

The Effects of a Simpler Criminal Procedure: Evidence from 1.1 Million Czech Cases

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Abstract

The paper estimates the effects of a simpler criminal procedure on case durations and the probabilities that the defendant is charged and convicted. The identification strategy exploits a policy reform in the Czech Republic as a quasi-natural experiment. The reform allowed petty offenses to be prosecuted via a simplified (fast-track) procedure but its actual implementation varied substantially across districts. We find very strong evidence that prosecuting a case via the fast-track procedure reduces the duration of the police/prosecutor phase of the procedure and increases the probability that the prosecutor charges the suspect at court. To a lesser extent, it also reduces the duration of the court phase of the procedure and increases the probability of conviction at trial. The fast-track procedure released resources that could potentially be spent on prosecuting serious cases. We investigate for possible spillover effects but find almost no evidence of such effects.

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1 Introduction

The design of the criminal procedure has to strike a delicate trade-off between competing objectives: assuring that 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 committing of the crime until 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 of evidence required to convict a defendant. However, collecting evidence and reaching a final verdict requires a substantial input of time and other resources from 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. A complex procedure with many steps increases the probability that the defendant will exploit 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 undesirable consequences, including 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 input.

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 an 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 decisions on merit, and in the cases that did proceed to decisions on merit, they were less likely to reverse or remand. Soares and Sviatchi (2010) evaluate the effects of 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 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 a shorter time and at a lower cost. It therefore releases resources that can be used to prosecute the remaining cases.¹ These cases can then also be 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 our best knowledge, there is no study empirically investigating 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 criminal case outcomes, namely 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 evidentially simple crimes to be prosecuted via a “fast-track” procedure. In practice, it became used most intensively for petty thefts, driving-related offenses and other simple crimes, typically if the offender is caught on the spot. 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.² 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 actual implementation of the fast-track procedure was gradual and varied substantially across judicial districts. To illustrate, 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

¹Easterbrook (1983). In contrast, Garoupa and Stephen (2008) give a more moderate view.

²Ministry of Justice of the Czech Republic (2001).

districts. Similar variation is observed in all offense categories, and it has persisted over time. Dušek (2015) documents 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 districts are rather small and numerous: there are 86 districts, with an average population of 110,000. The variation across districts is thus exploited to estimate its effects in a difference-in-differences framework.

The dataset is a full universe of cases prosecuted by prosecutors and all criminal cases adjudicated by the courts during 1998-2008. It contains detailed case-level information on 1.1 million cases: the exact legal definition of each offense, numerous case and offender characteristics, the dates of key procedural steps, procedural information including whether the case is prosecuted via the fast-track or conventional procedure, and final decisions (eg. charge, drop, convict, acquit). The data thus allows controlling in a rich way for the case and offender characteristics.

We present a simple theoretical framework in order to show that the simpler procedure could have affected the criminal case outcomes through two distinct effects: a direct effect on cases that are actually prosecuted by the fast track (eligible cases) spillover effect on ineligible, typically more serious cases. The spillover effects are driven by a higher overall use of the fast-track procedure. We then present two estimation approaches that disentangle the direct and the spillover effects.

We find very strong evidence of the direct effects, namely in the police/prosecutor phase of the procedure. Prosecuting a case through the fast-track reduces the case duration and increases the probability that the prosecutor submits charges to the court. These effects are found across almost all offense categories and our measures of the case suitability for the fast-track. We also find evidence of direct effects on the court duration and the probability of conviction at trial, although these are somewhat smaller in magnitude and breadth.

On the other hand, we find essentially no evidence of spillover effects. In some specifications, we find some, albeit rather weak, evidence of spillover effects with the “wrong” sign, whereby high use of the fast-track in some cases has a negative effect on other cases.

The overall effects of the fast-track procedure were economically significant. The duration of the case from offense to charges declined by 56 days, on average, in the offense categories with relatively high use of the fast-track, during the post-reform period. The fast-track procedure, as actually implemented, contributed 27 days to this decline. Similarly, the probability of charges increased by 11 percentage points, of which the fast-track accounts for 6 percentage points.

The paper is organized as follows. Section 2 describes the institutional setting of the fast-track procedure. Section 3 lays out the theoretical framework. Section 4 describes the data. In section 5, we describe the identification strategy, while the estimation methods and results are presented in section 6. In conclusions, we discuss the policy implications and alternative explanations for our findings.

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.³ 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 statutory sentence⁴ 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, 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 be from the very start supervised by the state attorney who would, at the end, review it and charge the defendant at court. The court could hold a preliminary hearing; then, at trial, the evidence would be re-presented and assessed by the judge. The deadlines faced by the law enforcers are reasonably flexible.⁵

³“Zkrácené přípravní řízení” in Czech. The reform was legislated by Act no. 265/2001.

⁴The Czech Criminal Code sets, for each offense, the minimum and maximum statutory sentence. The sentence imposed by the judge is a discretionary decision, which must strictly lie within this interval.

⁵For 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.

Under the fast-track procedure, the police accuses the defendant and hands the case over to the state attorney, who reviews the case and charges the defendant in the court. The text of the prosecution is simpler (containing 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 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 within two weeks of the crime being 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 officer⁷, although the prosecutor may reverse that decision. In practice, the two typically discuss each case informally. The letter of the legislation prescribes that all eligible cases should be prosecuted via the fast-track procedure. 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-à-vis the police, introduced some adversarial features, and shifted the burden of assessing the evidence from the police to the courts.

The reform appears to have had an effect on the crime rates. In a related paper (Dušek 2015) estimates its effects on crime rates, exploiting the variation in adoption across districts as 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 led 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.

⁶According 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.

⁷Only 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 prosecuted and punished according to the Criminal Code, it passes the case to the state police.

3 Theoretical framework

We present a very simple model in order to organize thinking about the predicted effects of introducing a simpler criminal procedure for petty crimes. The basic idea comes from Landes' (1971) model of optimal prosecutor behavior. Enforcement officials operate under resource constraints and have to allocate limited resources (their own time and budget) across individual cases. For simplicity, assume there are two types of case, 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 in court for a prosecutor, or the probabilities of conviction for a judge. The resource constraint is depicted as a curve PPF_1 in Figure 1, which shows the possible combinations of probabilities for serious and petty offenses. Allocating more resources to the enforcement of serious offenses increases p_H but requires a reduction in resources allocated to the enforcement of petty offenses 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 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 2), allowing the achievement of 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 outward shift of the PPF represents the resource-releasing hypothesis graphically.

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 serious crimes because petty crimes became relatively cheaper to enforce. The scale effect induces the official to allocate the released resources into both offense types and thus to increase output. Figure 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. We refer to the sum of the substitution and scale effects on petty offenses as the *direct effect*. The simpler procedure also has a *spillover effect* on serious offenses 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 2 is drawn such that the scale effect dominates, and the

probability p_H therefore increases).⁸

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 other, more serious offenses. The effect on serious offenses is thus ultimately an empirical question. Analogous predictions apply also to case durations, although with an opposite sign.

4 Data and summary statistics

The Czech Ministry of Justice provided us with two administrative databases of all criminal cases. One covers all cases closed by district prosecutors, and the other that were closed by district courts (including possible appeals) during 1998-2014. From these, we selected all cases where the prosecution started during 1998-2008 (the prosecutor database) and where the charges were bound over to the court during 1998-2008. Thus we cover four years before the reform (1998-2001) and the seven years afterward (2002-2008). The final regression-ready sample has over 1.1 million prosecutorial cases and over 950 thousand court cases.⁹

We use the following information about each case:

- Procedural dates: the date when the crime was committed, the date the prosecutor charged the defendant at court, and the date of final adjudication outcome.
- The final verdicts of the prosecutor (charging, dropping the charges, etc.) and the court (conviction, acquittal).
- For cases prosecuted after the reform, an indicator whether the case was prosecuted via the conventional or fast-track procedure.
- The legal definition of the offenses (the exact section and subsection of the Czech Criminal Code). We aggregate these very detailed definitions to 11 broader offense categories.¹⁰

⁸The theoretically predicted spillover effect is also the main reason why we do not exploit the variation in the fast-track implementation across offenses as an additional source of identifying variation in the empirical analysis.

⁹The reason for selecting cases up to 2008 is twofold. First, we observe the final outcomes for nearly all cases initiated during that period, with only a tiny fraction of cases not recorded in the database (those that laster for more than 6 years in case of the 2008 cases). Second, a new criminal procedure reform of 2009 expanded the range of eligible cases but also mitigated the incentives to process cases quickly. For that reason, we evaluate the effects of the reform only during the period covered by the same post-reform legislation, between 2002 and 2008.

¹⁰To classify offenses, we 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 fol-

- Case characteristics: number of charges per case, situational characteristics such as the presence of alcohol and basic victim information (man, woman, child) etc.
- Characteristics of the offender (gender, age, nationality, number of prior convictions).

We add to these data the maximum statutory sentences allowed by the Czech Criminal Code for each charge. This way, we can classify whether the case is potentially eligible for the fast-track procedure (the maximum statutory sentence up to 3 years) or ineligible (the maximum sentence exceeding 3 years).¹¹

The key outcome variables at the case level are:

- Duration from offense to charges (the police/prosecutor phase of the procedure)
- Duration from charges to final adjudication, incl. appeal (the court phase)
- Charges (the prosecutor files charges with court)
- Conviction (the final court verdict is conviction)

The main variation in the fast-track use occurs at the district level, and in our regressions, this is further broken down by case eligibility and offense category. We therefore construct several control variables at these aggregated levels:

- The share of cases prosecuted via the fast-track procedure.
- Total number of new cases.
- Number of new cases per individual prosecutor or court senate.¹²

Through the rest of the paper, the year of the case is based on when the procedural phase started. E.g., the 1998 cases in the prosecutorial phase include cases in which the police formally started the prosecution of a specific offender during 1998, and the 1998 cases in the

lowing the categorization of the Czech Criminal Code. We 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.

¹¹We use the term “potentially eligible” to emphasize that such cases meet one, although the most important criterion. The potentially eligible case need not be actually prosecuted via the fast track because it failed to meet the other criteria (evidentiary simplicity) or the police/prosecutor decided to prosecute it via the conventional procedure.

¹²The data contains an identifier for an individual prosecutor, and a court senate. The senate is the basic working units of Czech courts. It is a court department of typically 3 judges which is usually specialized in certain types of cases. Depending on the case difficulty, the case can be handled by a single judge or multiple judges; however, our data does not contain an identifier for the individual judge.

court phase include cases where the court received the charges from the prosecutor during the 1998. The outcomes of these cases may have been determined in 1998 or later years.

Table 1 shows the average characteristics of all cases, divided into the periods before and after the reform. The fast-track procedure was used to prosecute 24% of cases in total throughout the post-reform period. Potentially eligible cases made up 85 percent of cases before the reform; most of the cases are thus petty crimes punishable by up to three years. This share has increased to 88 percent, a rather slight overall shift in the composition of cases towards petty crimes. Among the case and offender characteristics, a noticeable difference between the pre-and post-reform period is an increase in the offenders' recidivism history.

The changes in the key outcomes of interest are illustrated in Figures 3 through 6. They show the evolution of the outcomes of interest, averaged at the country level, separately for potentially eligible and ineligible cases. We emphasize that the eligibility criterion is based on the statutory sentence only and expansion of the actual use of the fast-track procedure does not affect the composition of cases between these two groups. The potentially eligible cases still contain a large fraction of individual cases that are prosecuted via the conventional procedure. The duration of the police/prosecutor phase was on an increasing trend for both eligible and ineligible cases, reaching 300 and 450 days, respectively. It leveled off and later declined by 50 days for eligible cases but continued on the upward trend among the ineligible cases.

Figure 4 plots the probability of the key outcome of the prosecution, the probability of charges. The reform led to an immediate jump in this probability for the potentially eligible offenses by 9 percentage points. The probability of charges continued to grow throughout the post-reform period, reaching almost 90 percent. For ineligible offenses, the probability of charges also jumped up in the reform year but by a much smaller magnitude.

The duration of the procedure in court (Figure 5) declined by approximately 100 days for both eligible and ineligible offenses after the reform. Finally, the probability of conviction at court, conditional on being charged is depicted in (Figure 6). The U-shaped pattern is similar for both potentially eligible and ineligible offenses.

5 Empirical strategy

5.1 Key empirical issues

The empirical objective is to estimate the direct and spillover effects of the fast-track procedure, exploiting the variation between districts. The direct effect denotes the effect on an

individual case of the fast-track procedure being applied in that case. The spillover effect denotes the effect on an individual case of the overall usage of the fast-track in all other cases. The idea is that the magnitude of the spillover is determined by the total amount of time and other resources that were released by the fast-track. Before explaining the strategy that we actually use, it is illustrative to first consider the estimation we would prefer to perform if we had a perfect experiment available, and then to address the real-world complications.

The ideal experiment would involve randomization at two levels. First, districts would be randomly assigned the share of cases that they should prosecute via the fast-track. Second, districts would select individual cases for the fast-track at random. One would then estimate the direct effect by a dummy equal to one if an individual case is prosecuted via the fast-track. The spillover effect would be captured by a variable equal to the overall share of fast-track cases in a district.

The variation at the district level is more significant for our research question. We are attempting to identify the equilibrium effects of a simpler criminal procedure, and such effects occur at the jurisdiction level. With 86 districts, we have a large number of jurisdictions to estimate the effects. In the subsection below, we argue that the assignment of the fast-track shares at the district level can be regarded, for all practical purposes, as random.

5.2 Identifying variation

The actual adoption of the fast-track procedure was gradual and varied widely across offense categories and districts. Table 2 shows the mean, standard deviation, and the 5th and 95th percentiles of the share of fast-track cases in 2002 (the first post-reform year) and in 2008 (the last year in our data) at the district level. Only offense categories with a high use of the fast-track procedure are reported: thefts/burglaries, driving offenses,¹³ 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 percent fast-track share already in the first post-reform year - they are typically simple offenses with 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 average share of the fast-track cases was 23 percent, varying from 6 percent in the 5th percentile to 43 percent in

¹³The 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 order, and by far the most common violation involves driving with a suspended driver's license.

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 cases among the reported offense categories 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 or durations. They may also adopt other measures aimed at cutting case durations, introducing an omitted variable bias.

We 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 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 sub-units. Certain officers (e.g., patrol officers) can only prosecute via the fast track while others (investigative) have 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 cases 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 cases 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 adoption were partially driven by the relative overload of the police officers and prosecutors. Police officers in districts with higher caseload tended to adopt the fast track more intensively in order to take more cases "off the table". In districts with low caseload, the officers reported that there was no pressure to spend time and effort on learning and adopting 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 postulate 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.

An important institutional feature concerns the court trials. The courts must “stick” with the procedure set by the police/prosecutor. Hence the procedural choice is entirely exogenous from the courts’ point of view. Econometrically it is indeed exogenous unless the police and the prosecutor select the procedure based on the anticipation of the case duration or the case outcome at the court level.

We check for potential determinants of the fast-track adoption. We define the share of fast-track cases among offenses with a high fast-track use in the first post-adoption year (2002) as a measure of adoption intensity in a district. Figure 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 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, we estimated numerous regression specifications explaining the share of fast-track cases as a function of pre-reform levels of case duration, caseload, crime rates, or their pre-reform trends. None of these variables were statistically significant predictors of the intensity of fast-track adoption.

6 Estimation and results

While the key variation occurs at the district level, our regressions are estimated at the case level. The case-level data allows us to control in a rich way for the case and offender characteristics, so that differences in the case composition across districts do not represent a major empirical issue. When specifying the case-level regressions, one more issue needs to be addressed: the assignment of individual cases to the fast-track within the district is clearly non-random. By the last eligibility criterion, the fast-track cases are evidentially simple cases and would exhibit shorter duration and higher likelihoods of charges and convictions even in the absence of the fast-track. This feature would bias the coefficient on the fast-track case dummy away from zero. On the other hand, the districts that use the fast-track procedure intensively inevitably end up extending it to more sophisticated cases. This feature creates

an opposite bias.

We propose two approaches to dealing with the issue, and we describe them and present the results for each in turn.

6.1 Estimation 1: Marginal effects of more intensive use

The first approach estimates the marginal effect of a more intensive use of the fast-track on the outcomes of an average case within a reasonably narrow group of cases. It estimates the direct effect not by a dummy at the case level, but by the share of fast-track cases at level of the district, offense category and eligibility.

Specifically, we estimate a difference-in-differences specification, separately for each offense category, further divided into potentially eligible and ineligible offenses:

$$y_{ieojt} = \beta_{eo}^D s_{eojt} + \beta_{eo}^S \overline{s_{eojt}} + \gamma_o X_{ieojt} + \delta_o X_{jt} + \lambda_{eoj} + \lambda_{eot} + \epsilon_{ieojt} \quad (1)$$

where y_{ieojt} is the outcome variable for a case i of an eligibility statut e and offense category o in district j and year t .

The key treatment variable is s_{eojt} , the share of fast-track cases with an eligibility status e in offense category o , while $\overline{s_{eojt}}$ is share of the fast-track cases in a district in all cases *other than* eo . X_{ieojt} denotes a vector of case and offender characteristics and X_{jt} denotes characteristics of the criminal justice system in the district.¹⁴ λ_{eoj} and λ_{eot} are the district and year fixed effects, again specific for the offense and eligibility categories and ϵ_{ieojt} is the error term.

β_{eo}^D and β_{eo}^S are the key parameters of interest. β_{eo}^D has the interpretation of the direct effect: a change in outcome for offense \times eligibility category eo due to a one unit increase in the share of fast-track cases in that category. The treatment variable hence takes the same value within a district and offense/eligibility category, irrespective of whether an individual case is prosecuted via the fast-track or not. Unlike the fast-track case dummy, this treatment variable avoids the issue of the fast-track cases being inherently simpler due to unobservables. β_{eo}^D should thus be understood as the marginal effect of an additional fast-track case within a narrow offense category.

¹⁴The case and offender characteristics are: the number of charges per case, dummies for the presence of selected eight most frequent sections of the criminal code among charges, maximum statutory sentence, dummies for the situational information, number of prior convictions, dummy for a pretrial detention, defendant age, dummies for gender and foreign status, and dummies for educational categories. The district characteristics are: the number of cases per prosecutor or court senate, and the number of prosecutors or court senates, respectively.

β_{eo}^S has the interpretation of the spillover effect, as it captures the overall use of the fast track in the district in offense categories other than eo .

For ineligible offenses, there is only a spillover effect but no direct effect. When equation 1 is estimated for ineligible offenses, the term s_{eojt} is dropped. In both sets of regressions, the parameters of interest are identified by comparing the change in the outcome variable between districts with varying intensity of the fast-track adoption. The identifying assumption requires that the district-specific trends in unobservables are uncorrelated with the share of fast-track cases in a district. We 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.

6.2 Results 1: Marginal effects of more intensive use

The estimates are presented in Tables 3 through 6, one for each outcome variable. The upper panels show the estimates of β_{eo}^D and β_{eo}^S for potentially eligible offenses, and the lower panels the estimates of β_{eo}^S for ineligible offenses.¹⁵ The offense categories are arranged such that columns (1)-(5) include categories with a relatively high fast-track use while columns (6)-(11) include those with a sporadic fast-track use.

Table 3 reports the effects on the duration of the police/prosecutor phase, from offense to charges. All estimated direct effects are negative, significant at 1%, and range from -83 (driving offenses) to -332 (offenses against family). The magnitudes imply that an increase in the share of fast-track cases by 10 percentage points reduces the duration by 9 days for thefts, 20 days for other property/economic offenses, 8 days for driving offenses etc. The direct effects are systematically greater in offense categories with a sporadic use of the fast track (offenses against life and health, sex offenses, fraud/embezzlement etc). This suggests that the fast track had greater effect on the margin when used to prosecute somewhat more sophisticated and serious offenses.

The estimated spillover effects on the potentially eligible cases (second row of table 3) vary in sign and magnitude and are not statistically significant (with only one exception of the offenses against public order). Similarly, the lower panel shows the spillover effects on the more serious, ineligible cases. Again, the coefficients have varying signs and magnitudes, but are not statistically significant. Overall, they do not provide any consistent evidence of the expected spillover effect, reducing the case duration over the mechanical direct effect.

Table 4 reports the effects on the final outcome of the prosecutorial phase of the procedure,

¹⁵To save on space, the coefficients on the control variables are not reported. Full results are available upon request.

the decision to charge the defendant at court. Again, there is a very strong evidence of a direct effect: The estimated direct effects are all positive and they are statistically significant at all but two offense categories. They are significant at 1 percent in the five offense categories with a high fast-track use (columns 1-5). In terms of magnitude, a 10 percentage point increase in the share of fast-track cases is associated with an increase in the probability of charges by 0.9 percentage points (thefts), 2.7 percentage points (other property/economic crimes), 0.8 percentage points (driving offenses), etc.

Similarly to the previous table, the estimated spillover effects are insignificant and vary in size. This is the case of potentially eligible as well as ineligible cases.

In table 5 we report the estimates for the duration of the court phase, measured by the duration from charges to the final adjudication (incl. the resolution of a possible appeal). The direct effects are generally negative, and they are negative for all offense categories with a high fast-track use. However, they are smaller in magnitude (compared to the prosecutorial phase) and are significant only for two categories, thefts and sex offenses. In terms of magnitude, a 10 percentage point increase is associated with a reduction of the court duration by 8 days (thefts, property/economic offenses), 1 day (driving offenses), 5 days (offenses against personal liberty) etc.

The spillover effects on potentially eligible cases have generally an unexpected positive sign and are statistically significant for three offenses categories. Likewise, the estimated spillover effects on the ineligible cases give no evidence of a reduction in the court duration. Rather, they tend to indicate an increase in duration through the spillover effect, although statistically insignificant. Such “perverse” spillover effects may be explained by the reallocation of the court’s resources towards the fast-track cases. If the courts experience a large number of fast-track cases in some offense categories, they appear to prioritize to resolve these simple cases, thus prolonging the duration in the other categories. In the language of the model (section 3), the substitution effect dominates.

Last, the estimates for the probability of conviction are shown in table 6. There are positive and statistically significant direct effects in several offense categories. A 10 percentage point increase in the share of fast-track cases increases the probability of conviction by 0.9 percentage points (thefts), 1 percentage point (property/economic offenses), 0.7 percentage points (offenses against personal liberty) or 1.7 percentage points (offenses against life and health).¹⁶ The spillover effects tend to be negative and are also significant in several offense categories. For example, a 10 percentage point increase in the share of fast-track cases other than theft reduces the likelihood of conviction for thefts by 1.4 percentage points. The estimates for

¹⁶These estimates are potentially contaminated by a sample composition bias that, however, biases the estimates downwards. As the share of the fast-track cases increases, the composition of the fast-track cases shifts towards more complex cases, where we would expect a lower probability of conviction.

ineligible cases show no evidence of spillover effects.

6.3 Estimation 2: Deciles of fast-track suitability

The second estimation approach is based on the fact that some cases are more suitable for the fast-track procedure than others; such suitability can be captured by observables. The estimation of the direct effect compares the outcomes of the fast-track cases with non-fast-track cases that are otherwise similar in terms of their predicted suitability for the fast-track.

Specifically, we first estimate a linear probability model explaining the prosecution of each case through the fast-track procedure. As explanatory variables, we use the same case and offender controls used in regression 1 in the prosecutorial dataset. The regression is estimated separately for each offense category, and only on a subsample of potentially eligible cases. We only include cases from 2006-2008, when the implementation of the fast-track procedure appears to have reached a stable equilibrium. We emphasize that we do not include district fixed effects in order to avoid district differences in adopting the fast-track; we only want to explain the likelihood of the fast-track by the characteristics of the case as such. With these estimates, we predict the likelihood of the fast-track procedure for every potentially eligible case in the sample, before and after the reform. We label this predicted likelihood the “suitability” of the case for the fast-track procedure.

We then divide the potentially eligible cases into the deciles of the fast-track suitability. Note that several offense categories are generally mixed inside each decile. We create an additional group, the ineligible cases in which the likelihood of the fast-track is by construction zero. Then, within each decile d (plus the group of ineligible cases) we estimate the regression:

$$y_{idjt} = \beta_d^D D_{idjt} + \beta_d^{S^d} s_{djt} + \beta_d^{\overline{S}} \overline{s}_{djt} + \gamma_d X_{idjt} + \delta_d X_{jt} + \lambda_{do} + \lambda_{dj} + \lambda_{dt} + \epsilon_{idjt}$$

where the subscripts $idjt$ denote the case, decile, district and year, respectively. The treatment variable capturing the direct effect is D_{idjt} , a dummy for a given case being prosecuted via the fast-track. The parameter β_d^D hence measures the (decile-specific) direct effect on the cases actually prosecuted via the fast-track. Specification 6.3 allows for two types of spillover effects: variable s_{djt} denotes the share of fast-track cases within a given decile (and district and year). It captures the effect of a higher fast-track use in given decile on individual cases in that decile. For example, if there are many fast-track cases among the most suitable ones (decile 10), this affects the outcome of each individual case, irrespective of whether that case is prosecuted via the fast-track or not. The parameter $\beta_d^{S^d}$ can be thought of as a within-decile spillover effect. Last, the variable \overline{s}_{djt} denotes the share of fast-track cases in a particular

district and year in deciles *other than* decile d . The parameter β_d^{So} is thus a spillover effect associated with a high overall fast-track use in a district, conceptually analogous to the spillover effects in equation 1.

6.4 Results 2: Deciles of fast-track suitability

The estimates are presented in Tables 7 through 10, one for each outcome variable. The general pattern is very similar to the previous estimation approach: Very strong evidence of direct effects, especially in the prosecutorial phase, and virtually no evidence of the spillover effects.

In table 7, the direct effects on the duration from offense to charges are negative and significant at 1 % level in all suitability deciles. They are smaller in the upper five deciles (a reduction in duration by 130 to 191 if a case is prosecuted via the fast-track) than in the lower 5 deciles (cut in duration by 245 to 333 days). This indicates that the time-reduction benefits of the fast track increase with the case complexity. The within-decile spillover effects have varying signs, but they have a positive sign and are significant in the 6th to 8th deciles. The overall spillover effects are generally small and insignificant as well, but they are also positive, rather large, and significant in the two upper deciles. Both of these results may indicate a crowding-out effect; e.g., a large use of the fast-track overall (hence reaching the relatively more complex cases) shifts the prosecutorial priorities away from the simplest cases that would be prosecuted via the fast-track anyway.

The direct effects on the probability of charges (table 8) are also significant at 1% level in all deciles. Their magnitude varies between 5.5 to 11.8 percentage point increase in the likelihood of charges, and it tends to be the highest in the intermediate deciles.

The results for the court durations (table 9) also show strong direct effects across all deciles, negligible spillover effects within deciles, and generally insignificant overall spillover effects with the “wrong” sign. Finally, table 10 shows positive direct effects on the probability of conviction, especially in the top deciles. Charging the case through a fast-track procedure leads to more than 5 percentage points increase in the probability of conviction.

Notably, none of the tables above gives evidence of the expected spillover effects on the more serious, ineligible offenses (columns 1).

(ROBUSTNESS CHECK SECTION TBA)

6.5 Economic significance

The regressions show that the fast-track procedure has statistically significant effects, particularly in the prosecutorial phase. What was the total effect of the fast-track procedure, as actually implemented? In order to answer this question, we compare the change in actual outcomes with a change in counterfactual outcomes. To construct the counterfactual, we use the regression coefficients from tables 3 and 4 to predict the outcome 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 and the year dummies would have evolved as they actually did. Table 11 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. Among the listed high fast-track offenses, the duration from offense to charges declined by 55 days on average, of which 27 days is attributable to the fast-track procedure.

The bottom panel of Table 11 reports the results of an analogous counterfactual exercise for the probability of charges. In theft cases, it 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 offenses and offenses against personal liberty. Among all the offenses with a high fast-track use, the probability of charges increased by 11 percentage points, of which the fast-track accounts for 6 percentage points.

7 Conclusions

The paper provided evidence that introducing a simpler criminal procedure has some important effects on the outcomes of criminal cases. We attempted to distinguish the direct effects of a given case being prosecuted via the simpler procedure, and spillover effects of the overall use of the simpler procedure.

We find very strong evidence of the direct effects, namely in the police/prosecutor phase

of the procedure. Prosecuting a case through the fast-track reduces the case duration and increases the probability that the prosecutor will bring charges to the court. These effects are found across almost all offense categories and across all levels of the predicted suitability of the case for the fast-track. We also find evidence of direct effects on the court duration and the probability of conviction at trial, although these are somewhat smaller in magnitude and breadth.

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. Alternative criminal procedures such as the fast-track procedure, plea bargaining or penal order can indeed be thought of as a “technological improvement”. They do allow cases to be processed faster and also allow prosecutors to successfully complete a higher fraction of cases all the way to charging the defendant at court. On the policy side, the paper demonstrates that countries burdened with an 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.

On the other hand, there is a trade-off. The evidence of an increase in the probability of conviction at trial raises the possibility that the efficiency gains come at the expense of accuracy. The increase in the probability of conviction is consistent with two explanations that are not mutually exclusive: If the fast-track procedure eliminated several loopholes that the factually guilty defendants used to exploit to escape punishment, it reduced the incidence of Type II errors. If it abridged some procedural rights such that factually innocent defendants are more likely to get convicted, it increased the incidence of Type I errors. Assessing the relative importance of the two explanations is beyond the scope of this paper (and our data availability). But our results clearly point out that the procedural rules have an effect on conviction/acquittal outcomes.

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 that plea bargaining releases resources and increases the prosecutors’ productivity even in cases that reach trial. These beneficial spillovers onto more serious cases were in fact an explicit objective of the reform. However, we find essentially no evidence of such desirable spillover effects. In some specifications, we find some, albeit rather weak, evidence of “perverse” spillover effect whereby high use of the fast-track in some cases has a negative effect on other cases. Overall, the results give little empirical support for the resource-releasing hypotheses.

Why is there an absence of significant spillover effects? We discuss three possible explana-

tions. One, the adoption of the fast-track procedure (24 percent of cases throughout the post-reform period) was insufficient to produce visible spillover effects. The findings are thus highly context-dependent to the actual implementation of the fast-track in the Czech system, as opposed to the predominance of pleas bargaining in the U.S. system. Second, the potential benefits of the reform were unexploited. The Czech enforcement agencies tend to be rather compartmentalized, with individual police units, prosecutors, and courts specializing in certain types of cases. It is highly possible that the units working on the simple cases simply continued working on their simple cases under the fast-track, and their time was not reallocated to more complicated, ineligible cases. (Further research would be necessary to demonstrate this.)

Third, theoretical framework of section 3 provides the last 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 stem 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 demonstrated by Dušek (2015), it led 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. Likewise, some of our current results are consistent with the story that as the courts received more simple, fast-track case, they tended to “neglect” the other cases, and the outcomes of those cases worsened. The desired spillover effects on serious cases may thus have been largely undone by the reallocation of enforcement resources towards the simple cases.

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Figure 1: Constraints and preferences of enforcement officials

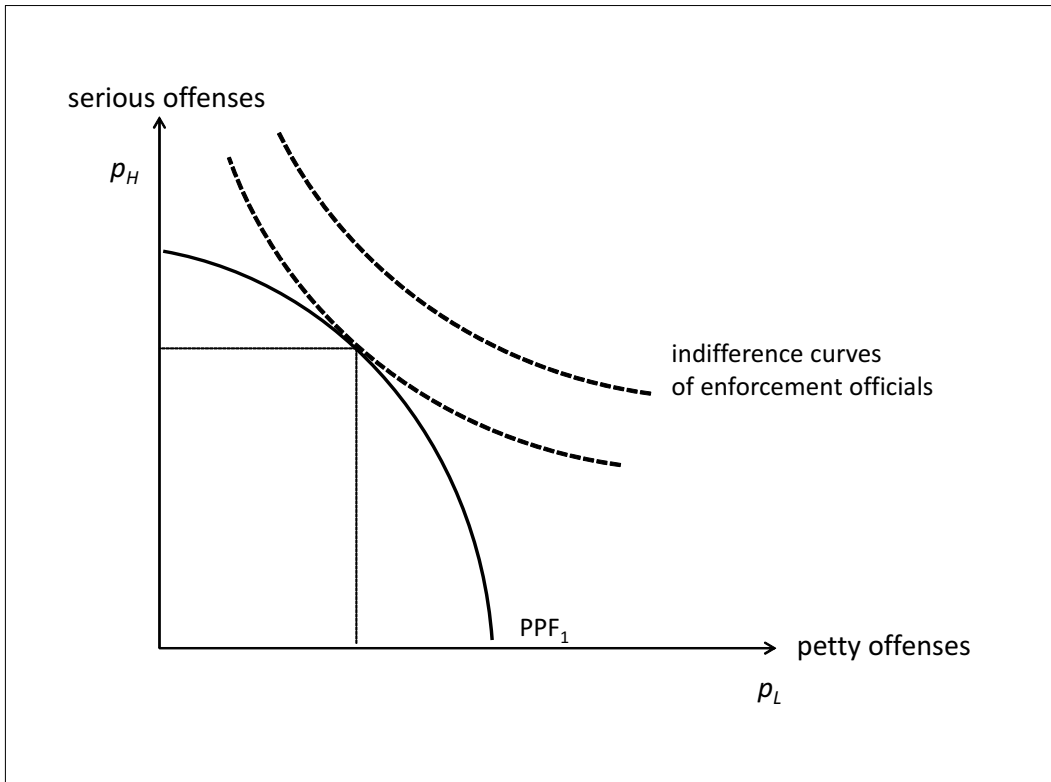


Figure 2: Response to a decrease in the cost of enforcing petty crimes

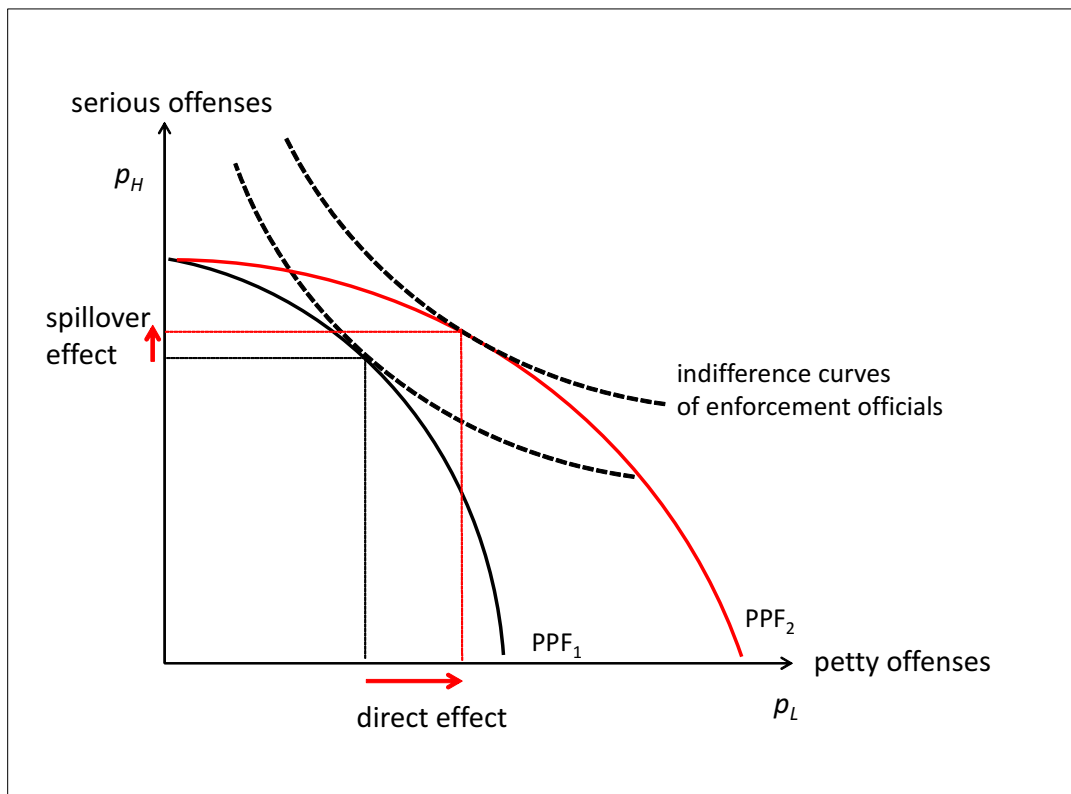


Figure 3: Average duration from offense to charges, by fast-track eligibility

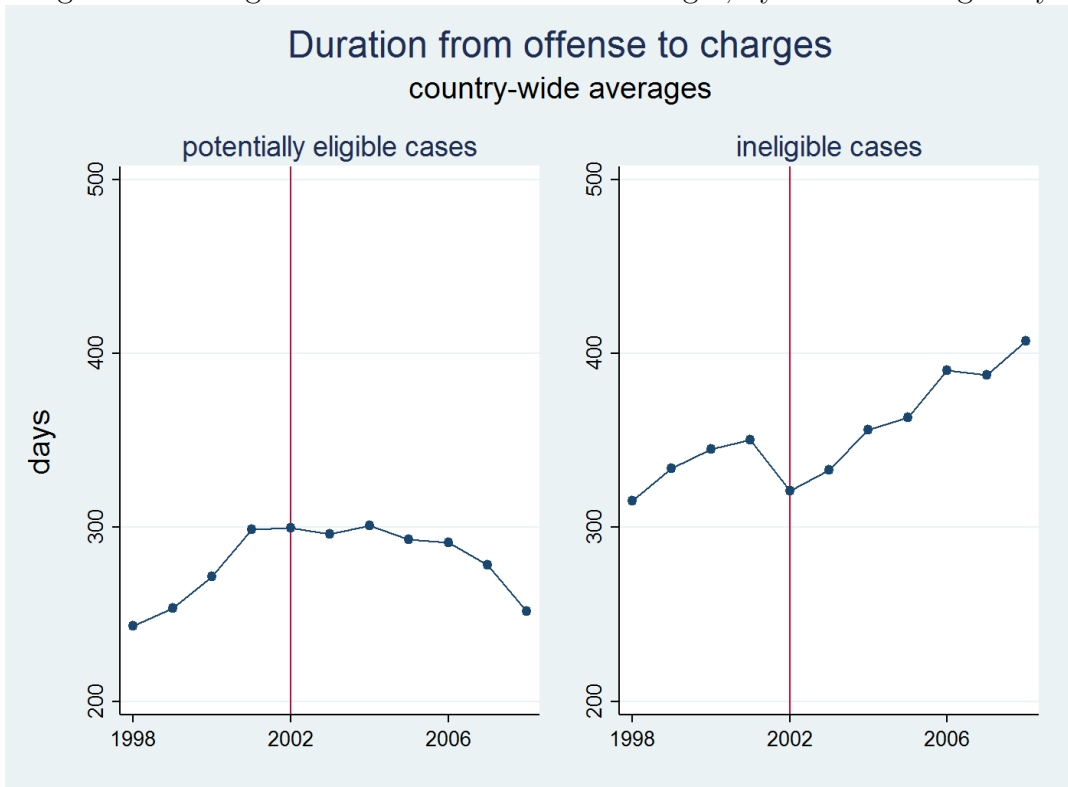


Figure 4: Average probability of charges, by fast-track eligibility

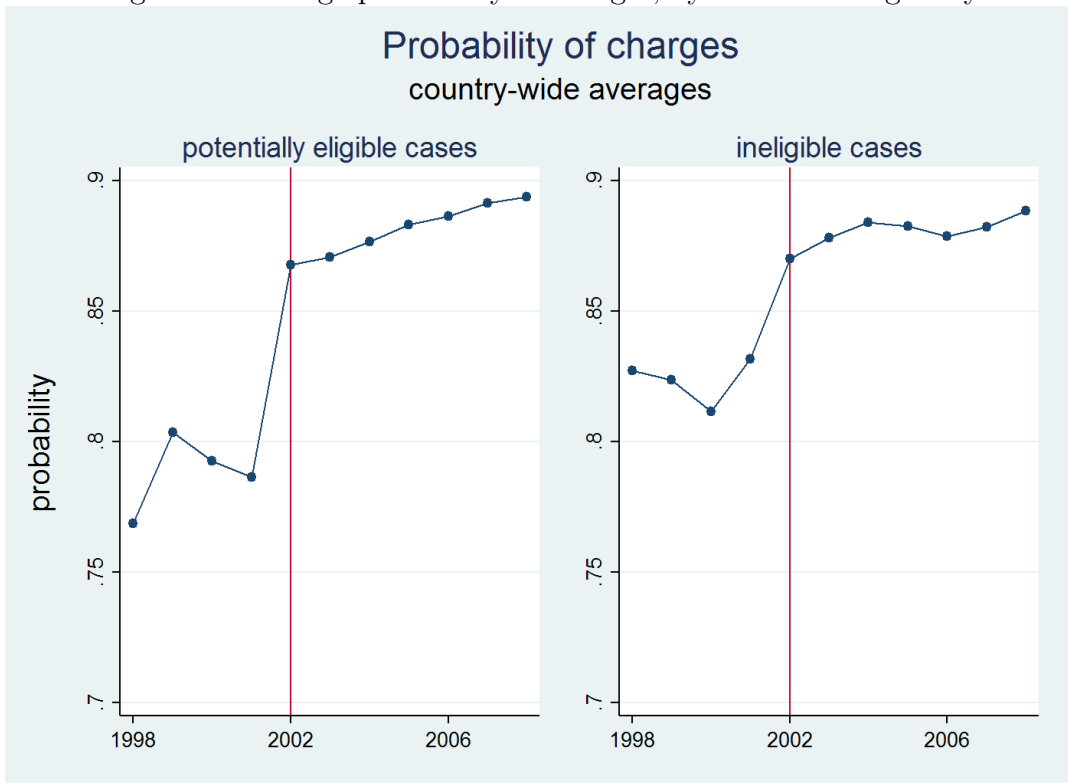


Figure 5: Average duration from charges to final adjudication, by fast-track eligibility

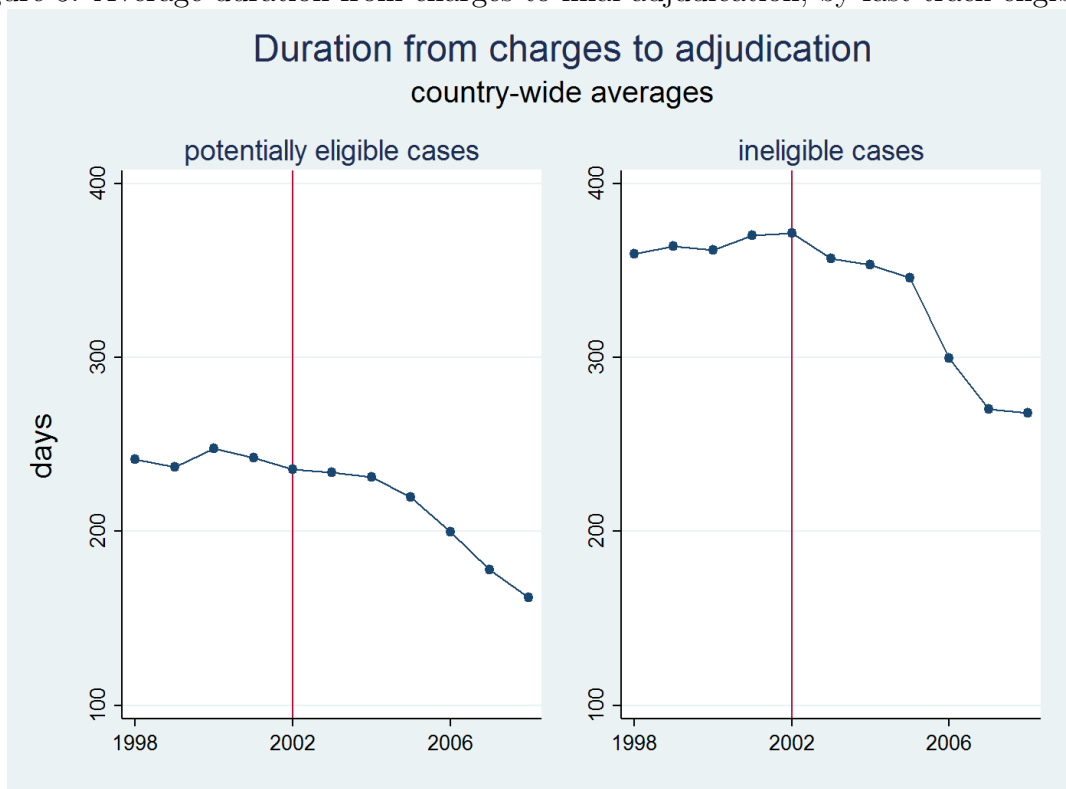


Figure 6: Average probability of conviction, by case eligibility

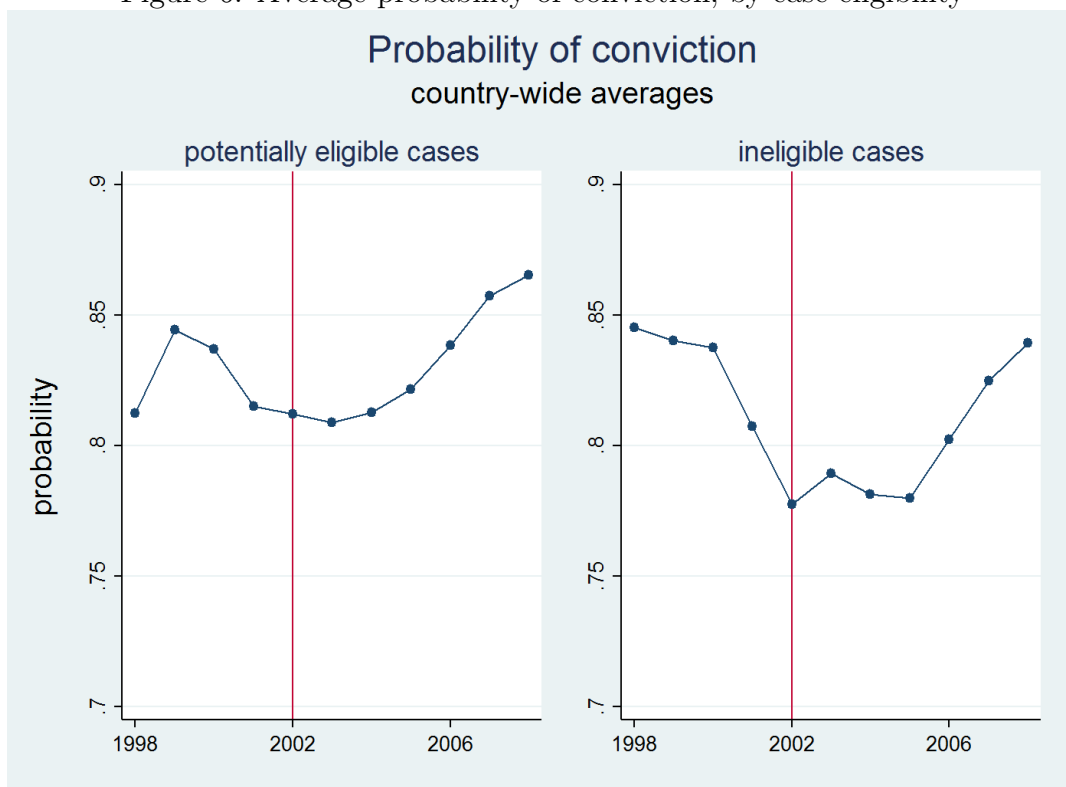


Figure 7: Endogeneity of fast-track adoption: levels

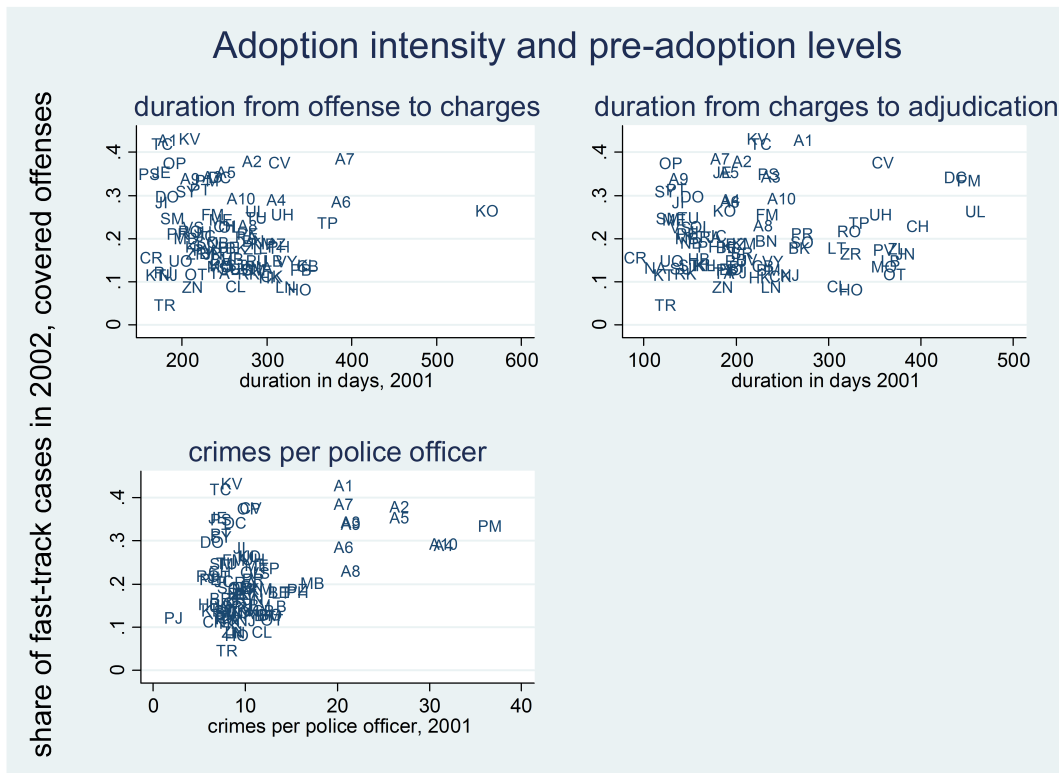


Figure 8: Endogeneity of fast-track adoption: trends

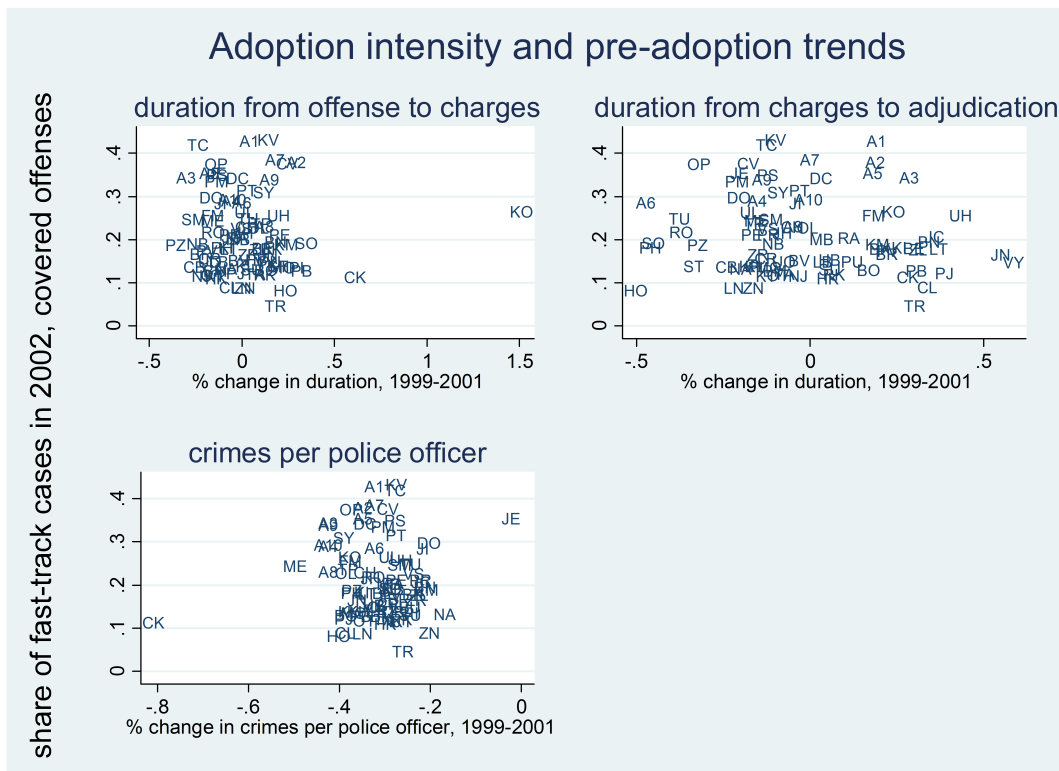


Table 1: Summary statistics

Variable	pre-reform			post-reform		
	obs	mean	sd	obs	mean	sd
fast-track cases (share)	386,619	0.00	0.00	739,616	0.24	0.43
potentially eligible case (share)	386,619	0.85	0.35	739,616	0.88	0.33
duration offense to charges (days)	341,743	278	304	738,849	296	405
probability of charges	386,619	0.79	0.40	739,616	0.88	0.32
duration charges to conviction (days)	314,927	259	375	646,688	221	319
probability of conviction	314,927	0.83	0.38	646,688	0.83	0.38
number of charges per case	386,619	1.33	0.70	739,616	1.30	0.66
offender female	386,619	0.11	0.31	739,616	0.13	0.33
offender foreign	386,619	0.06	0.24	739,616	0.05	0.22
offender age	386,617	30	11	739,616	32	11
number of offender's prior convictions	314,927	1.99	3.02	646,688	2.55	4.31

Table 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 high fast-track use 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 high fast-track use offenses	35	24	7	85

Note: The unit of observation for the summary statistics is district-year-offense category.

Table 3: Duration from offense to charges, marginal effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	theft/ burglary	property/ economic	driving	against personal liberty	against public order	robbery	against life/ health	sex offenses	against family	fraud/ embezzl.	against public safety
direct effect	-87.76*** (10.75)	-208.6*** (44.99)	-83.26*** (9.075)	-139.0*** (16.90)	-119.6*** (21.09)		-193.3*** (44.14)	-214.6*** (40.03)	-332.5*** (109.3)	-204.1*** (72.53)	-144.5*** (23.69)
spillover effect	121.8 (87.33)	-371.2 (331.6)	-108.7 (87.98)	158.8 (144.4)	103.5 (206.4)	95.29 (100.7)	269.2 (170.9)	66.12 (306.5)	-36.85 (401.7)	286.8 (380.0)	422.3*** (189.9)
observations	253,495	71,853	142,573	48,494	82,502	11,837	51,064	7,695	104,526	163,681	14,502
R-squared	0.129	0.419	0.105	0.113	0.129	0.435	0.121	0.121	0.093	0.121	0.129
potentially eligible cases											
ineligible cases											
spillover effect	120.2 (203.9)	-200.9 (309.8)	-74.65 (255.6)	-121.4 (155.5)	95.29 (100.7)	37.73 (42.81)	63.49** (31.59)	44.39 (118.5)	-489.8 (410.0)	112.2 (152.5)	142.4 (87.11)
observations	5,961	4,127	2,993	3,722	11,837	24,772	36,434	15,504	2,098	11,490	18,887
R-squared	0.209	0.375	0.169	0.281	0.435	0.113	0.145	0.132	0.237	0.132	0.165
district fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (clustered by district) in parentheses.

All regressions include control variables described in section 6.1

Table 4: Probability of charges, marginal effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	theft/ burglary	property/ economic	driving	against personal liberty	against public order	robbery	against life/ health	sex offenses	against family	fraud/ embezzl.	against public safety
direct effect	0.0905*** (0.0254)	0.272*** (0.0458)	0.0847*** (0.0247)	0.116*** (0.0260)	0.103** (0.0453)		0.112 (0.125)	0.0251 (0.0618)	0.102** (0.0436)	0.128** (0.0613)	0.139*** (0.0450)
spillover effect	0.0502 (0.266)	0.343 (0.242)	-0.00425 (0.145)	-0.00192 (0.285)	-0.0970 (0.300)	0.0962 (0.115)	-0.862* (0.484)	-0.170 (0.415)	0.270 (0.183)	-0.0752 (0.182)	0.0732 (0.449)
observations	264,355	76,927	144,194	50,866	86,965	12,229	55,029	8,336	109,654	167,768	15,320
R-squared	0.052	0.063	0.053	0.086	0.061	0.054	0.155	0.073	0.032	0.045	0.136
ineligible cases											
spillover effect	-0.0162 (0.0852)	0.141 (0.148)	0.00645 (0.170)	0.0647 (0.132)	0.0962 (0.115)	-0.00302 (0.0336)	-0.00833 (0.0832)	0.126* (0.0703)	-0.170 (0.179)	-0.00653 (0.0627)	-0.0260 (0.0823)
observations	6,255	4,516	3,128	3,812	12,229	25,665	37,901	16,902	2,130	12,273	19,461
R-squared	0.094	0.134	0.086	0.107	0.054	0.049	0.133	0.080	0.109	0.082	0.110
district fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (clustered by district) in parentheses.

All regressions include control variables described in section 6.1

Table 5: Duration from charges to adjudication, marginal effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	theft/ burglary	property/ economic	driving	against personal liberty	against public order	robbery	against life/ health	sex offenses	against family	fraud/ embezzl.	against public safety
potentially eligible cases											
direct effect	-81.94** (41.21)	-78.71 (56.27)	-8.244 (25.15)	-50.89 (52.92)	-40.21 (35.28)		-166.2 (120.3)	-213.5** (85.92)	57.87 (47.89)	2.615 (83.11)	7.378 (30.61)
spillover effect	152.9** (68.09)	101.3 (98.07)	43.29 (47.36)	138.1* (75.83)	8.488 (81.79)	109.2 (137.5)	7.024 (113.7)	175.4 (244.1)	161.5*** (57.45)	104.1 (90.57)	-412.7 (334.6)
observations	219,330	60,285	127,470	53,044	67,822	10,697	47,315	8,499	103,969	146,684	10,797
R-squared	0.254	0.268	0.129	0.259	0.225	0.455	0.186	0.391	0.215	0.264	0.244
ineligible cases											
spillover effect	503.7* (300.6)	168.4 (248.1)	180.3 (141.8)	156.0 (174.8)	109.2 (137.5)	-23.42 (92.91)	99.56 (86.45)	169.5 (132.1)	184.3 (145.1)	169.6 (137.6)	-91.61 (87.93)
observations	4,472	3,397	2,835	3,765	10,697	21,710	27,044	13,284	1,661	9,006	16,899
R-squared	0.406	0.413	0.291	0.363	0.455	0.381	0.224	0.369	0.386	0.376	0.326
district fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (clustered by district) in parentheses.

All regressions include control variables described in section 6.1

Table 6: Probability of conviction, marginal effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	theft/ burglary	property/ economic	driving	against personal liberty	against public order	robbery	against life/ health	sex offenses	against family	fraud/ embezzl.	against public safety
direct effect	0.0849** (0.0392)	0.0960** (0.0429)	0.0186 (0.0131)	0.0734*** (0.0257)	0.0245 (0.0285)		0.174** (0.0683)	-0.115 (0.0800)	-0.00801 (0.0361)	-0.0121 (0.0356)	-0.0527 (0.0477)
spillover effect	-0.141*** (0.0532)	-0.0292 (0.109)	-0.0567 (0.0405)	-0.149** (0.0640)	0.127 (0.0860)	0.000544 (0.0926)	0.122 (0.147)	0.236* (0.130)	-0.121** (0.0585)	-0.0170 (0.0853)	-0.533 (0.521)
observations	219,330	60,285	127,470	53,044	67,822	10,697	47,315	8,499	103,969	146,684	10,797
R-squared	0.051	0.091	0.047	0.072	0.063	0.135	0.120	0.074	0.040	0.050	0.103
ineligible cases											
spillover effect	0.221 (0.205)	-0.102 (0.237)	-0.227 (0.193)	0.199 (0.210)	0.000544 (0.0926)	0.0179 (0.0597)	-0.0587 (0.0503)	-0.0534 (0.0959)	-0.174 (0.217)	-0.116 (0.121)	0.00999 (0.0566)
observations	4,472	3,397	2,835	3,765	10,697	21,710	27,044	13,284	1,661	9,006	16,899
R-squared	0.164	0.147	0.088	0.159	0.135	0.102	0.111	0.117	0.255	0.085	0.092
district fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (clustered by district) in parentheses.

All regressions include control variables described in section 6.1

Table 7: Duration from offense to charges, by deciles of fast-track suitability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	ineligible cases	dec=1	dec=2	dec=3	dec=4	dec=5	dec=6	dec=7	dec=8	dec=9	dec=10
direct effect		-333.1*** (25.40)	-287.6*** (13.59)	-244.8*** (11.15)	-252.8*** (17.30)	-280.8*** (10.46)	-191.1*** (5.783)	-154.7*** (4.995)	-145.2*** (4.959)	-144.7*** (5.557)	-130.0*** (3.511)
spillover effect – decile		-48.77 (191.1)	-109.4 (116.6)	58.29 (65.98)	-53.63 (46.58)	103.4* (57.80)	111.0*** (27.07)	37.72*** (11.65)	24.94** (12.36)	19.52 (13.02)	6.721 (9.773)
spillover effect – overall	-45.03 (54.89)	-45.28 (80.48)	35.68 (65.52)	-15.31 (58.82)	40.45 (71.75)	-6.203 (77.53)	21.06 (40.02)	19.54 (20.71)	46.53 (28.24)	83.95*** (24.29)	82.19*** (21.25)
observations	107,101	87,370	92,225	92,345	88,877	66,436	76,945	57,109	92,010	93,187	96,877
R-squared	0.410	0.393	0.175	0.277	0.210	0.286	0.122	0.191	0.238	0.237	0.235
district fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
offense fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (clustered by district) in parentheses.

All regressions include control variables described in section 6.1.

The potentially eligible cases are divided into deciles of fast-track suitability, defined as the predicted likelihood of a case being prosecuted via the fast-track based on the observable characteristics of cases during 2006–2008.

Table 8: Probability of charges, by deciles of fast-track suitability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	ineligible cases	dec=1	dec=2	dec=3	dec=4	dec=5	dec=6	dec=7	dec=8	dec=9	dec=10
direct effect		0.0553*** (0.0115)	0.0884*** (0.00789)	0.0984*** (0.00625)	0.0955*** (0.00926)	0.103*** (0.00570)	0.118*** (0.00646)	0.109*** (0.00561)	0.0871*** (0.00593)	0.0984*** (0.00606)	0.0901*** (0.00571)
spillover effect – decile		0.102 (0.137)	0.0264 (0.0591)	0.0326 (0.0653)	0.0504 (0.0567)	0.0718 (0.0545)	-0.00931 (0.0404)	0.000410 (0.0255)	0.0336 (0.0290)	0.00516 (0.0222)	-0.0177 (0.0349)
spillover effect – overall		-0.0352 (0.0539)	-0.0316 (0.0407)	0.0379 (0.0471)	0.0379 (0.0517)	-0.0416 (0.0528)	0.103** (0.0480)	-0.0421 (0.0726)	-0.00553 (0.0570)	-0.0317 (0.0468)	0.0507 (0.0381)
observations	112,582	91,870	96,309	96,724	92,263	70,964	80,903	59,365	94,977	95,853	97,901
R-squared	0.096	0.196	0.053	0.066	0.056	0.071	0.087	0.119	0.077	0.083	0.072
district fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
offense fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (clustered by district) in parentheses.

All regressions include control variables described in section 6.1.

The potentially eligible cases are divided into deciles of fast-track suitability, defined as the predicted likelihood of a case being prosecuted via the fast-track based on the observable characteristics of cases during 2006–2008.

Table 9: Duration from charges to adjudication, by deciles of fast-track suitability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	potentially eligible cases										
ineligible cases	dec=1	dec=2	dec=3	dec=4	dec=5	dec=6	dec=7	dec=8	dec=9	dec=10	
direct effect	-54.96** (23.97)	-71.96*** (9.984)	-41.71*** (6.322)	-39.52*** (5.368)	-57.51*** (6.148)	-55.22*** (5.356)	-53.00*** (4.534)	-53.17*** (5.148)	-44.38*** (4.344)	-37.44*** (3.555)	
spillover effect – decile	289.9* (164.0)	-32.67 (105.7)	-41.62 (66.59)	20.26 (83.95)	-81.39 (86.72)	-0.175 (56.30)	27.42 (42.56)	-31.50 (38.19)	37.53 (45.99)	-30.44 (21.17)	
spillover effect – overall	-7.023 (76.17)	42.55 (81.80)	32.45 (78.38)	5.911 (102.5)	99.44 (90.04)	-6.246 (72.29)	19.78 (63.73)	119.1 (84.98)	99.55 (110.3)	117.1*** (43.16)	
observations	87,174	81,709	83,956	82,309	66,141	65,861	55,770	78,888	83,624	84,491	
R-squared	0.323	0.268	0.263	0.223	0.250	0.235	0.254	0.262	0.152	0.162	
district fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	
year fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	
offense fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (clustered by district) in parentheses.

All regressions include control variables described in section 6.1.

The potentially eligible cases are divided into deciles of fast-track suitability, defined as the predicted likelihood of a case being prosecuted via the fast-track based on the observable characteristics of cases during 2006–2008.

Table 10: Probability of conviction, by deciles of fast-track suitability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	ineligible cases	dec=1	dec=2	dec=3	dec=4	dec=5	dec=6	dec=7	dec=8	dec=9	dec=10
direct effect		0.0262* (0.0140)	0.0578*** (0.0106)	0.0106 (0.0118)	0.0456*** (0.00646)	0.0522*** (0.00763)	0.0718*** (0.00678)	0.0628*** (0.00493)	0.0553*** (0.00535)	0.0525*** (0.00472)	0.0529*** (0.00459)
spillover effect – decile		-0.0704 (0.115)	-0.0813 (0.0790)	-0.0240 (0.0446)	-0.0863** (0.0426)	-0.0531 (0.0572)	0.000665 (0.0354)	-0.0555 (0.0335)	0.0229 (0.0327)	-0.0229 (0.0305)	-0.0153 (0.0162)
spillover effect – overall		0.0683 (0.0444)	0.00256 (0.0520)	0.00790 (0.0435)	0.0663 (0.0560)	-0.108* (0.0583)	-0.0464 (0.0456)	-0.0751 (0.0554)	-0.0531 (0.0606)	0.0288 (0.0757)	-0.0352 (0.0310)
observations	87,174	81,709	83,956	83,847	82,309	66,141	65,861	55,770	78,888	83,624	84,491
R-squared	0.094	0.090	0.060	0.053	0.069	0.105	0.061	0.077	0.050	0.066	0.044
district fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
offense fe	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (clustered by district) in parentheses.

All regressions include control variables described in section 6.1.

The potentially eligible cases are divided into deciles of fast-track suitability, defined as the predicted likelihood of a case being prosecuted via the fast-track based on the observable characteristics of cases during 2006–2008.

Table 11: 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	fast-track accounts for
theft/burglary	167.79	-32.87	-12.76	-20.11	-20.11
driving offenses	163.22	-115.85	-55.82	-60.02	-60.02
against personal liberty	215.65	-39.51	-33.45	-6.06	-6.06
property/economic offenses	528.16	-71.72	-17.46	-54.26	-54.26
against public order	207.18	-19.92	-24.88	4.95	4.95
all high fast-track use offenses	256.40	-55.97	-28.87	-27.10	-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	fast-track accounts for
theft/burglary	0.66	0.17	0.10	0.07	0.07
driving	0.88	0.05	-0.04	0.09	0.09
against personal liberty	0.63	0.12	0.05	0.07	0.07
property/economic offenses	0.61	0.08	0.06	0.02	0.02
against public order	0.67	0.11	0.06	0.05	0.05
all high fast-track use offenses	0.69	0.11	0.05	0.06	0.06