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

Libor Dušek† and Josef Montag‡

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This 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 quasi-natural experiment in the implementation of the simplified procedure at the district level. We find robust evidence that prosecuting a case via the simplified 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 and increases the probability of conviction at trial. The simplified procedure, applicable to low severity crimes only, released resources that can be allocated to serious cases. However, we do not find evidence of such beneficial spillovers.

Keywords: Criminal procedure, law enforcement, courts, prosecutors.

JEL Classification: K14, K41, K42.

Highlights:

- We evaluate the effects of a simplified criminal procedure.
- It reduces the duration of the police/prosecutor phase of the procedure.
- It increases the probability that the suspect is charged.
- We find no evidence of beneficial spillover effects on serious cases.

1 Introduction

Crime enforcers in many countries struggle with heavy caseloads (European Commission on the Efficiency of Justice 2014, Transactional Records Access Clearinghouse 2014). This produces delays in court adjudication that have undesirable effects on the rule of law,

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†Corresponding author. Univesity of Economics, Prague and CERGE-EI; email: libor.dusek@vse.cz; postal address: Univesity of Economics, Nám. W. Churchilla 4, 13067, Prague 3, Czech Republic.

‡International School of Economics, Kazakh-British Technical University and Univesity of Economics, Prague; email: josef.montag@gmail.com.

quality of adjudication, and crime (Dušek 2015; Huang 2011; Listokin 2007; Pellegrina 2008). It is therefore important to understand the effects of policies addressing this issue. Many European countries introduced alternative criminal procedures during the 1990s and 2000s, such as the penal order, plea bargaining, and accelerated/simplified proceedings (Wade et al. 2008).[1] These procedures are administratively cheaper, less rigorous, and their application is typically limited to less serious offenses. Theoretically, resolving a case through the alternative procedure should result in a shorter case duration and a higher probability of conviction. Additionally, a more efficient criminal procedure releases resources that can be used in the enforcement of other cases, generating beneficial spillover effects.

Despite their proliferation throughout Europe, little is known about the effects and desirability of the alternative procedures. This paper addresses the gap in the literature by analyzing the effects of a reform aimed at increasing the efficiency of the criminal justice system in the Czech Republic. Specifically, the reform introduced a substantially more simplified criminal procedure applicable to evidentially simple low-severity cases. However, as low-severity cases represent the bulk of criminal courts' agenda, simplifying the adjudication process may potentially generate substantial efficiency improvements. The actual implementation of the reform resulted in a quasi-experiment that allows us to ascertain its causal effects on case durations and probabilities that a defendant is charged and convicted. To this end, we analyze administrative data covering the full universe of criminal cases from 1998 to 2008, containing a total of 1.1 million cases.

The literature to which this paper belongs is rather scant. 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. 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 imposed strict time limits, but did not change the criminal procedure itself. Although extreme delays have been eliminated, the reform did not result in shorter time elapsed in processing cases. Boari and Fiorentini (2001) study the introduction of plea bargaining in Italy. They find that due to lack of incentives on the side of prosecutors and on the side of defendants, the

[1] Among others, the penal order was implemented in Poland in 1997, France in 2002, the Netherlands in 2008, while it has been historically used in Germany, Switzerland and Sweden. The simplified/accelerated proceedings were implemented in Spain in 1988, Hungary in 1998, the Czech Republic and Slovakia in 2002, and Poland in 2007. Plea bargaining was implemented in Poland in 2003, France in 2004, Slovakia in 2005, and Germany in 2009 (Wade et al. 2008).

use of plea bargaining has been rather limited and has not brought desired improvements in the efficiency of the Italian legal system (see also Garoupa and Stephen 2008). Using district level data, Dušek (2015) studied the effects of the Czech simplified procedure on deterrence. His results indicate that the reform has resulted in a substantial increase in the number of recorded criminal offenses associated with driving, suggesting a reallocation of police enforcement efforts toward crimes with low enforcement costs. He finds only weak evidence of a deterrent effect on burglary and embezzlement. No study has yet investigated the empirical effects of a procedural simplification on criminal case *outcomes*. This paper fills this gap in the literature.

The criminal procedure reform in the Czech Republic was adopted in 2002. It allows evidentially simple low-severity crimes to be prosecuted via a simplified, fast-track procedure. When a case is prosecuted in the fast-track regime, several procedural steps are skipped and the paperwork is substantially simplified. In addition, strict deadlines are imposed. In practice, the fast track is primarily used in the prosecution of petty theft, driving-related offenses, and other minor offenses, typically if the offender is caught on the spot. The stated objective of the reform was to save resources in the enforcement of petty crimes and allow more resources to be allocated to the enforcement of serious crimes (Ministry of Justice of the Czech Republic 2001).

The actual implementation of the fast-track procedure was gradual and varied substantially across the 86 judicial districts – producing a quasi-experimental variation. To illustrate, the share of petty thefts prosecuted via the fast track in the first post-reform year was 27 percent on average. However, it varied from 8 to 49 percent across districts. This variation has persisted over time and is observed across different categories of fast-track eligible offenses. We exploit this variation to estimate the effects of the reform on case durations and probabilities of charges and conviction in a difference-in-differences framework.

Endogeneity of the intensity of fast track adoption at the district level presents a concern. However, we document that the intensity of fast track adoption was not related to the pre-reform trends in the outcomes. Based on interactions with practitioners, Dušek (2015) states that the variation across districts is largely due to "local law" – administrative and ideological preferences of district police chiefs and prosecutors. At the court level, the case is adjudicated according to the procedure that was submitted by the prosecutor; the procedure is thus exogenous from the judges' point of view. At the institutional level, it is important to note that criminal cases can only be investigated by the state police, which is subordinate to the central government. Thus, local politicians do not have direct influence over the law enforcement in their jurisdiction. Additionally, during the period of our analysis, the police districts differed from the regional government districts, in that district police chiefs did not have political counterparts at the regional level.

We present two estimation methodologies that disentangle two distinct effects: (i) direct effects on cases that are actually prosecuted by the fast track and (ii) spillover effects which are driven by the overall use of the fast-track procedure in a district. Both approaches yield very strong evidence of the direct effects, particularly in the police/prosecutor phase of the procedure. Prosecuting a case through the fast track significantly reduces the case duration and increases the probability that the prosecutor submits charges to the court. These effects are found across almost all 11 offense categories that we study. 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 less consistent. On the other hand, we find essentially no evidence of beneficial spillover effects consistent with the released resources being used for more effective enforcement of other cases. If anything, we find some evidence of undesirable spillover effects on case durations. Our estimates suggest that specialization of prosecutors and court senates decreased after the reform, possibly explaining the absence of beneficial spillovers.

2 Institutional background

2.1 Context of the reform

Criminal procedure is a rigorous and complex approach to arrive at judgement. This is because incorrect judgements in criminal cases are extremely costly to society. Previously, former communist countries had experienced abuse of the repressive apparatus for social control as well as for political and economic ends. After the fall of the iron curtain in 1989, a number of these countries introduced reforms of criminal procedure that were highly conservative and empowered defendants with extensive procedural rights. Wrongful convictions and eliminating possibilities of abuse of criminal justice were the dominant motives behind the design of criminal procedure reforms. This configuration, however, led to costly adjudication of criminal cases.

By the end of the 1990s, it became clear that imposing the requirement of such a complex procedure on all criminal cases was impractical. Many crimes, such as driving or administrative offenses, are relatively minor and are typically well supported by the evidence. In such cases, the probability and the social costs of judicial errors should be relatively small. Thus, using the standard criminal procedure may not be cost-justified as it consumes scarce resources and may constrain the ability of the criminal justice system from adjudicating simple cases, as well as more serious or complicated ones, resulting in underdeterrence.

2.2 The fast-track procedure

In an effort to address these issues, the Czech criminal procedure reform of 2002 introduced a simplified, or fast-track, criminal procedure.[2] The procedure reduces administrative paperwork, eliminates several procedural steps carried out by the prosecutor or the court, and imposes stricter deadlines. Only cases that meet the following eligibility criteria can, and should, be prosecuted via the fast-track procedure: (i) They fall into the jurisdiction of the district court (the lowest court level in the Czech court system). (ii) The maximum statutory sentence does not exceed three years of imprisonment.[3] (ii) The suspect was either identified while committing the crime or immediately afterwards, or the evidence identified 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 within two weeks.

Under the fast-track procedure, the police accuse the defendant and hand the case over to the state attorney, who reviews it and charges the defendant at the court. The text of the prosecution is less detailed and 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 also no closing speeches.

The deadlines for the fast track are far stricter as compared with the flexibility of the standard procedure: the police have to hand over the case to the prosecutor within two weeks of the crime being reported. The prosecutor may, upon request, extend the deadline by ten days at most; if the deadline is missed, the case reverts to the full-fledged standard

[2] *"Zkrácené přípravné řízení"* in Czech. The reform was legislated by Act no. 265/2001. For more detailed context of the reform see Dušek (2015). For more background on the use of alternative procedures in the international context see Dušek and Montag (2015), who also develop a unified theoretical framework for studying their optimal use and effects.

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

procedure. The risk of reverting the case to the time-consuming conventional procedure gives law enforcers strong incentives to meet the deadlines.

The decision whether to initiate the fast-track or conventional procedure rests with the district-level state police officer, although the prosecutor may reverse that decision. The letter of the law 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. However, once set, the court has to adjudicate the case through the procedure that was submitted by the prosecutor. This fact is important for the interpretation of our results.

3 Theoretical considerations

Landes' (1971) canonical paper was the first to model optimal prosecutor behavior under budget constraint. In that model, plea bargains release the prosecutors' resources. These savings can be invested in other cases, strengthening the evidence and increasing the total sum of sentences. The same economic argument extends to the other alternative procedures: they also allow enforcers to dispose of certain cases quickly and at low cost, thus releasing the enforcement resources. However, Landes (1971) does not consider the trade-offs involving judicial errors, particularly wrongful convictions, that are more likely to occur when cases are resolved through a less-than-trial procedure. These are, indeed, the main concerns relating to plea bargaining in the United States (Bibas 2004) or alternative procedures in Europe (Gilliéron 2013).

In Montag and Dušek (2015), we develop a comprehensive theory of the use of alternative criminal procedures that takes these trade-offs into account. Here we focus on the main intuitions and empirical predictions. A more detailed summary of the model and the reasoning behind the points relevant to this paper is provided in the Appendix.

A benevolent adjudicator faces a continuum of criminal cases that differ by evidence strength. The adjudicator faces budget constraint and her aim is to minimize the sum of judicial errors. Cases can be resolved via three different avenues: (i) a case can be dropped, (ii) it can be brought before trial, (iii) or resolved through alternative (less-than-trial) procedure. These options differ by administrative costs and the type of judicial errors they may generate. Dropping a case costs nothing but may result in a wrongful acquittal. The trial is the costliest option, and it produces additional evidence that provides a more precise signal of the defendant's guilt. The trial may result in a wrongful acquittal or wrongful conviction but the likelihood of these errors is small (if the courts were perfect, it would be zero).

The alternative procedure has positive costs but is cheaper than trial. It can be thought of simply as an administrative declaration of the guilt justified by the initial evidence. Alternative procedure may therefore result in a wrongful conviction.

Minimizing the sum of the social costs of judicial errors leads to two evidence standards: one for charging a defendant before the court and one for the alternative procedure. The cases with the evidence below the standard for charging are dropped. The cases with evidence above that standard are resolved at trial. Finally, the cases with the strongest evidence lead to a conviction through the alternative procedure. At these evidence standards, shifting the marginal defendant to trial would reduce the costs of judicial errors by the amount just equal to the additional costs of conducting the trial.

The model predicts the optimal adjustment in the evidence standards when the alternative procedure is introduced for low-severity offenses, as was the case of the Czech criminal procedure reform. The low-severity cases with the strongest evidence, previously decided at trial, are now resolved more cheaply through the alternative procedure. The probability of conviction for these cases thus rises. The cost saving then allows a reduction in the evidence standards for charging the low- and high-severity cases. Greater fractions of both low- and high-severity cases are charged, but these marginal cases would be those with relatively weak evidence; these marginal cases would then result in conviction at trial with lower probability than the cases that had reached trial previously. The model also predicts a quantitatively greater adjustment in the fraction of cases charged for the low-severity than for the high-severity cases.

This framework yields testable predictions for outcome variables that can be observed in the real-world data: the probabilities of charges and convictions. The testable predictions are summarized as follows.

A. Direct effects on cases potentially eligible to be decided via alternative procedure:
   (a) The probability of charges increases as the evidence standard for trial is lowered.
   (b) The probability of conviction increases as the cases decided through simplified procedure are more likely to result in conviction.

B. Spillover effects on potentially eligible and ineligible cases due to resources being released as a result of the overall use of the alternative procedure:
   (a) The probability of charges increases as the evidence standard for trial is lowered.
   (b) The probability of conviction decreases as the additional cases decided via courts will be evidentially weaker and more likely to result in acquittal.
   (c) These spillover effects are expected to be smaller than direct effects.

Although the models do not explicitly consider case durations, the expected effects are intuitive. The cases resolved via the alternative procedure should see a reduction in duration due to the procedure's administrative simplicity and lower costs. The procedure itself does not change for the other cases, but the time saved due to the use of the alternative procedure can be used to mitigate backlog, leading to earlier case completion. However, there is a possible mitigating factor in that the adjudicator would process the other cases more carefully, resulting in longer durations.

4 Data and summary statistics

The Czech Ministry of Justice provided us with two administrative databases of all criminal cases covering the period from 1998 to 2014. The first database contains cases closed by district prosecutors and the second contains cases that were adjudicated by district courts, including possible appeals. 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 seven years after (2002-2008).[4] The final regression-ready sample has over 1.1 million prosecutorial cases and over 950 thousand court cases.[5]

We use the following information about each case:

[4]Through the rest of the paper, the year of the case is based on when the respective procedural phase started. For example, 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 court phase include cases where the court received the charges from the prosecutor during 1998. The outcomes of these cases may have been determined in 1998 or in later years.

[5]The 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 lasted for more than 6 years with respect to 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 this reason, we evaluate the effects of the reform only during the period covered by the same post-reform legislation, between 2002 and 2008.

- Procedural dates: the date when the crime was committed, the date the prosecutor charged the defendant at court, and the date of the final judgment.
- The final decisions of the prosecutor and the courts' final judgments.
- 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.[6] In addition, we add dummies for the presence of each of the nine most frequent sections of the criminal code (in any of the charges), which controls for the offense types in more detail.
- Case characteristics: number of counts with which the defendant was charged, and situational characteristic dummies[7]
- Characteristics of the offender (gender, age, foreign nationality status, number of prior convictions).

We complement this data with 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 three years). We use the term "potentially eligible" to emphasize that such cases meet one, but 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 (simplicity of evidence) or the police/prosecutor decided to prosecute it via the conventional procedure.

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)

[6]To 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 following 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.

[7]The source data codes 32 various circumstances of the case, such as whether the victim was a man, woman, or child, the presence of alcohol or drugs, offense committed while driving etc. For each case, up to three indicators for the presence of any of these circumstanes is provided.

Our identifying variation of the use of the fast track is at the district level. For district we therefore construct district-level control variables, namely the total number of new cases and the number of new cases per individual prosecutor or court senate.[8]

Table 1 shows the average characteristics of all cases, divided into the periods before and after the reform. Potentially eligible cases accounted for 85 percent of cases before the reform—the vast majority of cases are thus lower severity crimes punishable by up to three years. This share has increased to 88 percent after the reform, a slight overall shift in the composition of cases towards petty crimes. During the post-reform period, the fast-track procedure was used to prosecute 24 percent of cases on average.

5 Empirical strategy

5.1 Key empirical issues

Our empirical objective is to estimate the effects of the fast-track procedure on case durations and probabilities of charges and conviction. Theoretically, as well as by the policy maker's intention, the use of fast track should have two types of effects: (i) direct effects and (ii) spillover effects.

Direct effects are effects of processing a case via the fast-track procedure on the outcomes of that very case. Spillover effects are district-level effects of the overall use of the fast-track procedure within a district. 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 use of the fast track in a district.

An ideal experiment that would allow us to estimate these effects 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 randomly select individual cases from the eligible cases to be processed via the fast track. One would then estimate the direct effect by an indicator variable 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.

[8]The 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.

The real-world reform, of course, did not generate such a pure experiment. However, we demonstrate in the subsection below that the assignment of the fast-track shares at the district level can be regarded, for all practical purposes, as good as random. In the next section, we deal with the issue that the assignment of individual cases into the fast-track is not random.

5.2 The identifying variation

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 judicial districts, we have a relatively large number of jurisdictions to estimate the effects.

For each offense category, Table 2 reports the mean, the standard deviation, and the 5^{th} and 95^{th} percentiles of the use of the fast track among the potentially eligible cases in 2002 (the first post-reform year) and in 2008 (the last year in our data). The actual adoption of the fast-track procedure varied widely across districts and was also gradual. The share of fast track in driving offenses, while 57 percent on average, was 28 percent in the 5^{th} percentile district and 84 percent in the 95^{th} percentile district. For theft, the initial average share of the fast-track cases was 27 percent, varying from 8 percent in the 5th percentile to 49 percent in the 95^{th} percentile. Six years later, there is an overall increase in the share of the fast-track cases, but the differences between districts persisted. For instance, the share of fast-track cases among all potentially eligible cases increased by 19 percentage points on average and this increase was almost the same at the 5th percentile and the 95^{th} percentile (18 percentage points and 20 percentage points, respectively). This variation naturally lends itself to estimating the effects of the reform in a difference-in-differences (DD) framework.

5.3 The identifying assumption

Although the letter of the law stipulates that all eligible cases should be prosecuted using the fast-track procedure, law enforcers in reality have some discretion as to whether a case will be prosecuted via the fast track or not. Endogeneity of adoption therefore presents a concern. More specifically, one may suspect that (i) the districts experiencing longer case durations or (ii) worsening case outcomes may adopt the fast-track procedure more intensively and may potentially implement other measures, introducing an omitted variable bias.

The first concern does not represent a real issue since any correlation between the levels of outcome variables and the intensity of adoption will be controlled for by fixed effects. With regard to the second concern, the key identifying assumption for the DD estimator is that the intensity of fast track adoption is uncorrelated with district-level trends in unobservables (Angrist and Pischke 2008). We emphasize that the identifying assumption does not require, for example, that the 2002 reform had no other effects that would be operating through different channels than the fast track; only that such effects be uncorrelated with the share of fast-track cases in a district.

The key assumption about trends can be tested by looking at pre-reform trends. Figure 1 plots the share of fast-track cases among potentially eligible cases in the first post-reform year (2002) against the percentage change in each of our outcome variables during the three years preceding the adoption. The figures do not reveal any apparent positive or negative relationship.

We also check for the potential correlation between the fast-track adoption and pre-reform trends formally through regressions reported in Table 3. We take all the cases where the prosecution started in 2002 and regress the decision to prosecute the case via the fast-track procedure (the fast-track dummy) on a rich set of case and defendant controls and district-level pre-reform trends in our four outcome variables. This way we can check whether the probability of prosecuting a case via the fast track is related to pre-adoption district trends in the outcome variable while controlling for any systematic differences in case composition or defendant characteristics.

The results are reported in Table 3. We first include each of the district trend variables separately. It is reassuring that the coefficient estimates are never statistically different from zero. In three cases out of four, the estimated coefficients are substantively small and in two cases the estimates are relatively precise. In the case of the pre-reform trend in the probability of charges, the positive coefficient is rather large in magnitude. This is to a certain degree driven by a single outlier district (Třebíč district) with a strong negative pre-reform trend and unusually low intensity of adoption. Dropping this outlier district decreases the coefficient by about one third.[9] In column five we estimate the regression

[9]We have also re-estimated all of our main models without the Třebíč district, but this does not affect the results appreciably. We therefore do not drop this district from the data. The estimates without Třebíč are available upon request.

with all four trend variables included and obtain quantitatively similar results. In summary, we do not find evidence that pre-reform trends would invalidate our DD strategy.

However, by looking at pre-reform trends, it is not possible to infer that there was not an unobserved shock that would affect the outcomes and would be correlated with the intensity of fast track adoption. Although it is not possible to test this complementary assumption in the data, we can gauge this concern by looking at the post-reform evolution in the observable variables that we believe might affect the outcomes and can in principle be correlated with the intensity of adoption. If the adoption and post-reform evolution of district-level observables were correlated (for example, high-adoption districts hiring more prosecutorial and judicial staff after the reform), it would be likely that the changes in unobservables are correlated with the adoption as well.

Table 4 reports the results of this check. Each of the columns shows a cross-sectional regression, where the 2001–2008 percentage change in a district-level variable is regressed against the share of fast-track cases among the potentially eligible cases in the first post-reform year. The variables are ordered to capture the caseload, case composition, and case characteristics. The coefficients on the fast track share are insignificant in 11 of the 12 regressions. The only exception is the share of driving cases, where the districts with initially high adoption experienced a relative decline in the fraction of driving cases. The regressions also have very low R^2, indicating that the post-adoption share of fast-track cases is almost entirely unrelated to the post-reform evolution of observables. All in all, we interpret the results of both exercises, reported in Tables 3 and 4, as providing enough confidence that the identifying assumption is satisfied.

Figures 2 through 5 show the evolution of the outcome variables and already provide cursory evidence of the effects. The districts are divided into three equally sized groups according to the ultimate intensity of the adoption of the fast-track procedure. The intensity is measured as a share of fast-track cases among the potentially eligible cases. The observations are further split by fast-track eligibility. We emphasize that the eligibility criterion is based on the statutory sentence only, therefore the composition of cases in these two groups does not change as the fast-track is used more intensively over time. Potentially eligible cases still contain a large fraction of individual cases that are prosecuted via the conventional procedure.

Figure 2 shows that the duration of the police/prosecutor phase was growing along similar trends in all three groups of districts and both case types. Over the post-reform years, the duration of the potentially eligible cases eventually declined by 62 days in the high-adoption districts, by 35 days in the medium adoption districts, and only 13 days in the low-adoption districts. No such diverging post-reform evolution is observed for the ineligible cases, where the duration continued to grow; if anything, duration increased more in the high-adoption districts. Figure 3 plots the probability of charges. Again, the districts exhibit near-identical trends up to 2001. The reform led to an immediate jump in this probability for the potentially eligible offenses, and the jump was somewhat more pronounced in the high- and medium-adoption districts (8.7 percentage points) than in the low-adoption districts (7.7 percentage points). Subsequently, the probability of charges continued to grow at a faster rate in the high- and medium-adoption districts.

The duration of the court procedure in potentially eligible cases, plotted in Figure 4, declined by 76 days in the high-adoption group and by somewhat less (60 and 64 days) in the medium and low adoption group. Among ineligible cases, the decline was actually less steep in the high-adoption districts. Finally, Figure 5 depicts the probability of conviction at trial. Among potentially eligible offenses, it exhibited a greater increse in high- and medium-adoption districts (by 6.8 and 5.4 percentage points, respectively) than in the low-adoption districts (3.2 percentage points).

6 Estimation and results

Although the identifying variation occurs at the district level, our regressions are estimated at the case level. Case-level data allows us to control for a rich set of the case and offender characteristics so that differences in the case composition across districts do not represent a major empirical issue.

However, when specifying the case-level regressions, one more issue needs to be addressed: The assignment of individual cases to the fast track within a district is clearly non-random. By the last eligibility criterion, fast-track cases are evidentially simple cases that would probably exhibit shorter duration and higher likelihoods of charges and convictions even in the absence of the fast-track procedure. This would bias the coefficient on the fast-track case dummy away from zero because the detailed case evidence is unobservable in our data. On the other hand, the districts that use the fast-track procedure intensively inevitably end up extending it to more sophisticated cases, creating a bias in the opposite direction. We employ two distinct methodologies that address these selection issues. We describe the two methodologies and present the results for each in turn, showing that both lead to very similar results.

6.1 Estimation 1: OLS estimator of marginal effects

The first methodology estimates the marginal effect of a more intensive use of the fast track on the outcomes of an average case within a narrow group of cases. Specifically, we estimate our difference-in-differences specification separately within each of our 11 offense categories. For potentially eligible cases we estimate

$$y_{ijt} = \beta_D FTshare^{o}_{jt} + \beta_S FTshare^{-o}_{jt} + \boldsymbol{\gamma' x_{ijt}} + \boldsymbol{\delta' x_{jt}} + \boldsymbol{\tau_t} + \boldsymbol{\phi_j} + \boldsymbol{\tau_j} t + \epsilon_{ijt}, \quad (1)$$

where y_{ijt} is the outcome variable for a case i in district j and year t. The key treatment variable is $FTshare^{o}_{jt}$, which is the share of fast-track cases among the potentially eligible cases in an offense category o. 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. $FTshare^{-o}_{jt}$ is the share of the fast-track cases in a district in all the other offense types - that is, in all offense categories other than o as well as the ineligible cases in an offense category o. We further include a vector of case and offender characteristics, $\boldsymbol{x_{ijt}}$, and characteristics of the criminal justice system in a district, $\boldsymbol{x_{jt}}$.[10] Vectors $\boldsymbol{\tau_t}$ and $\boldsymbol{\phi_t}$ are the year effects and district fixed effects, respectively, and we also include district-specific time trends, $\boldsymbol{\tau_j} t$. Finally, ϵ_{ijt} is the unexplained residual. The standard errors are clustered by district.

The coefficients of interest are β_D and β_S. β_D has the interpretation of the direct effect. Using a share of fast-track cases among eligible cases in an offense category, rather than a fast track indicator, alleviates the selection issues discussed above, but slightly alters the interpretation of the direct effect we are estimating: β_D is the marginal effect of a change in the outcome due to a one unit increase in the share of fast-track cases. β_S has the interpretation of the spillover effect, as it captures the effects of the overall intensity of fast track use in the district while excluding the eligible cases in offense category o.

[10]The case and offender characteristics are: the number of counts with which the offender was charged, dummies for the presence of the selected nine 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 nationality 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.

For ineligible offenses we estimate simply

$$y_{ijt} = \beta_S FTshare_{jt}^{-o} + \boldsymbol{\gamma}'\boldsymbol{x_{ijt}} + \boldsymbol{\delta}'\boldsymbol{x_{jt}} + \boldsymbol{\tau_t} + \boldsymbol{\phi_j} + \boldsymbol{\tau_j}t + \epsilon_{ijt}, \tag{2}$$

as there are no direct effects among ineligible offenses. Thus, for ineligible offenses, there can only be spillover effects.

6.2 Results 1: Baseline estimates of marginal effects

The estimates are presented in Tables 5 through 8, one for each outcome variable. The upper panels show the estimates of β_D and β_S for potentially eligible offenses, and the lower panels the estimates of β_S for ineligible offenses.[11] The offense categories are arranged so that columns (1) to (5) include categories with a relatively high fast track use while columns (6) to (11) include those with a less intensive fast track use, as reported in Table 2.

Table 5 reports the effects on the duration of the police/prosecutor phase, from offense to charges. All estimated direct effects are negative, significant at one-percent level, and range from −87 days (theft/burglary) to −336 days (fraud/embezzlement and offenses against public safety). 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, 23 days for other property/economic offenses, 9 days for driving offenses, and so on. The direct effects tend to be greater in offense categories with a sporadic use of the fast track (offenses against life and health, sex offenses, fraud/embezzlement). 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 5) are always positive, often an order of magnitude smaller than direct effects, but rarely significant. Similarly, the lower panel shows the spillover effects on the more serious, ineligible cases. The coefficients have varying signs and magnitudes, but are never statistically significant. Overall, they do not provide any evidence on the expected beneficial spillover effect on ineligible cases.

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

Table 6 reports the effects on the final outcome of the prosecutorial phase of the procedure; the decision to charge the defendant at court. There is substantial evidence of a direct effect: The estimated direct effects are all positive and they are highly statistically significant for all but three offense categories. In the five offense categories with a high fast track use (columns 1 through 5), the effects are always significant at one percent level and are substantively large. 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 for thefts, 2.3 percentage points for other property/economic crimes, one percentage point for driving offenses, and so on. The estimated spillover effects are insignificant and vary in size; in the case of high fast-track use offenses the estimated spillovers tend to be relatively small. Results for spillover effects on ineligible cases are similarly inconclusive.

In Table 7 we report the estimates for the duration of the court phase, measured by the duration from charges to the final adjudication (including a possible appeal). The direct effects are negative in most offense categories, particularly those with a high fast track use. However, they are smaller in magnitude (compared to the prosecutorial phase) and are significant only for two categories, property/economic offenses and sex offenses. In terms of magnitude, a 10 percentage point increase is associated with a reduction of the court duration by four days for thefts, ten days for property/economic offenses, 1.6 days for driving offenses, and so on.

The spillover effects on potentially eligible cases mostly have a positive sign and are seldom statistically significant. 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.

Last, the estimates for the probability of conviction are shown in Table 8. 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 0.6 percentage point for thefts, 1.5 percentage points for property/economic offenses, and 0.9 percentage point for offenses against personal liberty. However, we note that these estimates are potentially contaminated by a sample composition bias that 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. The spillover effects on potentially eligible cases tend to be positive but are not statistically significant. The estimates for ineligible cases do not reveal any systematic evidence of spillover effects.

6.3 Estimation 2: Matching-based four-step estimator

The second estimation approach proceeds in four steps: (i) We match the treated (fast-track) cases with observably similar pre-reform cases. (ii) Then we estimate the direct effect by nearest-neighbor matching, where the outcomes are already purged of year and district effects. (iii) We use the estimates of the direct effect to construct a measure of the resources released by the fast track use. (iv) Finally we estimate the spillover effects using this preferred measure of resources released.

6.3.1 Matching procedure

In the ideal experiment we lay out in Section 5.1, the direct effect would be estimated simply by comparing the cases prosecuted via the fast track with the other cases. Matching is a standard technique to reconstruct such an experiment. However, our setting is slightly more complicated: one cannot match a treated case in district j and year t with a control case in the same district j and year t. Cases selected for the fast track are evidentially simpler cases and our data does not contain details on evidence. One can, however, perform matching over time in order to find the most similar case occurring prior to the reform. The idea is to search for matches for treatment cases during a post-reform year ($t \geq 2002$) in district j among all cases in a preform year ($t < 2002$) in district j.

Matching cases over time, however, would lead to biased estimates of the treatment effect if the year effects or the district characteristics would differ over time. The outcome variable thus needs to be purged out of these time-varying effects. Therefore, in order to implement the matching procedure, we first regress each outcome variable in a full sample of potentially eligible cases on year dummies, district characteristics, case and offender characteristics and district dummies.[12] We use the residuals from this regression as the dependent variables on which we estimate the direct effects by matching.

The sample for the matching estimator consists of fast-track cases from the post-reform years and all (by definition untreated) potentially eligible cases from the pre-reform years.

[12]While the primary motivation is to remove the year fixed effects and the effects of a change in district characteristics, we nevertheless include the other control variables as well in order to obtain unbiased estimates of these effects.

Matching variables include the district and all case and offender characteristics used in the regressions with marginal effects (see Section 6.1). Nearest-neighbor matching assures that each treated observation has at least one match. This procedure is carried out separately by offense category and outcome variable. The identifying assumption is that changes in unobserved case characteristics within an offense category and district are uncorrelated with the intensity of fast track adoption.

6.3.2 Estimating the released resources

To estimate the spillover effects, we construct an alternative measure of the amount of resources released by fast track use. An ideal measure of saved resources should capture the amount of time and other resources saved by prosecuting cases via the fast track. Unfortunately, we have no information about the actual amount of time that each police officer, prosecutor, or judge spend on each case. In the previous section, we used the simple share of offenses prosecuted via the fast track (in all other offense categories). The disadvantage is that it gives each fast-track case an equal weight, while the resource-savings clearly depend also on the types of cases prosecuted via the fast track. In order to obtain a more precise measure of resources saved, we use our estimates of the direct effect on the case durations as proxies for the *relative* time savings due to fast track.

Specifically, we predict the duration from offense to charges for each case from regressions where the case and offender characteristics, district characteristics, district fixed effect and year effects as the right-hand side variables.[13] These regressions were again estimated separately by offense category. For each offense category o in each district i and year t we then compute the total predicted duration of all cases in categories other than o. Then, for each case prosecuted via fast track, we deduct the size of the direct effect, estimated by matching, from the predicted duration. Then we re-compute the total duration of cases in offense and eligibility categories other than o, with this reduction applied to fast-track cases.

The percentage difference between the predicted duration without and with the direct effects deducted is our proxy for the resources released by the fast track that we use to estimate the spillover effects in the prosecutor data.[14] This exercise is performed

[13]That is, regressions equivalent to equation (1), but without $FTshare^{o}_{jt}$ and $FTshare^{-o}_{jt}$ variables.

[14]An example illustrates how the spillover measure is constructed: Let a district have just two offense categories, A and B, with 10 cases per year of each. The predicted durations are 200 for A and 300 for B. The fast track is implemented in 50 percent of the A-cases and 20 percent of the B-cases, and the direct

analogously with the duration from charges to adjudication to construct the spillover measures in the court data. The spillover effects are estimated by the regression (1), except for the fast track shares which are replaced by the measures of the resources saved described above.

6.4 Results 2: Matching-based estimates

The estimates are presented in Tables 9 through 12, one for each outcome variable. The first row of results reports the direct effects estimated by matching. The second and third rows report the spillover effects on potentially eligible and ineligible cases estimated with the measure of resources released described above. While matching is a very different empirical strategy than the marginal effects in the previous section, the results exhibit one very similar pattern: very strong evidence of direct effects. The estimated direct effects are generally greater in magnitude and have smaller standard errors. There is again no evidence of spillover effects on the probabilities of charges and conviction. However, a greater fraction of the estimated coefficients points to undesirable spillover effects on case durations.

In Table 9, the direct effects on the duration from offense to charges are negative and significant at the one-percent level in all offense categories. They are all greater in magnitude than the corresponding direct effects in Table 5. The effect of prosecuting a case via the fast track varies from -105 (driving offenses) to -246 (offenses against family). The spillover effects are consistently positive and in four offense categories they are statistically significant. This gives some evidence of undesirable spillover effects whereby durations tend to be longer if the fast track is used more intensively in other offense types.

The direct effects on the probability of charges (Table 10) are also significant at one-percent level in all offense categories. Their magnitude varies from a 7.2 percentage point increase (driving offenses) to 17.3 percentage point increase (offenses against public safety); these are generally slightly smaller than the corresponding direct effects from Table 6.

effects estimated by matching are -80 for A and -110 for B. Hence the total predicted durations before substracting the direct effects are thus 2000 for A and 3000 for B. The direct effects reduce the total duration by 400 ($= 10 \times 0.5 \times 80$) for A-cases and by 220 ($= 10 \times 0.2 \times 110$) for B-cases. The measure of the spillover for A is the percentage reduction in total duration among the B-cases (0.073), and the measure of the spillover for B is the percentage reduction among the A cases (0.25).

The estimated direct effects on court durations (Table 11) are all significant at the one-percent level. In terms of magnitude, adjudicating the case via the fast track reduces the duration by 26 days (offenses against family) to 111 days (sex offenses). The spillover effects on potentially eligible cases carry a positive sign and the coefficients are statistically significant in seven offense categories. The spillover effects on ineligible cases also have positive signs (with one exception) and six of them are significant at the five or one-percentage level. In summary, there is no evidence of the desirable spillover effects. Rather the results give some evidence on the undesirable spillover effects whereby the courts may to have neglected more serious cases when they received a greater fraction of fast-track cases.[15]

Finally, Table 12 shows positive and significant direct effects on the probability of conviction in eight offense categories. Charging the case through a fast-track procedure leads to between a 4 to 6 percentage point increase in the probability of conviction.

6.5 Robustness checks

We performed several robustness checks, but none had an appreciable effect on our estimates. As mentioned in Section 5.3, the district of Třebíč is an outlier and we have therefore re-estimated all of our models without it; the results did not change. The capital city of Prague is another candidate outlier as it is socio-economically distinct, has a different composition of criminal cases, and has high (although not the highest) adoption of the fast-track procedure. We re-estimated the models while dropping the ten districts of Prague and the estimates were virtually unchanged.

Although we report the more conservative DD specifications with district-specific trends as our preferred models, we have also estimated more conventional specifications with simple district and year fixed effects, without district trends. Generally, these models produced slightly larger estimates of the direct effect of the fast track, compared with the reported specifications that include trends. The estimates of spillover effects were similarly inconclusive as in the reported regressions.

[15]The positive signs may also indicate that resources are invested in more intensive investigation of ineligible cases, possibly facilitating a higher success rate at trial (as would be predicted by Landes' 1971 model). However, this latter prediction is not supported by our results.

6.6 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 5 and 6 to predict the prosecutorial duration and the probability of charges in each case 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 effects would have evolved as they actually did.

Table 13 reports the results of these simulations, which we carried out for the potentially eligible cases. For example, the average duration from offense to charges for theft/burglary cases was 169 days in the last year before the reform. It declined by 37 days during the post-reform period. The regression estimates imply that in the absence of the fast-track procedure, the duration would have declined as well, but by 11 days only. The fast-track procedure, as actually implemented to prosecute theft/burglary cases, accounts for 26 days of the reduction in duration. The contribution of the fast-track procedure was particularly pronounced in driving offenses, offenses against personal liberty and public safety where it accounts for a reduction in case duration by 66, 12 and 28 days, respectively. Among all potentially eligible cases, the duration from offense to charges declined by 47 days on average, of which 18 days are attributable to the fast-track procedure.

The bottom panel of Table 13 reports the results of an analogous counterfactual exercise for the probability of charges. In potentially eligible theft cases, it increased by 11 percentage points during 2001-2008, from 82 percent to 93 percent and the fast-track procedure contributed 5 percentage points to this increase. It also had an economically large effect on property/economic offenses, driving offenses, and offenses against personal liberty (7 percentage points). Among all potentially eligible cases, the probaility of charges increased by 11 percentage points, of which the fast-track accounts for 5 percentage points.

6.7 Explaining the absence of the intended spillover effects

The main findings of the empirical analysis show strong evidence of the direct while the expected desirable spillover effects are not present. If anything, the spillover effects on case duration seem to go in the opposite direction than the theory would predict and the policy maker intended. Hence the question: why was there an absence of the spillover effects?

The theories considered in Section 3 and the Appendix offer a partial explanation. The Landes' (1971) resource-releasing hypothesis predicts an increase in the probability of conviction at trial, while the Dušek and Montag (2016) model predicts a decrease due to a reduced evidence standard for bringing charges. Since the factors considered by each of the models are likely to be at play in the real-world enforcement, it is possible that the opposing effects merely cancel out. However, both models unanimously predict a positive spillover effect on the probability of charges. A fuller explanation therefore requires a consideration of the institutional forces at play.

One possible explanation is a reallocation of enforcement towards petty crimes. Our theoretical model in fact predicts that, from the social point of view, the resources released by a more efficient procedure should be allocated primarily towards low-severity offenses. Such reallocation occured in the Czech case, as Dušek (2015) documents a more vigorous enforcement of driving-related offenses in districts with a higher fast-track adoption, leading to an absolute increase in the recorded driving offenses. Hence the potential of the simpler procedure to release resources for the enforcement of serious cases was partially undone by more vigorous enforcement of petty cases.

However, there still should be an absolute increase in the amount of resources allocated to more serious cases. Contrary to that, the estimated spillover effects on the case durations were generally positive, and many of them significant when our measure of spillover proxied for the resources released. This is consistent with the prosecutors and courts prioritizing fast-track cases and possibly neglecting the remaining cases. Our results suggest that the reallocation towards low-severity offenses may have exceeded the optimal level, possibly resulting in an absolute decrease in resources allocated to more severe cases.

The last explanation, one that we can test in our data, concerns the specialization of prosecutors and judges. In the Czech context, the allocation of cases to individual prosecutors and court senates is non-random and the prosecutors and senates tend to specialize in certain types of cases. For the spillover effects to materialize, an internal mechanisms would have to exist such that the resources released in prosecuting and adjudicating petty cases would be indeed allocated to the prosecution and adjudication of the serious cases. For example, when the fast-track procedure is implemented, this saves the time of prosecutors who specialize predominantly in simple, petty cases, while prosecutors specializing in complicated, serious cases are less affected. As a result, some cases may have been taken off the shoulders of prosecutors specializing in serious cases and allocated to those initially specializing in petty cases.

Such reallocation may either decrease specialization, if relatively serious cases are reallocated from the serious-case specialists, or increase specialization, if the relatively petty cases are reallocated from the serious-case specialists. Indeed, increasing returns from specialized human capital imply that deeper specialization would lead to higher productivity and be socially desirable (Becker 1985; Rosen 1983). However, managers in bureaucratic organizations, such as the police, or district prosecutor office, may lack incentives for exploiting such opportunities. In either case, this argument leads to an empirically testable prediction: the caseload of the prosecutors initially specializing in the serious cases should decrease.

Did such reallocation occur in practice? We construct a test that gauges the presence of case reallocation induced by the fast-track procedure. We exploit the fact that our data identifies individual prosecutors. First, we define the degree of specialization at the level of individual prosecutors in 2001, the last year prior to the reform. The measure is simply the share of ineligible cases handled by each prosecutor. Within each district, we classify the prosecutors into terciles of the share of ineligible cases in 2001. This choice is driven by the practical fact that most districts are small, with between five to nine prosecutors (the smallest district has only three). In the first, second, and third terciles, the average shares of ineligible cases are 4, 12, and 30 percent, respectively, in the pre-reform year. At the level of each prosecutor and year, we construct dependant variables that capture the case allocation and specialization: number of cases, share of ineligible cases, and the number of ineligible cases.

Then, in a subsample covering 2001-2008, we regress these dependant variables on a time trend, a time trend interacted with the tercile dummies, the district-level share of fast-track cases, and the share of fast-track cases interacted with the tercile dummies. Prosecutor fixed effects are also included to account for time-invariant specialization at the prosecutor level. Tercile-specific time trends should control for case composition changes resulting from prosecutors seniority or possible regression to the mean. The interactions between the fast-track share and the terciles are the key variable in these regressions.

If a greater intensity of the fast-track adoption in a district reduced the caseload of the prosecutors specializing in the serious cases, the coefficient on the interaction between the fast-track share and the top tercile would be negative.

Table 14 reports the results, separately for individual prosecutors and court senates. The third tercile denotes the highest degree of specialization in ineligible cases, and the first tercile is the omitted category in these regressions. Columns 1 and 5 document a mean reversion over time: Prosecutors that initially specialized most in ineligible cases (third tercile) experience a decline in the share and number of ineligible cases over time, relative to the time trend. In the second column, we can see that the prosecutors in the third tercile experienced a greater decline in the share of ineligible cases if the share of fast-track cases in a district was greater.

At the same time, in the districts with a higher fast track share, prosecutors specializing in ineligible cases did not experience a decline in the *number* of all cases (column 4) or the number of ineligible cases (column 6). If anything, the coefficients on the interactions of the second and third deciles and the fast track share are positive and the increase in total cases far exceeds the increase in ineligible cases, although the coefficients are not statistically significant. We therefore do not find evidence consistent with a released caseload pressure on prosecutors initially specializing in serious cases. Rather, the evidence suggests that the fast track generated an increase in petty cases, and such cases were allocated among the prosecutors in a way that mitigated their specialization.

We carried out the same test for the court senates (the right panel of Table 14). Similarly, the senates in the second and third deciles experienced a greater decline in the share of ineligible cases where in districts that adopted the fast-track procedure more intensively (column 8). Hence the reallocation also tended to mitigate specialization between senates. On the other hand, there is at least weak evidence that the reallocation actually tended to relieve the pressure on the senates initially specializing in ineligible cases. The coefficients on the interaction between the second and third terciles and the fast track share are negative in the regressions with the number of all cases (column 10) as well as the number of ineligible cases (column 12), and two of them are significant at the ten-percent level.

Taken together, it appears that the resource-releasing benefits of the simpler procedure were to a significant degree unexploited, and this fact may explain the absence of the spillover effects. Some of the resource-saving benefits were dissipated by an increased enforcement of petty driving offenses rather than serious offenses. The caseload was reallocated among individual senates in a way that mitigated the specialization in serious offenses, and prosecutors initially specializing in serious cases were not relieved from the caseload pressure.

7 Conclusions

This paper provides evidence that introducing a simpler criminal procedure has important effects on the outcomes of criminal cases. We 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. The model of Landes (1971) and our own model provide two complementary theoretical frameworks that predict positive direct effects on the probabilities of charges and conviction, positive spillover effects on the probability of charges, and differ in their predicted spillover effects on the probability of conviction.

Consistent with the theory, we find very strong evidence of the direct effects on the less serious offenses, particularly 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 all offense categories. 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 be thought of as technological improvements. They allow cases to be processed faster and allow prosecutors to successfully complete a higher fraction of cases all the way to charging the defendant at court. On the policy side, this 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.

Our results contrast with the earlier experience from the Speedy Trial Act in the United States or the introduction of plea bargaining in Italy. These reforms did not bring the desired improvements in the efficiency of criminal procedure. Earlier studies point to lack of incentives, particularly on the part of prosecutors, to use these innovations effectively (Boari and Fiorentini 2001; Bridges 1982). This was not the case with the introduction of the fast-track procedure in the Czech Republic. There were quite strong incentives to make use of the alternative procedure when applicable, not least for the fact that it is clearly less tedious to undertake, as is documented by the relatively high share of cases (20 percent) that were prosecuted and adjudicated via the fast track immediately after its introduction in 2002 as well as its increasing use thereafter, 39 percent by 2008. This highlights the role of incentives that participants in criminal procedure face and their indispensability when criminal procedure reforms are devised.

However, we find no evidence of beneficial spillover effects, even though these were an explicit objective of the reform. Thus, our results do not provide empirical support for the resource-releasing hypothesis implied by the theoretical models in Dušek and Montag (2016) and Landes (1971). On the other hand, they concur with the modest effects found in studies estimating the effects of hiring more judges on court output (Beenstock and Haitovsky 2004; Dimitrova-Grajzl, Grajzl, Sustersic, and Zajc 2012). Hiring more judges or procedural simplification are two alternative measures that release the resource constraint of enforcers. It is perhaps not surprising that if one measure does not bring significant results, neither does the other. The mechanisms through which enforcement authorities fail to translate additional resources into additional results are yet to be fully explored; we provided some evidence suggesting that the cases were not effeciently reallocated across individual prosecutors.

Last, our results clearly show out that the procedural rules have a significant effect on the ultimate case outcomes. The evidence of increases in the probabilities of charges and conviction imply that a higher fraction of accused defendants is eventually convicted. Hence it is possible that the efficiency gains come at the expense of accuracy. The increase in the probabilities of charges and conviction is consistent with two (not mutually exclusive) explanations: (i) If the fast-track procedure eliminated several loopholes that the factually guilty defendants had used to exploit to escape punishment, it reduced the incidence of wrongful acquittals. (ii) Alternatively, if it abridged some procedural rights such that factually innocent defendants became more likely to get convicted, it may have increased the incidence of wrongful convictions. Ascertaining the relative importance of these effects and factors behind them is an important question for future research.

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Figure 1: Endogeneity of fast track adoption: pre-reform trends

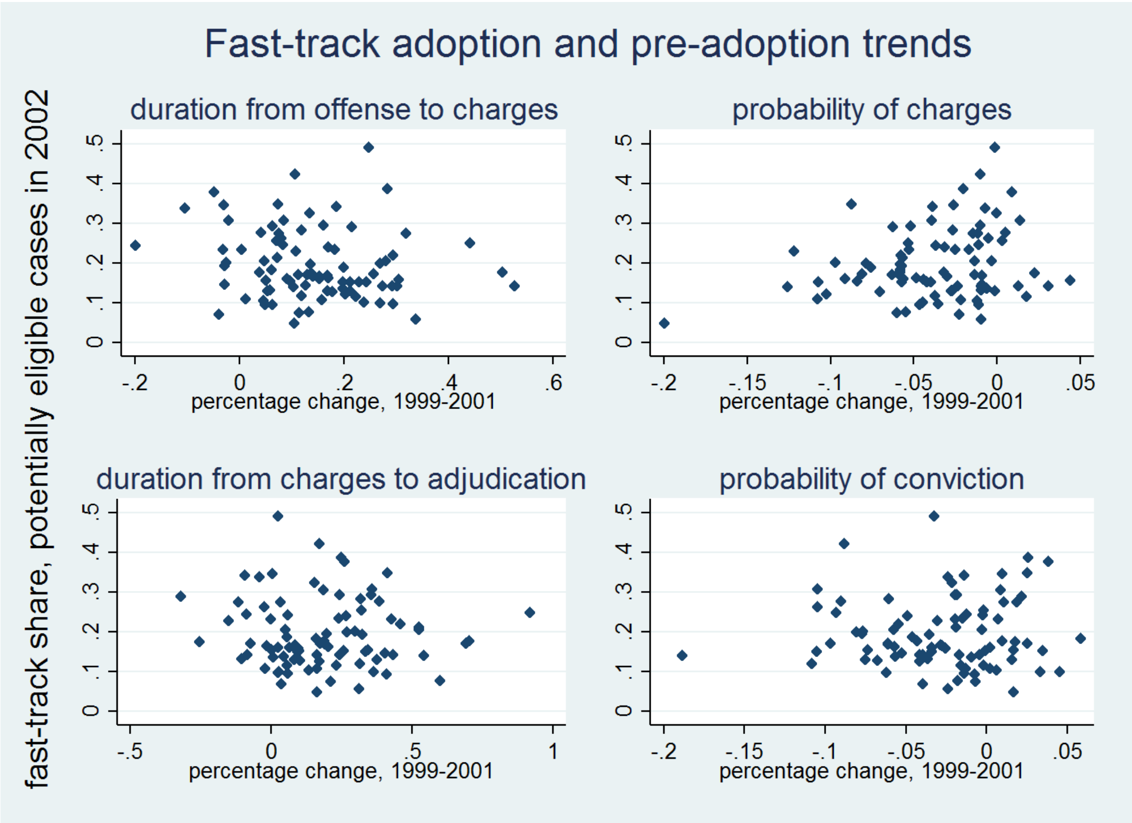

Figure 2: Average duration from offense to charges, by intensity of fast-track adoption

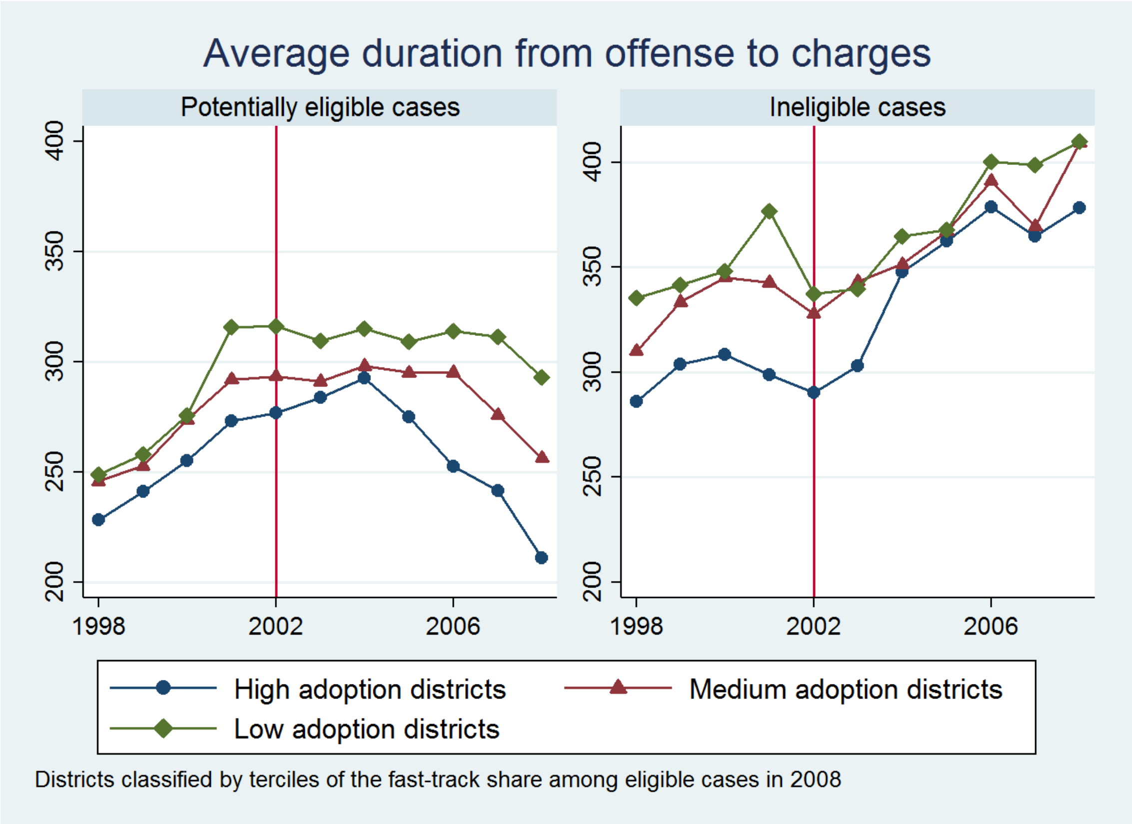

Figure 3: Average probability of charges, by intensity of fast-track adoption

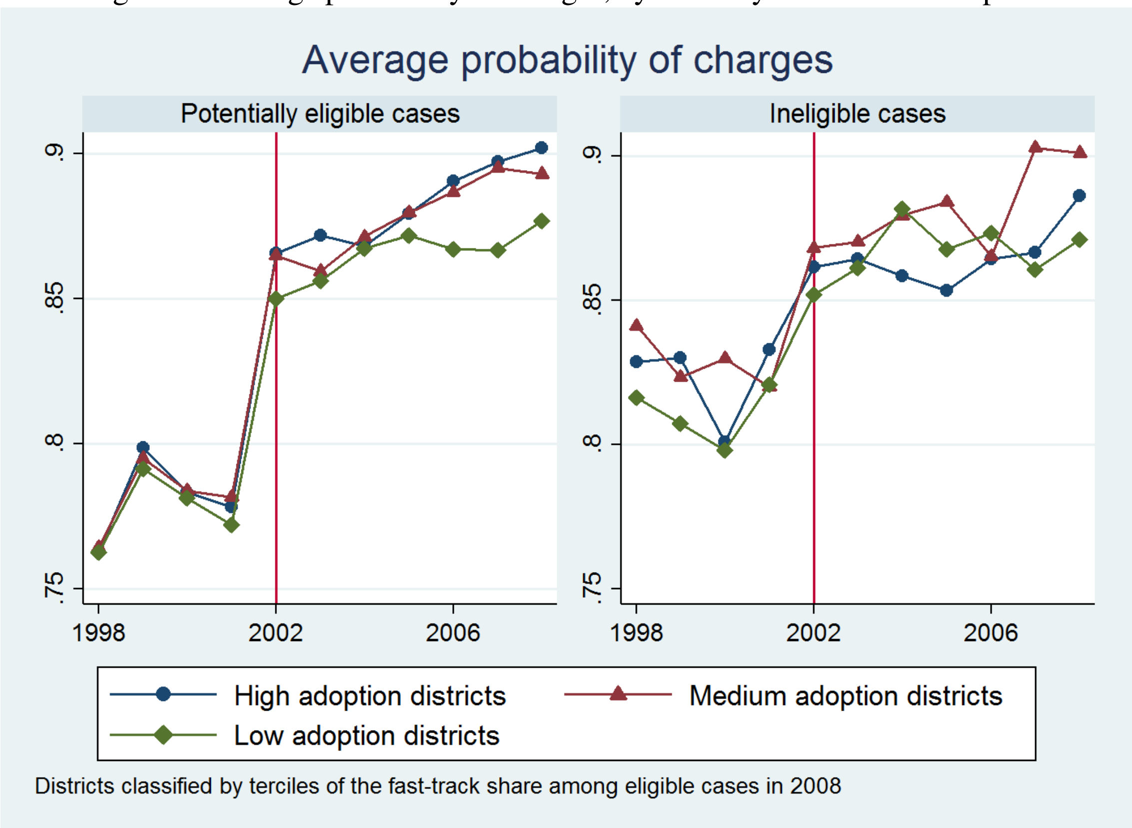

Figure 4: Average duration from charges to adjudication, by intensity of fast-track adoption

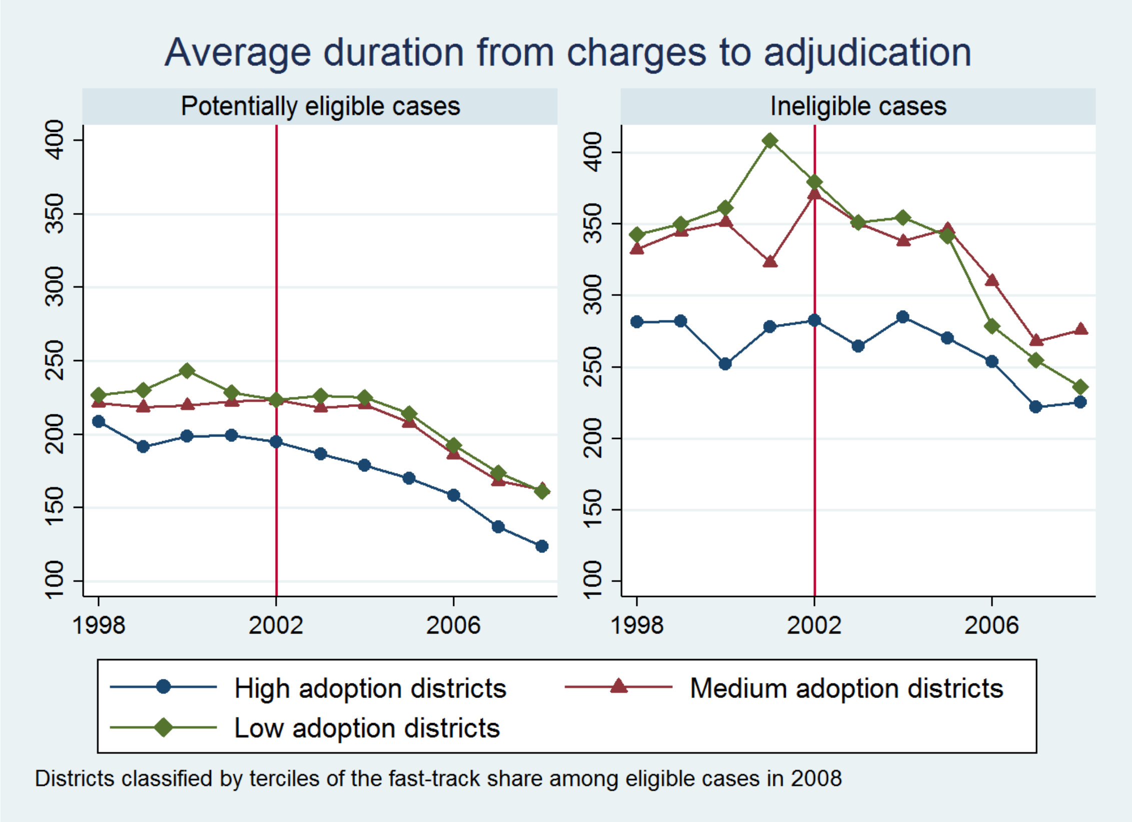

Figure 5: Average probability of conviction, by intensity of fast-track adoption

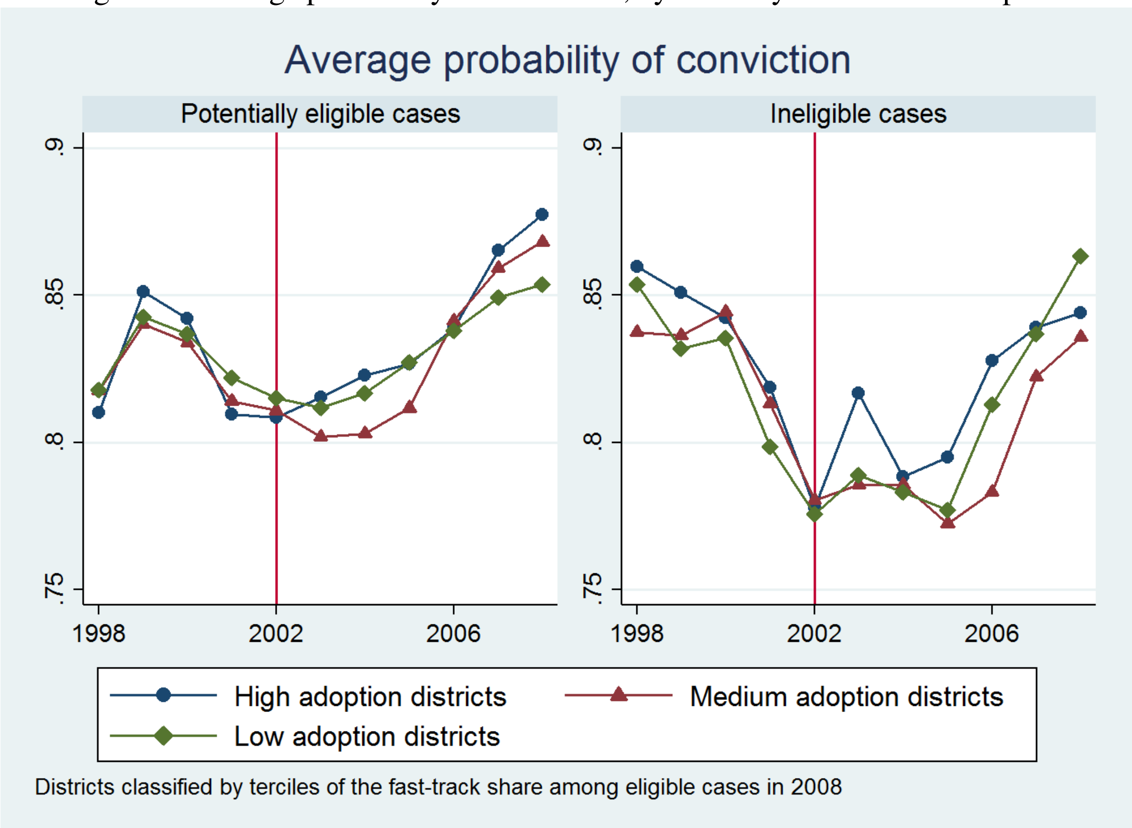

Table 1: Summary statistics

Variable	Pre-reform			Post-reform		
	Observations	Mean	S.D.	Observations	Mean	S.D.
Potentially eligible case (share)	386, 619	0.85	0.35	739, 616	0.88	0.33
Fast-track cases (share)	–	–	–	739, 616	0.24	0.43
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

Offense category	Share of fast-track cases in 2002				Share of fast-track cases in 2008			
	Mean	S.D.	P_5	P_{95}	Mean	S.D.	P_5	P_{95}
Theft/burglary	0.27	0.12	0.08	0.49	0.37	0.12	0.16	0.61
Property/economic offenses	0.16	0.10	0.03	0.32	0.21	0.12	0.03	0.42
Driving	0.57	0.17	0.28	0.84	0.76	0.12	0.58	0.92
Against personal liberty	0.20	0.11	0.03	0.41	0.27	0.14	0.10	0.53
Against public order	0.19	0.14	0.02	0.44	0.21	0.13	0.03	0.43
Against public safety	0.21	0.21	0.00	0.69	0.19	0.22	0.00	0.67
Against life or health	0.03	0.05	0.00	0.14	0.03	0.05	0.00	0.16
Sex offenses	0.06	0.11	0.00	0.33	0.09	0.17	0.00	0.50
Against family	0.04	0.09	0.00	0.25	0.03	0.07	0.00	0.19
Fraud/embezzlement	0.06	0.08	0.00	0.20	0.05	0.04	0.00	0.14
All potentially eligible cases	0.20	0.09	0.08	0.35	0.39	0.09	0.26	0.55

Note: The table shows the summary statistics of the shares of fast-track cases among potentially eligible cases at the level of district-year-offense category.

Table 3: Verifying the difference-in-differences assumptions.

	(1)	(2)	(3)	(4)	(5)
Outcome variable:	Fast-track case dummy in 2002	Fast-track case dummy in 2002	Fast-track case dummy in 2002	Fast-track case dummy in 2002	Fast-track case dummy in 2002
Percentage change (1999–2001) in:					
Duration from offense to charges	-0.066				-0.074
	(0.082)				(0.081)
Probability of charges		0.332			0.341
		(0.239)			(0.238)
Duration charges to adjudication			-0.051		-0.056
			(0.042)		(0.044)
Probability of conviction				0.034	-0.037
				(0.184)	(0.185)
Case controls	yes	yes	yes	yes	yes
Offense fixed effects	yes	yes	yes	yes	yes
R-squared	0.210	0.210	0.210	0.209	0.211
Observations	88,403	88,403	88,403	88,403	88,403

Note: The regressions are estimated on a subsample of potentially eligible cases initiated in 2002. The percentage changes are computed at the district level. The case controls include maximum statutory sentence, dummies for two, three, four, and more charges per case, situational dummies, dummies for the presence of each of the eight most frequent sections of the criminal code among the charges, defendant gender, foreign status and education levels, number of cases per prosecutor, and the number of prosecutors in a district. Standard errors clustered by district are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 4: Adoption and post-adoption change in observables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Percentage change in:	Number of new cases	Number of prosecutors	Number of new cases per prosecutors	Number of court senates	Share of pot. eligible cases	Share of robberies	Share of thefts	Share of driving	Number of charges per case	Max sentence	Number of prior conviction	Share of foreign
Fast-track share in 2002	-0.219	-0.425	0.272	-0.495	-0.036	-1.038	0.115	-6.749***	0.054	0.084	0.365	-0.478
	(0.312)	(0.348)	(0.441)	(0.431)	(0.052)	(1.020)	(0.200)	(2.443)	(0.061)	(0.086)	(0.299)	(0.769)
R-squared	0.006	0.018	0.004	0.015	0.006	0.012	0.004	0.083	0.009	0.011	0.017	0.005
Observations	86	86	86	86	86	84	86	86	86	86	86	86

Note: The unit of observation is a district. The regressions estimate the percentage change in the dependant variable over 2002-2008 against the share of fast-track cases among the potentially eligible cases in 2002 (the first post-reform year). The dependent variables in columns (9) to (12) denote district-level averages of case characteristics. Standard errors are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 5: Duration from offense to charges (marginal effects, in days)

	(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	−87***	−233***	−93***	−199***	−151***		−220***	−185***	−336***	−336***	−178***
	(18)	(67)	(12)	(22)	(29)		(42)	(44)	(96)	(71)	(23)
Spillover effect	27	142	23	116***	83*		50*	4	166***	123	42
	(30)	(103)	(20)	(35)	(48)		(27)	(102)	(61)	(79)	(69)
R-squared	0.155	0.424	0.110	0.122	0.134		0.131	0.141	0.100	0.127	0.141
Observations	253,522	71,839	142,575	48,461	82,508		51,056	7,706	104,521	163,679	14,516
Average duration 1999-2001	174	298	120	191	198		186	248	539	489	205
	Ineligible cases										
Spillover effect	203	228	-43	14	134	-5	35	-95	-246	29	124
	(180)	(435)	(274)	(180)	(137)	(50)	(39)	(149)	(616)	(170)	(108)
R-squared	0.243	0.404	0.207	0.362	0.466	0.125	0.154	0.151	0.281	0.149	0.183
Observations	5,961	4,128	2,993	3,722	11,834	24,771	36,433	15,501	2,098	11,491	18,888
Average duration 1999-2001	377	906	470	297	267	196	193	331	659	749	372
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
District trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: All regressions include case-level and district-level control variables: maximum statutory sentence, dummies for two, three, four, and more charges per case, situational dummies, dummies for the presence of each of the eight most frequent sections of the criminal code among the charges, defendant gender, foreigner status and education levels, number of cases per prosecutor, and the number of prosecutors in a district. Standard errors clustered by district are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 6: 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
	Potentially eligible cases										
Direct effect	0.094***	0.226***	0.096***	0.109***	0.112***		0.138	0.028	0.034	0.149**	0.178***
	(0.023)	(0.042)	(0.032)	(0.028)	(0.031)		(0.106)	(0.068)	(0.040)	(0.06)	(0.048)
Spillover effect	0.031	0.064	−0.009	0.126**	0.068		−0.04	0.064	0.057*	0.016	−0.173
	(0.036)	(0.068)	(0.043)	(0.049)	(0.059)		(0.104)	(0.123)	(0.031)	(0.048)	(0.119)
R-squared	0.055	0.070	0.058	0.092	0.069		0.165	0.093	0.035	0.050	0.151
Observations	264,398	76,912	144,196	50,819	86,982		55,010	8,347	109,649	167,763	15,336
Avg. probability 1999-2001	0.83	0.70	0.93	0.8	0.78		0.67	0.75	0.82	0.79	0.65
	Ineligible cases										
Spillover effect	0.002	0.098	−0.402**	0.01	0.02	0.058	0.065	0.057	−0.403	−0.064	0.009
	(0.085)	(0.212)	(0.201)	(0.148)	(0.129)	(0.049)	(0.086)	(0.091)	(0.251)	(0.073)	(0.087)
R-squared	0.117	0.161	0.123	0.163	0.069	0.058	0.144	0.091	0.184	0.096	0.123
Observations	6,255	4,517	3,128	3,812	12,226	25,664	37,901	16,897	2,130	12,274	19,463
Avg. probability 1999-2001	0.9	0.74	0.79	0.91	0.88	0.87	0.8	0.76	0.81	0.8	0.84
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
District trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: All regressions include case-level and district-level control variables: maximum statutory sentence, dummies for two, three, four, and more charges per case, situational dummies, dummies for the presence of each of the eight most frequent sections of the criminal code among the charges, defendant gender, foreigner status and education levels, number of cases per prosecutor, and the number of prosecutors in a district. Standard errors clustered by district are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 7: Duration from charges to adjudication (marginal effects, in days)

	(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	−45	−103**	−16	2	−51		−10	−188*	−30	22	−26
	(40)	(51)	(25)	(51)	(32)		(95)	(100)	(44)	(92)	(28)
Spillover effect	119**	52	47	87	62		-121*	260*	146***	58	-19
	(58)	(72)	(41)	(76)	(51)		(73)	(142)	(53)	(75)	(109)
R-squared	0.269	0.281	0.141	0.282	0.239		0.202	0.422	0.229	0.276	0.270
Observations	219,331	60,285	127,470	53,044	67,822		47,314	8,499	103,969	146,684	10,797
Average duration 1999-2001	254	289	136	254	230		253	370	208	267	210
						Ineligible cases					
Spillover effect	146	32	135	-151	152	69	112	185	50	93	-111
	(217)	(326)	(148)	(248)	(197)	(113)	(76)	(151)	(264)	(138)	(110)
R-squared	0.460	0.472	0.334	0.412	0.474	0.407	0.251	0.396	0.537	0.418	0.356
Observations	4,472	3,397	2,835	3,765	10,697	21,710	27,044	13,284	1,661	9,006	16,899
Average duration 1999-2001	373	646	233	414	286	391	308	444	417	478	298
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
District trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: All regressions include case-level and district-level control variables: maximum statutory sentence, dummies for two, three, four, and more charges per case, situational dummies, dummies for the presence of each of the eight most frequent sections of the criminal code among the charges, dummy for pretrial detention, defendant gender, foreigner and juvenile status, dummies for one, two, three, four,a and more prior convictions, number of cases per senate, and the number of senates in a district. Standard errors clustered by district are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 8: 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
	Potentially eligible cases										
Direct effect	0.055**	0.146***	0.02	0.087**	0.06*		0.064	−0.114	−0.019	−0.037	−0.021
	(0.024)	(0.041)	(0.014)	(0.038)	(0.031)		(0.082)	(0.091)	(0.049)	(0.038)	(0.043)
Spillover effect	-0.05	-0.043	0.001	-0.1*	-0.044		0.131*	-0.018	-0.018	0.06	0.059
	(0.042)	(0.068)	(0.038)	(0.053)	(0.059)		(0.078)	(0.141)	(0.043)	(0.043)	(0.134)
R-squared	0.054	0.095	0.051	0.075	0.068		0.124	0.089	0.043	0.053	0.118
Observations	219,331	60,285	127,470	53,044	67,822		47,314	8,499	103,969	146,684	10,797
Avg. probability 1999-2001	0.84	0.75	0.94	0.88	0.85		0.78	0.79	0.77	0.85	0.79
	Ineligible cases										
Spillover effect	0.039	0.136	-0.193	0.515	0.087	-0.026	-0.018	-0.016	-0.417	-0.110	-0.068
	(0.267)	(0.320)	(0.224)	(0.329)	(0.112)	(0.085)	(0.08)	(0.112)	(0.318)	(0.132)	(0.07)
R-squared	0.204	0.194	0.120	0.205	0.151	0.110	0.121	0.126	0.316	0.102	0.105
Observations	4,472	3,397	2,835	3,765	10,697	21,710	27,044	13,284	1,661	9,006	16,899
Avg. probability 1999-2001	0.84	0.63	0.86	0.88	0.89	0.87	0.83	0.74	0.85	0.75	0.9
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
District trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: All regressions include case-level and district-level control variables: maximum statutory sentence, dummies for two, three, four, and more charges per case, situational dummies, dummies for the presence of each of the eight most frequent sections of the criminal code among the charges, dummy for pretrial detention, defendant gender, foreigner and juvenile status, dummies for one, two, three, four,a and more prior convictions, number of cases per senate, and the number of senates in a district. Standard errors clustered by district are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 9: Duration offense to charges (matching-based four-step estimator, in days)

	(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
	A: Potentially eligible cases										
	Direct effects estimated by matching										
Direct effect	−132***	−214***	−105***	−145***	−130***		−144***	−163***	−246***	−242***	−127***
	(0.8)	(3.4)	(3.3)	(2.4)	(2.1)		(3.9)	(11.0)	(8.7)	(4.2)	(6.8)
Observations	253,522	71,839	142,575	48,461	82,508		51,056	7,706	104,521	163,679	14,516
	Spillover effects estimated by OLS										
Spillover effect	23	219	36	170***	162**		72***	42	123	200**	76
	(35)	(152)	(35)	(44)	(67)		(24)	(152)	(86)	(93)	(98)
R-squared	0.155	0.424	0.110	0.122	0.134		0.131	0.141	0.100	0.127	0.141
	B: Ineligible cases, spillover effects estimated by OLS										
Spillover effect	226	699	−133	3	273	−1	52	−180	−101	20	191
	(207)	(637)	(498)	(297)	(190)	(73)	(41)	(250)	(983)	(221)	(144)
R-squared	0.243	0.404	0.207	0.362	0.466	0.125	0.154	0.151	0.281	0.149	0.183
Observations	5,961	4,128	2,993	3,722	11,834	24,771	36,433	15,501	2,098	11,491	18,888
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
District trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: The direct effects are estimated by nearest-neighbor matching. The spillover effects are estimated by separate regressions using a measure of the resources released due to the fast-track procedure. See Section 6.3 for details. The regressions estimating the spillover effects also include case-level and district-level control variables: maximum statutory sentence, dummies for two, three, four, and more charges per case, situational dummies, dummies for the presence of each of the eight most frequent sections of the criminal code among the charges, defendant gender, foreigner status and education levels, number of cases per prosecutor, and the number of prosecutors in a district. Standard errors clustered by district are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 10: Probability of charges (matching-based four-step estimator)

	(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
	A: Potentially eligible cases										
	Direct effects estimated by matching										
Direct effect	0.076***	0.152***	0.072***	0.108***	0.102***		0.154***	0.058***	0.098***	0.094***	0.173***
	(0.001)	(0.005)	(0.003)	(0.005)	(0.004)		(0.012)	(0.022)	(0.005)	(0.004)	(0.012)
Observations	264,398	76,912	144,196	50,819	86,982		55,010	8,347	109,649	167,763	15,336
	Spillover effects estimated by OLS										
Spillover effect	0.036	0.121	−0.062	0.127*	0.054		−0.013	0.209	0.075*	−0.023	−0.190
	(0.06)	(0.079)	(0.073)	(0.07)	(0.088)		(0.074)	(0.175)	(0.041)	(0.058)	(0.162)
R-squared	0.055	0.070	0.058	0.092	0.069		0.165	0.093	0.035	0.050	0.150
	B: Ineligible cases, spillover effects estimated by OLS										
Spillover effect	-0.138	0.067	-0.724**	0.119	-0.029	0.093	0.105	0.003	-0.486	-0.057	-0.047
	(0.128)	(0.286)	(0.337)	(0.239)	(0.159)	(0.083)	(0.094)	(0.140)	(0.430)	(0.107)	(0.135)
R-squared	0.117	0.161	0.123	0.163	0.069	0.058	0.144	0.091	0.183	0.096	0.123
Observations	6,255	4,517	3,128	3,812	12,226	25,664	37,901	16,897	2,130	12,274	19,463
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
District trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: The direct effects are estimated by nearest-neighbor matching. The spillover effects are estimated by separate regressions using a measure of the resources released due to the fast-track procedure. See Section 6.3 for details. The regressions estimating the spillover effects also include case-level and district-level control variables: maximum statutory sentence, dummies for two, three, four, and more charges per case, situational dummies, dummies for the presence of each of the eight most frequent sections of the criminal code among the charges, defendant gender, foreigner status and education levels, number of cases per prosecutor, and the number of prosecutors in a district. Standard errors clustered by district are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 11: Duration from charges to adjudication (matching-based four-step estimator, in days)

	(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
	A: Potentially eligible cases										
	Direct effects estimated by matching										
Direct effect	−48***	−42***	−35***	−48***	−58***		−43***	−111***	−26***	−42***	−32***
	(1.8)	(4.4)	(4.7)	(4.7)	(4.9)		(9.6)	(24.6)	(4.6)	(4.1)	(9.2)
Observations	219,331	60,285	127,470	53,044	67,822		47,314	8,499	103,969	146,684	10,797
	Spillover effects estimated by OLS										
Spillover effect	255*	472***	84**	262*	341***		252**	609**	347***	387***	290
	(132)	(108)	(32)	(146)	(105)		(106)	(241)	(124)	(145)	(189)
R-squared	0.270	0.282	0.141	0.282	0.239		0.202	0.422	0.229	0.277	0.271
	B: Ineligible cases, spillover effects estimated by OLS										
Spillover effect	361	608	679***	-255	708*	475***	272**	635***	341***	326	397***
	(569)	(528)	(206)	(971)	(418)	(173)	(122)	(177)	(115)	(329)	(146)
R-squared	0.460	0.472	0.337	0.412	0.474	0.407	0.251	0.397	0.538	0.418	0.356
Observations	4,472	3,397	2,835	3,765	10,697	21,710	27,044	13,284	1,661	9,006	16,899
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
District trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: The direct effects are estimated by nearest-neighbor matching. The spillover effects are estimated by separate regressions using a measure of the resources released due to the fast-track procedure. See Section 6.3 for details. The regressions estimating the spillover effects also include case-level and district-level control variables: maximum statutory sentence, dummies for two, three, four, and more charges per case, situational dummies, dummies for the presence of each of the eight most frequent sections of the criminal code among the charges, dummy for pretrial detention, defendant gender, foreigner and juvenile status, dummies for one, two, three, four, and more prior convictions, number of cases per senate, and the number of senates in a district. Standard errors clustered by district are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 12: Probability of conviction (matching-based four-step estimator)

	(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
	A: Potentially eligible cases										
	Direct effects estimated by matching										
Direct effect	0.053***	0.061***	0.038***	0.051***	0.048***		0.046***	0.039	−0.009	0.049***	0.041**
	(0.002)	(0.006)	(0.003)	(0.007)	(0.006)		(0.015)	(0.036)	(0.009)	(0.005)	(0.017)
Observations	219,331	60,285	127,470	53,044	67,822		47,314	8,499	103,969	146,684	10,797
	Spillover effects estimated by OLS										
Spillover effect	-0.046	-0.098	-0.002	-0.049	-0.042		-0.026	0.101	-0.051	-0.037	0.059
	(0.061)	(0.077)	(0.031)	(0.066)	(0.124)		(0.061)	(0.132)	(0.06)	(0.04)	(0.142)
R-squared	0.054	0.095	0.051	0.075	0.067		0.124	0.089	0.043	0.053	0.118
	B: Ineligible cases, spillover effects estimated by OLS										
Spillover effect	-0.005	-0.510	0.118	1.048	0.415*	0.122	0.007	0.094	-0.557**	-0.140	0.159*
	(0.387)	(0.588)	(0.357)	(0.972)	(0.241)	(0.113)	(0.104)	(0.196)	(0.232)	(0.228)	(0.081)
R-squared	0.204	0.194	0.120	0.204	0.151	0.110	0.121	0.126	0.316	0.102	0.105
Observations	4,472	3,397	2,835	3,765	10,697	21,710	27,044	13,284	1,661	9,006	16,899
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
District trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: The direct effects are estimated by nearest-neighbor matching. The spillover effects are estimated by separate regressions using a measure of the resources released due to the fast-track procedure. See Section 6.3 for details. The regressions estimating the spillover effects also include case-level and district-level control variables: maximum statutory sentence, dummies for two, three, four, and more charges per case, situational dummies, dummies for the presence of each of the eight most frequent sections of the criminal code among the charges, dummy for pretrial detention, defendant gender, foreigner and juvenile status, dummies for one, two, three, four, and more prior convictions, number of cases per senate, and the number of senates in a district. Standard errors clustered by district are in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 13: Accounting for the effect of the fast-track procedure

	Duration from offense to charges				Probability of charges			
	Actual duration in 2001 (days)	Change in actual duration, 2001–2008	Change in counterfactual duration, 2001–2008	Fast track accounts for	Actual probability in 2001	Change in actual probability, 2001–2008	Change in counterfactual probability, 2001–2008	Fast track accounts for
Theft/burglary	169	−37	−11	−26	0.82	0.11	0.07	0.05
Property/economic offenses	359	0	−7	7	0.69	0.07	0.00	0.07
Driving	123	−77	−12	−66	0.93	0.03	−0.04	0.07
Against personal liberty	195	−28	−17	−12	0.81	0.09	0.02	0.07
Against public order	207	−19	−15	−4	0.77	0.08	0.05	0.03
Against public safety	191	32	50	−18	0.62	0.07	0.10	−0.03
Against life or health	190	−5	−19	14	0.66	−0.01	−0.00	−0.01
Sex offenses	255	34	46	−13	0.76	0.12	0.09	0.02
Against family	566	214	164	50	0.83	0.06	0.04	0.02
Fraud/embezzlement	524	104	74	30	0.78	0.08	0.07	0.01
All potentially eligible cases	299	−47	−29	−18	0.79	0.11	0.06	0.05

Note: Change in the counterfactual outcome denotes the average change of the predicted values of the outcome variables. The predictions are based on regressions (Tables 5 and 6), assuming that no case would be prosecuted by the fast-track procedure.

Table 14: Reallocation of cases at the prosecutor and court senate level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Prosecutors						Court senates					
Outcome variables:	Share of ineligible cases	Share of ineligible cases	Number of cases	Number of cases	Number of ineligible cases	Number of ineligible cases	Share of ineligible cases	Share of ineligible cases	Number of cases	Number of cases	Number of ineligible cases	Number of ineligible cases
Time trend	0.002*	-0.007**	0.4	-1.8	-0.1	-0.2	-0.001	-0.003	2.5	-5.7**	-0.8***	-1.1***
	(0.001)	(0.003)	(1.2)	(2.0)	(0.1)	(0.2)	(0.001)	(0.002)	(1.6)	(2.3)	(0.2)	(0.3)
2nd tercile	-0.004**	0.007*	-6.0***	-8.5***	-1.0***	-1.2***	-0.004***	-0.000	-4.4**	0.2	-0.6**	-0.5
× time trend	(0.002)	(0.004)	(1.6)	(2.4)	(0.2)	(0.3)	(0.002)	(0.003)	(2.0)	(3.1)	(0.3)	(0.4)
3rd tercile (specialists)	-0.024***	-0.003	-1.0	-2.1	-1.9***	-2.2***	-0.012***	-0.005*	-1.3	2.8	-1.4***	-0.7
× time trend	(0.003)	(0.005)	(1.7)	(2.5)	(0.3)	(0.4)	(0.002)	(0.003)	(2.2)	(3.2)	(0.3)	(0.5)
Fast-track share in a district		0.218***		52.8*		4.5		0.029		198.8***		7.0
		(0.065)		(32.0)		(4.1)		(0.035)		(41.2)		(5.2)
2nd tercile		-0.261***		61.7		4.7		-0.100**		-107.4*		-1.7
× fast-track share		(0.073)		(42.5)		(5.8)		(0.051)		(59.1)		(7.9)
3rd tercile (specialists)		-0.518***		28.9		7.4		-0.166***		-98.3		-16.5*
× fast-track share		(0.101)		(42.9)		(7.9)		(0.054)		(62.3)		(8.8)
Prosecutor / court senate FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	0.535	0.542	0.671	0.675	0.688	0.690	0.410	0.414	0.566	0.574	0.506	0.507
Observations	5,019	5,019	5,019	5,019	4,571	4,571	3,224	3,224	3,224	3,224	3,111	3,111

Note: The regressions are estimated at the level of the prosecutors / court senates and year, during 2001-2008 period. The terciles denote the location of each prosecutor / court senate in a within-district tercile of the share of ineligible cases in the last pre-reform year (2001). Robust standard errors clustered by the prosecutor or court senate in parentheses: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

A Appendix: Theoretical framework

We present a more detailed summary of our theoretical model (Dušek and Montag, 2016) with a focus on the predictions that are relevant for this paper.

A.1 Socially optimal use of trials and alternative criminal procedures

A benevolent adjudicator faces a continuum of criminal cases that differ by evidence strength p. The adjudicator faces budget constraint and her aim is to minimize the sum of judicial errors. Cases can be resolved via three different avenues: (i) a case can be dropped, (ii) it can be resolved at trial, (iii) or resolved through alternative (less-than-trial) procedure. These options differ by administrative costs and the type of judicial errors they may generate. Specifically, trial is the costliest option with a cost c_T, the alternative procedure has positive costs c_C but is cheaper than trial, whereas dropping a case costs nothing. The basic version of the model assumes for simplicity that the trials are perfect; that is, they always reveal the truth about the defendant's guilt or innocence.[16] It can be thought of simply as an administrative declaration of the guilt justified by the initial evidence. Alternative procedure may therefore result in a wrongful conviction, the social cost of which is w_c. Dropping a case may result in wrongful acquittal, the social cost of which is w_a.

The adjudicator decides through which avenue to resolve each case. For each offense severity category i, the model yields two standards of evidence which lead to optimal allocation of cases to trial and the alternative procedure

$$p_{Ti} w_{ai} = \lambda c_T, \qquad \text{and} \tag{3}$$

$$(1 - p_{Ci}) w_{ci} = \lambda (c_T - c_C), \tag{4}$$

where p_{Ti} is the probability of guilt (inferred from the evidence against the defendant) at which the adjudicator is indifferent between dropping a case and pursuing a trial. In the second condition, p_{Ci} is the strength of evidence at which the adjudicator is indifferent between pursuing a trial and convicting the defendant through the alternative procedure. At the optimal evidence standards, shifting the marginal defendant to trial would reduce the

[16]The extensions of the model incorporate imperfect trials but do not qualitatively affect the results.

expected costs of judicial errors by the amount equal to the additional costs of conducting the trial (scaled by the Lagrange multiplier λ).

The Lagrange multiplier is the monetary value of judicial errors that would be avoided as a result of increasing the budget by one dollar. With sufficient budget, $\lambda = 1$, the adjudicator merely compares the costs of judicial errors and the monetary costs of the trial. With an insufficient budget, the adjudicator acts as if the costs of the trial were greater than they nominally are, scaled up by a factor $\lambda > 1$. She conducts fewer trials, more cases are dropped, and more defendants are convicted through the alternative procedure than would be socially optimal. As a result, the total costs of judicial errors are greater.

A.2 Comparative statics of introducing alternative criminal procedure

Graphical representation of the model's comparative statics relevant for our empirical analysis is shown in Figure A.1. Panel A depicts the pre-reform situation when all criminal cases must have been decided through the full-fledged criminal trial. The resources are primarily channeled to the high-severity offenses, indexed by h, allowing for a lower evidence standard for pursuing trials and therefore fewer cases dropped for insufficient evidence. For low-severity offenses, indexed by l, the costs of wrongful acquittal are lower and therefore more cases get dropped as pursuing a trial for low-severity offenses with weak evidence, and thus small chance of conviction, is wasteful.

The effects of introducing alternative procedure are shown in panel B of Figure A.1. The fast-track procedure introduced in the Czech Republic in 2002 stipulates that only less serious cases with solid evidence are eligible. All other cases must be resolved via standard trial. Low-severity cases with evidence above p_{Cl} will be convicted through the alternative procedure, and the probability of conviction for such cases will rise. The alternative procedure saves costs releasing resources to be used elsewhere. As a result, λ decreases, reducing the evidence standards for charging, p_{Tl} andp_{Th}, for the low-severity as well as high-severity cases. Greater fraction of the low- and high-severity cases will be charged.

Note, however, that introducing alternative procedure will primarily affect the evidence standards for low-severity offenses, increasing the resources allocated therein. This is a direct implication of the optimality condition that the cost of wrongful acquittals should be equalized, on the margin, across offense severity categories. Mathematically,

$$dp_{Tl} w_{al} = dp_{Th} w_{ah}, \tag{5}$$

implying $dp_{Tl} > dp_{Th}$, as $w_{al} < w_{ah}$, and thus a greater change in the evidence standard for the low-severity offenses.

Figure A.1: The optimal reallocation of cases after introducing the alternative procedure