_{oit} = \beta_{2o} d_{oit} + \gamma_{2o} \log P^{C}_{oit-1} + \delta_{2o} log X_{it} + \lambda_{2oi} + \lambda_{2ot} + \epsilon_{2oit} \tag{3.2}$$

where y_{oit} denotes the crime rate (number of offenses per 100,000 inhabitants). In addition to socio-economic characteristics X, the regression includes lagged clearance rate P^C as a conventional measure of deterrence. β_{2o} is the parameter of interest. It is specific for each offense and, according to the prediction, should be positive for victim-reported crimes but could be negative for police-reported crimes. The system of equations 3.1-3.2 is estimated by 2SLS. Standard errors are clustered by district.

As an alternative, I also estimate a reduced-form model, that is, an OLS estimate of equation 3.2 where case duration is replaced by fast-track share. The reduced-form regression produces a difference-in-differences estimate of the reform on crime rates (or, more precisely, the effect of the intensity of adoption). The advantage of the IV specification is that case duration is a measure of the cost of crime (for offenders) or the cost of the procedure (for enforcement officials). The estimated parameters β_{2o} can be interpreted as behavioral parameters and can be potentially compared to similar estimates from other countries and legal contexts. The reduced-form specification gives smaller standard errors but the magnitude of the coefficients is context-specific to the particular procedural reform.

3.4 Results

3.4.1 IV and reduced-form estimates

The IV estimates for covered victim-reported crimes are presented in Table 3.2. In the first-stage regressions, all the coefficients on the share of fast-track cases are negative and significant at 1 percent. They are large in magnitude – a one-percentage point increase in the share of fast-track cases reduces the case duration by between 0.53 to 1.33 percent. The values of the F-test statistic exceed 10 for all offenses. The estimates of the first-stage regressions show that the share of fast-track cases is a strong instrument.

The IV estimates of the effect of case durations on crime rates are reported in the top row of Table 3.2. The coefficients are positive for aggravated assault, burglary, embezzlement and miscellaneous offenses, as expected. However, none of them are statistically significant. For comparison, I also show the "naive" OLS estimates of an equivalent regression (equation 3.2) in the bottom of the tables. The OLS coefficients should be biased upward because of the reverse causality from more crimes to longer procedure. Indeed, the OLS estimates are positive for 6 out of 8 offenses. They are statistically significant for theft and burglary, the two most common offenses, where the magnitudes imply that a 10-percent reduction in case duration is associated with a reduction in crime rate by half a percent. The IV procedure appears to remove the bias in the expected direction – the IV coefficients are smaller than OLS coefficients for all of these six offenses. For two remaining offenses (embezzlement and miscellaneous), the OLS have implausible negative values while the IV estimates are positive (but insignificant).

The results are very different for the covered police-reported offenses (Table 3.3), namely for two offenses associated with driving: obstruction of an official order and driving under the influence. The IV estimates are negative, very large, and significant at 1 percent. Their magnitudes imply that a 10-percent reduction in case duration increases the crime rate by 2.4 percent (obstruction) and 9.6 percent (DUI). A large negative effect of longer duration on crime rate is also found for violence against public officials and vandalism, although the coefficients are not statistically significant. The first-stage estimates show that the share of fast-track cases is an even stronger instrument for police-reported offenses than for victim-reported offenses.[26]

The reduced-form regressions are presented in Tables 3.4 (victim-reported offenses) and 3.5 (police-reported offenses). The estimated coefficients on the share of fast-track prosecutions have the expected negative sign for five out of the eight victim-reported offenses studied (aggravated assault, trespass, burglary, theft, and embezzlement). They are significant only for burglary and embezzlement. The coefficient of -0.32 for burglary implies that the burglary rate would be 32 percent lower if all cases were handled via the fast-track procedure, compared to what it would have been in its absence. (However, a 100% share may be beyond the realm of possibility; the actual share was 15% in 2008).

The second table once again shows positive, large, and statistically significant coefficients for obstruction of an official order and driving under the influence. These coefficients imply that a full adoption of the fast-track would increase the number of recorded obstruction and driving-under-the-influence crimes by 83 and 33 percent, respectively. Full adoption is not beyond the realm of possibility, as there are several districts where the share of fast-track cases exceeded 90 percent.

The results from both IV and reduced form regressions provide very strong evidence that a reduction in case duration led to an increase in the number of driving-related offenses that are most often discovered via police enforcement activity. Such an increase can be best explained by a substantial reallocation of police effort towards pursuing driving-related offenses, which the fast-track allowed to be "processed" at very low cost. The reallocation effect clearly dominates any deterrent effect. On the other hand, the results provide rather meagre evidence of any deterrent effect of shorter duration on ordinary, victim-reported offenses. Only the reduced form specification detected a statistically significant deterrent effect on burglary and embezzlement.

The coefficients on the lagged logarithm of the clearance rate are negative, as expected, and are statistically significant for property crimes in both IV and reduced-form regressions. They are thus in line with the findings of the conventional deterrence literature estimating the economics model of crime on regional data.[27] Interest-

[26]With the exception of violence against public officials, the coefficients are significant at 1 percent and the values of the F-test statistics exceed 20.

[27]See Tauchen (2010) for a recent review.

ingly, the coefficients on the lagged clearance rate from the conventional estimates of the economics model of crime on the Czech data (Dušek 2012) are greater in magnitude than the corresponding coefficients obtained here. This suggests that not including the duration of the criminal procedure in the conventional estimates may be inducing an ommitted variable bias.

3.4.2 Robustness checks and extensions

The results are robust to alternative specifications. In the first set of robustness checks I experimented with adding more instruments. I added the share of fast-track cases among all covered offense categories. If a district had a high overall share of fast-track cases, this could have reduced the case duration for offense o even though the share of fast-track cases in offense o was not particularly high.[28] In an alternative specification, I added average case characteristics, such as the number of charges per case, the share of offenders in pre-trial detention, the number of prior convictions, and the shares of female and foreign offenders. The latter set of instruments is somewhat controversial; while some of the case characteristics clearly affect durations[29] they are potentially correlated with unobservable determinants of crime rates. Nevertheless, the estimates of β_o^2 under both specifications were very similar to the estimates in Tables 3.2 and 3.3. They are -0.267 and -0.273 for obstruction of an official order and -0.882 and -0.760 for driving under the influence. They are generally positive but very small and insignificant for most victim-reported offenses.

The next set of checks exploits the variation between offenses. Offenses with a low share of the fast-track procedure naturally offer themselves as a "control group" for offenses with a high share. However, there is a reason for caution. The fast-track procedure could have had spillover effects onto other offenses[30] and if so, the other offenses would not constitute a valid control group.

[28] In the first-stage regressions, the coefficients on the overall share of fast-track cases are negative and significant for half of the offenses.

[29] In the first-stage regressions, the average number of charges per case was positively, and the share of offenders in pretrial detention negatively, related to duration.

[30] In a companion paper (Dušek 2013), I find that it had a positive spillover effect on the probability that a suspect is eventually charged for robbery and rape and a negative spillover effect on case durations for a majority of other offenses.

Specifically, I estimate the equation

$$log\, y_{oit} = \sum_o \beta_o s_{oit} \times D_o + \gamma log\, P^C_{oit-1} + \delta log X_{it} + \lambda_{ot} + \lambda_{io} + \epsilon_{oit} \tag{3.3}$$

The share of fast-track prosecutions s_{oit} is interacted with an offense dummy variable D_o so that we obtain estimates of β_o that are specific for each offense. The offense-year fixed effects λ_{ot} control for aggregate shocks to each offense, such as changes in enforcement policies at the national level.[31] They should be included to isolate possibly diverging trends between the covered crimes and other (more serious) crimes. The district-offense fixed effects λ_{oi} control for unobserved heterogeneity between districts in offense-specific crime rates. The parameter of interest is identified both from comparing the change in crime rates in high-adoption districts with the change in crime rates in low-adoption districts, and from comparing, within a district, the change in crime rates for high-adoption offenses with that for low-adoption offenses. In an alternative specification I add a district-reform dummy, i.e. an indicator for each district in all years before the reform and all years after the reform. The district-reform dummy captures possible district-specific shocks to overall crime rates that occurred after the reform.[32]

The results are shown in Tables 3.6 and 3.7. The rows represent alternative sets of fixed effects and show the coefficients on the fast-track share. Equation 3.3 was estimated separately on the samples of all victim-reported and all police-reported offenses, although the coefficients are only reported for the covered offenses.

For victim-reported offenses, the estimated effects are negative and significant for burglary and embezzlement. They are actually greater in magnitude than the corresponding difference-in-differences estimates in Table 3.4. Similarly, the estimated

[31]The most important change concerned driving under the influence (DUI). In July 2006, the government adopted a radical traffic law reform that drastically stiffened penalties for all traffic violations, including DUI. The deterrent effect of the reform resulted in a short-run reduction in road fatalities (Montag 2014). On the other hand, the definition of DUI as a criminal offense was expanded. Before the reform, DUI by ordinary (non-professional) drivers would be regarded as a criminal offense only if it was a repeated offense. Afterwards, every incident of DUI was regarded as a criminal offense. The change in definition is largely responsible for the aggregate boost in the number of DUI offenses in crime statistics since 2006.

[32]In principle, a specification with district-year and offense-year fixed effects would be most desirable since it would control both for common shocks at district level as well as for offense-specific trends at national level. When estimated, none of the coefficients on the fast-track share or the clearance rate were significant. The set of dummies appears to be so extensive that it removes most variation from the data.

effects are positive and significant for obstruction and driving under the influence, and they are also greater in magnitude than the corresponding difference-in-differences estimates from Table 3.4. This indicates that the "control" offenses were moved in the opposite direction (at least in relative terms) to the covered offenses in high-adoption districts.

The fast-track procedure was targeted at less serious, simpler crimes. However, it may have had a spillover effect on other crimes. The reduction in the time cost of prosecuting covered crimes may lead to a shorter case duration or higher probability of punishment for other crimes, with a corresponding deterrent effect. On the other hand, offenders may substitute away from other crimes. I checked for presence of the spillover effect by regressing the rates of other offenses against a measure of overall fast-track use, i.e. the share of fast-track cases among covered crimes in each district and year. A negative and statistically significant spillover effect was detected for two offenses, robbery and fraud. In terms of magnitude, a 10-percentage point increase in the share of fast-track cases is associated with a 4-percentage point reduction in the robbery rate.[33]

3.5 Conclusions

This paper has provided evidence that reducing the duration of criminal procedure has some important effects, by exploiting a major criminal procedure reform in the Czech Republic as a "quasi-natural experiment".

A shorter criminal procedure increases the costs of committing the crime for the criminals and reduces the costs of prosecuting the criminals for the law enforcers. The findings show that the law enforcers are very responsive to case duration, consistent with the resource reallocation hypothesis. The police responded to shorter procedure by pursuing offenses that could be prosecuted quickly and at low cost more vigorously. The number of two particular offenses that are recorded mostly as a result of police enforcement effort – obstruction of an official order and driving under the influence – rose relatively more in districts with high fast-track adoption. There is an economic reason why the reallocation was directed towards offenses as-

[33] Detailed results are available upon request.

sociated with driving, and not towards other police-reported offenses. Allocating more resources to the enforcement of driving-related offenses presumably leads to a more predictable and larger increase in the number of captured offenders than allocating the same resources towards capturing, say, drug gangs or street vandals.

The IV estimates of the reallocation effects imply that a 10-percent reduction in case duration increases the recorded crime rate by 2.4 percent (obstruction) and 9.6 percent (DUI). In order to evaluate the economic significance of these estimates, I compare the actual crime rates with crime rates predicted under the assumption that the share of fast-track cases remained zero throughout the post-reform period while the socio-economic controls and the year dummies evolved as they actually did.[34]

The number of cases of obstruction of an official order was 64 per 100,000 in 2001, rose to 113 in 2005 and then declined to 49 by 2008. (The decline is caused by a change in the traffic law in 2006 which made it easier for the police to punish delinquent drivers through other routes; this legal change is captured by the year dummies.) The predicted crime rates under the assumption of zero fast-track share are 85 for 2005 and 27 for 2008. In the absence of the fast-track procedure, the number of recorded obstructions would have been lower by 24% (2005) or 44% (2008). In a similar vain, the number of recorded driving-under-the-influence cases was 5 in 2001, rose to 10 by 2005, and then exploded to 112 following a new traffic law that extended the legal definition of this criminal offense from repeat drunk drivers to all drunk drivers. The predicted crime rates under the assumption zero fast-track share are 8 for 2005 and 74 for 2008. The number of recorded driving-under-the-influence cases would have been lower by 20% (2005) or 34% (2008).

I find rather meagre evidence of the deterrent effect of shorter procedure on victim-reported offenses that the reform explicitly targeted. The IV estimates have the expected sign for burglary, embezzlement, and other property crimes, but they are statistically insignificant. The reduced-form estimates show that the fast-track procedure had some deterrent effect on burglary and embezzlement. It is worth considering why precisely these two crimes exhibit some deterrent effect. Possibly, crimes

[34]That is, I first generate predicted values of the logarithm of the case duration based on the first-stage regression under the assumption that the share of fast-track cases is zero during the post-reform year. Then I generate predicted values of the logarithm of the crime rates based on the second-stage IV estimates and the predicted durations from the previous step.

that require prior planning, or crimes that are more likely to be committed after a cost-benefit analysis, are more responsive to changes in the costs of committing those crimes, including discounted costs of future punishment.[35]

The estimated deterrent effects are economically significant. The estimates for burglary imply that the fast-track procedure as actually adopted reduced the burglary rate by 4.8 percent. The number of burglaries - as well as most other "ordinary" crimes - declined gradually during the post-reform period. The estimate implies that the fast-track procedure accounts for 23% of the decline in burglaries during the 2002–2008 period and 11% of the decline in embezzlements.

The lack of strong evidence as to the deterrent effect on property crimes contrasts with Pellegrina (2008) who also uses an IV strategy but finds a deterrent effect. One reason for the difference may lie in my research design. Pellegrina (2008) takes the conventional wisdom that peripheral courts are less efficient than main courts, and the fact that the peripheral courts are established far away from provincial centers and in less populated areas. She then uses the distance from the provincial center and the area of the provincial district as an instrument for duration. It could be argued whether these geographical measures are uncorrelated with the unobservable determinants of crime rates. The identification strategy in this paper is based on an explicit quasi-experimental design. A reform that was adopted with varying intensity in different districts for plausibly exogenous reasons generated a variation in duration across time and districts.

Second, the findings in this paper are of course context-specific to the 2002 Czech criminal procedure reform. The case durations of the covered offenses declined by about 150 days after the reform (Figure 3.1), which is an impressive accomplishment. Still, the deterrent effect on victim-reported crimes may have been limited. The lack of salience to the offenders is one possible factor. The reform was not advertised to the general public, and the fast-track procedure in practice covered only between 10 and 40 percent of offenses. The offenders may only have learned gradually about the

[35]Burglary and embezzlement have a relatively low share of fast-track cases among the covered victim-reported offenses. This fact alone is not in contradiction with the finding of a relatively larger deterrent effect. The estimated coefficients in the reduced-form regressions do not denote the change in the crime rates due to the reform, but the change in crime rates due to an increase in the utilization of the fast-track procedure by one percentage point. By construction of the estimators, had the actual share of fast-track cases been higher for burglary and embezzlement, the coefficients should have been unaffected (although the post-reform reduction in crime rates should have been greater).

change in the swiftness of punishment through their own experience or the experience of their peers (Glaeser, Sacerdote and Scheinkman 1996). Also, the time span from offense to final adjudication is still about 300 days (victim-reported offenses) or 200 days (police-reported offenses). If offenders discount the future heavily, the perceived increase in the severity of punishment may be small if the punishment is still imposed 200 days after the offense. The underlying deterrent effect of a shorter procedure may be highly non-linear and may be most pronounced at very short durations.

To conclude, I discuss some normative implications. A shorter and simpler criminal procedure is, *ceteris paribus*, desirable in its own right. Any deterrent effect it may have on crime is simply an added benefit. The reallocation of enforcement towards crimes with simpler procedure has ambiguous welfare consequences. The previous literature analyzed such reallocation in the context of the U.S. war on drugs, with a generally negative normative assessment. The main reasons are that enforcement was reallocated towards drug crimes that are not necessarily desirable to be deterred, and that the reallocation led to an increase in other crimes (Benson et al (1992)). In the Czech context, the enforcement shifted towards offenses that are clearly desirable to deter, and I do not find that it led to any increase in other crimes. From this perspective, the shorter procedure as implemented in the Czech context appears to be an improvement. Yet, the increased caseload of driving offenses has inevitably employed additional resources from the police, prosecutors, and courts, and it is possible that allocating resources towards enforcement of some other crimes could have been more efficient.

3.6 Figures and tables

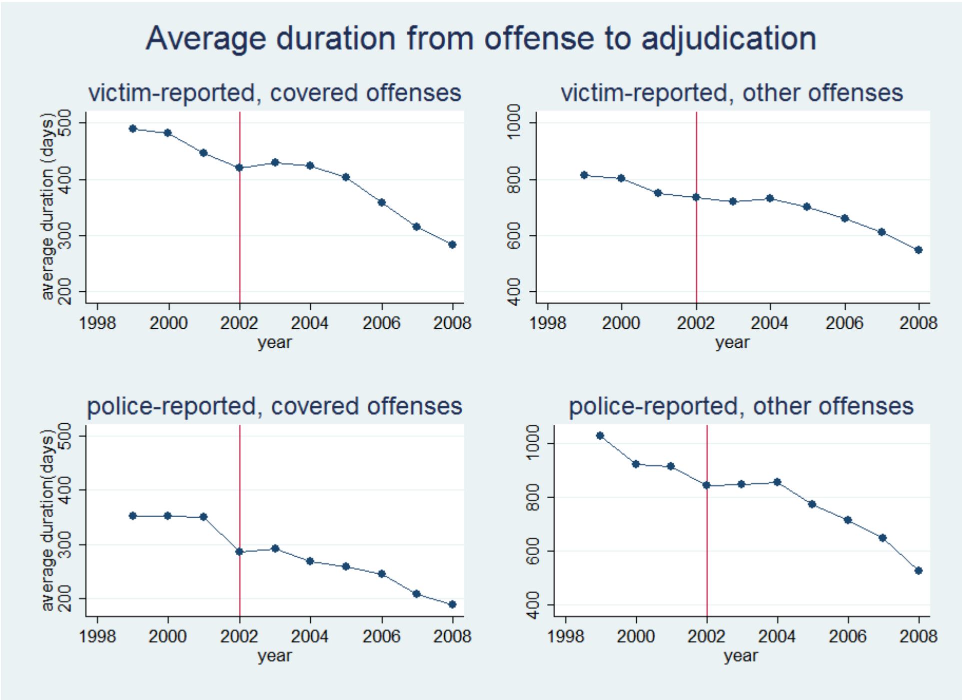

Figure 3.1: Duration from offense to charges

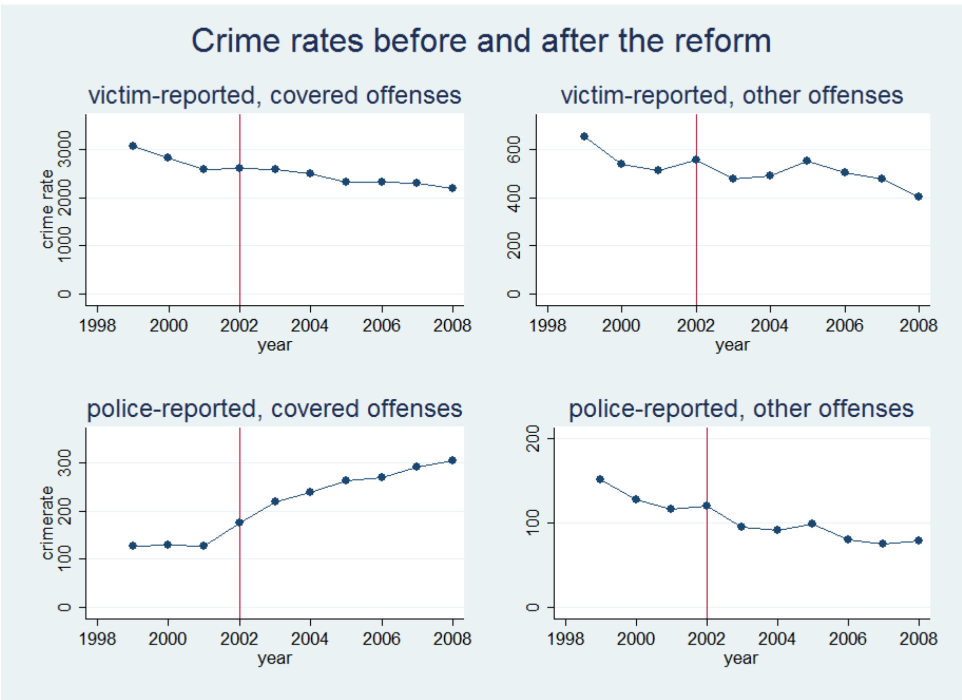

Figure 3.2: Crime rates before and after the reform

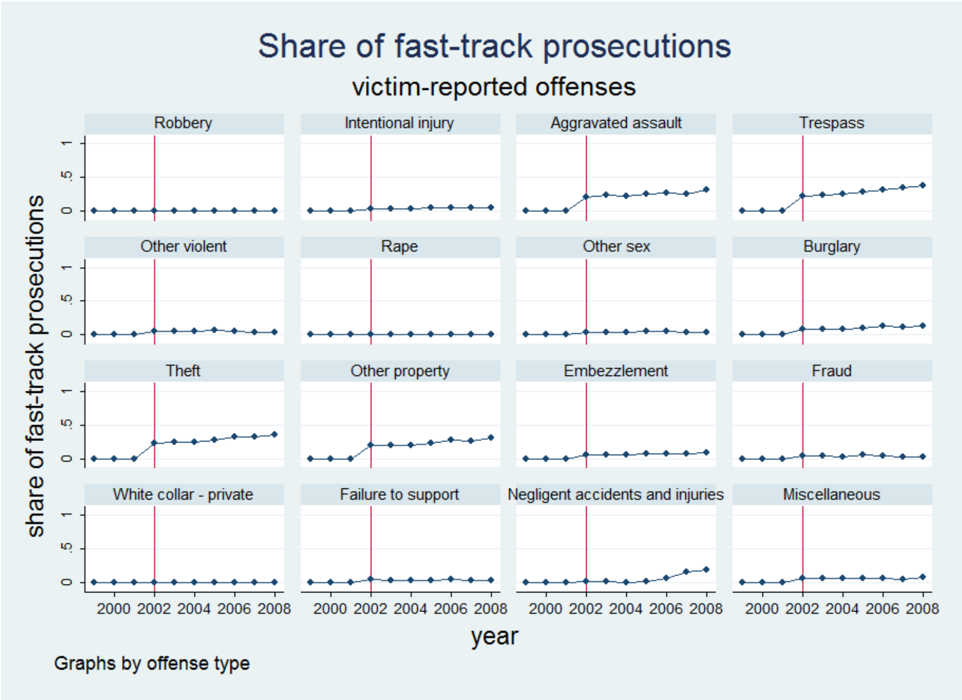

Figure 3.3: Adoption of the fast-track procedure by offense type

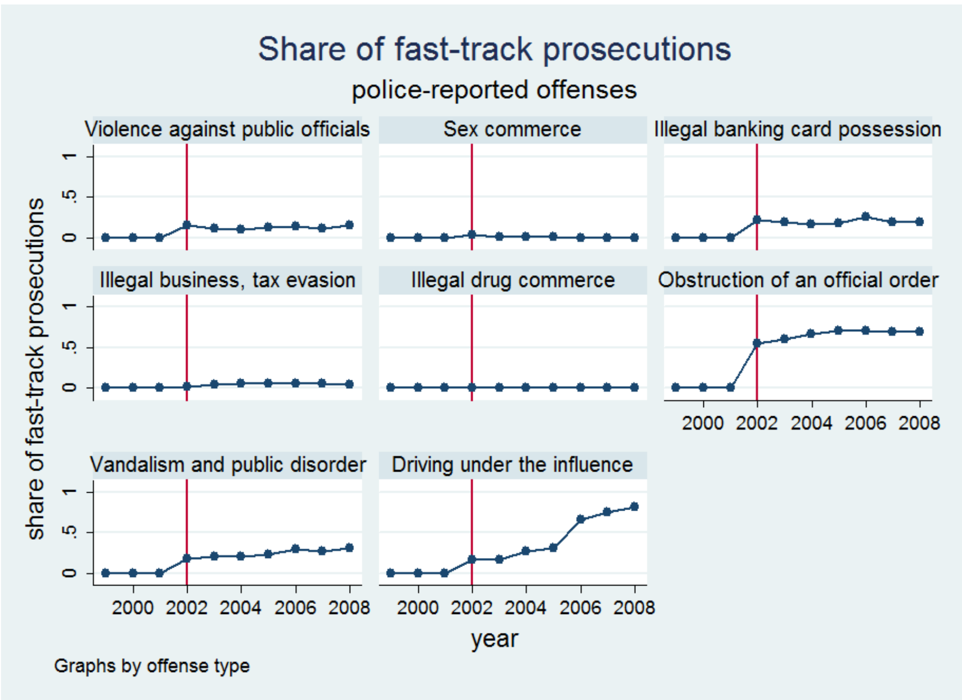

Figure 3.4: Adoption of the fast-track procedure by offense type

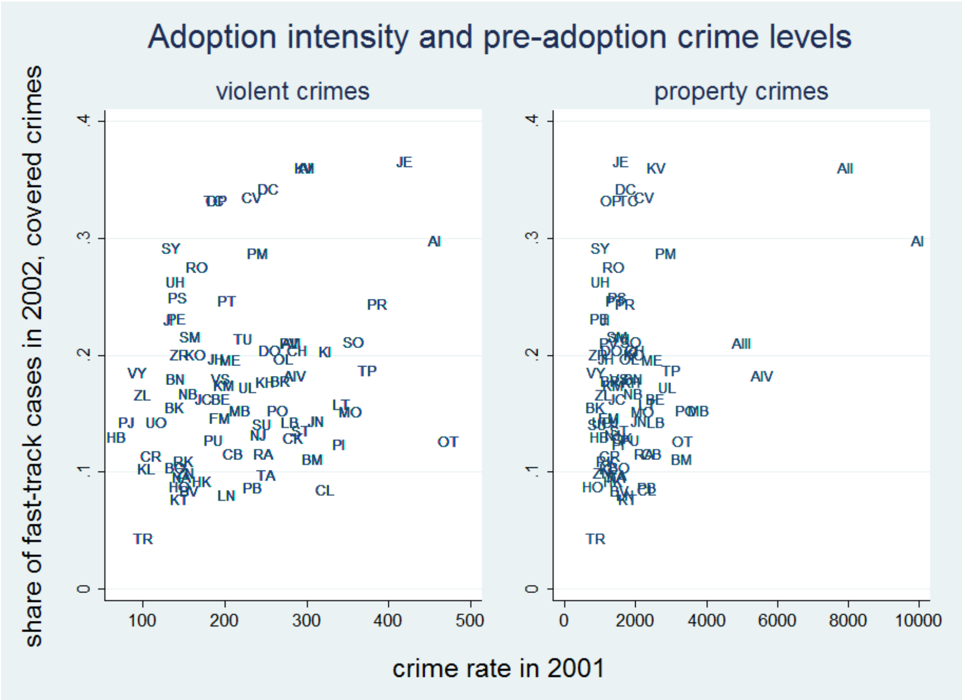

Figure 3.5: Endogeneity of fast-track adoption: crime levels

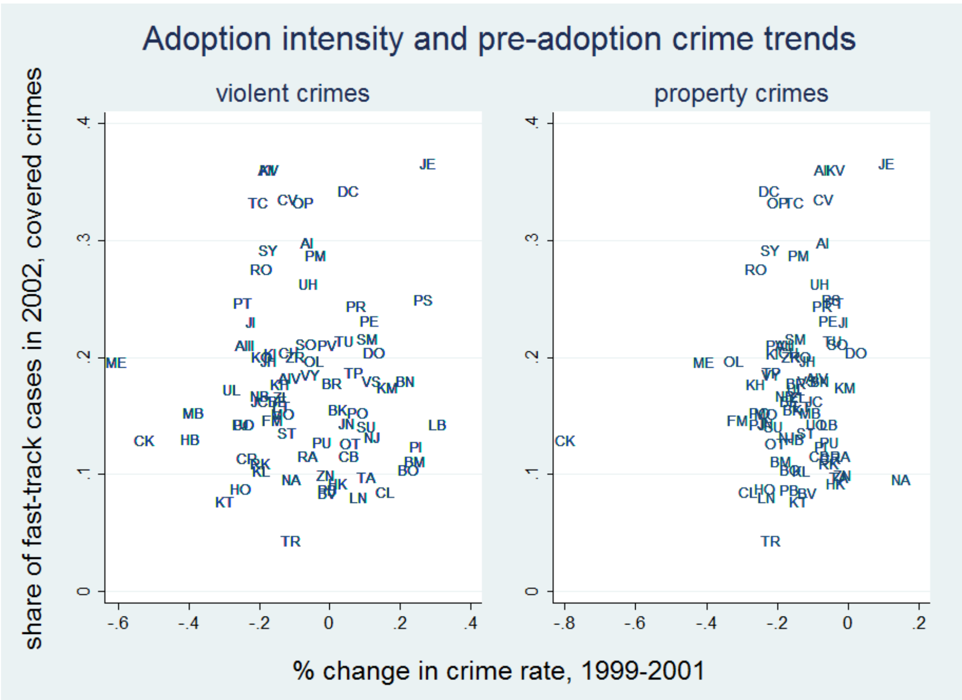

Figure 3.6: Endogeneity of fast-track adoption: crime levels

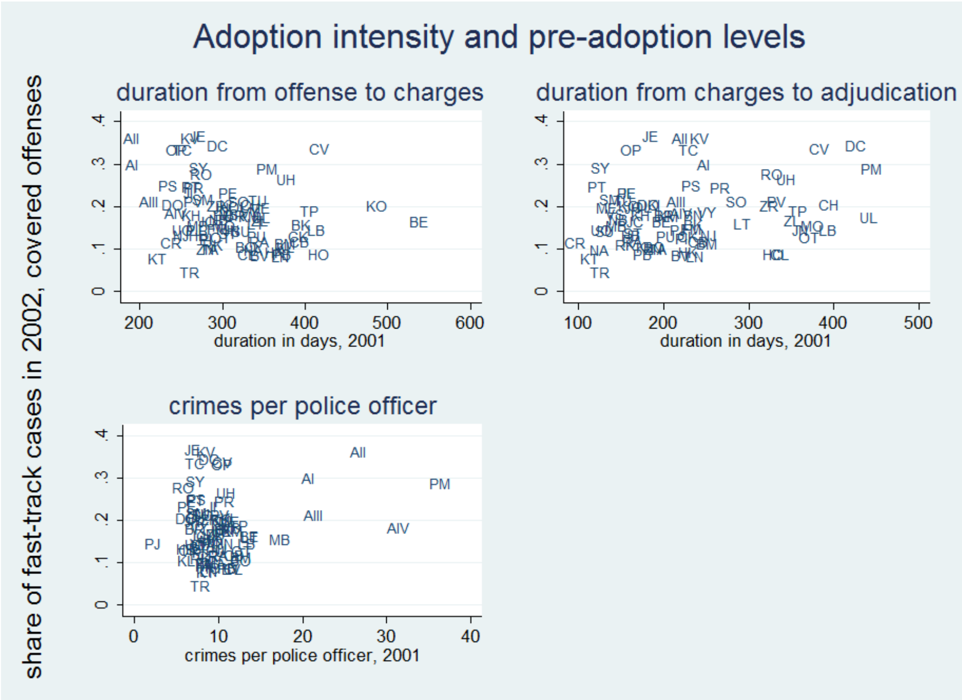

Figure 3.7: Endogeneity of fast-track adoption: levels of case duration and caseload

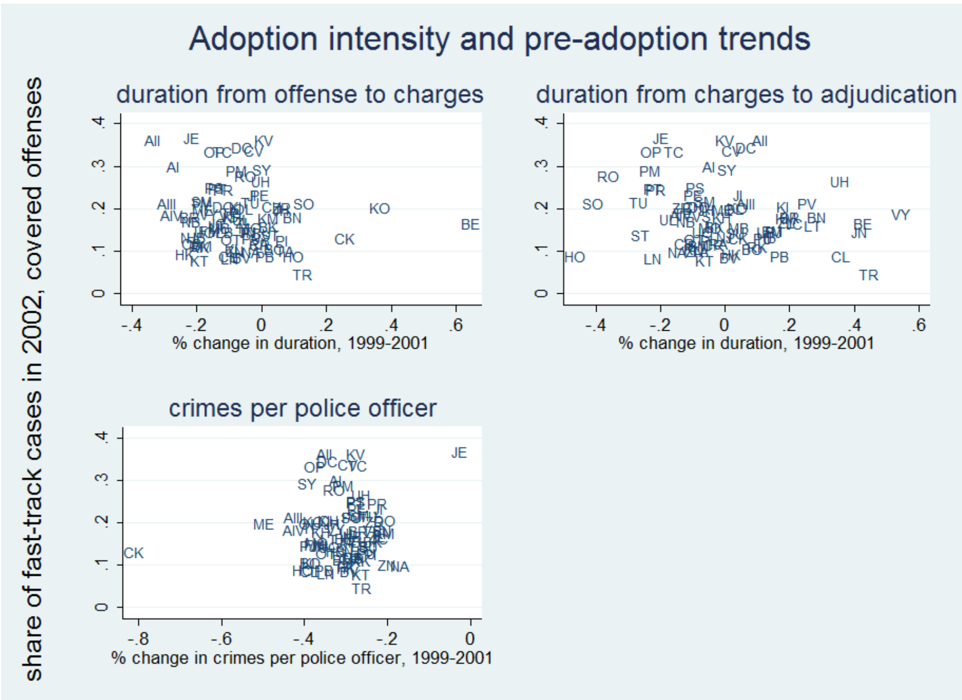

Figure 3.8: Endogeneity of fast-track adoption: levels of case duration and caseload

Table 3.1: Variation in the use of fast-track procedure across districts

Share of fast-track prosecutions in 2002 (%)

offense type	mean	s.d.	5th percentile	95th percentile	crime rate
Aggravated assault	20	17	0	57	27
Trespass	24	15	4	53	34
Violence against public officials	14	19	0	56	12
Burglary	9	6	1	20	704
Theft	21	9	7	39	1600
Illegal banking card possession	17	21	0	60	23
Other property	19	15	0	45	96
Embezzlement	6	7	0	21	78
Obstruction of an official order	55	16	27	77	81
Driving under the influence	17	22	0	62	7
Vandalism and public disorder	19	14	0	43	54
Negligent accidents and injuries	1	5	0	6	79
Miscellaneous	7	7	0	20	60

Share of fast-track prosecutions in 2008 (%)

offense type	mean	s.d.	5th percentile	95th percentile	crime rate
Aggravated assault	33	24	0	71	17
Trespass	40	21	10	78	24
Violence against public officials	15	19	0	43	9
Burglary	15	10	3	35	510
Theft	34	11	15	52	1410
Illegal banking card possession	17	20	0	50	75
Other property	28	16	0	51	122
Embezzlement	11	9	0	30	44
Obstruction of an official order	54	26	8	93	51
Driving under the influence	81	15	38	96	110
Vandalism and public disorder	30	18	6	60	67
Negligent accidents and injuries	18	19	0	53	107
Miscellaneous	7	10	0	25	42

Table 3.2: IV (duration instrumented by the share of fast-track cases), covered victim-reported crimes

	(1) aggravated assault	(2) trespass	(3) burglary	(4) theft	(5) other property	(6) embezzl.	(7) negligent accidents	(8) misc.
IV: 2nd stage								
log duration	0.008	−0.143	0.043	−0.065	−0.176	0.206	−0.003	0.070
	(0.131)	(0.176)	(0.075)	(0.057)	(0.122)	(0.309)	(0.089)	(0.141)
log clearance	0.087	−0.073	−0.158***	−0.224**	−0.071*	0.168	0.146	−0.157*
(lagged)	(0.180)	(0.097)	(0.042)	(0.091)	(0.041)	(0.126)	(0.154)	(0.084)
obs	649	697	697	700	698	700	314	700
R-squared	0.19	0.14	0.55	0.31	0.53	0.56	0.52	0.38
IV: 1st stage								
share of fast-track cases	−1.045***	−0.782***	−0.829***	−1.331***	−0.859***	−0.537***	−0.693***	−1.045***
	(0.191)	(0.130)	(0.171)	(0.251)	(0.177)	(0.156)	(0.212)	(0.128)
partial R^2	0.10	0.12	0.10	0.24	0.07	0.03	0.07	0.13
F-test	30.14	36.33	23.61	28.28	23.71	11.87	10.70	66.80
Hausman χ^2	0.003	−2.385	0.000	−5.749	1.212	−1.674	0.000	1.932
OLS:								
log duration	0.036	0.025	0.044*	0.056**	0.026	−0.198***	0.001	−0.100**
	(0.032)	(0.061)	(0.024)	(0.023)	(0.030)	(0.069)	(0.024)	(0.046)

The dependant variable is the logarithm of the crime rate. Robust standard errors in parentheses.
Regressions include socio-economic controls, year and district dummies
*** p<0.01, ** p<0.05, * p<0.1

Table 3.3: IV (duration instrumented by the share of fast-track cases), covered police-reported crimes

	(1) violence against public officials	(2) banking card possession	(3) obstruction of official order	(4) vandalism	(5) driving under the influence
IV: 2nd stage					
log duration	−0.454	0.093	−0.243**	−0.219	−0.956**
	(0.429)	(0.178)	(0.106)	(0.185)	(0.390)
log clearance	−0.037	−0.189***	−0.739	−0.106	−0.318
(lagged)	(0.077)	(0.047)	(0.740)	(0.087)	(0.277)
obs	652	548	700	696	655
R-squared	−0.12	0.78	0.49	0.29	0.84
IV: 1st stage					
share of fast-track cases	−0.362**	−0.733***	−1.607***	−0.958***	−0.638***
	(0.152)	(0.148)	(0.260)	(0.140)	(0.079)
partial R^2	0.02	0.07	0.303	0.13	0.12
F-test	5.69	24.66	38.15	46.82	75.11
Hausman χ^2	5.605	0.055	−0.062	0.064	7.54
OLS:					
log duration	0.005	0.019	−0.179***	−0.093	−0.188
	(0.044)	(0.049)	(0.046)	(0.062)	(0.121)

The dependant variable is the logarithm of the crime rate. Robust standard errors in parentheses.
Regressions include socio-economic controls, year and district dummies.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Difference-in-differences (reduced-form), covered victim-reported crimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log (crime rate)	aggravated assault	trespass	burglary	theft	other property	embezzlement	negligent accidents	miscellaneous
share of fast-track cases	−0.067	−0.180	−0.320*	−0.109	0.028	−0.692**	0.121	0.020
	(0.123)	(0.114)	(0.184)	(0.089)	(0.071)	(0.276)	(0.162)	(0.225)
lagged clearance rate (log)	0.246	−0.066	−0.153***	−0.221**	−0.058	0.183	0.104	−0.171*
	(0.210)	(0.100)	(0.044)	(0.091)	(0.039)	(0.118)	(0.132)	(0.090)
observations	708	711	711	711	711	711	711	711
R-squared	0.72	0.66	0.91	0.96	0.92	0.75	0.70	0.75
district fe	yes	yes	yes	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes	yes	yes	yes

Robust standard errors in parentheses. Regressions include socio-economic control variables.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.5: Difference-in-differences (reduced-form), covered police-reported crimes

log (crime rate)	(1) violence against public officials	(2) illegal banking card possession	(3) obstruction of official order	(4) vandalism and public disorder	(5) driving under the influence
share of fast-track cases	0.029	−0.013	0.829***	−0.163	0.326*
	(0.155)	(0.192)	(0.169)	(0.162)	(0.189)
lagged clearance rate (log)	−0.072	−0.094**	−0.766	−0.087	−0.547**
	(0.071)	(0.039)	(0.664)	(0.090)	(0.256)
observations	699	646	710	711	674
R-squared	0.67	0.86	0.73	0.68	0.87
district fe	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes

Robust standard errors in parentheses. Regressions include socio-economic control variables.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Triple-differences (reduced-form), covered victim-reported crimes

log (crime rate)	(1) aggravated assault	(2) trespass	(3) burglary	(4) theft	(5) other property	(6) embezzlement	(7) negligent accidents	(8) miscellaneous
offense-year district-offense	−0.033 (0.136)	−0.157 (0.126)	−0.442** (0.203)	−0.050 (0.095)	0.036 (0.083)	−0.740*** (0.261)	0.103 (0.159)	−0.060 (0.237)
offense-year distric-reform district-offense	−0.020 (0.130)	−0.106 (0.128)	−0.416** (0.208)	−0.053 (0.091)	0.036 (0.076)	−0.713*** (0.269)	0.174 (0.160)	−0.020 (0.234)

The table reports offense-specific coefficients on the share of fast-track cases from equation 3.3.
All regressions also include the log of clearance rates, socio-economic controls, and dummy variables. The dummy variable sets are indicated in the row headings. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Triple-differences (reduced-form), covered police-reported crimes

log (crime rate)	(1) violence against public officials	(2) illegal banking card possession	(3) obstruction of official order	(4) vandalism and public disorder	(5) driving under the influence
offense-year district-offense	−0.017	−0.253	0.897***	−0.192	0.415**
	(0.150)	(0.186)	(0.174)	(0.170)	(0.186)
offense-year distric-reform district-offense	−0.019	−0.230	0.988***	−0.197	0.414**
	(0.153)	(0.183)	(0.182)	(0.176)	(0.187)

The table reports offense-specific coefficients on the share of fast-track cases from equation 3.3.
All regressions also include the log of clearance rates, socio-economic controls, and dummy variables.
The dummy variable sets are indicated in the row headings. Robust standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.8: Classification of offenses

broad crime category	offense type	police-reported or victim-reported	covered offense
violent	robbery	victim-reported	no
	intentional injury	victim-reported	no
	rape	victim-reported	no
	other violent offenses	victim-reported	no
	other sex offenses	victim-reported	no
	aggravated assault	victim-reported	yes
	trespass	victim-reported	yes
	sex commerce	police-reported	no
	violence against public officials	police-reported	yes
property	burglary	victim-reported	yes
	theft	victim-reported	yes
	other property offenses	victim-reported	yes
	illegal banking card possession	police-reported	yes
white-collar	fraud	victim-reported	no
	other white-collar	victim-reported	no
	embezzlement	victim-reported	yes
	illegal business, tax evasion	police-reported	no
other	failure to support	victim-reported	no
	negligent accidents and injuries	victim-reported	yes
	miscellaneous	victim-reported	yes
	illegal drug commerce	police-reported	no
	obstruction of official order	police-reported	yes
	driving under the influence	police-reported	yes
	vandalism	police-reported	yes

Chapter 4

The Effects of a Simpler Criminal Procedure on Criminal Case Outcomes: Evidence from Czech District-Level Data[1]

4.1 Introduction

The design of the criminal procedure has to strike a delicate trade-off between competing objectives: assuring that the guilty defendants are convicted; assuring that innocent defendants are acquitted; economizing on the costs of police, prosecutors, judges, defendants, and attorneys; and minimizing the duration of the procedure from the commission of the crime till the actual imposition of the punishment.

The trade-off between the first two objectives has been studied extensively in the theoretical law and economics literature. Most papers (e.g. Andreoni 1991, Rizzolli 2011, Kaplow 2012) search for the optimal standard of proof, that is, the level

[1] Published in Dušek, L., The Effects of a Simpler Criminal Procedure on Criminal Case Outcomes: Evidence from Czech District-Level Data, CERGE-EI working papers (accepted, expected publication December 2014). I appreciate comments from Alena Bicakova, Patrick Gaule, Josef Montag, and participants at the EALE conference and seminars at NF VSE and Copenhagen Business School. I am particularly grateful to Petr Koucky from the Ministry of Justice and Vladimir Stolin from the Police Headquarters for making the data available and to Branislav Zudel, Marek Pekoc, and Vojtech Zika for excellent research assistance. Financial support from IGA grant no. IG505023 is acknowledged.

of evidence required to convict a defendant. However, collecting the evidence and reaching a final verdict requires a substantial input of time and other resources of the policemen, prosecutors and judges. The rules of the criminal procedure guide and constrain the actions of the enforcement officials. Rules that are more formal and grant defendants more procedural rights may lead to more precise verdicts; on the other hand, they may lead to very expensive and lengthy criminal trials. Lengthy and formalistic procedure may also negatively affect the probability of punishment. As the time passes, the quality of the evidence deteriorates or the defendant is more likely to turn fugitive. Complex procedure with many steps increases the probability that the defendant exploits a procedural loophole or witnesses modify their original testimonies.

Delays in the criminal justice process are a serious problem in most countries, and they have many undesirable consequences, including the effects on crime (Pellegrina 2008). Many countries take policy measures to reduce the duration of the criminal procedure. There are two broad approaches to doing so:

- Hiring more policemen, prosecutors and judges – i.e., using more inputs to produce more enforcement output, holding the production technology constant.
- Simplifying the procedure – i.e., changing the production technology, therefore allowing more enforcement output to be produced with the same amount of inputs.

Recent studies on the efficacy of the first approach include Beenstock and Haitovsky (2004) and Dimitrova-Grajzl et al (2012) who investigate the effects of hiring more judges (in Israel and Slovenia, respectively) on the number of cases that are resolved. Both find that an increase in the number of judges has a very small effect on the number of cases resolved and the pending caseload, the extra manpower being largely offset by a reduced productivity per judge and by increased number of cases filed. Huang (2011) investigates the reverse case, when the caseload of two U.S. federal courts of appeals increased suddenly by 40 percent due to a flood of immigration cases. This had an effect on the outcomes of non-immigration cases, where the courts were more likely to dismiss the cases before reaching the decision on merits, and in the cases that proceeded to the decision on merits, they were less likely to reverse or remand. Soares and Sviatchi (2010) evaluate the effects of a technological modernization in Costa Rican courts, finding an increase in clearance rates and a reduction in administrative costs per case.

The economics literature on the effects the second approach has been centered around plea bargaining, a distinctly American procedure. The standard economic argument favors plea bargaining because it achieves convictions of the offenders who do plead guilty in shorter time and at lower cost. It therefore releases resources that can be used to prosecute the remaining cases.[2] These cases can then be also resolved in a shorter time and with a higher probability of conviction at trial. Plea bargaining thus produces an important spillover effect on other cases.

Boari and Fiorentini (2001) is a rare empirical assessment of the effects of plea bargaining, exploiting the transplantation of plea bargaining in Italy. Bridges (1982) evaluates a procedural reform that explicitly sought to shorten the duration of criminal cases: the Speedy Trial Act in the United States. The Act, however, administratively imposed strict time limits instead of simplifying the procedure per se. To my best knowledge, there is no study investigating empirically the effects of a procedural simplification within the traditional civil law framework of a public prosecutor and mandatory court trial.

This paper fills this gap in the literature. It exploits a criminal procedure reform in the Czech Republic as a "quasi-natural experiment" to test the effects of a simpler criminal procedure on the criminal case outcomes, namely the case duration, the probability that an identified suspect is charged at court, and the probability that a charged suspect is convicted at trial. The reform was adopted in 2002. It allowed evidentiarily simple crimes to be prosecuted via a "fast-track" procedure. The new procedure removed several procedural steps and substantially simplified the paperwork. The stated objectives of the reform were to save resources in the enforcement of petty crimes and to release resources for the enforcement of serious crimes.[3] In this sense, the introduction of the fast-track procedure is economically similar to introducing plea bargaining, although only for a limited number of offenses.

The number of cases in a given offense category that are actually prosecuted via the fast-track depends on the number of cases that meet the eligibility criteria and on a

[2]Easterbrook (1983). In contrast, Garoupa and Stephen (2008) give a more moderate view.
[3]Ministry of Justice of the Czech Republic (2001).

discretionary decision of the police officer to prosecute the case via the fast track. In practice, the implementation of the fast-track was gradual and varied substantially across offenses and districts. The fast-track became used most intensively for thefts and for offenses related to driving because these are exactly the offenses where the offender is prosecuted after being caught on the spot and the evidence is thus straightforward. The share of thefts prosecuted via the fast-track was 23 percent on average in the first post-reform year, while it varied from 6 to 43 percent across districts. Similar variation is observed for all offenses, and it persisted over time. In previous research (Dušek 2014), I document that the variation across districts is largely due to "local law" – administrative and ideological preferences of police officers and prosecutors. Importantly, the intensity of fast-track adoption was not related to the pre-reform trends in the case duration or crime rates in a district.

The variation across districts is exploited to estimate its effects on the criminal case outcomes in a difference-in-differences framework. The dataset is a panel of 86 Czech districts and 11 offense categories covering 1998-2008. It contains detailed information on the criminal justice process: number of cases handled by the prosecutor, number of cases prosecuted via the fast-track or conventional procedure, fraction of defendants that were charged and eventually convicted. It also contains detailed information on durations (e.g. average time from offense to charges and final adjudication) and average characteristics of the offender and the case.

I present a simple theoretical framework in order to show that the reform could have affected the criminal case outcomes through two distinct effects: A direct effect on offenses that are prosecuted relatively intensively through the fast-track procedure (covered offenses) and a spillover effect on other offenses (typically more serious offenses) that are prosecuted via the fast-track procedure only sporadically or not at all. Then I estimate these effects by regressing the case outcomes on average case, offender and district characteristics, year dummies and interaction of district dummies with a time trend. Most importantly, the regressions include the share of fast-track cases in a given offense (to capture the direct effect) and the overall share of fast-track cases (to capture the spillover effect).

I find statistically and economically significant direct effects on the duration of the case from offense to charges. A 10-percentage point increase in the share of fast-track cases translates into a reduction in the duration by 9 to 35 days for the covered offenses. The durations were declining throughout the post-reform period, from 256 to 200 days on average for covered offenses. The estimates imply that the fast-track procedure, as actually implemented, contributed 27 days to this decline. The direct effects on the duration of the court phase (from charges to final adjudication) are smaller in magnitude and significant only for two offense categories.

I also find significant positive direct effects on the probability that the identified suspect is eventually charged, which was 69 percent on average before the reform for covered offenses. A 10 percentage point increase in the fast-track share translates into an increase in this probability by 1 to 3 percentage points, depending on the offense. The probability of charges increased by 11 percentage points during the post-reform period, and the estimates imply that the fast-track contributed 6 percentage points to this increase.

I find no evidence of spillover effects on case durations and the probability of conviction at trial. I find some but statistically weak evidence of a spillover effect on the probability of charges, particularly for robbery, sex crimes, and crimes against family.

These findings give some empirical insights into the economics of criminal procedure. Criminal justice systems that are burdened with a large case load can simultaneously improve case durations and the productivity of law enforcers by simplifying the criminal procedure. The lack of evidence of spillover effects does not provide support for the resource-releasing hypothesis put that the standard economic models use in defence of plea bargaining. I would argue that cost-reducing procedural innovation, such as the fast-track procedure or plea bargaining, does not only release enforcement resources, but it also leads to their reallocation. If the costs of enforcing petty cases fall, enforcers have an incentive to pursue the petty cases more vigorously. This reallocation – undesirable from the policy perspective – mitigates the spillover effect. The previous findings (Dušek 2014) of a substantial increase in the number of driving-related offenses is consistent with the reallocation hypothesis and provides a possible explanation for the absence of the spillover effect.

4.2 Institutional background

Prior to the 2002 reform the Czech Criminal Procedure Code prescribed a unified procedure applicable to all crimes. Practitioners generally agreed that the procedure was unnecessarily burdensome, lengthy and expensive for less serious crimes and for crimes where the evidence clearly indicated guilt. The reform introduced a so-called fast-track criminal procedure.[4] Only cases that meet the eligibility criteria: can be prosecuted via the fast-track procedure

1) They fall into the jurisdiction of the district court (i.e., the lowest court level).

2) The maximum punishment set by the Criminal Code does not exceed three years of imprisonment.

3) The suspect was either identified while committing the crime or immediately after, or the evidence revealed in the early stage of the investigation is sufficient to prosecute the suspect and there is a reasonable chance that the suspect can be brought to trial in two weeks.

The fast-track procedure reduced administrative paperwork, eliminated several procedural steps carried out by the prosecutor or the court, and imposed stricter deadlines. Under the conventional procedure, the police, upon identifying the suspect based on the collected evidence, would formally accuse the defendant. From that point on, the police would essentially repeat the collection of evidence (e.g., interrogating witnesses again) while the suspect has broad procedural rights (e.g., to read and comment on the testimonies provided by the witnesses). The case would then be bound over to the state attorney who would review it and charge the defendant at court. The court could hold a preliminary hearing; then, at trial, the evidence would be re-presented again and assessed by the judge. The deadlines faced by the law enforcers are fairly flexible.[5]

Under the fast-track procedure, the police accuses the defendant, hands the case over to the state attorney who reviews the case and charges the defendant at the court. The text of the prosecution is simpler (contains the description of the case and

[4] *"Zkrácené přípravné řízení"* in Czech. The reform was legislated by the Act No. 265/2001.

[5] For example, the police are supposed to hand over the less serious cases to the prosecutor within 2 months. However, if they fail to meet the deadline, they have to merely justify that to the prosecutor who sets a new deadline.

the proposed punishment, but not the legal justification and the description of the evidence). The trial is also simplified: with the consent of the defendant, the judge may declare certain facts of the case indisputable and hence the evidence need not be presented at trial; there are no closing speeches etc. The deadlines are far stricter; the police have to hand over the case to the prosecutor in two weeks since the crime was reported. The prosecutor may, upon request, prolong the deadline by ten days at most; if the deadline is missed, the case reverts to the conventional procedure. The risk of reverting the case to the time-consuming conventional procedure gives the law enforcers strong incentives to meet the deadlines.[6]

The decision whether to initiate the fast-track or conventional procedure rests with the district-level state police officer[7], although the prosecutor may reverse that decision. In practice, the two typically discuss each case informally but reversals of the initial police officer's decisions are rare. The letter of the legislation prescribes that all eligible cases should be prosecuted via the fast-track. In reality, the officers exercise discretion and cases that are eligible for fast-track may be prosecuted via the conventional procedure. Once set, the procedure "sticks" with the case. The court has to adjudicate the case through the procedure that was submitted by the prosecutor.

The reform also made some changes to the conventional procedure. For example, it enhanced the powers of the prosecutor vis-a-vis the police, introduced some adversarial features, and shifted the burden of assessing the evidence from the police to the courts.

The reform was well received by the police and prosecutors. As the main advantages, they report that the fast-track significantly shortened the procedure, reduced the case backlog, and allowed investigative officers to focus on more complicated serious cases.[8] It allowed police officers at the local level to handle far more criminal cases. These police officers emphasized their satisfaction from handling criminal cases from the first contact with the crime all the way through the prosecution; under the con-

[6] According to the conversations with the practitioners, the fast-track cases are typically handed over to the court either in a day or two, or at the two-week deadline.

[7] Only the state police officers can handle criminal cases. Many cities have a city police, but its authority is limited to minor violations punishable by fines (e.g., traffic violations, loitering, graffiti). When the city police discovers an act that should be prosectued and punished according to the Criminal Code, it passes the case to the state police.

[8] Zeman et al (2008), my own interviews with police offcers.

ventional procedure they would have to pass the case to a higher-level investigative officer without seeing the final result. A new criminal procedure reform of 2009 further expanded the range of offenses that can be prosecuted via the fast-track but also mitigated the incentives for the police officers to process cases quickly. For that reason, I evaluate the effects of the reform only during the period covered by the same post-reform legislation, between 2002 and 2008.

The reform appears to have had an effect on the crime rates. In a related paper (Dušek 2014) I estimate its effects on crime rates, exploiting the variation in adoption across districts like in this paper. The fast-track procedure had a rather limited deterrent effect on some less serious crimes, namely burglary and embezzlement. However, it also lead to a substantial increase in offenses related to driving that are discovered and recorded mainly through the police's enforcement effort. The last finding is best rationalized as the reallocation of the police enforcement efforts towards crimes that became "cheaper" to prosecute.

4.3 Theoretical framework

I present a very simple model in order to organize thinking about the predicted effects of a procedural reform described above. The basic idea comes from Landes (1971) model of optimal prosecutor behavior. I modify his model to explicitly show the effects of a reduction in the cost of enforcement for a subset of offenses. The enforcement officials operate under resource constraint and have to allocate limited resources (their own time and material inputs) across individual cases. For simplicity, assume there are two types of cases, serious offenses (H) and petty offenses (L). The output of the enforcement official's work is summarized in "success rates" p_H and p_L, which would be the probabilities of identifying a suspect for a police officer, the probabilities of charging the defendant at court for a prosecutor, or the probabilities of conviction for a judge. The resource constraint is depicted as a curve PPF_1 in Figure 4.1, which shows the possible combinations of probabilities for serious and petty offenses. Allocating more resources into the enforcement of serious offenses increases p_H but requires a reduction in resources allocated to the enforcement of petty resources and hence reduces p_L. The official is maximizing an objective function which is increasing in the probabilities of conviction; the exact shape of the function depends on the number of and harm from the petty and serious crimes, and possibly also his private objectives. The objective function is characterized by an indifference curve in Figure 4.1.

A procedural innovation such as the fast-track procedure reduces the cost of enforcement but only for the petty crimes. It shifts the resource constraint outwards (PPF_2 in Figure 4.2), allowing to achieve higher probabilities of conviction for both types of offenses with the same total resources. It also rotates the constraints such that it becomes flatter: the relative costs of enforcing petty offenses fall, allowing the enforcement official to achieve a greater increase in p_L when shifting the same amount of resources from serious cases.

The optimal response of the enforcement official is driven by substitution and scale effects. The substitution effect induces the enforcement official to shift resources towards petty crimes and away from the serious crimes because the petty crimes became relatively cheaper to enforce. The scale effect induces the official allocate the released resources into both offense types and thus to increase output. Figure 4.2 depicts the resulting equilibrium response. The new procedure has an unambiguous effect on petty offenses, leading to an increase in p_L because the substitution effect reinforces the scale effect. I refer to the sum of the substitution and scale effects on petty offenses as the direct effect. Less obviously, the new procedure has a spillover effect on the serious offenses as well through the behavioral response of the enforcement official. The spillover effect has a theoretically ambiguous direction because the substitution effect mitigates the scale effect (Figure 4.2 is drawn such that the scale effect dominates, and the probability p_H therefore increases).[9]

To summarize, the model predicts that the fast-track procedure should increase the probability of conviction for the petty offenses that were actually covered by the fast-track procedure, while it may increase or decrease the probability of conviction for the other, more serious offenses. The effect on the serious offenses is thus ultimately an empirical question.

[9] The theoretically predicted spillover effect is also the main reason why I do not exploit the variation in the share of fast-track cases across offenses as an additional source of identifying variation in the empirical analysis.

4.4 Empirical methodology

4.4.1 Data and summary statistics

The dataset used in the analysis covers four years before the procedural reform (1998-2001) and 7 years afterward (2002-2008). The unit of observation is a court district and an offense category. There are 86 court districts with an average population of about 120,000 and 11 broader offense categories. The primary data sources are the administrative databases of prosecutorial and court cases provided by the Ministry of Justice. The database records every criminal case that reached the final decision of the prosecutor in the police/prosecutor phase or the final adjudication (including possible appeals) in the court phase. The databases contain the following information about the cases:

- The date when the crime was committed, the police accused the defendant, the prosecutor charged the defendant, the date when the case was received by the court, and the date of final adjudication outcome.
- The legal definition of the offenses (the exact section and subsection of the Czech Criminal Code). I aggregate these very detailed definitions to 11 broader offense categories.[10]
- The final verdicts of the prosecutor (charging, dropping the charges, etc) and the court (guilt, acquittal, the type and severity of punishment).
- Basic characteristics of the offender (gender, age, number of prior convictions).
- For cases prosecuted after the reform, an indicator whether the case was prosecuted via the conventional or fast-track procedure.

I constructed the following variables at the level of the district-year-offense, where year indicates the year when the offense was committed for the police/prosecutor phase and the year when the prosecutor bound the case over to the court for the court phase:

[10]To classify offenses, I separate several narrow offense definitions that are numerous (e.g., theft/burglary, robbery, driving offenses) and then assign the remaining less numerous offenses into broader categories following the broad categorization of the Czech Criminal Code. I exclude murders from the analysis. They are by default adjudicated by the higher-level courts, and the identifying variation at the district level therefore is not available.

- The share of cases prosecuted via the fast-track.

- Case durations: the average duration in days from offense to charges (when the prosecutor binds over the case to the court) and duration from charges to final adjudication.

- Case outcomes: the probability of charges (the fraction of accused offenders who were ultimately charged) and the conditional probability of conviction (the fraction of charged offenders who were ultimately convicted).

- Offender characteristics: the average age of the offender, the share of women and foreigners among offenders, and the average number of prior convictions.

- Case characteristics: the average number of charges per cases (many offenders face multiple charges), the average maximum statutory sentence, and the fraction of defendants in pre-trial detention.

The case outcomes potentially depend on the caseload; I therefore construct three district-level measures of caseload: total number of crimes per police officer, total number of cases per prosecutor, and total number of cases per court senate.[11]

Table 4.1 shows the average characteristics of all cases, divided into the periods before and after the reform. The case duration before the reform was, on average, 362 days (police/prosecutor phase) and 249 days (court phase). These durations declined to 310 and 209 during the post-reform period. Likewise, the probability that the prosecutor brings charges averaged 68 percent before the reform and increased to 78 percent afterwards. The probability of conviction averaged around 82 percent both before and after the reform. Among the case characteristics, a noticeable difference between the pre-and post-reform period is the decline in the fraction of defendants held in pre-trial detention and an increase in recidivism history. The fast-track procedure was used to prosecute 15% of cases in total throughout the post-reform period.

[11]The number of crimes and the number of police officers were obtained from the statistical records of the Police of the Czech Republic. The number of prosecutors in a district is contained indirectly in the prosecutor database because it reports a unique prosecutor identifier. The number of judges in a district is not, unfortunately, available in the court database. I therefore construct a proxy measure of court caseload, the number of cases per senate. The judges in a given court are always divided into senates, with a senate comprising typically of three judges. Finally, I also include the total district population as another control variable in the regressions.

A cursory preview of the results is provided by Figures 4.3 through 4.6. They show the evolution of the outcomes of interest, averaged at the country level. The offenses are divided into "covered" and "other" depending on whether they had above-median or below-median share of the fast-track cases by the end of the sample period. The covered offenses still contain a large fraction of individual cases that are prosecuted via the conventional procedure, and the other offenses may contain a few cases that are prosecuted via the fast-track; the two offense groups differ in the intensity of the actual use of the fast-track. Hence there is no composition effect that would drive diverging trend between the two groups. The duration of the police/prosecutor phase of covered offenses started declining exactly when the reform was implemented. It declined by more than 100 days by 2008, even though with a temporary rebound in the 3rd and 4th year after the reform. The duration of other offenses also declined, but it had been on a declining trend before the reform and stopped declining shortly afterwards. The duration of the procedure in court (Figure 4.4) declined by approximately 100 for covered offenses and by half as many days for other offenses.

Figure 4.5 plots the probability of charges. The reform led to an immediate jump in this probability for the covered offenses by 10 percentage points. The probability of charges continued to grow throughout the post-reform period until reaching 85 percent. For other offenses, the probability of charges rose only slightly with the reform and then levelled off.

Finally, we can observe the trends in the probability of conviction at court, conditional on being charged (Figure 4.6). For covered offenses, it rose gradually by 7 percentage points (to almost 90 percent) since the reform, reversing the prior downward trend. The pattern is similar for other offenses, but less pronounced in magnitude.

4.4.2 Identifying variation

The actual adoption of the fast-track procedure was gradual and varied widely across offenses and districts. The main reasons for such variation are the differences among offenses in the share of cases that are eligible for the fast-track, and differences between districts in exercising the discretion to prosecute cases via the fast-track.

The latter variation allows identifying the causal effects of the fast-track procedure.

Table 4.2 shows the mean, standard deviation, and the 5th and 95th percentiles of the share of fast-track cases for the covered offenses in 2002 (the first post-reform year) and in 2008 (the last year in our data) at the district level. The fast-track procedure became used relatively heavily in prosecuting thefts/burglaries, driving offenses,[12] crimes against personal liberty, other economic/property offenses and crimes against public order. The share of the fast-track cases is highest for offenses that are typically discovered and recorded by capturing the offender, when the identity of the offender is immediately known. In particular, driving offenses had a 53% fast-track share already in the first post-reform year - they are typically simple offenses with a straightforward evidence.

The 5th and 95th percentiles demonstrate the variation in adoption. The share of fast-track in driving offenses, while 53 percent on average, was 28 percent in the 5th percentile district and 82 percent in the 95th percentile district. For theft, the initial share of the fast-track cases was 23 percent, varying from 6 percent in the 5th percentile to 43 percent in the 95th percentile. Six years later, there is an overall increase in the share of the fast-track cases, but it occurs mainly through an even higher usage among the districts at the top of the distribution. E.g.,the share of fast-track among all covered offenses increased by 13 percentage points both on average, but only by 5 percentage points at the 5th percentile and as much as 20 percentage points at the 95th percentile.

Endogeneity of adoption presents a concern. The law enforcers choose whether to prosecute cases via the fast-track procedure. Naturally, one may suspect that the districts experiencing higher crime levels, rising crime trends, heavy case backlog, or long case durations may adopt the fast-track procedure more intensively as a measure to cut crime. They may also adopt other measures aimed at cutting case durations, introducing an omitted variable bias.

I interviewed several Ministry of Interior, Police, and State Attorney officials to collect anecdotal evidence about the causes of the large variation across districts. In their view the differences between districts were driven first and foremost by

[12]The driving offenses include predominantly two narrower offense categories: driving-under-the-influence, and obstruction of an official order. The latter is committed by not obeying a court's restraining, and by far the most common violation involves driving with a suspended driver's license.

bureaucratic inertia and ideological preferences - certain police chiefs and prosecutors being more willing to experiment with new methods than others. To a secondary degree, they were a by-product of internal guidelines that divide tasks and case types between various police subunits. Certain officers (e.g., patrol officers) can only prosecute via the fast-track while others (investigative) have a discretion. The share of fast-track cases in a district is then in part determined by the share of less serious crimes that "land on the desk" of the investigative vs patrol officers. The experts reported that the investigative units generally disdain the fast-track procedure as a matter of their professional culture. In districts where the guidelines allocate more petty crimes to the investigative units, the share of fast-track prosecutions is lower. Many factors determine the allocation of labor in the guidelines other than the concerns about the use of the fast-track procedure; the resulting share of fast-track prosecutions is ancillary to those factors. There was also no political pressure from the central or regional governments to adopt the fast-track procedure intensively in specific districts; the police districts were actually different from the political districts at the time of the reform and the police chiefs did not have counterparts in elected political officials.

According to the narrative evidence, the differences in the adoption were partially driven by the relative overload of the police officers and prosecutors. Police officers in districts with higher case load tended to adopt the fast-track more intensively in order to put more cases "off the table". In districts with low case load, the officers reported that there was no pressure to spend time and effort to learn and adopt the new procedure. The last explanation posits a relationship between the adoption intensity and the number of crimes per police officers. Excessive length of the criminal procedure was not mentioned as a factor influencing adoption. None of the anecdotal explanations postulates a correlation between the adoption intensity and the *trends* in case durations or other outcome variables. This is important for the identification strategy. A spurious correlation between the adoption intensity and trends in outcomes would lead to biased estimates in the difference-in-differences framework.

I check for potential determinants of the fast-track adoption. I define the share of fast-track cases among covered offenses in the first post-adoption year (2002) as a measure of adoption intensity in a district. Figure 4.7 plots this measure against the duration from offense to charges, duration from charges to final adjudication, and caseload (crimes per police officer) in the last pre-adoption year. It indicates that adoption is positively but very weakly related to the duration of the court phase of the procedure and to the caseload per police officer. The relationship with load is driven by several outliers (the Prague districts and Pilsen) that have very high caseload and were above-average (but not the highest) adopters. Figure 4.8 shows that the fast-track adoption was not related to the percentage changes in durations and load during the three years preceding the adoption. To check for endogeneity rigorously, I estimated numerous regression specifications explaining the share of fast-track cases as a function of pre-reform levels of case duration, caseload, crime rates socio-economic variables, or their pre-reform trends. None of these variables were statistically significant predictors of the intensity of fast-track adoption.

4.4.3 Estimation

The variation between districts naturally calls for the difference-in-differences estimator. For covered offenses, I am able to estimate both direct and spillover effects. I estimate the following equation separately for each covered offense category:

$$y_{oit} = \beta_{do} s_{oit} + \beta_{so} \overline{s_{it}} + \gamma_o X_{oit} + \delta_o log X_{it} + \lambda_{oi} + \lambda_{ot} + \epsilon_{oit} \tag{4.1}$$

where y_{oit} is the outcome variable for offense o in district i in year t, s_{oit} is the share of fast-track cases in offense o, while $\overline{s_{it}}$ is the average share of the fast-track case across all offenses (it is therefore the same for all offenses in district i). X_{oit} denotes average characteristics of cases and X_{it} denotes characteristics of the criminal justice system in the district.[13] λ_{oi} and λ_{ot} are the district and year fixed effects, and ϵ_{oit} is the error term. β_{do} and β_{so} are the key parameters of interest. β_{do} has the interpretation of the direct effect: a change in outcome for offense o due to a one-percentage point increase in the share of fast-track cases in offense o. The estimates of the spillover effects are based on the idea that the magnitude of the spillover is determined by

[13] The average case and offender characteristics are: the number of charges per case, maximum statutory sentence, number of prior convictions, share of defendants in pretrial detention, defendant age, and the shares of women and foreigners among defendants. The district characteristics are: the number of crimes per police officer, number of cases per prosecutor and per court senate, and district population.

the total amount of time and other resources that were released by the fast-track. That in turn is determined by the overall share of the fast-track cases in the district $\overline{s_{it}}$, not the share for the particular offense. β_{so} thus has the interpretation of the spillover effect.

For other offenses, I estimate the spillover effect only. The estimating equation is the same as equation 4.1, except the term $\beta_{do}s_{oit}$ is omitted.[14]

In both sets of regressions, the parameters of interest are identified from comparing the change in the outcome variable in high-adoption districts with the change in the outcomes in low-adoption districts. The identifying assumption requires that the district-specific trends in unobservables are uncorrelated with the share of fast-track cases in a district. I emphasize that the identifying assumption does not require that the 2002 reform had no other effects; only that such effects be uncorrelated with the share of fast-track cases in a district. Standard errors are clustered by district.

4.5 Results

4.5.1 Covered offenses

The estimates of the direct and spillover effects on covered offenses are presented in Tables 4.3 (durations) and 4.4 (probabilities). To save on space, the rows show the estimates of β_{do} and β_{so}, while the coefficients on the control variables are not reported.[15] The first row of Table 4.3 shows the effects on the duration from offense to charges. All estimated direct effects are negative, significant at 1%, and range from -90 (driving offenses) to -346 (property/economic offenses). The size of the coefficient for, for example, theft/burglary implies that an increase in the share of fast-track cases by 10 percentage points is associated with a reduction in total duration by 12 days. In a similar vein, a 10-percentage points increase in the share of fast-track cases is associated with a reduction in the duration from offense to charges by 9 days for trespass, 20 days for offenses against personal liberty, 35 days

[14]Most of the other offenses contain a small but non-zero fraction of fast-track cases and thus the direct effects can, in principle, be estimated. However, the share of fast-track cases is too small to generate a measurable impact on the outcomes. I estimated the specification 4.1 for other offenses as well; however, the estimated direct effects were either small and insignificant or had implausibly large values.

[15]Full results are available upon request.

for other property/economic offenses, and by 14 for offenses against public order.

The second row reports the estimated spillover effects. I find no evidence of a negative spillover effects on covered offenses. The coefficients on the fast-track share in all offenses are positive but not statistically significant for four out of the five covered offenses. The main effect of the reform on covered offenses appears to be driven purely by economizing resources in enforcing each covered offense.

The lower panel of Table 4.3 reports the effects on the duration of the court phase, from charges to final adjudication. All estimated direct effects are also negative, and they are statistically significant at 5% for theft/burglary and offenses against personal liberty. They imply a reduction in the court duration by 13 days associated with a 10 percentage point increase in the share of fast-track cases. The estimated spillover effects are not statistically significant at a 5% level, and they have a positive sign.

The top panel of Table 4.4 reports the estimated effects on the probability that the defendant is charged, conditional on being identified as suspect. The direct effects are positive, significant at 1% for four out of five offenses, and large in magnitude. A 10 percentage point increase in the share of fast-track cases is associated with an increase in the probability of charges by 1.3 percentage points for theft/burglary, 2.2 percentage points for driving offenses, 2.3 percentage points for offenses against personal liberty, and 3 percentage points for other property/economic offenses. The spillover effects have varying signs. The only significant spillover effect is found for driving offenses and it is negative, implying a perverse spillover effect that actually lengthens duration. Finally, the regressions in the bottom panel of Table 4.4 reveal no discernible direct or spillover effects on the probability of conviction at trial, conditional on the case reaching the trial. This is an important finding. The fast-track procedure simplified many steps and could have potentially deteriorated the defendants' rights to the extent that more of them would be (unjustly) convicted at trial. The last finding indicates that it was not the case and the courts appear to have applied the same standard for conviction in the fast-track cases as in the conventional cases.

The results for covered offenses are consistent with the hypothesis that the simplified criminal procedure had desirable causal effects on the petty offenses that the reform explicitly targeted. It significantly reduced case durations, particularly in the police/prosecutor phase of the procedure. It also significantly increased the probability that the prosecutor successfully completes the prosecution and brings the charges to the court.

4.5.2 Other offenses

The estimates of the spillover effects on the duration of other, non-covered offenses are presented in Table 4.5. There is only one negative and statistically significant spillover effect: on the duration from offense to charges for sex offenses (a 10 percentage point increase in the overall fast-track share being associated with shortening the duration by 34 days). The spillover effects on the duration from offense to charges are small, insignificant, and with varying signs for all the remaining offenses. The same is true of the estimated spillover effects on the duration from charges to final adjudication. Not only are the estimated spillover effects insignificant, but the regressions explaining the case durations of other offenses have generally lower explanatory power than corresponding regressions for covered offenses.

The evidence of spillover effects on the probability of charges or the probability of conviction is also very weak (Table 4.6). The estimated effect on the probability of charges is positive and economically significant for all offenses but one, in particular for robbery (0.08), sex offenses (0.12), offenses against family (0.09) and against public safety (0.12). However, it is statistically significant only for offenses against family. No spillover effects are detected for the probability that the defendant is convicted at trial. All the estimates are statistically insignificant and generally small in magnitude.

I experimented with alternative specifications that could potentially gauge the spillover effects more precisely. In one, the overall share of the fast-track cases was replaced with the share of fast-track cases among the covered offenses only. In another, I constructed the average share of fast-track cases weighted by an index of case difficulty. The fast-track procedure plausibly released more enforcement resources in districts that implemented it on relatively more difficult cases, holding the overall share of fast-track cases constant. Using the case-level data, I construct an index of case difficulty at the level of a very narrow offense definition (the section and subsection of the criminal code). The index is equal to the average case duration in the last pre-reform year. The weighted share of fast-track cases is then constructed at the level of offense, district, and year. Nevertheless, this procedure produced estimates of the spillover effects that were very similar to the specifications using a simple fast-track share.

4.6 Conclusions

The paper provided evidence that introducing a simpler criminal procedure has some important effects on the outcomes of criminal cases. In the Czech context, the simpler procedure was implemented on a subset of petty offenses. The main finding is that it reduced the duration of the criminal procedure precisely for those (covered) offenses. The reduction in duration was particularly concentrated in the police/prosecutor phase of the procedure.

The estimated direct effects on covered offenses are economically significant. In order to evaluate their economic significance, I compare the change in actual duration with a change in counterfactual duration. To construct the counterfactual, I use the regression coefficients from Table 4.3 to predict the duration after the reform, under the assumption that the share of fast-track cases would have remained zero throughout the post-reform period while the case, offender, and district characteristics, the year dummies and district trends would have evolved as they actually did. Table 4.7 reports the results of these simulations. For example, the average duration from offense to charges for theft/burglary cases was 168 days in the last year before the reform. It declined by 33 days during the post-reform period. The regressions estimates imply that in the absence of the fast-track procedure, the duration would have declined as well, but by 13 days only. The fast-track procedure, as actually implemented to prosecute theft/burglary cases, accounts for 20 days of the reduction in duration. The contribution of the fast-track procedure was particularly pronounced in driving offenses and other property/economic offense, where it accounts for a reduction in case duration by 60 and 54 days, respectively. On average, the duration from offense to charges declined by 55 days, of which 27 days is attributable to the fast-track procedure.

The second main finding is a direct effect on the probability that the accused defendant is eventually charged with court. The fast-track procedure can therefore be thought of as a "technological improvement" that allowed the police and prosecutors to successfully complete a higher fraction of cases all the way to charging the defendant. As for the economic significance, the bottom panel of Table 4.7 reports the results of an analogous counterfactual exercise. The probability of charges in theft cases increased by 17 percentage points during 2001-2008, from 66 percent to 83 percent. If the share of fast-track cases were zero, it would have increased by 10 percentage points only. The fast-track procedure thus contributed 7 percentage points to this increase. It had a similarly large effect on driving offense and offenses against personal liberty. On average, it accounts for 6 percentage points out of the 11 percentage points actual increase in the probability of charges.

The particular findings are of course context-specific to the Czech criminal procedure reform. However, they provide insights into some general questions in the economics of criminal procedure. On the policy side, the reform demonstrates that countries burdened with overly lengthy and ineffective criminal justice process do not necessarily have to hire more police officers, prosecutors, or judges. Simplifying the procedure can reduce the procedural delays and increase the output of the enforcement officials. The lack of an effect on the probability of conviction at trial indicates that such an improvement in productivity can be achieved without compromising the standard of proof.

The reform saved enforcement resources in a subset of cases (petty offenses). In this sense, it was conceptually similar to introducing plea bargaining. The estimates of the spillover effects thus provide an indirect test of the hypothesis the plea bargaining releases resources and increases the prosecutors ’ productivity even in cases that reach trial. These beneficial spillovers onto other, more serious offenses, was in fact an explicit objective of the reform. However, I find essentially no evidence of such spillover effects (with a possible exception of a statistically weak spillover on the probability of charges for several serious offenses).

Why is there an absence of significant spillover effects? The theoretical framework of section 4.3 provides a possible explanation. The spillover effect is a product of two underlying behavioral responses: the scale and substitution effects. The benefits of plea bargaining and similar cost-reducing procedural alternatives are stemming only from the scale effect. The substitution effect, however, is driven by the incentive of the law enforcers to allocate resources towards offenses that are "cheap" (from their perspective) to enforce. The magnitude of the substitution effect depends on the willingness of the enforcement officials to substitute the enforcement of the petty offenses for the serious offenses. The fast-track procedure reduced the relative cost of prosecuting petty offenses. As I demonstrate in Dušek (2014), it lead to an increase in the number of (recorded) driving offenses, a finding that is consistent with a fairly sizable willingness to substitute the enforcement of various types of offenses depending on the cost of enforcement. The potential spillover effects on serious offenses may thus have been largely undone by the reallocation of enforcement resources towards petty offenses.

4.7 Figures and Tables

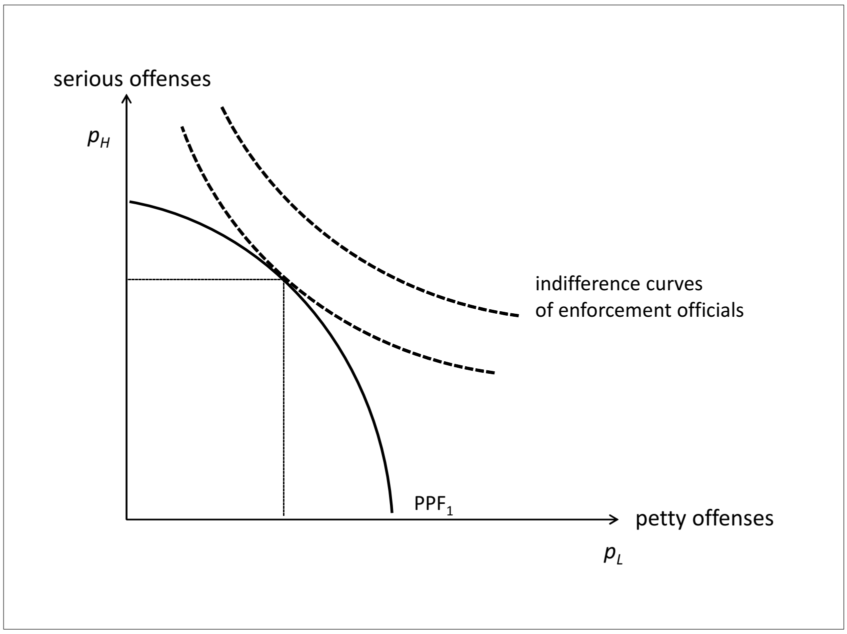

Figure 4.1: Constraints and preferences of enforcement officials

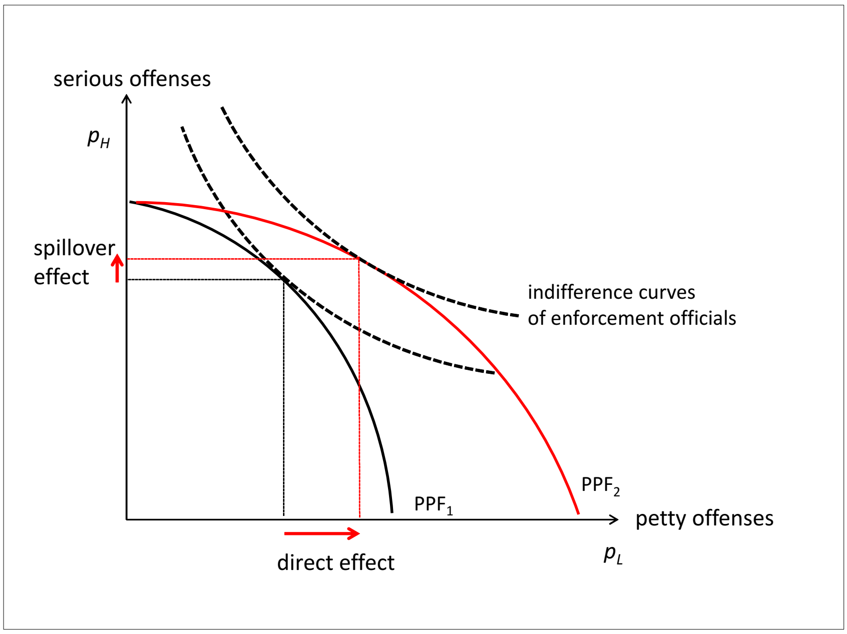

Figure 4.2: Response to a decrease in the cost of enforcing petty crimes

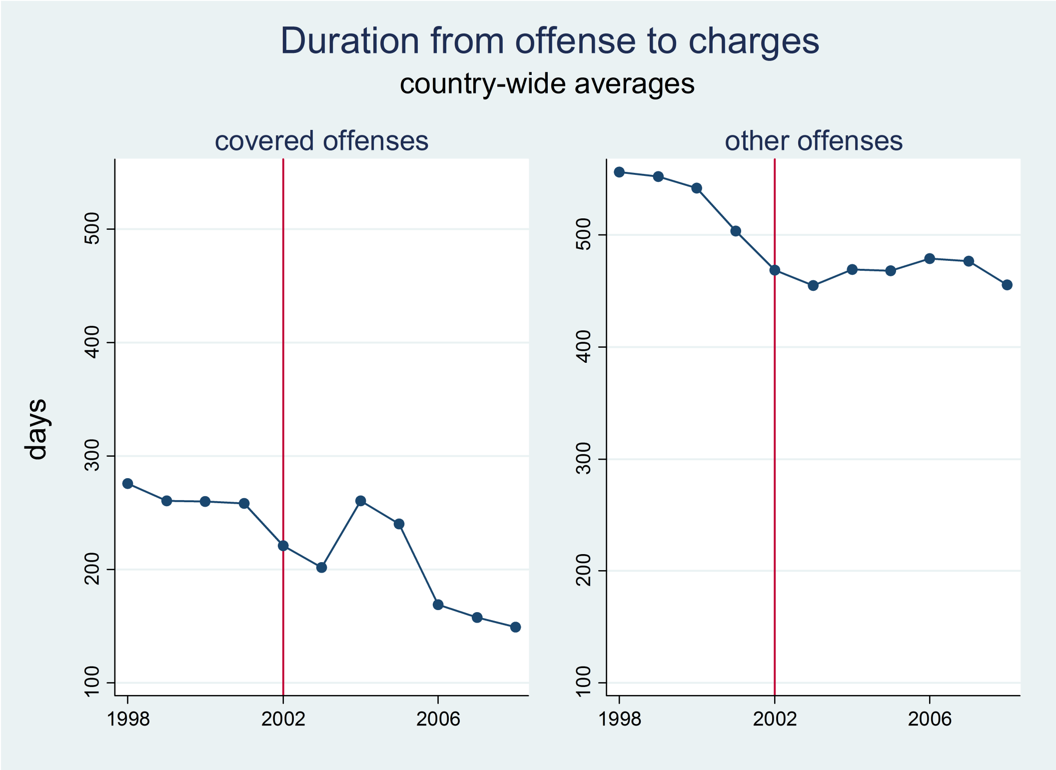

Figure 4.3: Average duration from offense to charges, by offense types

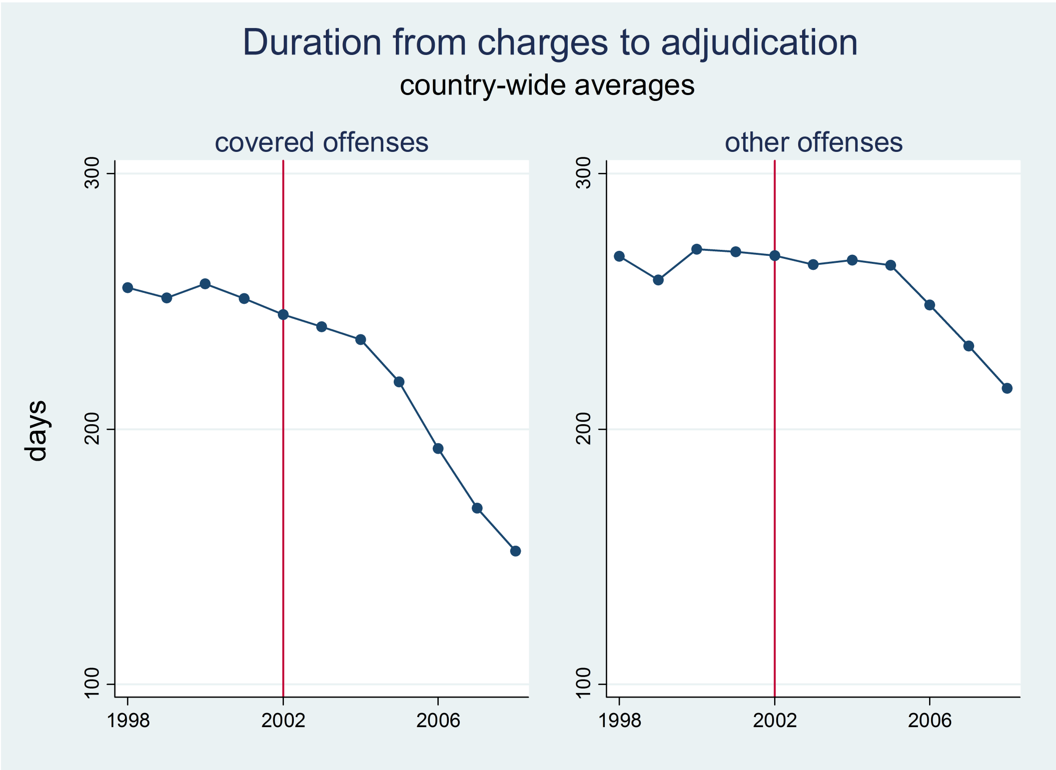

Figure 4.4: Average duration from charges to final adjudication, by offense types

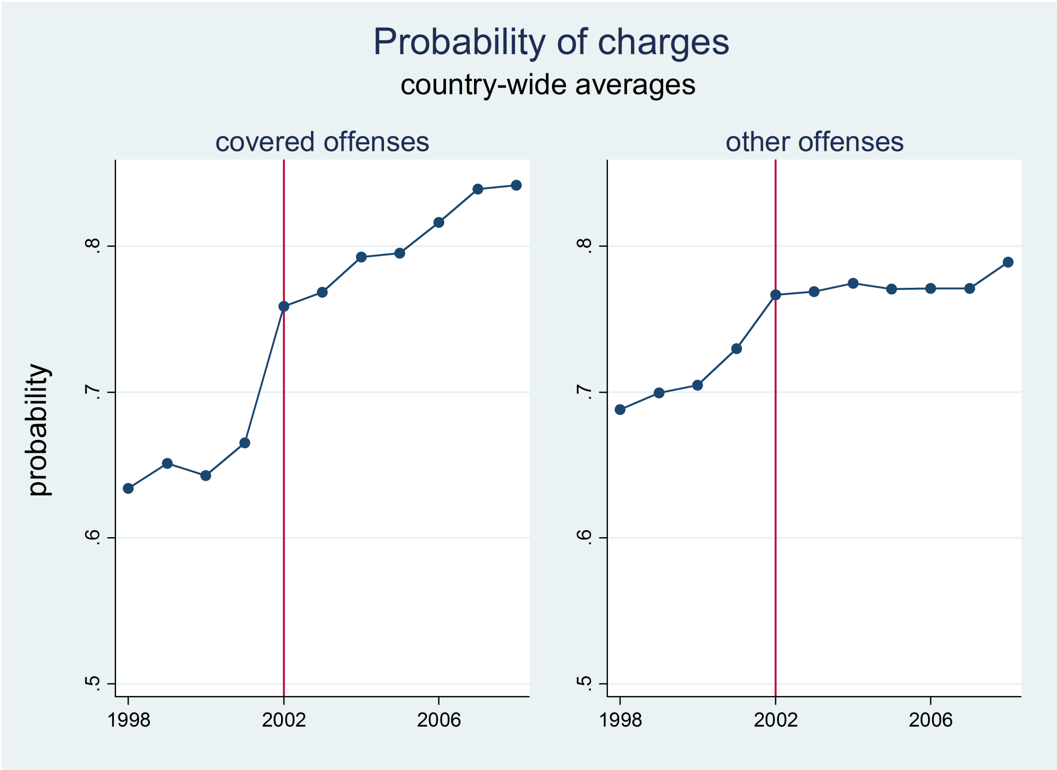

Figure 4.5: Average probability of charges, by offense types

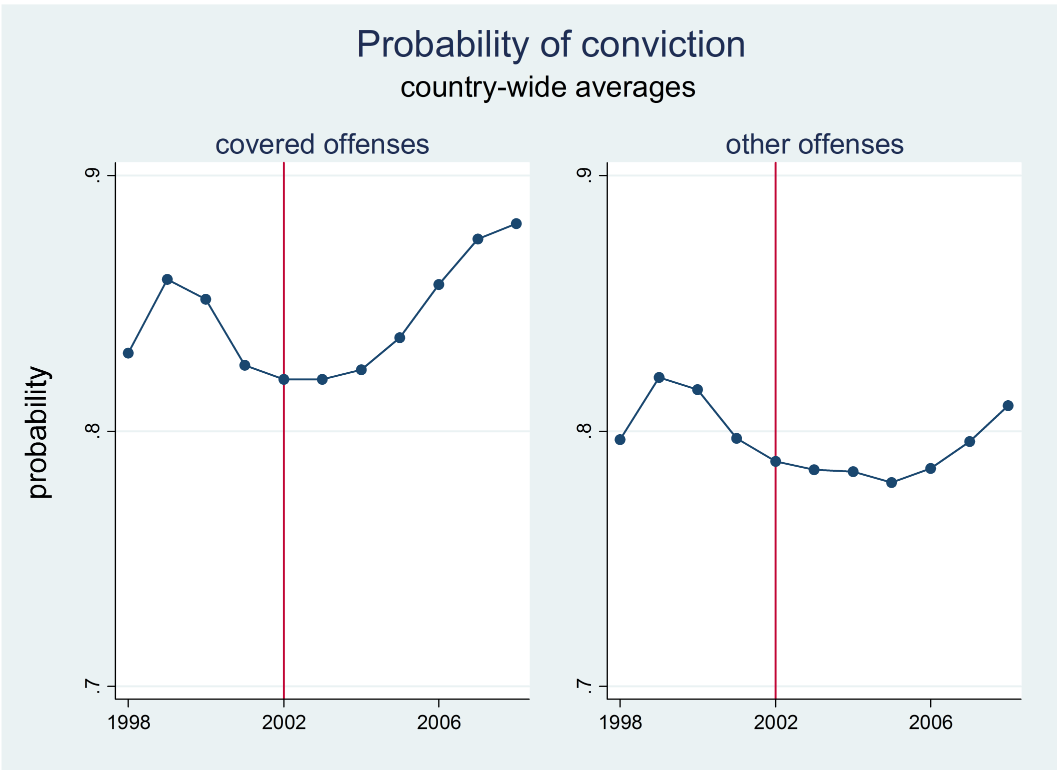

Figure 4.6: Average probability of conviction, by offense types

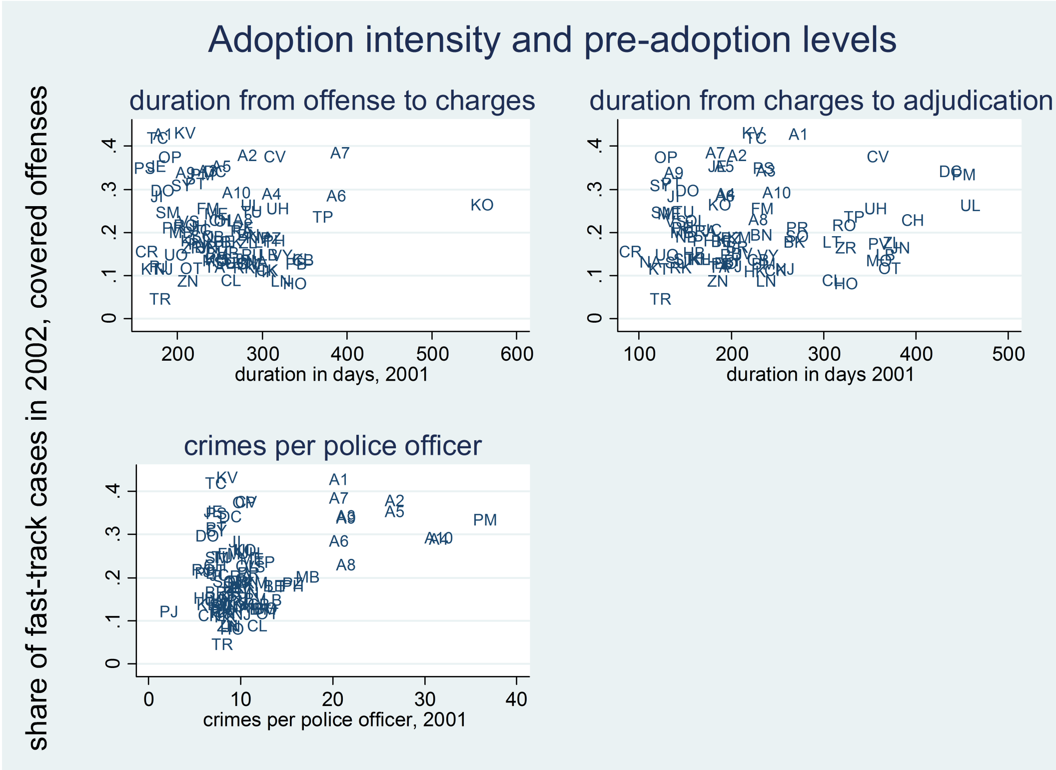

Figure 4.7: Endogeneity of fast-track adoption: levels

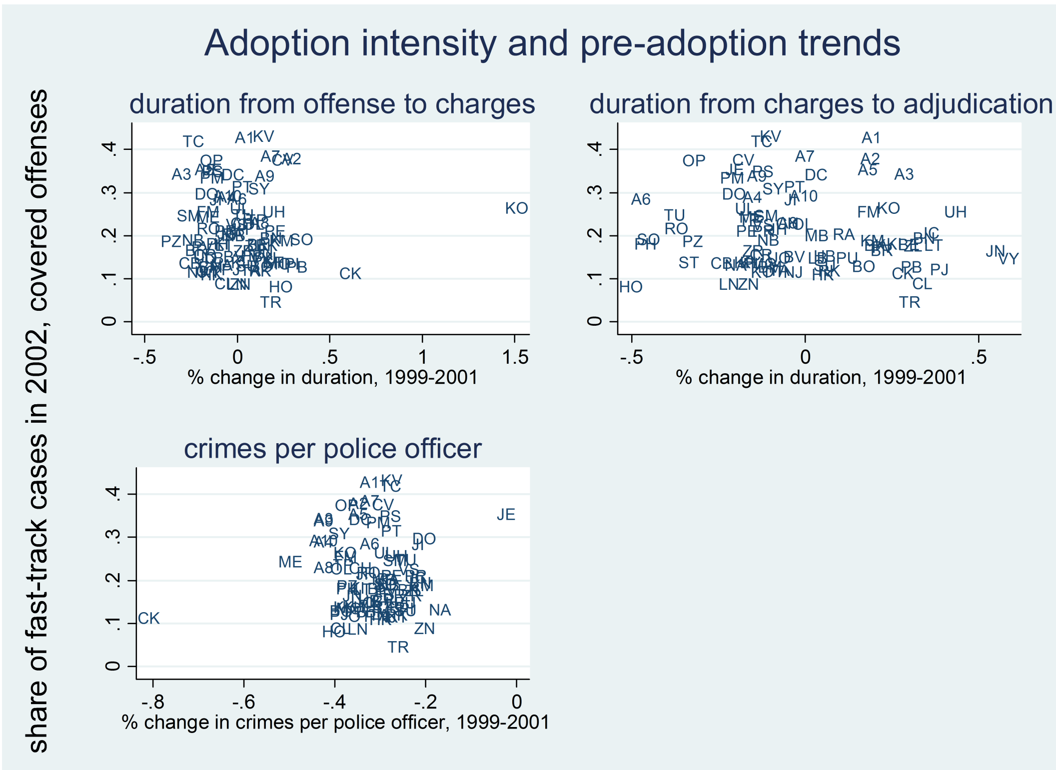

Figure 4.8: Endogeneity of fast-track adoption: trends

Table 4.1: Summary statistics

	pre-reform			**post-reform**		
variable	obs	mean	sd	obs	mean	sd
share of fast-track cases	3782	0	0.02	6618	0.15	0.20
duration from offense to charges (days)	3778	362.25	250.15	6617	309.75	220.20
duration from charges to adjudication (days)	3775	248.95	161.43	6612	212.03	126.61
probability of charges	3782	0.68	0.15	6618	0.78	0.14
probability of convictions	3775	0.83	0.12	6612	0.82	0.11
number of charges per case	3782	1.34	0.32	6618	1.36	0.31
share of defendants in pre-trial detention	3775	0.16	0.19	6612	0.10	0.15
number of prior convictions	3775	1.91	0.94	6612	2.38	1.05
share of female defendants	3782	0.10	0.07	6618	0.11	0.08
share of foreign defendants	3782	0.06	0.09	6618	0.06	0.09
defendant age	3782	29.79	4.17	6618	31.08	4.30
crimerate (offeses per 100,000)	3784	0.04	0.03	6622	0.03	0.02
crimes per police officers	3784	16.25	11.18	6622	10.95	5.93
cases per prosecutor	3784	104.91	36.54	6622	97.62	30.02
cases per court senate	3784	144.23	35.54	6622	417.92	549.29

Note: summary statistics computed at the level of district-year-offense.

Table 4.2: Variation across districts

Share of fast-track cases in 2002 (%)

offense category	mean	sd	p5	p95
theft/burglary	23	11	6	43
driving offenses	53	17	28	82
against personal liberty	12	8	2	28
property/economic offenses	10	8	2	26
against public order	14	10	2	32
all covered offenses	22	19	2	65

Share of fast-track cases in 2008 (%)

offense category	mean	sd	p5	p95
theft/burglary	35	11	19	57
driving offenses	76	11	60	92
against personal liberty	21	12	5	45
property/economic offenses	21	12	6	45
against public order	23	12	5	46
all covered offenses	35	24	7	85

Table 4.3: Effects on covered offenses: Case durations

	(1)	(2)	(3)	(4)	(5)
outcome variable:	theft/ burglary	driving offenses	against personal liberty	property/economic offenses	against public order
duration from offense to charges					
direct effect: fast-track share, offense-specific	−116.6***	−90.19***	−202.3***	−346.0***	−138.8***
	(22.37)	(16.37)	(42.74)	(73.22)	(42.01)
spillover effect: fast-track share, all cases	62.63	12.55	103.5*	−2.978	105.1
	(40.23)	(42.86)	(58.84)	(78.43)	(68.18)
R-squared	0.757	0.798	0.625	0.617	0.561
duration from charges to adjudication					
direct effect: fast-track share, offense-specific	−133.3***	−25.66	−133.9**	−55.05	−48.09
	(50.22)	(23.77)	(63.91)	(66.58)	(35.60)
spillover effect: fast-track share, all cases	105.8*	57.75	32.88	40.31	53.44
	(56.98)	(39.17)	(74.85)	(80.19)	(60.82)
R-squared	0.718	0.607	0.706	0.633	0.619
observations	946	946	946	946	946

The table reports the coefficients on the share of fast-track cases and their standard errors (clustered by district). The unit of observation is district, offense, and year. All regressions include district trends and year fixed effects. All regressions include the following average characteristics of cases and districts: crimes per police officer, number of cases per prosecutor/senate, district population, number of charges per case, maximum statutory sentence, number of prior convictions, share of defendants in pretrial detention, defendant age, and the shares of women and foreigners among defendants.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4.4: Effects on covered offenses: Probabilities of case outcomes

	(1)	(2)	(3)	(4)	(5)
outcome variables:	theft/ burglary	driving offenses	against personal liberty	property/economic offenses	against public order
probability of charges					
direct effect: fast-track share, offense specific	0.136***	0.215***	0.226***	0.301***	0.0952*
	(0.0379)	(0.0495)	(0.0483)	(0.0575)	(0.0513)
spillover effect: fast-track share, all cases	0.0472	−0.151**	0.0698	−0.0387	0.0954
	(0.0485)	(0.0628)	(0.0716)	(0.0652)	(0.0856)
R-squared	0.848	0.661	0.650	0.757	0.692
probability of conviction					
direct effect: fast-track share, offense specific	0.0619	0.0271	0.0376	0.00382	0.0331
	(0.0374)	(0.0199)	(0.0480)	(0.0521)	(0.0446)
spillover effect: fast-track share, all cases	−0.0513	−0.0454	0.0365	−0.0885	−0.0218
	(0.0524)	(0.0402)	(0.0706)	(0.0780)	(0.0697)
R-squared	0.559	0.406	0.448	0.561	0.460
observations	946	946	946	946	946

The table reports the coefficients on the share of fast-track cases and their standard errors (clustered by district). The unit of observation is district, offense, and year. All regressions include district trends and year fixed effects. All regressions include the following average characteristics of cases and districts: crimes per police officer, number of cases per prosecutor/senate, district population, number of charges per case, maximum statutory sentence, number of prior convictions, share of defendants in pretrial detention, defendant age, and the shares of women and foreigners among defendants.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4.5: Effects on other offenses: Case durations

	(1)	(2)	(3)	(4)	(5)	(6)
outcome variables:	robbery	fraud/ embezzlement	against life or health	sex offenses	against family	against public safety
duration from offense to charges						
spillover effect: fast-track share, all cases	56.14	94.14	48.80	−344.1**	−2.112	−68.76
	(102.5)	(77.98)	(52.20)	(168.6)	(91.72)	(88.50)
R-squared	0.299	0.581	0.693	0.226	0.650	0.494
duration from charges to adjudication						
spillover effect: fast-track share, all cases	−92.14	1.765	−52.80	149.1	8.567	−58.75
	(97.45)	(65.84)	(60.27)	(96.73)	(40.38)	(82.66)
R-squared	0.620	0.704	0.693	0.636	0.696	0.536
observations	936	946	946	929	946	945

The table reports the coefficients on the share of fast-track cases and their standard errors (clustered by district).
The unit of observation is district, offense, and year. All regressions include district trends and year fixed effects.
All regressions include the following average characteristics of cases and districts: crimes per police officer, number of cases per prosecutor/senate, district population, number of charges per case, maximum statutory sentence, number of prior convictions, share of defendants in pretrial detention, defendant age, and the shares of women and foreigners among defendants.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4.6: Effects on other offenses: Probabilities of case outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
outcome variables:	robbery	fraud/ embezzlement	against life or health	sex offenses	against family	against public safety
probability of charges						
spillover effect: fast-track share, all cases	0.0836	0.0460	−0.0377	0.119	0.0988***	0.120
	(0.0748)	(0.0457)	(0.0884)	(0.143)	(0.0325)	(0.101)
R-squared	0.368	0.577	0.635	0.306	0.562	0.582
probability of conviction						
spillover effect: fast-track share, all cases	0.0347	−0.0227	0.0328	−0.130	−0.0316	0.0269
	(0.0949)	(0.0638)	(0.0585)	(0.113)	(0.0395)	(0.0754)
R-squared	0.290	0.442	0.592	0.240	0.469	0.354
observations	936	946	946	929	946	945

The table reports the coefficients on the share of fast-track cases and their standard errors (clustered by district). The unit of observation is district, offense, and year. All regressions include district trends and year fixed effects. All regressions include the following average characteristics of cases and districts: crimes per police officer, number of cases per prosecutor/senate, district population, number of charges per case, maximum statutory sentence, number of prior convictions, share of defendants in pretrial detention, defendant age, and the shares of women and foreigners among defendants.

*** p<0.01, ** p<0.05, * p<0.1

Table 4.7: Accounting for the effect of the fast-track procedure

Duration from offense to charges

offense category	actual duration 2001	change in actual duration, 2001–2008	change in counterfactual duration, 2001–2008	fast-track accounts for
theft/burglary	167.79	−32.87	−12.76	−20.11
driving offenses	163.22	−115.85	−55.82	−60.02
against personal liberty	215.65	−39.51	−33.45	−6.06
property/economic offenses	528.16	−71.72	−17.46	−54.26
against public order	207.18	−19.92	−24.88	4.95
all covered offenses	256.40	−55.97	−28.87	−27.10

Probability of charges

offense category	actual probability 2001	change in actual probability, 2001–2008	change in counterfactual probability, 2001–2008	fast-track accounts for
theft/burglary	0.66	0.17	0.10	0.07
driving	0.88	0.05	−0.04	0.09
against personal liberty	0.63	0.12	0.05	0.07
property/economic offenses	0.61	0.08	0.06	0.02
against public order	0.67	0.11	0.06	0.05
all covered offenses	0.69	0.11	0.05	0.06

Chapter 5

Responses to More Severe Punishment in the Courtroom: Evidence from Truth-in-Sentencing Laws[1]

5.1 Introduction

Laws that impose more severe punishments on criminals sometimes bring unexpected consequences. Their direct objective – to deter and incapacitate offenders by keeping them longer in prison – may be mitigated by the behavioral responses of judges, jurors, and prosecutors who exercise a certain amount of discretion at various stages of the criminal procedure. Judges and jurors may become more reluctant to convict, judges may impose a shorter sentence, and prosecutors may adjust their plea bargaining tactics. Understanding the character and empirical magnitude of the behavioral responses has important policy implications. Since legislators cannot fully control the choices of judges, jurors, and prosecutors, they should take the

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mitigating responses into account when designing sentencing policies. Legislating longer sentences may be undesirable both on the grounds of efficiency as well as fairness if the mitigating responses are large enough.

This paper presents evidence on mitigating responses by evaluating the effects of the so-called Truth-in-Sentencing (TIS) laws on the outcomes of criminal cases. The TIS laws, adopted by many U.S. states during the 1990s, require convicted offenders to serve at least 85 percent of their imposed prison sentence. This implies a stark increase in the fraction of the sentence served compared to the 1980s and early 1990s when prisoners served 48 percent on average (Ditton and Wilson 1999), mostly due to discretionary early release by parole officers and by prison overcrowding. If the probability of conviction and the imposed sentences had not changed after introducing the TIS laws, an offender could spend 70 percent more time in prison than previously expected.

Several states (e.g., Pennsylvania and Utah) and the federal government imposed TIS-type requirements prior to the 1990s (U.S. Department of Justice, 1993). The Federal Violent Crime Control and Law Enforcement Act of 1994[2] encouraged more states to adopt such provisions by introducing the so-called Violent Offenders Incarceration and the Truth-in-Sentencing Incentive Grant Program. To be eligible for the TIS grant, a state had to implement a TIS law that required offenders convicted of a Part I violent crime[3] to serve no less than 85 percent of the sentence imposed, or a similar law that effectively resulted in such offenders serving on average at least 85 percent of the sentence.[4]

The timing of adoption of the TIS laws by individual states varied (see Table 5.1). While only two states (plus the District of Columbia) had TIS-type provisions in the early 1990s, eleven other states adopted the TIS laws within one year of the passage of the Violent Crime Control and Law Enforcement Act of 1994. Twenty-seven states and the District of Columbia met the eligibility criteria by 1998.[5] The states also varied in the scope of coverage of the TIS laws; the 85 percent requirement applied to Part I violent felonies in all adopting states, but in some states it applied

[2]Public Law 103–322, Sept. 13, 1994 (the "1994 Crime Act").
[3]Part I violent crime includes murder, rape, robbery, and assault.
[4]For more detail on the criteria, see the U.S. Department of Justice (2005).
[5]These states received \$2.7 billion in total during 1996–2001 through the VOI/TIS grant program (U.S. Department of Justice, 2005).

to other crimes as well.

The variation among the states in the timing of adoption and the types of crime covered allows us to identify the effects of the TIS laws on case outcomes using a difference-in-differences-in-differences estimator. The data set – State Court Processing Statistics (SCPS) – consists of a large sample of felony cases from the most populous counties in the United States and allows us to control for many observable case characteristics.

The paper contributes to the empirical literature on behavioral responses in criminal procedure in several ways. First, it captures the various margins of responses in the criminal justice process in two simple summary measures. One measure is the change in the probability that an arrested offender is eventually convicted, irrespective of whether at trial or by pleading guilty. Indeed, we find that it fell by 9 percent. The other measure is the change in the imposed sentence that an arrested offender receives at the final disposition of the case, which is either the actual sentence imposed on a convicted defendant or a zero sentence imposed on an offender that is not convicted. It gives a particularly useful summary of the behavioral responses as the changes in the probability of dismissal, guilty plea, conviction at trial, and the sentence imposed upon conviction are reflected into the sentence that is ultimately imposed. It can also be interpreted as a change in the sentence that an offender can expect conditional on arrest. The TIS laws reduced the imposed sentence conditional on arrest by 8 percent according to our most preferred specifications.

The behavioral responses mitigated the intended effect of the TIS laws to impose more severe punishment. In the absence of behavioral responses, the sentence actually served, conditional on arrest, would have increased by 70 percent on average. As the sentence imposed, conditional on arrest, fell by 8 percent, the sentence that an arrested offender can expect actually to serve increased not by 70 percent but by "only" 56 percent.[6] Therefore, the unintended behavioral responses removed about one-fifth of the intended increase in the severity of punishment. The mitigating responses are empirically relevant and should to be taken into account when designing

[6]The expected sentence actually served was 50 percent of the sentence imposed upon conviction times the probability of being convicted prior to the adoption of the TIS laws. In the absence of behavioral responses, it would rise to 85 percent, a 70-percent increase. The behavioral responses reduced the product of the sentence and the probability by 8 percent. Hence, the new sentence actually served, conditional on arrest, increased to 78 percent (92 percent of 85), which is 56 percent higher than the pre-TIS law level.

sentencing policies.

Second, the paper provides one of the first empirical tests of Andreoni's (1991) proposition that more severe punishment should lead to a lower probability of conviction. While the proposition is widely accepted as theory, empirical evidence has been scant at best. We identified only two empirical studies using data on actual cases. Snyder (1990) finds a reduction in the probability of conviction in antitrust cases as the level of charges for certain antitrust violations was raised from misdemeanor to felony. Bjerk (2005), who explores primarily the response of prosecutors to the three-strikes laws, also tests whether offenders qualifying for a third-strike offense face a lower probability of conviction at trial but does not find any significant effect.[7] We do find a significant decrease in the overall probability that an arrested defendant is convicted. Further, when investigating the particular channels behind this overall effect, we find that TIS laws reduced the probability of conviction through a higher probability that the case is dismissed, through a lower the probability that the defendant pleads guilty, and, to a lesser extent, through a lower probability of conviction at trial.

Third, the paper adds new results to the empirical literature on the behavioral responses of prosecutors. One line of the literature finds that prosecutors "exploit" enhanced statutory sentences, consistently with models of the prosecutors that maximize the total punishment imposed. Kuziemko (2006) shows that defendants in murder cases in New York were accepting plea bargains with harsher terms after the state reintroduced the death penalty in 1995, while the likelihood that the defendant would plead guilty did not change. Kessler and Piehl (1998) find that California's Proposition 8, a popular initiative that mandated enhanced sentences for offenders with certain criminal histories caused an increase in sentences for those crimes that were subject to Proposition 8 as well as for crimes that were factually similar but were not subject to Proposition 8.

Another line of the literature instead finds that the prosecutors mitigate enhanced statutory sentences, which is rather consistent with the view that prosecutors use

[7] Bjerk's result may plausibly be explained by sample selection. The three-strikes laws made it more likely that a felony defendant with two prior strikes would have charges reduced to a misdemeanor (resulting in cases with relatively stronger evidence being prosecuted as felonies) and that he would not accept the plea bargain (resulting in cases with relatively stronger evidence being continued to trial). The shift in the distribution of cases reaching trial shifts the probability of conviction upward, offsetting the predicted behavioral response.

their discretion to apply broader social norms of justice and fairness in punishment. Bjerk (2005) studies the impact of the three-strikes laws which dramatically enhanced prison sentences for criminals with at least two prior violent felony convictions. The prosecutors became more likely to reduce the charge from felony to misdemeanor when the defendant was at risk of receiving a three-strike sentence. Walsh (2004) documents that between 25–45 percent of offenders eligible for a three-strike sentence in urban counties in California have their prior strikes dismissed.[8]

According to our findings, the probability that the defendant would plead guilty decreased by 10 percent, and the probability that the prosecutors would reduce charges from felony to misdemeanor decreased by 4 percent. Pleading guilty apparently became a less favorable alternative to trial; these findings rather support the "exploiting" view of the prosecutors.

Fourth, the paper provides interesting results on the heterogeneity of the responses. The TIS laws were designed primarily to punish violent criminals more severely, although about one third of the states apply them to non-violent crimes as well. The behavioral responses to the TIS laws were more pronounced for non-violent crimes, i.e., those crimes at which the laws were not primarily targeted. Judges and prosecutors appear to respond more strongly when the actual content of the law deviates from its stated objectives.[9]

Last, the paper also provides several policy-relevant findings about the effects of the TIS laws themselves. So far, Shepherd (2002) has analyzed their deterrence effects. Using a county-level panel, she estimates the effect of the TIS laws on crime rates, arrest rates, and the median prison sentences. She finds that the arrest rates increased with the introduction of TIS laws as the states that introduced the TIS laws tended to adopt a "tough on crime" attitude, and the police made more effort to

[8] The findings by Bjerk (2005) and Walsh (2004) can alternatively be rationalized as an optimal response by prosecutors who maximize the average sentence or number of convictions at trial subject to the resource constraint. Realizing that the judge or jury will be very reluctant to convict a defendant with two prior strikes when the punishment for the third-strike offense is deemed too severe (typically a situation when the third strike is relatively a petty crime), the prosecutor anticipates that winning the case would require substantial resources that would no longer be available for other cases. Offering "softer" terms to the defendant is then optimal even for a prosecutor who maximizes the average sentence and does not necessarily indicate an intentional objective to mitigate very long sentences.

[9] Such a selective response is presumably possible only if the judges and prosecutors share the stated objective of the legislation, which apparently was the case with the TIS laws (Shepherd, 2002).

arrest. Similarly she finds an increase in the imposed prison sentences. Her estimates can be interpreted as evidence of judges and prosecutors not offsetting an increase in the severity of punishment; alternatively, they can be interpreted as evidence of other "tough on crime" policies that were correlated with the adoption of the TIS laws. Our empirical strategy differs from that of Shepherd; we use case-level as opposed to county-level data and our "difference-in-differences-in-differences" estimator allows us to control for the unobservable "tough on crime" policies. In addition, we provide new findings of a substantial reduction in the probability of conviction and an overall reduction in the sentence imposed conditional on arrest. Our other findings, namely the reduction in the plea rate and an overall increase in the sentences imposed upon conviction, generally concur with those of Shepherd. Owens (2010), using the same data set as we do, detects a particular response to the TIS laws in the criminal procedure – people who were arrested for an offense covered by the TIS law but pleaded guilty to a misdemeanor (to which the TIS requirement does not apply) were punished with relatively harsher sentences. Our paper instead evaluates the impact of the TIS laws on on a broader range of case outcomes and on the overall punitiveness of the criminal justice process.

5.2 Theoretical predictions

This section discusses the behavioral responses to the TIS laws predicted by the theoretical literature. Simple expressions of measurable case outcomes organize our thinking:

$$S^S = S^C \cdot f, \tag{5.1}$$

$$E[S^S|arrest] = p \cdot S^C \cdot f = \big(p \cdot S^C + (1-p) \cdot 0\big) \cdot f = S^A \cdot f. \tag{5.2}$$

The punishment suffered by a convicted defendant is the sentence actually served in prison S^S, which is a product of the sentence imposed upon conviction S^C and the fraction of the sentence that is actually served f. The expected punishment facing an arrested defendant is the expected sentence actually served in prison $E[S^S|arrest]$, which in turn, is the product of the probability p that he is convicted (conditional on arrest), the sentence imposed if convicted, and the fraction actually served. The sentence if not convicted is, of course, zero. Adding the outcome under non-conviction to the expression in equation 5.2 shows straightforwardly that the expected sentence actually served in prison can also be stated as the expected sentence imposed (conditional on arrest) S^A multiplied by the fraction of the sentence served. The variable S^A summarizes adjustments in the probability and the sentence into a single measure of punishment that is produced as an "output" of the criminal procedure.

TIS laws exogenously shifted the fraction f upwards by a certain percentage, and they would have, *ceteris paribus*, mechanically increased the expected sentence actually served $E[S^S|arrest]$ by that same percentage. However, the probability of conviction and the sentence upon conviction are determined endogenously, and as a result, $E[S^S|arrest]$ may have increased by less than the mechanical change. We estimate how the variables that are determined inside the courtroom, p, S^C, and S^A, respond to a change in f. (We unfortunately cannot estimate the effect of the TIS laws on $E[S^S|arrest]$ since the data on prison releases do not cover enough years after the adoption of the laws.)

The predicted effect of the TIS laws on the probability of conviction follows a well-known model by Andreoni (1991). As the sentence actually served in prison increases, the social cost of convicting an innocent defendant also increases. The judge or jury who cares about the social costs of wrongful conviction then requires a higher standard of proof to convict a defendant.[10] The conviction rate among the cases resolved at trial should therefore fall. A similar trade-off may operate at other stages of the criminal procedure, such as the decision whether to dismiss a case.

In the plea bargaining process, changes in case outcomes reflect behavioral responses of the prosecutor (the terms of the plea bargain he offers) and the defendant (willingness to accept the terms). The predicted effects also depend on a particular model of the prosecutor, where the existing literature offers two broad views: According to one, the prosecutors are maximizing a well-defined deterrence objective, such as the total punishment imposed.[11] According to another, they pursue broader objectives of justice and fairness and apply punishment in accordance with these objectives.[12] Even though some predictions are ambiguous, certain observed effects

[10] Ezra and Wickelgren (2005) reach the same prediction in an alternative model where the population of defendants is endogenous.

[11] The classical references Landes (1971), Easterbrook (1983), and Reinganum (1988, 2000).

[12] Grossman and Katz (1983) and Miceli (1996) formally model the justice-pursuing prosecutor.

of the TIS laws allow us to discriminate between these alternative views. A reduction in the plea rate is predicted by the "maximizing" view of the prosecutors, while an increase is possible under both views. A decrease in the probability that the prosecutor reduces charges is predicted by the "maximizing" view and an increase by the "justice-pursuing" view.

In the "maximizing" models of the prosecutorial behavior, the prosecutor typically offers a sentence that makes the defendant indifferent between accepting the plea or going to trial.[13] If the TIS law applies irrespective of whether the defendant pleads guilty or is convicted at trial, the sentence to be actually served S^S rises mechanically as f increases for both trial and plea convictions. The prosecutor then need not adjust the imposed sentence S^C to make the defendant indifferent.[14] However, pleading guilty frequently implies being convicted of less severe charges compared to a potential conviction at trial. In such situations, the TIS laws may apply under the trial conviction but need not apply under the plea conviction. A maximizing prosecutor should then offer a longer sentence S^C or be less likely to reduce the charges. The prosecutor essentially "exploits" the increased gap between the actual sentence served under trial and under the plea and offers the defendant less favorable terms in the plea bargain.

The predicted impact on the defendants' plea choice is theoretically ambiguous. On the one hand, they would be more likely to plead guilty if the TIS law applies only to the trial sentence. On the other hand, the plea rate may fall if the prosecutors offer tougher bargains because of the TIS laws. Likewise, the defendants would be less

They assume that the prosecutor's utility depends on the deviation of the actual sentence from the sentence that she deems is most appropriate. The prosecutor also suffers disutility if an innocent defendant is convicted. The structure of the two models leads to either pooling or separating equilibria: either all defendants or the guilty defendants accept the plea. For that reason, their testable predictions correspond only indirectly to the testable predictions in our context. For example, in Miceli (1996), the plea sentence stops increasing in the statutory sentence and the equilibrium shifts from separating to pooling if the statutory sentence exceeds a certain threshold. In our context, these predictions would imply that the sentence would not change in response to the TIS law, and the probability of accepting the plea bargain would rise if the prosecutors find the new sentences to be too harsh.

[13] If the offenders are of different types (e.g., when they have imperfect information about the strength of evidence against them) and the prosecutor cannot distinguish their type, the optimal sentence offered involves only a marginal defendant being indifferent between the plea and trial: While defendants who think the case against them is weak strictly prefer a trial, those who think the case against them is strong strictly prefer pleading guilty.

[14] Whether he would optimally adjust the offered sentence upward or downward depends on the details of the model. For example, the very basic version of the Landes (1971) model with risk-neutral defendants and positive costs of trial predicts that the prosecutor should reduce the maximum sentence offered.

likely to accept the plea if they take into account that the probability of conviction at trial decreases.

In the "justice-pursuing" view of the prosecutors, the prosecutors may regard the increase in f as a departure from the prevailing norms of justice and use their discretion to mitigate its impact. They would then offer a shorter sentence S^C and be more likely to reduce charges. As a result, the defendants should be more likely to accept plea bargains.

In the sentencing stage, the judges may offset a higher fraction of the sentence actually served in prison simply by imposing shorter sentences. This would be particularly the case if they regard the mandated increase in the fraction of the sentence served as unjust.[15]

The preceding discussion of the particular behavioral responses implies predictions for the sign of our summary measures. The overall probability p that an arrested offender is convicted (by pleading guilty or at trial) is expected most likely to fall, although there is a theoretical possibility that it could rise if the prosecutors are mitigating the increased actual sentences and defendants become sufficiently more likely to accept plea bargains. The expected imposed sentence conditional on arrest S^A should most likely decrease as the probability of conviction decreases and the judges also reduce the sentences; however, there is a theoretical possibility that it may rise if the prosecutors offer sufficiently harsher sentences in plea bargaining.

5.3 Data and empirical strategy

We use the *State Court Processing Statistics: Felony Defendants in Large Urban Counties (SCPS),* an individual-level data set on approximately 100,000 criminal cases in state courts.[16] The sample covers 45 counties selected from 75 percent of the most populous counties in the United States. It tracks cases that were filed in May of every even year from 1990 to 2002. The universe of the data set is cases

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[15] The legal literature has been concerned with the sentencing implications of parole releases (see Genego, Goldberger, and Jackson (1975) for an early example). The empirical evidence on the relationship between sentences imposed by judges and the anticipation that the offender will be released early is, to our best knowledge, missing.

[16] The data are collected by the Bureau of Justice Statistics. ICPSR study #2038.

initiated by a felony arrest.[17] Due to missing values for relevant variables in some observations, the sample used in regressions has over 83,000 observations.

The SCPS data set contains rich information on each case: offender characteristics such as age, sex, and detailed prior record, information about the procedural aspects of the case (pretrial detention, type of attorney), and the final disposition of the cases including the length of the maximum jail or prison sentence, if applicable. The offenses are divided into 16 categories: violent crimes (murder, rape, robbery, assault, and other violent crimes) and non-violent crimes (burglary, larceny-theft, motor vehicle theft, fraud, other property crime, drug sales, other drug crimes, and four other minor categories). The data are summarized in the first column of Table 5.2.

Figures 5.1 through 5.4 illustrate the effects of the TIS laws on several case outcomes. Figure 5.1 shows the evolution of the average probability of conviction, conditional on arrest, across all states that adopted the TIS laws between 1990 and 2000. The time axis measures the number of years before and after the adoption of the TIS law.[18] The left panel depicts the violent crime cases which all became eligible under TIS laws. The right panel depicts the non-violent crime cases, which are further divided into those that became eligible under TIS laws and those that did not.[19] The probability of conviction was increasing in all types of cases prior to adoption. It declined by 5 percentage points after TIS adoption in violent cases and by 16 percentage points in non-violent cases eligible under TIS law. However, there was no such decline in non-violent cases that were not eligible under TIS law.

The probability that a case is dismissed (Figure 5.2) had been declining prior to the adoption of the TIS law in all types of cases and the trend was reversed in eligible cases – both violent and non-violent – after adoption. Again, there was no such trend reversal in ineligible non-violent cases. The probability of accepting a plea

[17] About 15 % of cases end up adjudicated as misdemeanors.

[18] The SCPS data records observations on arrests made in May every even year, hence we observe different "distances" from the time of adoption for different states depending on the exact time of adoption. If a state adopted the TIS law in an even year (and no later than in May), zero on the time axis indicates the year of adoption, two indicates two years since adoption and so on. If a state adopted the TIS law in an odd year (or after June in an even year), the first time we observe cases for such a state is already one year after the adoption. Hence zero on the time axis indicates the first year since adoption, two indicates the third year, etc.

[19] For the years before the adoption of the TIS laws, a case is classified as eligible under the TIS law if it would have been eligible under the TIS law that was eventually adopted in that state.

bargain (Figure 5.3) declined only temporarily in violent crime cases. It declined permanently by a full 15 percentage points in TIS-eligible non-violent cases while it increased slightly in ineligible non-violent cases. The picture is different for the length of the prison sentence imposed on convicted defendants (Figure 5.4). The length of sentence for violent crimes did not change noticeably. For non-violent crimes, it declined, albeit temporarily, in eligible cases but not in ineligible cases. However, the decline appears to be a continuation of a prior downward trend in eligible cases.

The figures indicate that the TIS laws had the predicted effects on the probabilities of conviction, dismissal, and plea, that such effects were more pronounced in non-violent than in violent cases, and that they had rather limited effects on the length of the sentence upon conviction. These preliminary findings are generally supported in the regression specifications that follow.

The empirical strategy is based on a "quasi natural experiment", which compares the treatment cases (those covered by the TIS laws) with appropriately chosen control cases. We adopt two alternative "difference-in-differences-in-differences" (D-i-D-i-D) estimators, formally stated as

$$Y_{icst} = f(TIS_{icst}, TISstate_{st}, X_{icst}, \lambda_{ct}, \lambda_{av}, \epsilon_{icst}), \tag{5.3}$$

$$Y_{icst} = f(TIS_{icst}, TISstate_{st} \times violent_{icst}, X_{icst}, \lambda_{ct}, \lambda_{av}, \epsilon_{icst}), \tag{5.4}$$

where i, c, s, and t denote the individual case, offense type, state, and year, respectively. Additionally, a denotes county and v denotes violent crime. Y_{icst} stands for the outcome variable, and TIS_{icst} is a dummy variable indicating whether the individual case is covered by the TIS law; that is, the case is eligible under the TIS law and the TIS law is in force.[20] $TISstate_{st}$ is a dummy variable equal to one if a state has the TIS law in force. $TISstate_{st} \times violent_{icst}$ is a dummy variable equal to one if a state has adopted the TIS laws and a given offense is a violent felony.

[20]The TIS case dummy may change for a given case during the criminal process. For example, the person may be arrested for a violent felony, and if convicted for a violent felony, the TIS law would apply. However, he may be convicted for a misdemeanor, and the TIS law would no longer apply. In the regressions, we set the TIS law according to the offense type that the offender is charged with at the relevant stage of the criminal process.

X_{icst} is a vector of individual characteristics of the offender and the case.[21] Finally, we include offense-year fixed effects λ_{ct} and county-violent crime fixed effects λ_{av}.[22] The offense-year fixed effects control for unobserved heterogeneity at the level of each offense and year. Compared to commonly used offense and year fixed effects, they impose less restrictive assumptions on the structure of the unobservables and allow, for example, separate national trends in the outcomes of criminal cases for each offense. The county-violent crime fixed effects control for unobserved heterogeneity at the county level, further disaggregated for violent and non-violent crimes. In alternative specifications, we include state-offense fixed effects instead.[23] ϵ_{icst} is an error term.

We use the D-i-D-i-D estimator, as opposed to the more conventional difference-in-differences (D-i-D) estimator since the identifying assumption for the latter is unlikely to hold. It would require that there was no differential change between the adopting and non-adopting states in the unobservables that affect outcomes in the offenses covered by the TIS laws after the adopting states implemented them. However, the states adopting the TIS laws may have adopted other "tough on crime" policies precisely because the objective of the laws was to punish certain crimes more severely. If that was the case, the error term may be correlated with the TIS_{icst} case dummy variable.

Our first specification (equation 5.3) therefore includes a TIS state control (variable $TISstate_{st}$). It captures the effect of state-specific unobservable variables that are potentially correlated with the adoption of the TIS laws and affect all crimes equally. The effect of the TIS laws is estimated from a within-state comparison of the change in the outcome for the crimes covered by the TIS laws with the crimes that are not covered. It is identified under the assumption that *within a state* the unobservable characteristics of TIS offenses and other offenses followed the same trend even though

[21] Prior felony convictions (measured by dummies for 1, 2, and 3 or more prior convictions), number of prior misdemeanor convictions, log age, log age interacted with the prior conviction dummies, gender dummy, race/origin dummies (white non-hispanic, black non-hispanic, hispanic, and other), and type of attorney (public, private, assigned, *pro se*, and others) are included in the X vector.

[22] Represented by the interactions of county dummies with a dummy variable equal to one for violent offense and zero for other offenses.

[23] Ideally, we would include the county-offense fixed effects. However, there are too few observations for many county-offense combinations which prevent a meaningful estimation. The county-violent crime fixed effects or state-offense fixed effects are therefore workable compromises, still superior to a specification with only county or state fixed effects which assumes away any differences in unobserved heterogeneity between offense types within states.

they may not follow the same trend in the adopting and non-adopting states. In other words, the adopting states may have gotten "tougher on crime" than the non-adopting states but then did so equally for all crimes.

The second specification (equation 5.4) exploits the fact that violent felonies are covered by the TIS laws in all states that adopted them while property, drug, and other non-violent crimes are covered only in some states. It includes a TIS state – violent crime interaction (variable $TISstate_{st} \times violent_{icst}$,) which captures the effect of unobservables that are correlated with the adoption of the TIS laws and affect violent crimes only. The effect of the TIS laws is estimated from a between-state comparison of the change in the outcome for non-violent crimes in the states that imposed the TIS requirement on both violent and non-violent crimes with the states that imposed the TIS laws on violent crimes only. The estimates are identified under the assumption the adopting states may have gotten "tougher" on violent than on non-violent crimes but must have gotten proportionately tougher on violent crimes irrespective of whether they imposed the TIS laws on all crimes or just violent crimes.[24]

A possible change in the sample composition poses a concern. The TIS laws have been accompanied by more intensive policing (Shepherd 2002). As the police arrests a larger fraction of offenders, it is possible that it also arrests a different sample of offenders; namely, the marginal offenders now being apprehended are likely to be those who are more difficult to identify. The evidence against such offenders is likely to be weaker, and they are less likely to be convicted. As a result, the average probability of conviction may fall even in the absence of any behavioral response. The importance of this problem can be checked by comparing the observable characteristics of cases before and after the adoption of the TIS laws; presumably, should there be a change in the sample composition of observables, it is quite likely that the unobservables changed as well. Table 5.2 show the sample means for the observable characteristics of cases in the last year in the SCPS data set before the TIS laws were adopted and in the first year after the adoption.[25] The table does not

[24] Admittedly, the estimates are not identified if states imposed the TIS laws on certain crimes and targeted other "tough on crime" policies on the same crimes. Unfortunately, there is no case-level variation within a particular crime (which would be the case if the TIS laws applied only to offenders with certain characteristics, for example).

[25] The data set records arrests made in May of an even year. For the two states that adopted the TIS laws in the first few months of an even year, we use the observations two years after the

show discernible changes in the observable characteristics. The only exception is the share of defendants who use a public defender, which rose by 10 percentage points in violent and by 11 percentage points in non-violent crime cases. This may indeed reflect a change in the strength of evidence, but the bias would rather go against the predicted effects (public defenders tending to represent in less defensible cases). We further address the sample composition issue in two robustness checks (section 5.4.7) with little effect on the results.

5.4 Results

This section presents the results in two steps: First, we present the summary measures: the reduction in overall probability of conviction conditional on arrest and the decrease in the length of sentence imposed conditional on arrest. Then we investigate specific channels behind the two summary findings[26] and the heterogeneity of behavioral responses across different offense categories.

5.4.1 Probability of conviction conditional on arrest

Our first summary measure of the effects of the TIS laws is the change in the probability that an arrested offender is eventually convicted, irrespective of whether via plea bargaining or conviction at trial. The marginal effects from probit estimates are presented in Table 5.3. They imply a reduction in the probability of conviction by 9 percent. This result is robust to alternative specifications – controlling for the TIS state or the TIS state – violent crime interaction (columns 1 and 2) or for replacing

adoption to allow the effect of TIS laws to be fully realized for the purpose of this before-after comparison.

[26]Two of the specific channels (the probability of conviction at trial and the length of sentence upon conviction) are estimated on sub-samples of cases at different stages of the criminal procedure. The natural concern is that results for those channels are possibly affected by sample selection. The TIS laws may have changed the distribution of unobservable characteristics of cases that result in conviction or that proceed to trial. For example, if the TIS laws reduce the fraction of cases settled in plea bargaining, the marginal offenders now proceeding to trial would face longer potential sentences than the average offender previously proceeding to trial. Unfortunately, we do not have instruments that would be correlated with the likelihood that the case proceeds to the subsequent stage and at the same time would not be correlated with the error term in the outcome equation in that stage. We still think it is preferable to present such results as tentative evidence and interpret them with caution. The majority of the channels (the probability that the case is dismissed, the probability of pleading guilty, and the probability that the prosecutor reduces charges) are estimated on the full sample and hence are not affected by sample selection.

the county-violent crime fixed effects with state-offense fixed effects (columns 3 and 4).[27] In all specifications, the marginal effects of the TIS case dummy are significant at the 1 percent level.

We also report the marginal effects of the TIS state and the TIS state – violent crime controls to demonstrate the appropriateness of the D-i-D-i-D estimator.[28] The coefficients of these two controls imply that the introduction of the TIS laws was associated with an overall increase in the probability of conviction, including the cases that were not subject to the TIS laws, on the order of 4 to 11 percent. Correspondingly, our estimates are different from the simple D-i-D estimates; when we exclude the $TISstate_{st}$ or the $TISstate_{st} \times violent_{icst}$ controls such that the specification is reduced to D-i-D, the marginal effect of the TIS_{icst} dummy becomes smaller in magnitude (-0.069). Even though these regressions do not directly estimate the choices by judges and juries, they nevertheless provide strong support for Andreoni's prediction in the sense that the criminal justice system convicts less if the sentences to be actually served are raised.

5.4.2 Sentence imposed conditional on arrest

The second summary measure of the behavioral responses to the TIS laws is the change in the sentence imposed conditional on arrest S^A. It is obtained by estimating equations 5.3 and 5.4 on the full sample of arrests, the dependent variable being the logarithm of the maximum prison or jail sentence imposed (in months). If the defendant was not convicted, the sentence in the regressions is set to zero.[29]

We estimate Tobit and quantile regressions instead of the conventional OLS for several reasons. The observed sentences are naturally censored at zero. They should also be censored at a very high sentence length since the requirement to serve 85 percent out of a 70-year maximum sentence may be of little practical significance. We therefore run Tobit regressions with the lower bound set at zero and the upper bound

[27] We also estimated alternative specifications which included a dummy variable for the presence of sentencing guidelines in a state and its interaction with the TIS case dummy; the key findings of the effects of the TIS laws were unaffected.

[28] The marginal effects on the two controls are 0.041 and 0.07 in the specification with county-violent crime fixed effects, and 0.1 and 0.11 in the specification with the state-offense fixed effects.

[29] The sentence is set to zero if the defendant was convicted but was punished with a fine instead of a prison or jail sentence. To deal with the logarithm of zero, we add one month to each sentence.

at 55 years.[30] Also, we expect the impact of the TIS laws to be more pronounced the longer the potential sentence is since the difference between serving, say 5 weeks or 8.5 weeks out of a 10-week maximum sentence may not be of such a concern to the judge than the difference between serving, say 5 years or 8.5 years out of a 10-year maximum sentence. The natural tool to address this issue is a quantile regression estimated at several quantiles. It predicts a change in a given quantile of the distribution of the dependant variable due to a change in the independent variable.

Table 5.4 shows the Tobit estimates. In the specifications with the offense-year and county-violent crime fixed effects, the marginal effect of the TIS case dummy is -0.114 when the TIS state control is included (column 1) and -0.097 when the TIS state-violent crime control is included (column 2). Both are significant at the1 percent level. In the specifications with offense-year and state-offense fixed effects, the marginal effects are smaller in magnitude (-0.083 and -0.039 for the respective controls, columns 3 and 4, and significant at the level of 1 and 10 percent).[31]

The estimates of the quantile regressions for the 75th and 90th quantiles are shown in Table 5.5.[32] They demonstrate that the behavioral response leading to shorter expected sentences was concentrated on the longest sentences, conditional on other factors. The marginal effects of the TIS case dummy are several times smaller in magnitude at the 75th quantile (columns 1 and 3) than at the 90th quantile although all of them are statistically significant at 1 percent level.

Both sets of regressions show fairly consistently that offenders covered by the TIS laws experienced a reduction in the sentence that they can expect at the time of arrest compared to offenders not covered. The reduction was not trivial; we regard the average of the four Tobit estimates (8.3 percent) as the most preferred "summary" result.

[30] As an alternative, we estimated the Tobit model with the lower bound equal to 4.5 months – treating non-convictions and convictions with short sentences as equivalent outcomes, with little effect on the results.

[31] The marginal effects of the TIS state and TIS state – violent crime interaction controls are positive as expected and significant at the 1 percent level. The unobserved factors that they capture increased the expected sentence by between 10 to 23 percent, depending on the specification.

[32] The quantile regressions are estimated at the 75th and 90th percentiles only. They could not be estimated at lower quantiles since zero sentence represents most observations for the 50th or lower quantiles, leaving almost no variation in the dependant variable.

5.4.3 Probability of conviction disentangled

The TIS laws may have reduced the likelihood of eventual conviction through three channels: a lower probability of conviction at trial, a higher probability that the case is dismissed before reaching a verdict on merits, or a lower probability that the offender accepts a plea bargain. The first two columns of Table 5.7 estimate the effect of the TIS laws on the probability of conviction at trial. They show a statistically significant reduction (by 9.8 percent) in the specification with the TIS state control and smaller and insignificant (5.2 percent) reduction in the specification with the TIS state – violent crime interaction.[33]

Columns 3 and 4 of Table 5.7 estimate the magnitude of the second channel by probit regressions with a dependant variable equal to one if the case was dismissed. The marginal effects of the TIS case dummy are 0.051 and 0.035 in the two basic specifications, and both are significant at the 1 percent level.[34] The tendency to convict less apparently applies to other stages of adjudication and not just to conviction/acquittal verdicts at trial. Unfortunately, we cannot say to what extent the higher probability of a dismissal is due to more dismissals by the judges during the pre-trial reviews and preliminary hearings or by the prosecutors since both are theoretically plausible.

5.4.4 Plea bargaining

The next set of probit regressions estimates the effect of the TIS laws on the likelihood that the case outcome is a guilty plea (columns 5 and 6). The estimates show a 9.5 percent reduction in the specification with the TIS state control and an 11 percent reduction in the specification with the TIS state – violent crime interaction.

The reduction in guilty pleas did not come about mechanically due to the fact that more cases were dismissed and therefore fewer cases were left to be potentially

[33]The results have to be interpreted with caution since the trial cases consist of a highly selected sample. The selection, however, rather induces an upward bias. As the TIS laws induced fewer cases to be resolved through plea bargaining, the marginal defendants who would have plead guilty now proceed to trial. However, the evidence against such defendants would be stronger than the average defendants who proceed to trial, implying an increase in the probability of conviction. The relatively small sample size (4363 cases) inevitably limits the statistical significance of the results.

[34]The coefficients on the TIS state and TIS state – violent crime interaction controls are negative, again indicating a presence of other "tough on crime" factors that tended to reduce dismissals.

resolved through plea bargaining. When the regressions are re-estimated on a subsample of cases that were resolved either through plea bargaining or at trial, the marginal effects of the TIS case dummy are statistically significant at the 1 percent level although somewhat smaller in magnitude (-4.1 and -7.2 percent in the two alternative specifications).[35]

As the data do not record the exact terms that the defendants were offered in the plea bargaining process, we can only partially infer whether the reduced probability of accepting a plea bargain is due to the defendants being less willing to plead guilty holding the terms of the plea bargain constant or due to the prosecutors offering relatively worse terms. The SCPS data allow us to check two channels through which the prosecutors can make the bargains less generous: by being less likely to reduce the charge from felony to misdemeanor (while all defendants in the data set were initially arrested with a felony charge) or by being less likely to reduce the charge to a felony which carries a shorter sentence. Results from a probit dependant variable equal to one if the case was adjudicated as a misdemeanor (columns 1 and 2) of Table 5.6 show a significant reduction in the likelihood that the charges would be reduced to a misdemeanor (by 4 or 2.7 percent, respectively, depending on the controls). The next two columns report marginal effects from probit regressions where the dependant variable is equal to one if the predicted sentence for the offense for which the case was adjudicated is shorter than the predicted sentence for the offense for which the defendant was arrested.[36] The sample is restricted to cases that were adjudicated as felonies (to isolate the reductions to a misdemeanor which we already estimated) and that resulted in conviction since only for conviction cases is the adjudication offense recorded in the SCPS data set. The results show a reduction in the likelihood of reducing charges by 2.3 percent when the TIS state control is included and a smaller (and insignificant) reduction when the TIS state – violent crime control is included.

[35] Detailed results are available upon request.

[36] The dependant variable was constructed as follows: First, we regressed the logarithm of the sentence as a function of offense dummies, year dummies, and county-violent crime dummies in a sample of cases that resulted in a conviction via plea bargaining. Second, we use the coefficients from this regression to predict, for each case in the sample, the sentence for which the defendant was arrested and the sentence for which the case was adjudicated. Third, if the latter predicted sentence is shorter than the former, the variable categorizing whether charges were reduced is equal to one. Across the sample, 11 percent of defendants who are convicted of a felony are convicted of a felony with a shorter sentence than for which they were arrested.

These findings are qualitatively similar to Kessler and Piehl (1998) and tend to support the "maximizing" view of the prosecutors as opposed to the "justice-pursuing" view of the prosecutors. The prosecutors appear to have "exploited" the increase in the severity of punishment by the TIS laws by offering the defendants harsher terms which they, in turn, became less likely to accept. The contrast to Bjerk's (2005) finding that the prosecutors got "softer" in response to the three-strikes laws warrants further discussion. The difference in results can hardly be attributed to the differences in empirical methodology as Bjerk (2005) adopts a very similar D-i-D-i-D empirical strategy, uses the same data, but estimates the prosecutors' response to a different punishment-enhancing policy. We instead hypothesize that the responses of prosecutors (and other enforcement agents in general) to enhanced legislated sentences are inevitably context-specific. If prosecutors regard more severe sentences as unjust, the tendency to "pursue justice" would dominate, and their actions would mitigate the increased severity. On the other hand, if more severe sentences conform to the prosecutors' norms of justice (in a given context), the desire to mitigate is absent, and we observe responses consistent with narrow maximization objectives. The prosecutors apparently shared the objectives of the TIS legislation (Shepherd, 2002), which possibly explains why their observed responses are consistent with the prosecutorial maximization in the context of the TIS laws but not in other contexts.

5.4.5 Length of sentence imposed upon conviction

The last two columns of Table 5.7 show the effects of the TIS laws in the last stage of the criminal procedure, i.e., the sentencing of the defendants who were convicted.[37] Additional control variables are introduced: The plea dummy captures the difference between the sentence in plea and trial cases while its interaction with the TIS case dummy allows us to see whether the TIS laws had a differential impact on sentencing in plea cases vis-à-vis the trial cases. The marginal effects of the TIS case dummy are positive and significant at the 1 percent level (0.223 and 0.260). The marginal effects of the plea-TIS interactions are negative but small and insignificant, -0.053 and -0.058, implying that the TIS laws did not have a discernibly differential effect

[37]The Tobit regressions are equivalent to those estimating the sentence conditional on arrest except that we add a dummy variable for whether the defendant pleaded guilty and an interaction of the plea dummy with the TIS case dummy.

on the sentence length in cases resolved through plea bargaining or trial. The positive coefficient on the TIS case dummy was obtained also when we experimented with alternative specifications.[38]

These results do not support the prediction that the judges would mitigate a higher fraction of the sentence served by imposing shorter sentences.[39] One explanation is that our TIS case dummy is still partially correlated with other "tough on crime" policies even after controlling for the presence of the TIS law in the state, and the resulting upward bias is greater than the behavioral response. The second explanation comes from sample selection for which we were unable to correct. As the cases covered by the TIS laws are more likely to be dismissed, the relatively weaker cases that would have received relatively shorter sentences drop out of the sample. Also, defendants covered by the TIS laws are more likely to reject the plea bargain and go to trial. All else equal (including a sentence received if pleading guilty), the marginal defendant who was indifferent between a guilty plea and a trial expects to receive a longer sentence at trial than an inframarginal defendant who strictly prefers going to trial. If the TIS laws shifted the marginal defendant to choose going to trial, the average sentence at trial would then rise, and the average sentence in plea bargains would fall, as the results suggest.

5.4.6 Offense-specific effects

We also estimate the impacts of the TIS laws specific to individual crime categories: murder, violent crime (other than murder), property, drug, and other crime.[40] Table 5.8 reports the main results from regressions that are equivalent to those in Tables 5.3-5.6, except that the single TIS dummy variable is replaced by interactions of the

[38]Such as including dummy variables for the presence of sentencing guidelines in the state, their interaction with the TIS case dummy, or including state-offense fixed effects instead of county-violent crime fixed effects.

[39]The only rather weak indicators of the offsetting behavior are the offense-specific effects of the TIS laws (Table 5.8). For violent crimes, there is indeed a large and negative effect on the sentence length.

[40]The "violent crime" (other than murder) category includes rape, robbery, assault, and other violent crime; the "property crime" category includes burglary, larceny-theft, motor vehicle theft, forgery, fraud, and other property crime; the "drug crime" category includes drug sales and other drug offenses; the "other crime" category includes weapons-related offenses, driving-related offenses, and other offenses.

TIS dummy with the dummies indicating the five offense categories.[41]

The TIS laws affected the two main outcomes of interest, the probability of conviction conditional on arrest and the sentence imposed conditional on arrest, predominantly among non-violent crimes. The probability of conviction declined by 13.6, 6.9, and 14.5 percent for property, drug, and other crimes, respectively; the sentence conditional on arrest declined by 15.3, 9.9, and 14.3 percent. The estimated effects are significant at the 1 percent level. For violent crimes (other than murder), the results indicate a smaller (5 percent) reduction in the probability of conviction but no significant effect on the sentence imposed conditional on arrest. Almost no estimates are significant for murder.

Similar patterns apply to the particular channels behind the summary measures. The estimated effects of the TIS laws on the increase in the probability that a case is dismissed, the reduction in the probability that the defendant accepts the plea bargain, and the reduction in the probability that charges are reduced to misdemeanor are all larger in magnitude and have smaller standard errors for non-violent crimes than for the violent crimes. On the contrary, the estimates for the sentence imposed upon conviction show large reductions in the sentence for violent crimes but are not significant and have different signs for other crimes.

5.4.7 Robustness checks

Our main results are generally robust to alternative specifications. The first set of robustness checks addresses the concern that the TIS laws altered the distribution of the unobserved characteristics of arrests. If the police make more arrests and the marginal arrests tend to be cases with weaker evidence than the average cases, the probability of conviction would fall. This mechanism may explain the observed increase in the probability that the case is dismissed as the judges and prosecutors "weed out" some of the marginal arrests with particularly weak evidence. If, however, the judges and prosecutors apply the same standard for dismissing the case, the distribution of the strength of evidence in the sub-sample of cases that proceed beyond dismissal should remain constant. Our first robustness check exploits

[41] It is impossible to estimate the specification with the $TISstate_{st} \times violent_{icst}$ interaction variable because all states that adopted the TIS laws covered all violent crimes. The effects on violent crimes overall and sub-categories of violent crimes cannot be separated.

this plausible assumption by re-estimating the model on a sub-sample of cases that were not dismissed.[42] The estimated marginal effects of the TIS case dummy on the probability of conviction are -0.05 and -0.059, depending on the specification (columns 1 and 2 of Table 5.9). They are somewhat smaller than the estimates obtained from the full sample[43] but remain highly statistically significant. Interestingly, the effects of the TIS state and TIS state – violent controls vanish. Likewise for the sentence conditional on arrest, the marginal effects of the TIS case dummy are somewhat smaller than the full sample estimates (-0.095 and -0.082), but they are not different in the statistical sense.

The second robustness check exploits information about the pretrial phase of the case. The defendant is more likely to be released on bail, and the terms of the pre-trial release tend to be more favorable if the evidence is weak or the case is less serious. Should the judges apply the same standards in the pre-trial release decisions under the TIS laws as they did before, the information about pre-trial release is a relevant control for the strength and seriousness of the cases. The SCPS data contain information about the type of pre-trial release granted,[44] the amount of bail set, and the behavior of the defendant during the pre-trial phase.[45] In columns 5–8, we re-estimate the model with dummy variables for each release type, the amount of bail set, and a dummy variable equal to one if the defendant failed to appear.[46] Including these controls has essentially no effect on the estimates in the probability of conviction regressions. In the sentence conditional on arrest regressions, the marginal effect of the TIS case dummy is the same (0.114) when the TIS state control is included and slightly smaller (0.077) when the TIS state x violent crime interaction is included.

The third robustness check addresses the concern that the TIS state and the TIS state – violent crime control may not adequately capture the unobservables affecting the outcomes of violent crimes. We therefore estimate the model on a sub-sample

[42] As a result, the sample is reduced to approximately 62,000 observations.

[43] The confidence intervals of the marginal effects obtained from the full sample do not overlap with the confidence intervals of the marginal effects obtained from the sample excluding the dismissed cases.

[44] The types of pretrial release are categorized as follows: financial release, nonfinancial release, emergency release, held on bail, denied bail, release conditions unknown, detained but reasons unknown.

[45] Whether he failed to appear, became a fugitive, or was re-arrested.

[46] The failure to appear is likely a good indicator of strength of the evidence and eventual conviction.

of non-violent crimes only, reducing the estimator to a simple D-i-D. It comes at a cost of dropping the crimes for which the TIS laws were designed but at a benefit of keeping the crimes for which any confounding effects are likely to be less serious. The estimated effects (-0.098 for the probability of conviction and -0.129 for the sentence conditional on arrest) are similar to those obtained in the full sample and to the offense-specific effects reported for non-violent crimes in Table 5.8.

The last set of checks exploits the variation in the intensity of the TIS laws. There are two sources of such variation. First, while most states followed the federal law and required offenders to serve 85 percent of the sentence, 3 states in our sample opted for 100 percent[47] and 2 states for 50 percent only.[48] Second, the fraction of the time actually served had varied among states and offenses prior to the adoption of the TIS laws. We expect the TIS laws to "bite" more if the offenders had previously served a shorter fraction of the sentence. We ran the same set of regressions, where we replaced the TIS dummy variable (and all interactions) with a continuous variable equal to the predicted fraction of the sentence served.

The predicted fraction is constructed as follows: For cases not covered by the TIS laws, it is computed from the National Corrections Reporting Program (NCRP) data series, individual-level data on approximately 2.9 million prisoners released from prison between 1989 and 2002.[49] The data were collected at the time of release and contain information on the individual characteristics of prisoners, the offense for which they were sentenced, the maximum and minimum sentence to which they were sentenced and the time served under the current admission. The predicted fraction of the sentence served is calculated by dividing the time served by the maximum sentence for each offender and then taking the average for each state-year-offense combination. The information about the time of admission to prison allows us to distinguish which prisoners were sentenced under the TIS laws and which were not. The number of observations for some states[50] is too small to predict the fraction for each state-year-offense. These states were dropped, reducing the number of observations used in the regressions by 7 percent. For cases covered by the TIS laws, we set the predicted fraction to the minimum fraction required by the TIS

[47] Georgia, Pennsylvania, and Virginia.
[48] Indiana, Maryland.
[49] The data is available at http://www.icpsr.umich.edu/cocoon/ICPSR/SERIES/00038.xml.
[50] Arizona, Connecticut, the District of Columbia, Indiana, and Pennsylvania.

legislation in the respective state for the respective offense.[51]

The results are presented in Table 5.10.[52] They are qualitatively and quantitatively similar for the following outcomes of interest: probability of conviction conditional on arrest, probability of conviction at trial, and the probability of reducing charges to a misdemeanor. For example, the marginal effect of the predicted fraction on the probability of conviction conditional on arrest is -0.074, which implies approximately a 2.5 percent reduction in that probability.[53] The marginal effect on the probability of conviction at trial implies a 12 percent reduction in that probability.

Qualitatively the same but quantitatively different estimates are found for the probability of a guilty plea – the effect is also negative but very small and statistically insignificant. For three outcomes the specification with the expected fraction implies qualitatively different results than the TIS dummy: The effects on the sentence conditional on arrest and the probability that the case would be dismissed are statistically insignificant and have the opposite sign. The effect on the sentence imposed upon conviction is negative, statistically significant, and large in magnitude. The last result is at least consistent with the theoretical prediction that judges should respond to the TIS laws by imposing shorter sentences, which was not confirmed in the main regressions (Table 5.7).

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[51]Ideally, we would like to use the predicted fraction served for cases covered by the TIS laws as well. However, we have two reasons why we prefer the legislated rather than predicted fraction. First, the predicted fraction is likely to be downward-biased for the cases covered by the TIS laws. New admissions to prison covered by the TIS laws occur only after the TIS laws are in force (1994 or later in most states). The NCRP data set therefore cannot record releases of prisoners who served 8 or more years post-TIS (and actually more than mere 2 years for those admitted to prison in 2000). Missing observations for releases after 2002 induces a downward bias in the estimate of the fraction since we are more likely to observe prisoners who were released early. Due to this limitation we are also unable to observe post-TIS fraction of the sentence served for very long maximum sentences. Second, it may be more plausible to assume that agents in the criminal process acted upon the expectation that the post-TIS offenders would serve the legislated minimum fraction rather than the ex-post realizations of the fraction.

[52]Due to space limitations, only the coefficients on the expected fraction served and their standard errors are reported. Full results are available upon request.

[53]The TIS laws raised the expected fraction of the sentence served from approximately 50% to 85%, i.e., by approximately 0.35. The coefficients on the fraction served should therefore be divided by 1/0.35 (approximately 3) to obtain estimates comparable to those on the TIS dummy variable.

5.5 Conclusions

Our evaluation of the impacts of the Truth-in-Sentencing laws produced consistent evidence on several channels of behavioral responses to more severe punishment in the criminal justice process. Requiring offenders to serve a higher fraction of their sentence in prison significantly reduced the probability that an arrested offender is convicted. This result represents one of the first empirical tests of the popular Andreoni (1991) model. Moreover, the magnitude of the reduction (9 percent) is empirically relevant and suggests that this line of behavioral response should be seriously considered in the design of sentencing policies.

The overall effect of the TIS laws was a reduction in the imposed sentence expected upon arrest. The stated intention of the TIS laws to increase criminal punishment was therefore mitigated by the behavioral responses on several margins. The magnitude of the mitigating effect is empirically relevant as well. In the absence of the behavioral responses, the increase in the fraction of the sentence served to 85 percent would have increased the expected sentence actually served by 70 percent on average. The behavioral responses reduced the expected imposed sentence conditional on arrest by 8 percent, which implies that the expected sentence actually served rose by "only" 56 percent.[54] The behavioral responses have therefore undone about one-fifth of the intended direct effect of the TIS laws. Also, they inevitably increased the disparities in punishment. Because of the TIS laws, a higher fraction of defendants walk away with no punishment at all while a smaller fraction of those who are convicted are punished much more severely.

Last, the results give an interesting perspective on the behavioral responses of the judges and prosecutors. The behavioral responses were most pronounced for non-violent crimes but small or insignificant for violent crimes. The primary goal of the TIS laws was to punish violent offenders more heavily. If the judges and prosecutors share that goal, they may not apply any offsetting behavior in violent crime cases, but they may have as well regarded the TIS laws as unnecessarily overreaching when they were applied to non-violent crimes. The offsetting behavior is then a

[54] The expected sentence actually served was $pS^C \cdot 0.5 = S^A \cdot 0.5$ in the absence of the TIS laws (0.5 being the average fraction of the sentence served). In the absence of behavioral responses, it would rise to $S^A \cdot 0.85$, a 70-percent increase. The behavioral responses reduced S^A by 8 percent. Hence the new sentence actually served, conditional on arrest, increased to $S^A \cdot 0.92 \cdot 0.85$, which is 56 percent higher than the pre-TIS law level.

logical reaction. A similar conclusion can be drawn when comparing our finding that the prosecutor got "tougher" in plea bargaining in response to the TIS laws with Bjerk's (2005) finding that the prosecutors instead got "softer" in response to the three-strikes laws. Judges and prosecutors do respond to more severe sentences, but they do so selectively. Alternative models of judicial and prosecutorial behavior need not be, after all, mutually exclusive but may correctly characterize the behavior of even the same individual judges and prosecutors depending on the context of the particular legislation.

5.6 Figures and tables

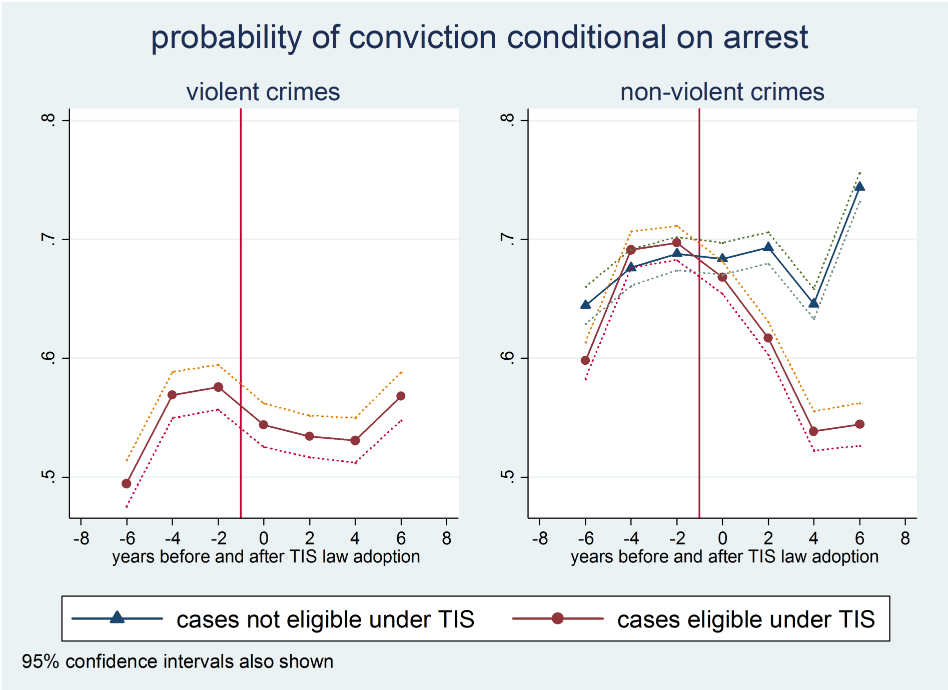

Figure 5.1: Probability of conviction

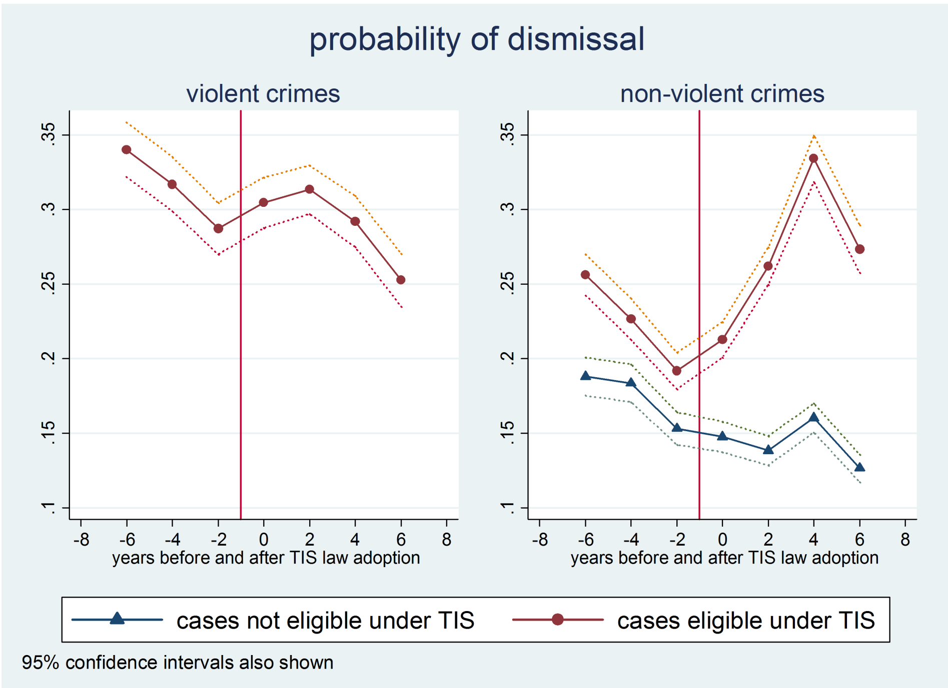

Figure 5.2: Probability of dismissal

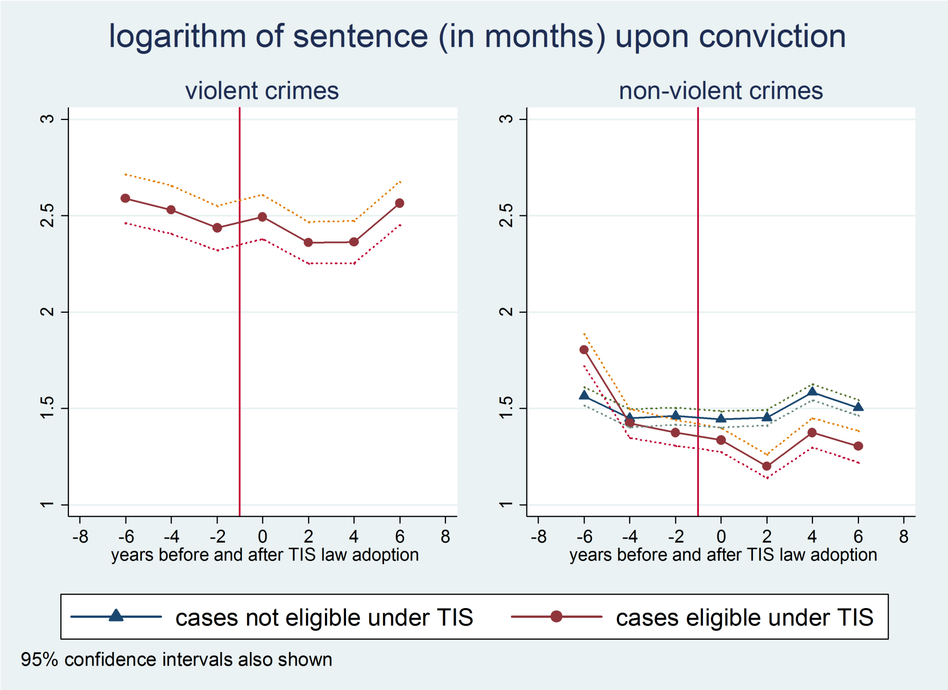

Figure 5.3: Probability of accepting a plea bargain

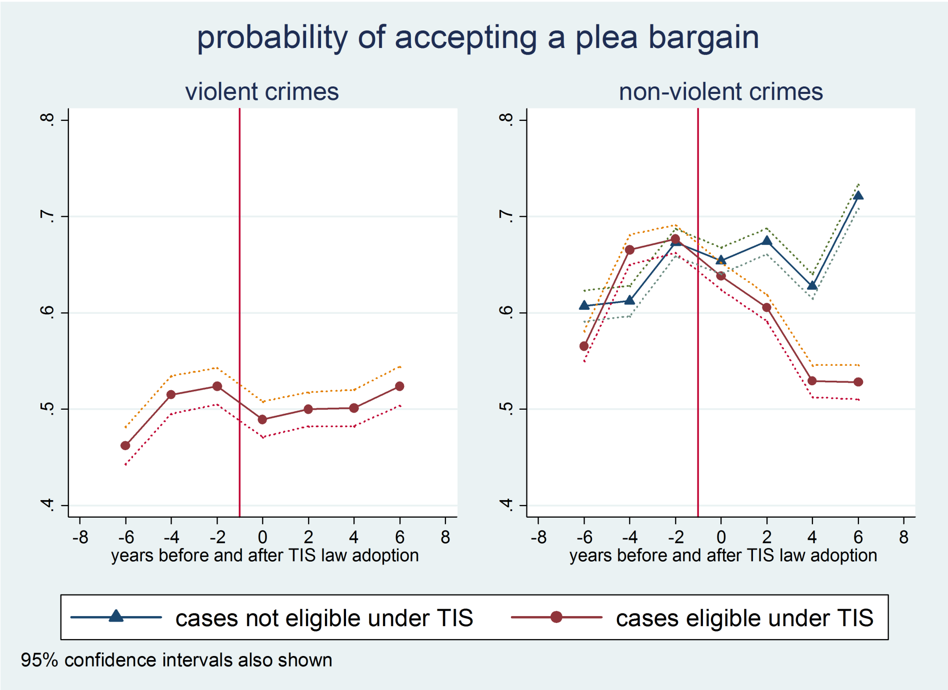

Figure 5.4: Logarithm of sentence (in months) upon conviction

Table 5.1: Adoption of the TIS laws

State	Year of introduction	Requirement(%)	Type of crime covered
Alabama	NA		
Arizona	1994	85	all
California	1994	85	violent felony
Connecticut	1996	85	violent felony
District of Columbia	1989	85	violent felony
Florida	1995	85	all
Georgia	1995	85	violent felony
Hawaii	NA		
Illinois	1995	85	all
Indiana	NA		
Kentucky	1998	85	violent felony
Massachusetts	NA		
Maryland	NA		
Michigan	1994	85	part I violent
Missouri	1994	85	repeat or dangerous felony
New Jersey	1997	85	violent felony
New York	1995	85	violent felony
Ohio	1996	85	felony
Pennsylvania	1911	100	part I violent
Tennessee	1995	85	violent felony
Texas	1993	50	aggravated
Utah	1985	85	all
Virginia	1995	100	felony
Washington	1990	85	part I violent
Wisconsin	1999	100	felony

Sources:
United States General Accounting Office: Truth In Sentencing: Availability of Federal Funds Influenced Laws in Some States, Report to Congressional Requesters, February 1998.
Chen, Elsa: Impact of Three Strikes and Truth in Sentencing on the Volume and Composition of Correctional Populations, Report Submitted to the National Institute of Justice, March 2000.
Table includes only the states covered in the SCPS data set.

Table 5.2: Summary Statistics

	Mean	violent crime		non-violent crime	
Case Outcomes	all	last year before TIS adoption	first year after TIS adoption	last year before TIS adoption	first year after TIS adoption
% convicted cases/arrest	67.31	64.24	60.06	75.59	72.33
	(46.90)	(47.94)	(48.99)	(42.96)	(44.74)
sentence/arrest in months	15.06	29.17	24.23	15.19	11.66
	(69.19)	(117.58)	(114.15)	(71.44)	(54.96)
% convicted cases/trial	80.23	77.01	75.25	76.30	78.59
	(39.83)	(42.19)	(43.26)	(42.62)	(41.08)
% dismissed or acquitted	25.71	32.01	36.62	18.03	22.30
	(43.70)	(46.66)	(48.19)	(38.44)	(41.63)
% pleaded guilty	63.01	58.24	53.80	73.48	68.96
	(48.28)	(49.33)	(49.87)	(44.15)	(46.27)
plea sentence/plea conviction	18.32	31.73	27.98	18.17	14.66
in months	(59.79)	(83.27)	(88.01)	(72.29)	(59.31)
trial sentence/trial conviction	82.32	178.36	149.08	87.40	46.29
in months	(229.83)	(369.52)	(354.35)	(228.13)	(123.49)
Individual Characteristics					
age	29.99	28.32	29.68	29.56	30.137
	(10.30)	(10.31)	(11.25)	(9.54)	(10.05)
% black	36.30	41.97	37.55	34.07	34.11
	(48.09)	(49.36)	(48.43)	(47.40)	(47.41)
% hispanic	21.07	21.40	22.26	21.81	21.65
	(40.78)	(41.02)	(41.61)	(41.30)	(41.19)
% women	16.72	10.99	13.10	16.41	18.07
	(37.31)	(31.28)	(33.75)	(37.04)	(38.49)
prior felony convictions	1.07	0.86	0.84	1.05	1.03
	(1.91)	(1.63)	(1.60)	(1.85)	(1.76)
prior misdemeanor convictions	1.61	1.45	1.50	1.75	1.67
	(2.58)	(2.53)	(2.51)	(2.76)	(2.67)
public defender (%)	40.35	40.17	53.04	42.37	54.83
	(49.06)	(49.04)	(49.92)	(49.42)	(49.77)
private attorney (%)	13.12	14.74	12.26	14.11	13.17
	(33.77)	(35.46)	(32.81)	(34.82)	(33.81)
assigned attorney (%)	11.09	11.87	13.94	12.22	10.80
	(31.40)	(32.34)	(34.65)	(32.75)	(31.04)
# Observations/arrest	83506	2402	2381	7628	7937
# Observations/ trial	4482	187	198	211	341
# Observations/trial conviction	3567	144	146	161	267
# Observations/ plea conviction	52387	1395	1281	5578	5470

Standard errors in parentheses.
Only states that eventually adopted the TIS laws are included in the summary statistics for a comparison between before and after TIS. To calculate the overall means of the variables, additional states that did not introduce TIS (Alabama, Indiana, Hawaii, Massachusetts, Maryland, and Texas) are also included.

Table 5.3: Probit Estimates, Probability of Conviction Conditional on Arrest

	1	2	3	4
TIS case	−0.094***	−0.088***	−0.093***	−0.061***
	(0.010)	(0.010)	(0.010)	(0.009)
TISstate	0.042***		0.105***	
	(0.011)		(0.010)	
TISstate x violent		0.070***		0.108***
		(0.017)		(0.015)
offense x year dummies	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	No	No
state x offense dummies	No	No	Yes	Yes
# observations	83,506	83,506	83,437	83,437
pseudo R^2	0.153	0.153	0.140	0.139

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects on the probability and their standard errors (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, and types of attorneys).

Table 5.4: Tobit Estimates, Imposed Sentence Conditional on Arrest (all cases)

	1	2	3	4
TIS case	−0.114***	−0.097***	−0.083***	−0.040
	(0.026)	(0.025)	(0.026)	(0.025)
TISstate	0.106***		0.185***	
	(0.028)		(0.032)	
TISstate x violent		0.172***		0.233***
		(0.058)		(0.061)
offense x year dummies	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	No	No
state x offense dummies	No	No	Yes	Yes
# observations	83,244	83,244	83,244	83,244
pseudo R^2	0.095	0.095	0.093	0.093

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects on the sentence and their standard errors (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, and types of attorneys).

Table 5.5: Quantile Estimates, Imposed Sentence Conditional on Arrest

	1	2	3	4
TIS case	−0.111***	−0.0161***	−0.394***	−0.183***
	(0.000)	(0.000)	(0.045)	(0.046)
TISstate	0.421***		0.513***	
	(0.000)		(0.048)	
TISstate x violent		0.258***		0.215**
		(0.000)		(0.097)
offense x year dummies	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	Yes	Yes
quantile	75%	75%	90%	90%
# observations	83,244	83,244	83,244	83,244
pseudo R^2	0.236	0.236	0.194	0.193

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
The reported coefficients denote the marginal effects on the probability.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, and types of attorneys).

Table 5.6: Probit Estimates, Probability of Reducing Charges

	1	2	3	4
Dependent Variable	Misdemeanor		Felony with shorter sentence	
TIS case	−0.040***	−0.027***	−0.023***	−0.011
	(0.006)	(0.005)	(0.008)	(0.008)
TIS state	0.030***		0.018**	
	(0.007)		(0.009)	
TISstate x violent		0.011		−0.011
		(0.013)		(0.015)
offense x year dummies	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	Yes	Yes
# observations	83,245	83,245	36,851	36,851
pseudo R^2	0.194	0.194	0.118	0.118

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects on the probability and standard errors (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, and types of attorneys).

Table 5.7: Probablity of Specific Case Outcomes and Length of Sentence upon Conviction

	1	2	3	4	5	6	7	8
Dependent Variable	Conviction at trial		Dismissed		Plea guilty		Sentence upon conviction	
	Probit		Probit		Probit		Tobit	
TIS case	−0.098**	−0.052	0.051***	0.036***	−0.095***	−0.110***	0.223***	0.260***
	(0.049)	(0.044)	(0.009)	(0.009)	(0.011)	(0.010)	(0.071)	(0.072)
TISstate	0.075**		−0.048***		0.005		0.025	
	(0.038)		(0.009)		(0.011)		(0.034)	
TISstate x violent		0.035		−0.044***		0.071***		−0.133*
		(0.051)		(0.015)		(0.018)		(0.079)
Plea							−0.379***	−0.379***
							(0.034)	(0.034)
Plea x TIS case							−0.053	−0.059
							(0.063)	(0.063)
county x violent dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
offense x year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# observations	4,363	4,363	83,506	83,506	83,506	83,506	55,954	55,954
pseudo R^2	0.185	0.184	0.166	0.166	0.129	0.129	0.112	0.112

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects and standard errors (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, and types of attorneys).

Table 5.8: Offense-Specific Effects

Dependent Variable	Sample	Offense Categories				
		murder	other violent	property	drug	other
Probability of conviction	all	0.066	−0.050**	−0.136***	−0.070***	−0.145***
		(0.057)	(0.020)	(0.014)	(0.013)	(0.020)
Expected imposed sentence	all	0.138	−0.027	−0.153***	−0.100***	−0.144***
		(0.177)	(0.054)	(0.030)	(0.029)	(0.041)
Maximum sentence imposed	convicted	0.449*	−0.580**	0.041	−0.087	0.289*
		(0.241)	(0.262)	(0.129)	(0.155)	(0.164)
Probability of conviction	trial	0.114***	−0.114	−0.190**	−0.049	−0.031
		(0.042)	(0.074)	(0.080)	(0.065)	(0.082)
Probability of a guilty plea	all	−0.009	−0.023	−0.139***	−0.071***	−0.157***
		(0.067)	(0.021)	(0.014)	(0.013)	(0.020)
Probability of dismissed	all	−0.045	0.036*	0.065***	0.047***	0.060***
		(0.051)	(0.019)	(0.012)	(0.012)	(0.017)
Probability of reducing charges	all	−0.012	−0.043***	−0.029***	−0.047***	−0.028***
		(0.070)	(0.010)	(0.006)	(0.005)	(0.009)

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects and their standard errors (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, and types of attorneys), offense-year dummies, county dummies interacted with violent crime dummies, and the interaction term of the TIS dummy and each crime category type.

Table 5.9: Robustness Checks

	1	2	3	4	5	6	7	8	9	10
	Dismissed cases excluded				Pre-trial covariates included				Non-violent crimes only	
	Length of Sentence/arrest		Conviction/arrest		Length of Sentence/arrest		Conviction/arrest		Sentence	Conviction
TIS case	−0.096***	−0.082***	−0.050***	−0.060***	−0.115***	−0.077***	−0.092***	−0.083***	−0.126***	−0.098***
	(0.033)	(0.032)	(0.007)	(0.007)	(0.025)	(0.024)	(0.011)	(0.010)	(0.025)	(0.011)
TISstate	0.081**		0.000		0.148***		0.048***		0.088***	0.029***
	(0.035)		(0.004)		(0.027)		(0.011)		(0.028)	(0.011)
TISstate x violent		0.129*		0.017***		0.173***		0.070***		
		(0.072)		(0.005)		(0.056)		(0.017)		
offense x year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# observations	61,773	61,773	61,231	61,231	81,796	81,796	82,053	82,053	62,572	62,767

* significant at 10%; ** significant at 5%; *** significant at 1%

Marginal effects and standard errors (in parentheses) are reported.

All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, and types of attorneys).

Table 5.10: Estimates of the TIS Effect Using the Predicted Fraction of the Sentence Served

Dependent Variable	Sample	Regression	Specification	
			TISstate	TISstate x violent
Probability of conviction	all cases	probit	−0.075***	−0.078***
			(0.021)	(0.021)
Expected imposed sentence	all cases	tobit	0.098	0.110*
			(0.060)	(0.060)
Expected imposed sentence	convicted cases	tobit	−0.297*	−0.298*
			(0.161)	(0.158)
Probability of conviction	trial cases	probit	−0.360***	−0.285***
			(0.090)	(0.083)
Probability of a guilty plea	all cases	probit	−0.003	−0.033
			(0.022)	(0.022)
Probability of dismissal	all cases	probit	−0.027	−0.036*
			(0.019)	(0.0190)
Probability of reducing charges	all cases	probit	−0.059***	−0.049***
to misdemeanor			(0.015)	(0.015)

* significant at 10%; ** significant at 5%; *** significant at 1%
The table reports the marginal effects and standard errors (in parenthese) on the fraction of the predicted sentence served in regressions that are equivalent to regressions in Tables 5.3 through 5.6 except that the TIS case dummy is replaced with the fraction of the expected sentence served.
Specification "TISstate" denotes regressions controlling for the presence of the TIS law in the state (equation 5.3). Specification "TIS state x violent" denotes regressions controlling for an interaction of the TIS state dummy and a violent crime dummy (equation 5.4).
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, and types of attorneys), offense-year dummies, and county dummies interacted with violent crime dummies.

Chapter 6

An Experimental Comparison of Adversarial Versus Inquisitorial Procedural Regimes[1]

6.1 Introduction

Public policy is implemented through the legal process, in a variety of institutional settings involving courts, administrative agencies, and other bodies. The predominant form of implementation is the process known to lawyers as adjudication, which resolves a dispute between parties by ascertaining and applying a legal decision rule to historical facts determined through a fact-finding apparatus. Most of the institutional details of differing adjudicatory systems are given by their respective approaches to the fact-finding function.

In contemporary legal systems, there are two principal models of adjudication: (1) the "adversarial" model prevailing in Anglo-American law, emphasizing the contesting parties' autonomy and control of legal proceedings; and (2) the "inquisitorial"

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model influencing the Civil Law systems of continental Europe and elsewhere – and sometimes used in administrative adjudication in the United States – emphasizing control by a disinterested decision-maker or judge. There is much prior literature devoted to a comparison of the relative attributes and performance of the adversarial and inquisitorial systems. Most of this literature consists of purely descriptive comparisons or normative critiques of one system or another, and, despite a growing theoretical literature in recent years, there has been little attention to empirical or experimental analysis of the relative efficiency of the two systems. [2]

In this paper, we seek to open the empirical debate by reporting the results of a series of economic experiments comparing adversarial with inquisitorial adjudication primarily in terms of their relative fact-finding efficiency, as given by their respective tendencies to reveal pertinent hidden information to a decision maker, which has been the subject of competing hypotheses in prior literature. On the one hand, Tullock (1980) argues that inquisitorial proceedings are likely to be more revealing and therefore more accurate, because "in adversarial proceedings, a great deal of the resources are put in by someone who is attempting to mislead" (p.96). Tullock's argument is based upon the supposition that litigation typically involves a "Mr. Right" (the party who should win) and a "Mr. Wrong" (the party who should lose), with "Mr. Wrong" engaged primarily in obfuscation. He also appears to assume implicitly that "Mr. Wrong" typically is in possession of private, discrediting information. On the other hand, Milgrom and Roberts (1986) present a game-theoretic model, motivated by institutional arrangements in certain settings of regulation by administrative agencies, in which sufficiently opposed interests between adverse

[2]The legal literature is voluminous. For a relatively recent exchange reviewing the arguments on each side in the specific context of comparing German and American procedure, see the exchange between Langbein (1985) and Gross (1987). Classic critiques of the adversarial system are given by Pound (1917) and Frank (1949), and defenses are given by Fuller (1978) and Landsman (1984). Theoretical analyses include Tullock (1980) and Milgrom and Roberts (1986), both discussed below, as well as Froeb and Kobayashi (1993; 1996) and Shin (1998). The only related experimental work of which we are aware is Thibault and Walker (1975), which differs markedly in its focus from our investigations. Thibault and Walker report comparative experimental findings designed to study differences between "party" and "court" control of information transmission, and the effect of pretrial bias on outcomes as between the two systems, within a psychology-based experimental design in which subjects were given no payoffs or misled as to the basis for the payoffs. Thibault and Walker's findings on revelation were consistent with the theoretical economics literature, especially Milgrom and Roberts (1986), in the sense that the potential of ex post verification affected each adversarial party's selection of information to disclose to the tribunal. Similarly, the findings on pretrial bias are consistent with Froeb and Kobayashi (1996), in that adversarial presentation reduced the effect of pretrial bias on the ex post decision. For a critique of Thibault and Walker's representation of the inquisitorial system, see Damaska (1975).

parties ensure the full revelation of information even to a relatively unsophisticated decision maker, at least where both parties have access to the same information and where the parties' reports are verifiable. [3]

Our first set of experiments begins the process of investigating the competing theoretical hypotheses by approximating the conditions envisaged by Tullock, with stylized rules of adversarial versus inquisitorial procedure. Under experimental conditions (1) exaggerating the characteristic features of the two systems into the extremes of party control (adversarial) versus judge control (inquisitorial), (2) embodying the assumption of an unambiguously "Right" and "Wrong" party under full information, and (3) distributing asymmetric information between two opposing and self-interested parties such that "Mr. Wrong" is given private and discrediting information, we compare the results in terms of both revelation and accuracy across the two rule systems. Our results show that, under these conditions, the judge-controlled "inquisitorial" system is both more revealing and more accurate than the party-controlled "adversarial" system.

We then conducted a second set of experiments under a differing information structure more closely resembling the assumptions of Milgrom and Roberts, by endowing "Mr. Right" with a clue to the content of the discrediting information possessed by "Mr. Wrong." [4] Under this structure of asymmetric but correlated information between the parties, the relative performance of the adversarial and inquisitorial systems is completely reversed. With this information structure, the party-controlled "adversarial" system now is both more revealing and more accurate than the judge-controlled "inquisitorial" system.

Under both sets of experiments, our findings are that adversarial and inquisitorial

[3] While Milgrom and Roberts' model assumes costlessly supplied information, Froeb and Kobayashi (1993; 1996) extend the analysis to settings in which parties incur costs to produce statistical evidence that is selectively reported to a "naive" (1993) and even a "biased" (1996) decision maker. Within limiting assumptions of symmetrical party access to information at constant marginal cost, Froeb and Kobayashi's theoretical findings are similar to Milgrom and Roberts. Similar results also are obtained under costly and asymmetrical information by Shin (1998) within a signaling model where the decisionmaker is able to draw inferences from an adversarial party's failure to produce evidence.

[4] While Milgrom and Roberts (1986) assume strictly costless access, later work by Froeb and Kobayashi (1993, 1996) predicts the same result under costly and symmetric access (see note 4, above). Our experimental conditions take that hypothesis one step further, by introducing a structure of "correlated" information similar to McAfee and Reny (1992), in which Mr. Right is given costly but asymmetric access. These conditions differ from the theoretical model of Shin (1998), in that we suppress burdens of proof under both adversarial and inquisitorial procedures.

procedures produced significantly different outcomes, and that their relative efficiency depends significantly upon the ex ante structure of information available to the parties. Future experimental and theoretical work will be required to investigate whether other variables significantly affect comparative results.

6.2 The Experiments

6.2.1 Revelation with Private Information

As indicated above, our initial experiment was designed primarily to test the hypothesis of Tullock (1980). Accordingly, the experiment centered on two case scenarios that were drawn to identify an unambiguous "Mr. Right" and "Mr. Wrong" under full information, and, in both cases, "Mr. Wrong" was provided with private and discrediting information. However, “Mr. Right” was not provided with enough private information to know ex ante whether he or she in fact was “Mr. Right.” [5]

Both case scenarios were then subjected to treatment under both of two rule regimes, representing adversarial and inquisitorial procedures, with each observation involving three subjects randomly assigned the roles of complaining party, defending party, and referee. [6] Under both regimes, there was a strong anti-perjury rule, with perjury defined to include embellishment as well as falsification, and punishable by forfeiture of the offending party’s full potential payoff.

For each observation of the experiment, subjects were assigned randomly to rule systems and roles. Each of the six roles – the three roles of referee, complaining party, and defending party, under each of the two rule systems – were given separate instruction sheets. For each case scenario, all three subjects were given sheet of "basic information" that included the simple decision rule to be applied by the referee. Each of the contending parties was given a second sheet of "additional in-

[5] If anything, the private information conveyed to “Mr. Right” might have suggested to that subject that he or she in fact would turn out to be “Mr. Wrong,” by indicating that, in the absence of further information, that subject would lose the case. In addition, Mr. Right’s private information included both useful and false “leads” to the hidden information, whereas Mr Wrong’s private information left no doubt that Mr. Wrong was wrong, i.e., would (or, under the decision rule, should) lose the case if the hidden information were revealed.

[6] Each subject played two rounds of the experiment, with randomized reassignment of roles between the two rounds, as among the six possible roles of complaining party, defending party, and referee under each of the two rule systems.

formation," which in the case of "Mr. Wrong" included the hidden fact discrediting that subject's position. The parties' payoffs were structured to be completely dependent upon the referee's award dividing a stake between the parties, while the referee's payoff, in the baseline case, was dependent strictly upon the "accuracy" of the award, in relation to the correct application of the given decision rule under full information.

Subjects played both scenarios in two sequential rounds of each experimental session, with learning effects minimized by the randomized re-assignment of roles and rule systems between rounds, by isolating subjects between rounds, and by alternating the sequence of the scenarios across successive experimental sessions. At the end of the second round, the subjects were paid and discharged.

Each experimental group was placed in a separate room and observed by a monitor, whose role was to police time limits and enforce the role limitations and the anti-perjury rule. The same time limit was provided under both rule systems. In the adversarial regime, the total time was divided equally between the parties, and the referee's role was completely passive during the questioning phase. In the inquisitorial regime, the allocation of time among the parties was left to the referee, who was the only questioner permitted.

Aside from the anti-perjury rule, neither rule system involved any explicit rules of evidence or burdens of proof. To randomize against the possibility that the given decision rule might be interpreted as implicitly casting the a burden of proof against the complaining party, the full-information "Mr. Right" was the complaining party in Scenario 1 and the defending party in Scenario 2. [7]

Monitors were instructed to take the observation on "revelation," defined to be disclosure through questioning of the hidden information discrediting "Mr. Wrong" in each case, specified as a 0-1 variable. [8] Monitors also were instructed to report, in the case of "questioning referee," whether the referee asked an open-ended question of one or both parties, essentially inviting that party to volunteer whatever

[7] Given this conscious design to eliminate the effects of burdens of proof, particularly the burden producing evidence, our experimental results are not a direct test of the model proposed by Shin (1998).

[8] The subjective aspect of these observations is one possible weakness in the experimental results, as the monitors' assignments were not randomized. However, different monitors were employed in different experimental sessions, without any detectable differences in results.

information that party thought should be brought to the referee's attention.

"Accuracy" was observed on the basis of the referee's announced award, as compared with the full-information "correct" answer that "Mr. Right" should be awarded the entire stake, and "Mr. Wrong" should be awarded nothing. The parties' payoffs were determined by the referee's award, and the referees' payoffs were determined by the correspondence between the referee's award and the predetermined "correct" outcome. Thus, in an experimental round involving a stake of $20, a precisely accurate decision produced both an "accuracy" score and a referee payoff of $20; while an entirely inaccurate decision produced an "accuracy" score and a referee payoff of 0.

A significant aspect of the experimental design was to set referees' maximum payoffs equal to the parties' maximum payoffs, thus presenting strong incentives to referees.

A total of 56 observations were taken on two days in March 1996 – March 4 and March 23 – at the University of Arizona Economic Sciences Laboratory.[9] Subjects were recruited from the general University community; law students and economics graduate students were used as monitors. Two different payoff levels were used: on March 4, maximum payoffs were $10 per round; while on March 23, maximum payoffs were raised to $20 per round for all three roles. In the data tables and figures presented below, both sets of observations are normalized to a $20.00 payoff level in terms of the "accuracy" measure, and we analyze the effect of payoff levels. (In all experiments each participant received a $5 fee for simply showing up for the experiment.)

In a variant designed to test the effects of the referee's payoff structure on fact-finding efficiency, we took a small number (8) of additional observations on February 23, 1998, under a modified referee payoff structure that guaranteed a fixed $10 payoff to the referee without regard to the accuracy of the decision, while leaving the parties' payoff structure and level ($20) unchanged. These observations were limited to the inquisitorial system, on the rationale that revelation would be most sensitive to the referee's incentives under that system.

[9] An initial day of preliminary experimentation on March 1, 1996, was used to refine the experimental instruments and design. Data from that day are not reported below.

6.2.2 Revelation with Correlated Information

For our second set of experiments, we made one change in the experimental instruments for each case scenario, to modify the "additional information" given to "Mr. Right" to supply that subject a clue to the hidden information possessed by "Mr. Wrong." Otherwise, the experimental design and conditions remained the same as under private information.

Under this correlated information structure, we took an additional 42 observations (21 for each rule system) in experimental sessions occurring on June 16, 1997 and February 20, 1998, at the Economic Sciences Laboratory at the University of Arizona. In each of these sessions, maximum payoffs per round remained at $20 for both parties and referees, and the referees' incentive structure remained the same.

6.3 Experimental Findings and Analysis

Our principal experimental findings are that fact-finding efficiency in adjudication is significantly affected by both the rule system and information structure, and that the relative fact-finding efficiency of adversarial versus inquisitorial rule systems is profoundly affected by the information structure. Under the "private" information structure, inquisitorial procedure was superior in revealing hidden information; while under the "correlated" information structure, adversarial procedure was superior in revealing hidden information. Similar results were obtained under the second measurement variable of "accuracy," although the exact relationship between revelation and accuracy remains somewhat ambiguous under our results. While revelation significantly contributed to accuracy under both rule systems, accuracy also is influenced by factors other than revelation in both systems.

6.3.1 Revelation under Private Information

Under private information, the experimental inquisitorial procedure produced more revelation than adversarial procedure. The comparative descriptive statistics are presented in Table 6.1, and the mean results displayed in Figure 6.1.

In Table 6.1, the means, standard deviations, and number of observations are tabulated for each rule system and each of the two scenarios. Figure 6.1 is a bar chart of the relative mean revelation of the two rule systems, combining both scenarios. As revelation is a 0-1 variable in each case, its mean represents the percentage of cases in which revelation was achieved.

As is obvious from the descriptive statistics, revelation rates are markedly higher in inquisitorial than in adversarial procedure under this information structure: inquisitorial procedure produced revelation in 28% of cases, versus only 7% for adversarial procedure. However, the standard deviations are high, the two scenarios appear to differ, and, as discussed above, payoff levels varied across experimental sessions.

To analyze all of these effects, we ran a logit regression on revelation as the dependent variable, with dummy variables representing the rule system, the scenario, and the payoff levels. Those results, reported in Table 6.2, show that only the coefficient on the rule system is significant at the 10% level ($p = .07$). Higher payoff level has the correct sign, but neither payoff level nor scenario has a significant coefficient at conventional levels.

As indicated by the descriptive statistics, the regression output shows a negative and significant effect of adversarial presentation on revelation, under the "private" information structure. In order to assess the importance of the referee's payoff structure to these results, we added the 8 observations taken under a "fixed" payoff structure for the referee, and re-ran this same logit regression with a new dummy variable entitled "Fixed Payoff," which identified those observations. The results are given in Table 6.3.

The results under this re-specified model are identical to the logit model above (Table 6.2), except that the additional variable for fixed-payoff had a coefficient with a sign in the expected negative direction, but statistically insignificant.

6.3.2 Revelation under Correlated Information

In the second set of experiments, changing the information structure from private to correlated information completely reversed the relative performance of the two rule systems: in this context, adversarial procedure produced dramatically higher rates of revelation than inquisitorial procedure. These results are summarized in Table

6.4 and Figure 6.2.

As shown in Figure 6.2, under the correlated information structure, adversarial procedure produced revelation in 71% of cases, while inquisitorial procedure produced revelation in only 14% of cases. In the Table 6.5 the coefficient on the rule system again is shown to be statistically significant by a logit regression of revelation against dummy variables representing the rule system and scenario. [10]

As shown by the regression results, scenario remains insignificant, but the coefficient on rule system, i.e. Adversarial, is highly significant (p=.002). However, under the correlated information structure, the sign has now changed from negative to positive, thus showing that adversarial procedures now produce superior revelation under this information structure.

6.3.3 Revelation and Accuracy

In both sets of experiments, in addition to measuring whether or not the hidden information was revealed explicitly, we also measured the correctness or accuracy of the referees' decisions. The alternative variable of "Accuracy" was defined as the degree of correspondence (in dollars) between the referee's actual award and the predetermined "correct" decision under full information and a proper application of the given decision rule, normalized to the $20 maximum payoff level used in most rounds of the experiments. [11] Thus, a completely accurate decision by the referee received an "accuracy" value of 20, a completely inaccurate decision received an "accuracy" value of 0, and so on.

The general results of the experiments showed a pattern of relative accuracy that involved the same rank-ordering as revelation, as summarized in Table 6.6.

Each cell of Table 6.6 reflects an accuracy relationship consistent with the revelation outcomes: under private information, inquisitorial procedure is more accurate than adversarial procedure; while under correlated information, adversarial procedure is more accurate than inquisitorial procedure; and overall, the correlated information structure produces more accuracy than the private information structure.

[10] Unlike the private information observations, under correlated information the payoff levels remained constant at $20.00 per round for each subject.

[11] Observations from the first experiment session of March 4, 1996 – involving payoff levels of $10 – are normalized to the $20 level in Table 6.6.

However, while the relative rankings are the same for accuracy as for revelation, the relative magnitudes differ. When measuring revelation, the superior system under each information structure produces dramatically higher levels than the inferior system: four times higher for inquisitorial procedure under private information; and five times higher for adversarial procedure under correlated information. When measuring accuracy, the differences are all in the same directions, but much smaller. Of course, in order for the accuracy ratio to be the same as the revelation ratio, accuracy would have to be perfect when revelation took place, and completely inaccurate when revelation did not take place.

A more interesting comparison would be with the accuracy level that results from "getting it right" (perfect accuracy) in the presence of revelation and random accuracy in the absence of revelation. Table 6.7 displays this comparison with the rows labeled "Forecast" generated as described above and those labeled "Experiment" containing the accuracy levels from our experiments.

An interesting feature of this comparison is that the Forecast accuracy levels are always higher, and in some cases substantially higher, than the accuracy level achieved in the experiment. Our subjects appear to make less-than-perfect use of the revealed information. Or perhaps our subjects are particularly bad at guessing in the cases where there is no revelation. In fact our subjects evidence both defects: the accuracy level in those case with revelation is only 15 and in those cases without revelation it is only about 7.5.

In order to more precisely investigate the effects of revelation on accuracy, we ran an ordinary least squares regression including all 106 observations in the series (64 for private information, including the 8 fixed-structure referee payoff observations; plus 42 for correlated information) The model specified accuracy as a function of scenario, revelation, payoff level, payoff structure, rule system, and a new variable named "Information," taking the value of 0 for the private information cases and 1 for the correlated information cases. The results of that analysis are given in Table 6.8.

In this model, as expected, the coefficient on revelation was highly significant ($p < .001$) with a large positive coefficient. Higher payoff level also was significant ($p = .06$) and positive. As accuracy – not revelation – was most directly correlated with the referees' payoffs, it is comforting to observe more accuracy supplied as the returns to accuracy increase. While the scenario difference was marginally significant ($p = .095$), the coefficients on all of the other three variables—fixed payoff, rule system, and information structure – were statistically insignificant.

These results suggest that most of the difference in accuracy between adversarial and inquisitorial procedure is explained by their relative revelatory efficiency under given information structures, as both rule system and information structure are insignificant after controlling for revelation. However it is worth noting that the R^2 of the regression is quite modest (0.20).

6.4 Discussion

The experimental work reported here represents only a starting point for a more rigorous investigation of the comparative features of adversarial and inquisitorial procedures. Our considerations of experimental design, the variations of parameters across experimental sessions, and differing models in the theoretical literature, all suggest future directions of research.

6.4.1 Revelation, Accuracy, and Information Structure

Our principal experimental findings are that the performance of adjudicatory systems are profoundly affected by the ex ante information structure, both in absolute terms and in the relative performance of adversarial versus inquisitorial procedure. These results held for both revelation and accuracy, as summarized in the bar charts provided in Figures 6.3 and 6.4, below.

A comparison of Figures 6.3 and 6.4 shows the relationship of the comparative rule system results for revelation and accuracy. As noted above, the relative performance of inquisitorial and adversarial rule systems followed the same pattern for both revelation and accuracy. However, after controlling for revelation (Table 6.8), the difference in accuracy as across the rule systems (and information structures) was not statistically significant at conventional levels.

To some extent, these results reflect the inherent definitional differences between revelation and accuracy in our experiment. Revelation is the more direct measure of fact-finding efficiency as such – it simply measures whether hidden information was brought out explicitly. Accuracy is a composite of both explicit fact-finding efficiency and diligence in applying the decision rule to the revealed facts.[12] While revelation was by far the most powerful influence on accuracy, the analysis of accuracy results (Table 6.8) shows that payoff levels and scenario difficulty also were statistically significant.

The analysis of our accuracy results suggests that the rule system and information structure were important to accuracy basically through their effects on revelation. The analysis of revelation shows that both rule system and information structure were important to revelation, and the relative performance of the rule systems was profoundly a function of the information structure.

The importance of information structure to the relative fact-finding efficiency of inquisitorial and adversarial procedure has implications for institutional design. In particular, our analysis shows that the fact-finding efficiency of adversarial procedure depends critically upon information structure. In terms of relative performance, the adversarial system was ten times more efficient in revelation under "correlated" as opposed to "private" information. This suggests that certain institutional adjuncts to the Anglo-American adversarial system – notably the expansive pre-trial "discovery" system in civil litigation – may be crucial to its fact-finding efficiency.[13]

In contrast, our experimental results showed that inquisitorial procedure was relatively more efficient under "private" as opposed to "correlated" information, though the relative difference was smaller and in the opposite direction. In our results, the fact-finding efficiency of the "questioning referee" (inquisitorial judge) actually dropped under "correlated" information. While this result may be an artifact of the relatively small number of observations in each cell of the analysis, it also might suggest that inquisitorial judges could be "overloaded" in correlated-information case, without the assistance of adversarial party presentations on the facts. Conversely, it suggests that the best case for inquisitorial procedure may be situations in which

[12] This of course abstracts from the point in Shin (1998) that inference from what is not revealed by the presumptively better informed party is also an important determinant of accuracy.

[13] The leading model of this system is that provided by Rules 26–37 and 45 of the Federal Rules of Civil Procedure, which authorize parties to compel pre-trial disclosure of information possessed by other parties to the lawsuit and even by non-parties, mostly on the initiative of the adversarial parties and, at least traditionally, with little intervention by the court.

– for cost reasons or otherwise – the institutions can not be arranged to promote the correlated information structure.

Our experimental design suppressed considerations of the cost of producing one or another of the information structures tested. Future experimental research could investigate this factor, through a design that permitted the parties or judge to "purchase" additional information prior to the definitive adjudication.

6.4.2 Payoff Levels and Structure

As reported in the previous section, a change in general payoff levels from \$10.00 to \$20.00 for all subjects had no significant effect on revelation (Tables 6.2 and 6.3), but did have a significant effect on accuracy after controlling for revelation (Table 6.8). This is interesting not in that it supports the rather unsurprising conclusion that, ceteris paribus, raising subjects' payoffs produces more effort, but rather that such "local" incentive effects appear to be swamped by institutional factors in the case of revelation.

In terms of payoff structure, our limited investigation of 8 cases of fixed referee payoffs, revealed negative but statistically insignificant effects on both revelation (Table 6.3) and on accuracy after controlling for revelation (Table 6.8). The lack of a significant effect may be due to the very small number of observations, as there is a basis in theory to expect that judges' payoff structures may have profound effects on the performance of adjudicatory systems. [14]

In most of our experimental observations, judges were permitted to achieve maximum payoffs by coming as close as possible to externally-verifiable "accuracy." However, even so, and even with highly transparent decision rules that directed their attention to the potential relevance of hidden information, the levels of revelation and accuracy were surprisingly low under both systems, with the possible exception of adversarial procedure in a correlated information structure. If judges' incentives were disconnected from "accuracy," then presumably the rates of revelation and accuracy would drop still further. Such disconnection might even change the observed

[14] Posner (1992, p.520) argues that one of differences between adversarial and inquisitorial procedure may lie in the relatively heavier reliance of the inquisitorial system on the effectiveness of public-sector judges, whose incentives may not be well-aligned with the social interest in accuracy.

relative performance of the two systems.

Furthermore, judges' payoffs in actual procedural systems may produce even more severe effects, by being inversely related to "public" expenditure, i.e., trial time, via such "throughput" measures as the number of case dispositions per time period that are commonly encountered in annual reports of judicial performance. Our small number of observations with fixed payoffs and our focus on experimental design emphasizing rule system did not permit us to test the potential effects of judicial incentives in anything like a comprehensive manner. Our results, however, are suggestive of fruitful directions for future research.

6.4.3 Expertise and Types of Disputes

Our experimental design sought to suppress the effect of either legal or fact-finding expertise on the part of judges and advocates. Expert advocacy (professional lawyers) is argued to be especially important to adversarial systems (Bundy and Elhauge, 1991), and has been shown empirically to affect outcomes by Ashenfelter and Bloom (1990). Similarly, judicial expertise has been argued as one of the "advantages" of inquisitorial procedure (Langbein, 1985).

While our design was not intended to address these issues, some of our findings suggest the roles that expertise may play. In our experiment, there was an extremely low incidence in within the inquisitorial rule system of the subjects playing the role of referee asking an "open-ended" question of one or both parties. Presumably, "expert" judges would ask that type of question in almost every case. Nevertheless, under "private" information, the "inexpert" subjects playing the roles of referee in the inquisitorial system achieved more revelation than the similarly "inexpert" subjects playing the role of advocate in the adversarial system. Similarly, under "correlated" information, the "inexpert" advocates achieved more revelation in the adversarial system than did the "inexpert" referee in the inquisitorial system. Furthermore, the relative differences in revelation rates could be consistent with the hypothesis that expert advocacy is relatively more important to the operation of adversarial systems than expert judging is to inquisitorial systems, even if the inquiry is confined to fact-finding alone.

If the range of inquiry is broadened to include the ascertainment and application of legal rules as well as facts, then expertise could operate in a different way, with possibly different results in terms of the legal accuracy of adjudication. Both Thibault and Walker (1978) and Langbein (1985) argue that inquisitorial procedure may be more appropriate to certain disputes or components of disputes – those focusing on the determination of fact alone, under agreed rules – than on disputes where the legal rule is unclear or the legal significance of historical facts (whether or not disputed) is unsettled or ambiguous. In other words, if there is no unambiguous “Mr. Right” and “Mr. Wrong” under rules that are settled and agreed upon by the parties ex ante lite, the relative results of two systems may differ.

As the "legal" component becomes relatively more important, the two rule systems could have different attributes in terms of “legal” as opposed to “factual” accuracy. Based upon anecdotal observations by monitors, in our experiment subjects playing the role of party-advocates under the adversarial system sometimes used their questioning period to advance what were in essence legal arguments to the passive referee. This effect may have influenced outcomes where questioning failed to achieve revelation.

However, in our experiments, these influences were suggestive and anecdotal only. Further research will be required in order to establish the effects, if any, of legal or fact-finding expertise on the relative performance of adjudicatory systems.

6.5 Concluding Remarks

By reporting the results of this experimental series, we hope to generate broader interest in applying the methods of experimental economics to questions of legal infrastructure, particularly in the field of legal procedure. Much work remains to be done, even within the relatively narrow domain of comparing adversarial and inquisitorial methods for revealing factual information to decision makers, which was the focus of our investigation.

Our principal experimental finding is that information structure profoundly affects the relative fact-finding efficiency of adversarial versus inquisitorial procedure. Inquisitorial methods resulted in a higher rate of revelation than adversarial methods under conditions of private and asymmetric discrediting information in the possession of an interested party. Adversarial methods resulted in a higher rate of revelation than inquisitorial methods under conditions of correlated information in which both interested parties had access (albeit unequal access) to the hidden information. These findings have implications for institutional design, in terms of the relative fact-finding strengths and weaknesses of adversarial and inquisitorial procedures.

Our experimental findings may be strictly limited by the incentive and information structure conditions imposed by the experiment. In particular, modifying the incentive structure of the decision maker, allowing costly information structures, or permitting legal or fact-finding expertise to be applied by the parties or judge, may produce very different experimental findings. Further experimental and theoretical research will be required to establish the significance of these and other conditions that we have not taken into account. Until that work is completed, perhaps the most important implication of our findings is that fully developed experimental research can and should be an important input into future reform efforts, particularly given the dearth of other empirical work in this field and the difficulties encountered by empirical research using data from functioning judicial systems.

6.6 Appendix

In this Appendix, we provide a more detailed description of the procedures and instruments used in our experimental research.

In general, all aspects of our experimental design and management were affected by the need to draw a balance between the level of abstraction desirable for isolating the variables of interest and the richness of institutional detail necessary to a role-playing experiment. Thus, while we sought to abstract away from some of the institutional details and terminology associated with legal procedure, in order to motivate the subjects it was necessary to convey that they were involved in a conflict to be decided by a third party. Accordingly, the experimental instruments used terminology such as "referee" rather than "judge," and referred to the two rule systems as "questioning parties" rather than "adversarial" and "questioning referee" rather than "inquisitorial," in order to minimize the secondary connotations associated with the more pointed terminology. In that same vein, students or law (or pre-law) or economics were screened from the subject population. Otherwise, subjects were recruited from the general University community in accordance with the standard procedures of the Economic Sciences Laboratory at the University of Arizona, where experimental sessions were held in March 1996, June 1997, and February 1998.

Nevertheless, it was necessary to provide enough institutional detail to motivate the subjects to undertake essentially a role-playing exercise. Accordingly, the experiments were built around two case scenarios involving gender-neutral characters to be played by subjects, entitled “Chris and Leslie” (Scenario 1) and “Jan and Pat” (Scenario 2), both involving a loss to one of the parties (“complaining party”) followed by a request for compensation from the other (“defending party”), to be decided by a third subject (“referee”).

Case scenario 1, entitled “Chris and Leslie,” involved a complaint by a farmer against a pesticide supplier that the pesticide killed off a local pheasant population relied upon by the farmer for supplemental income. “Mr. Wrong” was the defending party Leslie, the pesticide supplier, who was provided with private information that the pesticide in question had not been tested properly for toxicity to other animals, as the testing technician had been intoxicated at the time of the tests. Under full information, the correct decision in this scenario was to award the full stake to Chris, the complaining party.

Case scenario 2, entitled “Jan and Pat,” involved a complaint by a farmer against a veterinarian alleging that the veterinarian’s malpractice killed the farmer’s cow. In this case, the complaining party Jan, the farmer, was “Mr. Wrong,” who was supplied with private information that a self-administered home remedy had killed the cow. Under full information, the correct decision in this scenario was to award the full stake to Pat, the defending party.

In the experimental sessions, subjects were assigned at random to play one of six roles—complaining party, defending party, or referee, under either “questioning parties” or “questioning referee”—as to each scenario, which were successively in two rounds per experimental session. Learning effects were sought to be minimized by alternating the sequence of the scenarios across successive experimental sessions, by the randomized re-assignment of roles and rule systems between rounds, and by isolating subjects between rounds. Subjects were told the payoff levels in advance (in most sessions, a maximum of $20.00 per round, times two rounds, plus a $5.00 show-up fee). At the end of the second round, the subjects were paid and discharged, and were then disqualified from further participation.

At the beginning of each round, each of the six roles were provided with instruction sheets explaining the rule system and their role within that system. Adversarial procedure was represented by the system called "questioning parties," in which the "referee" is entirely passive during the hearing, and merely decides the case afterward. Inquisitorial procedure was represented by "questioning referee," in which only the referee may ask questions, and the parties are relegated essentially to the role of interested witnesses. The same time limits were provided under both systems. In "questioning parties," the total time was divided equally between the parties, with provision for both an initial "case in chief" phase and a follow-up "rebuttal" phase. In "questioning referee," the time allocation was left to the referee. Neither rule system involved any explicit burden of proof (though the decision rule might be interpreted as implicitly casting the burden on the complaining party) or any explicit rules of evidence, except for a strong anti-perjury rule.

The three subjects involved in each observation were isolated in a separate room with a fourth person as "monitor." The role of the monitor was to enforce time limits, role limitations, and the anti-perjury rule, and to record the observations on both revelation (whether the hidden information possessed by "Mr. Wrong" was brought out during the questioning phase) and accuracy (based on the referee's award of the stake). [15]

Each experimental round began with the distribution of information sheets on both roles and "basic information" on the case scenario being played, which included a description of the decision rule to be applied by the referee, which was made known to both parties and referee. [16] The subject playing referee received only the "basic information," whereas each of the parties received a sheet of "additional information."

[15] The subjective aspect of these observations is one possible weakness in the experimental results, as the monitors' assignments were not randomized. However, different monitors were employed in different experimental sessions, without any detectable differences in results.

[16] In case scenario 1 (Chris and Leslie; "Pheasant"), the decision rule was stated as "Leslie has to pay compensation only if Leslie's company failed to perform the standard testing or if the standard testing was performed improperly." In case scenario 2 (Jan and Pat; "Cow"), the decision rule was "Jan is entitled to compensation only if improper performance of veterinary services caused Jan's cow to die."

In the case of the party who was “Mr. Wrong” in the scenario, this “additional information” included the private and discrediting information that would cause “Mr. Wrong” to lose the case under the decision rule, and advised “Mr. Wrong” of the significance of that fact. However, none of the materials given to subjects specified that one of the contesting parties necessarily was “right” and the other “wrong” under full information, and both the instructions and case scenarios permitted compromise solutions by the referee, albeit solutions that deviated from the given decision rule.

In the first set of “private information” experiments, the additional information given to “Mr. Right” was completely ambiguous as to who, if anyone, was in the right. [17] In the second set of “correlated information” experiments, the only change was to add a clue to the additional information supplied to “Mr. Right” that, if pursued effectively, would result in the revelation of the hidden and discrediting information possessed by “Mr. Wrong.” [18]

Following a questioning phase, conducted exclusively by the referee in “questioning referee” (inquisitorial system) and exclusively by the parties in “questioning parties” (adversarial system), there was a decision phase after which the referee announced her or his decision. The subjects were then randomly re-assigned to new roles to play the second scenario. At the end of the second round, subjects were informed of the “correct” outcomes, and received their payoffs for both rounds combined.

[17] If anything, the private information conveyed to “Mr. Right” might have suggested to that subject that he or she in fact would turn out to be “Mr. Wrong,” by indicating that, in the absence of further information, that subject would lose the case. In addition, Mr. Right’s private information included both useful and false “leads” to the hidden information, whereas Mr. Wrong’s private information left no doubt that Mr. Wrong was wrong, i.e., would (or, under the decision rule, should) lose the case if the hidden information were revealed.

[18] The following are exact transcriptions of the supplemental information provided to "Mr. Right" in each scenario under the “correlated information” variant: Scenario 1 (Chris and Leslie; the “Pheasant” scenario): "[Y]ou are aware that Leslie’s testing technician, Jordan, was an alcoholic. Since the events in question here, Jordan was killed in an automobile accident that police reports indicated had been caused by Jordan’s drunken driving. One week ago, you received an anonymous telephone call from a woman who identified herself only as one of Leslie’s employees, who told you that, after Jordan’s death, Leslie discovered that Jordan did not properly perform the product testing, and, upon re-testing, that the pesticide in question was shown to be poisonous to other plants and animals by the standard tests." Scenario 2 (Jan and Pat; the “Cow” scenario): "Yesterday, you received a telephone call from Fran, who said that he/she had heard from Jan’s neighbors that Jan had used a home remedy on the cow, and that, after the cow’s death, Jan found out that the home remedy is always fatal to cows, whether or not they are sick. However, neither Fran nor the neighbor are available to give this information to the referee."

6.7 Figures and tables

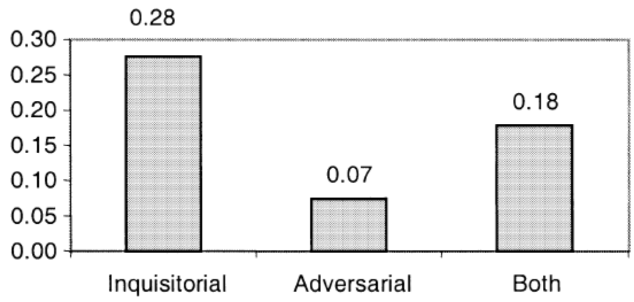

Figure 6.1: Revelation rates: Private Information

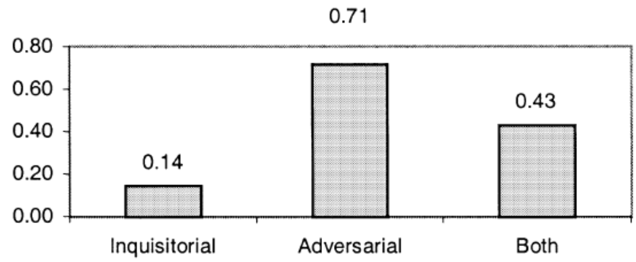

Figure 6.2: Revelation rates: Correlated Information

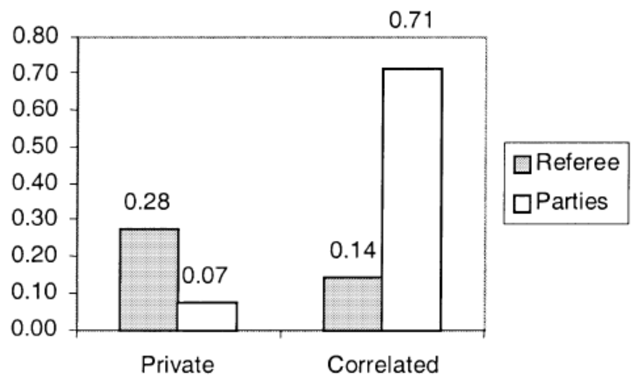

Figure 6.3: Comparative Revelation Rates: Private and Correlated Information

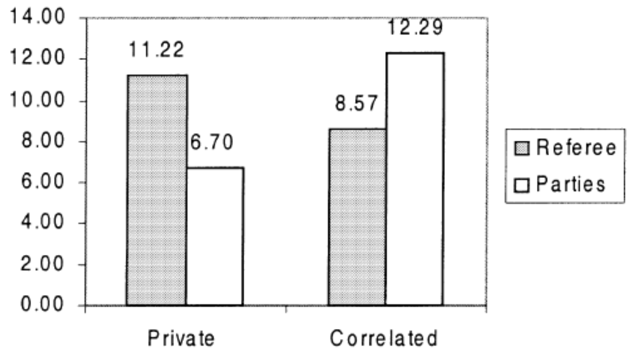

Figure 6.4: Comparative Accuracy Rates: Private and Correlated Information

Table 6.1: Revelation under Private Information

		Scenarios		
		1	**2**	**Both**
Inquistitorial	Mean	0.40	0.14	0.28
	STD	(0.51)	(0.36)	(0.45)
	# of Obs.	15	14	29
Adversarial	Mean	0.08	0.07	0.07
	STD	(0.28)	(0.27)	(0.27)
	# of Obs.	13	14	27
Both	Mean	0.25	0.11	0.18
	STD	(0.44)	(0.31)	(0.39)
	# of Obs.	28	28	56

Table 6.2: Logit Regression – Private Information

Variable	**Coefficient**	**Std. Error**	***t*-Statistic**	**Prob.**
C	−1.030	1.282	−0.803	.425
Higher Payoffs	0.033	0.075	0.434	.666
Adversarial	−1.575	0.858	−1.836	.072
Scenario 2	−1.053	0.779	−1.352	.182
Log-likelihood	−23.148			
Obs with Dep = 1	10			
Obs with Dep = 0	46			
Dependent Variable is REVELATION				
Included observations: 56				

Table 6.3: Logit Regression – Private Information with Differing Referee Payoff Structure

Variable	**Coefficient**	**Std. Error**	***t*-Statistic**	**Prob.**
C	−1.030	1.282	−0.803	.425
Higher Payoffs	0.033	0.075	0.434	.666
Adversarial	−1.575	0.858	−1.836	.072
Scenario 2	−1.053	0.779	−1.352	.182
Fixed Payoff	−0.516	1.287	−0.401	.690
Log-likelihood	−26.163			
Obs with Dep = 1	11			
Obs with Dep = 0	53			
Dependent Variable is REVELATION				
Included observations: 64				

Table 6.4: Revelation under Correlated Information

		Scenarios		
		1	**2**	**Both**
Inquistitorial	Mean	0.09	0.20	0.14
	STD	(0.30)	(0.42)	(0.36)
	# of Obs.	11	10	21
Adversarial	Mean	0.60	0.82	0.71
	STD	(0.52)	(0.40)	(0.46)
	# of Obs.	10	11	21
Both	Mean	0.33	0.52	0.43
	STD	(0.48)	(0.51)	(0.50)
	# of Obs.	21	21	42

Table 6.5: Logit Regression – Correlated Information

Variable	**Coefficient**	**Std. Error**	***t*-Statistic**	**Prob.**
C	−2.377	0.825	−2.882	.006
Adversarial	2.810	0.828	3.394	.002
Scenario 2	1.031	0.804	1.283	.207
Log-likelihood	−20.307			
Obs with Dep = 1	18			
Obs with Dep = 0	24			
Dependent Variable is REVELATION				
Included observations: 42				

Table 6.6: Comparative Accuracy Rates: Private and Correlated Information

		Information		
Rule System:		**Private**	**Correlated**	**Both**
Inquistitorial	Mean	11.22	8.57	10.11
	STD	(7.93)	(9.13)	(8.47)
	# of Obs.	29	21	50
Adversarial	Mean	6.70	12.29	9.15
	STD	(6.53)	(6.98)	(7.22)
	# of Obs.	27	21	48
Both	Mean	9.04	10.43	9.64
	STD	(7.58)	(8.24)	(7.86)
	# of Obs.	56	42	98

Table 6.7: Comparative Accuracy Rates: Private and Correlated Information: Experiment versus Forecast from "Perfect"Accuracy

		Information		
Rule System:		**Private**	**Correlated**	**Both**
Inquistitorial	Experiment	11.22	8.57	10.11
	Forecast	12.80	11.40	12.20
Adversarial	Experiment	6.70	12.29	9.15
	Forecast	10.70	17.10	13.50
Both	Experiment	9.04	10.43	9.64
	Forecast	11.80	14.30	12.90

Table 6.8: Least Square Regression: Accuracy All Observations

Dependent Variable is ACCURACY **Included observations: 106**				
Variable	**Coefficient**	**Std. Error**	***t*-Statistic**	**Prob.**
C	2.058	3.207	0.642	.523
Scenario 2	2.393	1.421	1.683	.095
Revelation	6.553	1.616	4.056	.000
Higher Payoffs	0.360	0.189	1.907	.060
Fixed Payoffs	−3.960	2.968	−1.335	.185
Adversarial	−1.890	1.438	−1.300	.197
Information	−1.890	1.719	−1.099	.274
R^2	0.203	Mean dependent var		9.552
Adjusted R^2	0.154	F-statistic		4.194

Chapter 7

Conclusion

I presented five empirical essays on the economics of crime and law enforcement, a product of a research agenda spanning several years. While each essay studied a different aspect of law enforcement systems, I think that their findings can be summarized into a common denominator: In law enforcement, rules and incentives matter. And they matter for criminals and law enforcers alike.

In *Chapter 2*, I exploit the very large drop in deterrence in the Czech Republic after 1989 to gain new evidence on the relationship between the probability and severity of punishment and crime. I find significant effects of deterrence on robbery, theft, intentional injury and failure to support. The estimated elasticities are similar to those reported in region-level regressions from other countries. I attempt to decompose the contribution of reduced deterrence to the post-revolution growth in crime rates. Weaker deterrence accounts for approximately one quarter of the four-fold increase in robberies and one half of the ten-fold increase in thefts.

The effect of the duration of criminal procedure on crime rates was investigated in *Chapter 3*. The findings differ importantly by the types of offenses. I find some but rather weak evidence that shorter duration has a deterrent effect on burglary and embezzlement. However, I find very strong evidence that shorter duration leads to an increase in (recorded) criminal offenses associated with driving, namely driving with a suspended license and driving under the influence. The key reason behind the last finding is that the number of (recorded) driving-related offenses is a product of the underlying criminal behavior and, more importantly, of the police's enforcement effort. Even though the shorter duration may have had a deterrent effect on driving offenses, the police responded by enforcing the driving offenses far more vigorously. The finding is consistent with a rational response of police officers to the new procedure. When making the decision whether to make road checks and arrest delinquent drivers, the police have to take into account that "processing" the offender will take a certain amount of paperwork. When a simplified procedure substantially reduced the paperwork, they responded by reallocating their resources towards enforcing driving offenses.

In *Chapter 4* I evaluate the effects of the same reform on criminal case outcomes. The law enforcers responded to the reduction in the cost of the criminal procedure by producing more output: for offenses that were relatively intensively prosecuted via the fast-track procedure, the duration of the procedure fell and the probability that the prosecutor brings charges to court increased. However, releasing resources from enforcing petty crimes had almost no discernible spillover effect on other, more serious crimes.

Chapter 5 tested a prediction that the severity of punishment has a negative causal effect on the probability of conviction. We found that the Truth-in-Sentencing Laws reduced the probability that an arrested defendant is convicted by 9 percent through a variety of legal channels (prosecutors more likely to drop the charges, defendants less likely to plea guilty, and judges/juries less likely to convict at trial) in those cases that were covered by the Truth-in-Sentencing laws. We thus provide rare empirical evidence that the legislator's intent to impose more severe punishment is partially offset by a reduced probability that the defendant is convicted. The primary objective of the Truth-in-Sentencing Laws was to punish violent crimes more severely, but several states extended them to non-violent crimes as well. Interestingly, we find that the offsetting effect was most pronounced for the non-violent crimes. The behavioral responses of law enforces to legislative changes appear to depend on the extent to which they share the objectives of the legislation. The prosecutors and judges shared the primary objective of the Truth-in-Sentencing Laws, but they found their extension to non-violent crimes unnecessarily overreaching and responded by softening their severity.

Chapter 6 compared the adversarial and inquisitorial legal procedure in their ability to reveal a decisive piece of evidence in an experimental trial. It turned out that the results depend significantly on the distribution of information across parties. In experimental settings where “Mr. Right’s” private information contained no clue towards the information possessed by “Mr. Wrong”, the inquisitorial procedure led to a somewhat higher revelation of the decisive information. However, in experimental settings where “Mr. Right’s” information contained a clue pointing to the incriminating information possessed by “Mr. Wrong”, the adversarial procedure significantly outperformed the inquisitorial one.

The empirical papers collected in the habilitation thesis demonstrate that the quasi-experimental and experimental research design can be fruitfully applied to answer novel question about the functioning of the law enforcement system. The results demonstrate that substantive and procedural laws alter incentives faced by both criminals and law enforcement officials. In turn, they have significant effects on their behavior and on the outcomes of the law enforcement systems; effects that are not necessarily obvious but that can be grasped with rigorous economic analysis.

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