

The Effects of a Shorter Criminal Procedure on Crime Rates

*Narges Khoshalhan, Rouhollah Sepehri**

Law Department, Naragh Branch, Islamic Azad University, Naragh, Iran

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ABSTRACT

Objective: A shorter and simpler criminal procedure may aspect crime rates by increasing the perceived severity of punishment and by inducing a reallocation of police enforcement resources. **Methodology:** I investigate the impacts of a criminal procedure reform in the Iran that allowed certain less serious effect to be prosecuted via a simplified (fast-track) procedure. The share of cases actually prosecuted via the fast-track procedure varied substantially across police districts and openness, which provides the basis for the identification strategy. The shorter procedure had very deferent effects on ordinary crimes reported by the victims and on crimes that are identified mostly by the enforcement effort of the police. **Results:** Specifically, it led to a substantial increase in the number of recorded criminal offenses associated with driving. This finding is best rationalized by a reallocation of police enforcement effort towards crimes that have low enforcement costs. **Conclusion:** I also find some but rather weak evidence of a deterrent effect on burglary and embezzlement.

1. Introduction

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

The length and complexity of the criminal procedure has implications for the behaviour of offenders and law enforcement officials. The offender discounts, at the time of committing the offense, the severity of punishment by the length of the time elapsed between the offense and the actual imposition of the punishment. Punishment imposed shortly after the offense is effectively more severe and should have a greater deterrent effect on crime. This deterrent effect should be enhanced by the fact that offenders tend to discount the future much more heavily than law-abiding citizens¹ (Herrnstein (1983), Wilson and Herrnstein (1985) and Nagin and Polansky (2004)). The economic model of crime therefore predicts a causal relationship from speedier criminal procedure to lower crime rates.

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

Two papers tested empirically the deterrent effect of a shorter criminal procedure. Pellegrina (2008) exploits cross-sectional variation in the length of criminal trials across provinces in Italy to detect a positive and statistically significant relationship between the length of trials and the rate of thefts, robberies, fraud, and racketeering. Soares and Sviatschi (2010) find a similar relationship between the rate at which courts process the criminal caseload

* Corresponding author: Rsepehrii@gmail.com

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(which is indirectly linked to the length of the procedure) and crime rates in a panel of cantons in Cost Rica. The reallocation of the enforcement effort in response to changes in the price of enforcement was investigated by Benson et al., (1992) and Baicker and Jacobson (2007). They find that when the local police departments in the U.S. were provided with the authority to keep the revenue from assets forfeited in drug enforcement, they shifted their enforcement resources towards the drug crimes and away from non-drug crimes.

Estimating the effects of case duration on crime rates is beset with a simultaneity problem: higher crime rates increase the caseload of the police and courts who then take more time to process the cases. An exogenous variation in case durations is needed to identify the causal effect on crime rates. The Iran criminal procedure reform, adopted in 2002, provides a quasi-natural experiment. It prescribed that certain less serious crimes can be prosecuted via a "fast-track" procedure, with fewer procedural steps, substantially less paperwork, and stricter deadlines. The stated objectives were to reduce case durations, save resources in the enforcement of less serious crimes, and free up resources for the enforcement of serious crimes.³ After the reform, the average duration of the procedure (from offense to final adjudication) declined by about a third for offenses that were relatively extensively covered by the fast-track.

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

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

The outcome variable of interest - the officially recorded crime rates - is a joint product of the underlying true crime rate and the police discretion in discovering and recording the crime. The deterrent effect of a shorter procedure should reduce the number of recorded crimes. The enforcement reallocation effect should increase the number of recorded crimes but only to the extent that the police can influence it. Offenses such as thefts or robberies are typically reported to the police by the victims⁴ and the reallocation effect should be relatively weak. I expect the estimated effect of shorter duration on victim-reported offenses to be negative (but still underestimate the true deterrent effect). On the other hand, crimes such as drug offenses or driving offenses are discovered almost exclusively through the police enforcement efforts. The police have substantial discretion in influencing the recorded number of such crimes. The reallocation effect may even dominate the deterrent effect. If it does, the estimated effect of shorter duration on police-reported offenses would be positive (and still underestimate the true reallocation effect).

The strongest and most robust result is that the reduction in case duration substantially increased the number of two police-reported offenses associated with driving: driving under the influence and obstruction of an official order (a criminal offense that is committed by a failure to comply with a court order, and most frequently is committed by drivers who continue driving with a suspended driving license⁵). The estimates are statistically and economically significant. They imply that in the absence of the reform, the number of recorded driving-under-influence cases would have been 20–34 percent below its actual level several years after the reform, and the number of recorded obstruction cases would have been 24–44 percent below. I also find a negative effect of shorter case duration on burglaries and embezzlements but it is not robust to regression specification.

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

2. Materials and methods

2.1. Data

The empirical work uses statistical records of the Police of the Iran and the Ministry of Justice aggregated at the level of a district, year, and offense. There are 79 police districts with a population of about 125,000 on average. The dataset covers three years before the reform (1999–2001) and seven years afterward (2002–2008). It contains the number of cases that passed through individual stages of the criminal procedure, starting with the number of offenses reported to the police, the number of cases when the suspect was identified, the number of prosecutions carried by the conventional and fast-track procedure, etc. The classification of offenses is very detailed. There are between 167 to 175 offense definitions, depending on the year. I aggregate these detailed offenses into 23 somewhat broader (and conventional) offense categories and also drop some obscure or rare offenses. The list of offense categories used in the analysis is given in Table 1. The Ministry of Justice records contain procedural information on each criminal case, including the dates of the offense, charges, and final adjudication. I aggregated the records in order to obtain average case durations at the level of the offense, district, and year.

Figure 2 plots the raw data on crime rates (number of offenses per 100,000), divided into police/victim reported offenses and covered/other offenses. It previews the key results. The number of covered victim-reported offenses was declining gradually throughout most of the sample period (by a third in total). Other victim-reported offenses were also on an overall downward trend. The rate of covered police-reported offenses was stable before the reform. It jumped up from 127 to 175 in the first post-reform year, and continued to rise at a slower rate thereafter. On the other hand, the police-reported offenses that were generally not covered by the reform were on a declining trend before the reform and declined in several steps afterwards.

2.2. Identifying variation

The identification strategy is based on the fact that the actual adoption of the fast-track procedure varied widely across offenses and districts. The adoption is measured by the share of cases that are prosecuted via the fast-track procedure in all prosecuted cases. Figures 3 and 4 show the share of fast-track prosecutions for each offense, aggregated at the national level. The fast-track procedure became used relatively heavily in prosecuting aggravated assault, trespass, burglary, thefts, other property crimes, embezzlement, illegal possession of a banking card, obstruction of an official order, vandalism, and driving under the influence. The police-reported offenses exhibit higher share of the fast-track because such offenses are typically discovered and recorded when the offender is captured, therefore the identity of the offender is immediately known. Obstruction of an official order has had by far the highest share of fast-track from the beginning. It is an administratively simple offense and the evidence is usually straightforward.

The variation across districts is presented in Table 2. It shows the mean, standard deviation, and the 5th and 95th percentiles of the share of fast-track prosecutions for the covered offenses in 2002 (the first post-reform year) and in 2008 (the last year in our data) at the district level. The fast-track procedure immediately became the prevalent method for prosecuting obstructions of an official order, with 55 percent on average, and 27 percent in the 5th percentile district. For theft, the initial share of the fast-track prosecutions was 21 percent, varying from 7 percent in the 5th percentile to 39 percent in the 95th percentile. Six years later, there is an overall increase in the adoption of the fast-track procedure for all offenses, but it occurs mainly through an even higher usage among the districts at the top of the distribution. The share of fast-track theft cases increased by 13 percentage points both on average and at the 95th percentile, but only by 8 percentage points at the 5th percentile. The share of fast-track prosecutions was still zero in the districts at the 5th percentile for many offenses six years since the reform.

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

I interviewed several Ministry of Interior, Police, and State Attorney officials to collect anecdotal evidence about the causes of the large variation across districts. In their view the differences between districts were driven first and foremost by bureaucratic inertia — some police officers and prosecutors were more willing to experiment with new methods than others. To a secondary degree, they were a by-product of the division of labor between patrol and investigative police units. Internal police guidelines divide the workload between these units, and such guidelines are issued by central, regional, and district chiefs, with an increasing level of detail. The investigative units generally disdain the fast-track procedure as a matter of their professional culture. In districts where the guidelines allocate pettier crimes to the investigative units, the share of fast-track prosecutions is lower. Many factors determine the allocation of labor in the guidelines other than the concerns about the use of the fast-track procedure; the resulting share of fast-track prosecutions is ancillary to those factors. There was also no political pressure from the central government or the regional governments to adopt the fast-track procedure intensively in specific districts.

Last, the practitioners indicated that differences in the adoption could be caused by the relative overload of the police officers and prosecutors. Police officers in districts with higher case load tended to adopt the fast-track more intensively in order to put more cases "off the table". In districts with low case load, the officers reported there was essentially no pressure to spend time and effort to learn and adopt the new procedure. The last explanation posits a relationship between the adoption intensity, (relative) staffing of the police, and crime levels. Importantly for the identification strategy, none of the anecdotal explanations posits a relationship between the adoption intensity and the trends in crime rates. Long case durations were never mentioned as a factor influencing adoption (Figure 4).

I check whether the intensity of adoption is correlated with the crime rates prior to adoption. The adoption intensity in a district is measured by the share of fast-track cases among covered offenses in the first post-adoption year (2002). Figure 5 plots the violent and property crime rates in each district in the last pre-reform year (2001) against the adoption intensity. There is a positive correlation for violent crimes. For property crimes, the visible positive correlation is driven by the four Prague districts (AI-AIV) that have the highest property crime rates and two of them were also among the most fervent adopters.

Figure 6 plots an equivalent picture for the percentage changes in crime rates during the pre-reform (1999-2001) period. The adoption intensity is unrelated to the pre-reform trends in the crime rates.

In a similar manner, the intensity of adoption is plotted against the case durations and caseload (crimes per police officer) in the last pre-adoption year (Figure 7). It indicates that adoption is positively but very weakly related to the duration of the court phase of the procedure and to the caseload per police officer. The relationship with load is driven by a five outliers (four Prague districts and Pilsen) that have very high caseload and were above-average (but not the highest) adopters. Figure 8 shows that the fast-track adoption was not related to the percentage changes in durations and load during the three years preceding the adoption.

3. Discussion and results

3.1. IV and reduced-form estimates

The IV estimates for covered victim-reported crimes are presented in Table 3. In the first-stage regressions, all the coefficients on the share of fast-track cases are negative and significant at 1 percent. They are large in magnitude - a one-percentage point increase in the share of fast-track cases reduces the case duration by between 0.53 to 1.33 percent. The values of the F-test statistic exceed 10 for all offenses. The estimates of the first-stage regressions show that the share of fast-track cases is a strong instrument.

The IV estimates of the effect of case durations on crime rates are reported in the top row of Table 3. The coefficients are positive for aggravated assault, burglary, embezzlement and miscellaneous offenses, as expected. However, none of them is statistically significant. For comparison, I also show the "naive" OLS estimates of an equivalent regression (equation 2) in the bottom of the tables. The OLS coefficients should be biased upward because of the reverse causality from more crimes to longer procedure. Indeed, the OLS estimates are positive for 6 out of 8 offenses. They are statistically significant for theft and burglary, the two most common offenses, where the magnitudes imply that a 10-percent reduction in case duration is associated with a reduction in crime rates by half a percent. The IV procedure appears to be removing the bias in the expected direction - the IV coefficients are smaller than OLS coefficients for all of these six offenses. For two remaining offenses (embezzlement and miscellaneous), the OLS have implausible negative values while the IV estimates are positive (but insignificant).

The results are very different for the covered police-reported offenses (Table 4), namely for two offenses associated with driving: obstruction of an official order and driving under the influence. The IV estimates are negative, very large, and significant at 1 percent. Their magnitudes imply that a 10-percent reduction in case duration increases the crime rate by 2.4 percent (obstruction) and 9.6 percent (DUI). A large negative effect of longer duration on crime rates is also found for violence against public officials and vandalism, although the coefficients are not statistically significant. The first-stage estimates show that the share of fast-track cases is an even stronger instrument for police-reported offenses than for victim-reported offenses. The reduced-form regressions are presented in Tables 5 (victim-reported offenses) and 6 (police-reported offenses). The estimated coefficients on the share of fast-track prosecutions have the expected negative sign for five out of the eight victim-reported offenses studied (aggravated assault, trespass, burglary, theft, and embezzlement). They are significant only for burglary and embezzlement. The coefficient of -0.32 for burglary implies that the burglary rate would be 32 percent lower if all cases were handled via the fast-track procedure, compared to what it would have been in the absence of the fast-track. (However, a 100% share may be beyond the realm of possibility; the actual share was 15% in 2008).

The second table again shows positive, large, and statistically significant coefficients for obstruction of an official order and driving under the influence. These coefficients imply that a full adoption of the fast-track would increase the number of recorded obstruction and driving-under-influence crimes by 83 and 33 percent, respectively. Full adoption is not beyond the realm of possibility as there are several districts where the share of fast-track exceeded 90 percent. (Choi, 2015)

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

4. Conclusion

The paper provided evidence that reducing the duration of criminal procedure has some important effects by exploiting a major criminal procedure reform in the Iran as a "quasi natural experiment".

Shorter criminal procedure increases the costs of committing the crime for the criminals and reduces the costs of prosecuting the criminals for the law enforcers. The findings show that the law enforcers are very responsive to the case duration, consistently with the resource reallocation hypothesis. The police responded to a shorter procedure by pursuing more vigorously those offenses that could suddenly be prosecuted quickly and at low cost. The number of two particular offenses that are recorded mostly through the police enforcement effort - obstruction of an official order and driving under the influence - rose relatively more in districts with high fast-track adoption. There is an economic reason why the reallocation was directed towards the offenses associated with driving and not towards other police-reported offenses. Allocating more resources to the enforcement of driving-related offenses presumably leads to a more predictable and larger increase in the number of captured offenders than allocating the same resources towards capturing, say, drug gangs or street vandals.

The IV estimates of the reallocation effects imply that a 10-percent reduction in case duration increases the recorded crime rate by 2.4 percent (obstruction) and 9.6 percent (DUI). In order to evaluate the economic significance of these estimates, I compare the actual crime rates with predicted crime rates under the assumption that the share of fast-track cases would have remained zero throughout the post-reform period while the socio-economic controls and the year dummies would have evolved as they actually did. (Asthana, 2017)

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

I find rather meagre evidence of the deterrent effect of shorter procedure on the victim-reported offenses that the reform explicitly targeted. The IV estimates are small and statistically insignificant. The reduced-form estimates show that the fast-track had some deterrent effect on burglary and embezzlement. Those estimated deterrent effects are economically significant. The estimates for burglary implies that the fast-track procedure as actually adopted reduced the burglary rate by 4.8 percent. The number of burglaries - as well as most other "ordinary" crimes - was declining gradually during the

post-reform period. The estimate implies that the fast-track accounts for 23 percent of the decline in burglaries during the 2002-2008 period and 11% of the decline in embezzlements.

The lack of strong evidence on the deterrent effect on property crimes contrasts with Pellegrina (2008) who also uses an IV strategy but finds a deterrent effect. One reason for the difference may lie in the research design. Pellegrina (2008) takes the conventional wisdom that peripheral courts are less efficient than the main courts, and the fact that the peripheral courts are being established far away from the provincial centres and in less populated areas. Then she uses the distance from the provincial centre and the area of the provincial district as an instrument for duration. Arguments could be made whether these geographical measures are indeed un-correlated with the unobservable determinants of crime rates. The identification strategy in this paper is based on an explicit quasi-experimental design. A reform that was adopted with varying intensity in different districts for plausibly exogenous reasons generated a variation in duration across time and districts. (Ahmed, 2015)

Second, the findings in this paper are of course context-specific to the 2002 Iran criminal procedure reform. The case durations of covered offenses declined by about 150 days after the reform (Figure 1) which is an impressive accomplishment. Still, the deterrence effects on victim-reported crimes may have been limited. The lack of salience to the offenders is one possible factor. The reform was not advertised to the general public, and the fast-track procedure in practice covered between 10 to 40 percent of offenses. The offenders may have learned only gradually about the change in the swiftness of punishment through their own experience or the experience of their peers (Glaeser et al., 1996). Also, the time span from offense to final adjudication is still about 300 days (victim-reported offenses) or 200 days (police-reported offenses). If offenders discount the future heavily, the perceived increase in the severity of punishment may be small if the punishment is still imposed 200 days after the offense. The underlying deterrence effect of a shorter procedure may be highly non-linear and may be most pronounced at very short durations.

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

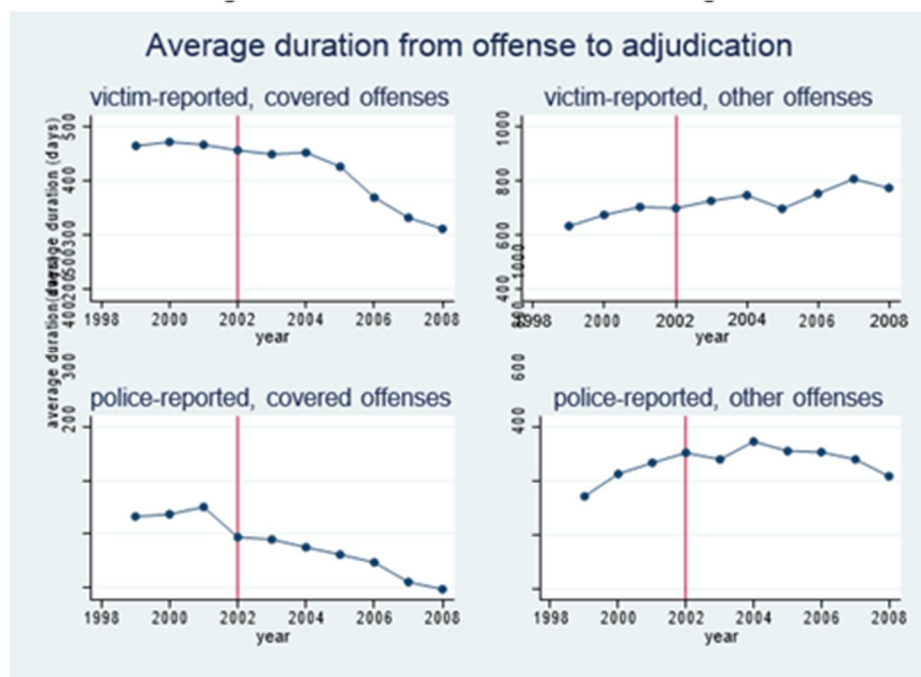


Figure 1: Duration of offense charges

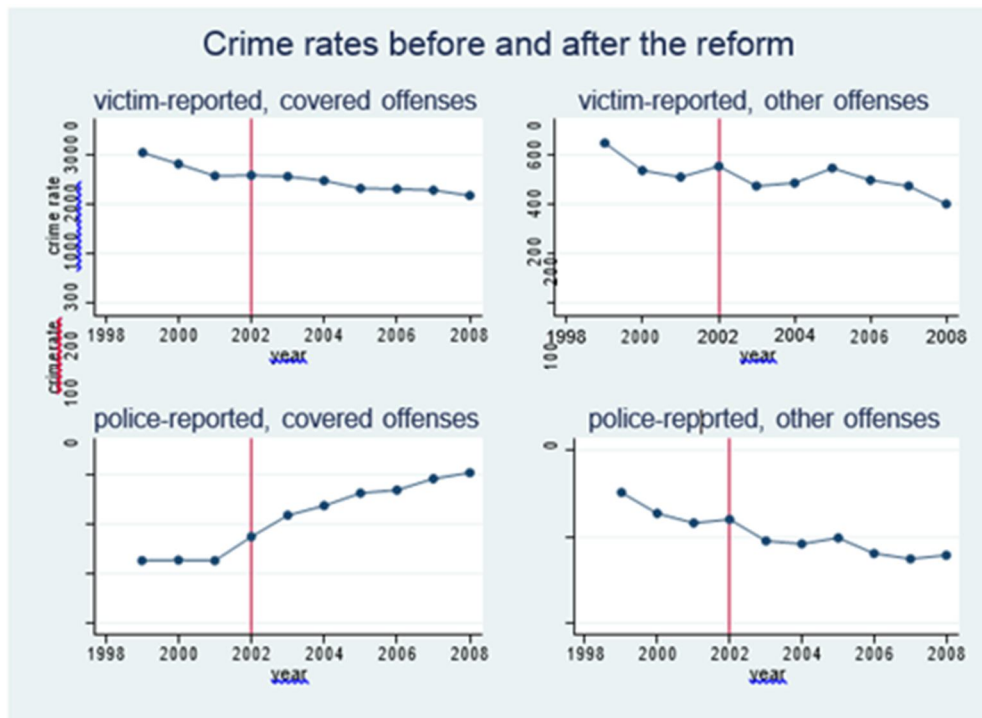


Figure 2: Crime rate before and after the reform

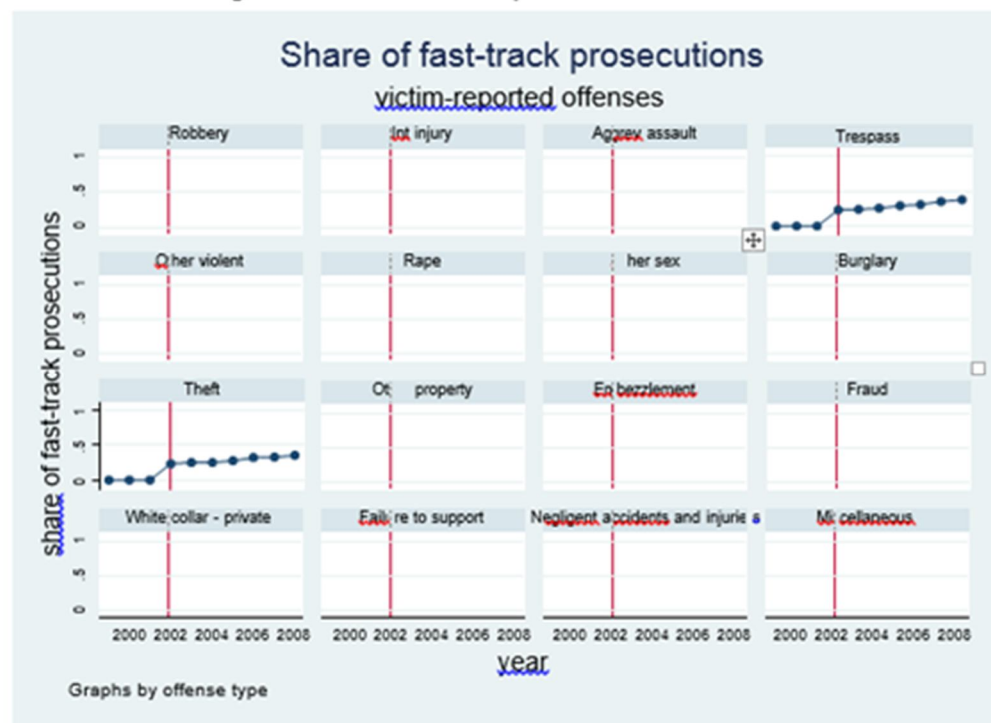


Figure 3: Gradual adoption the fast-track

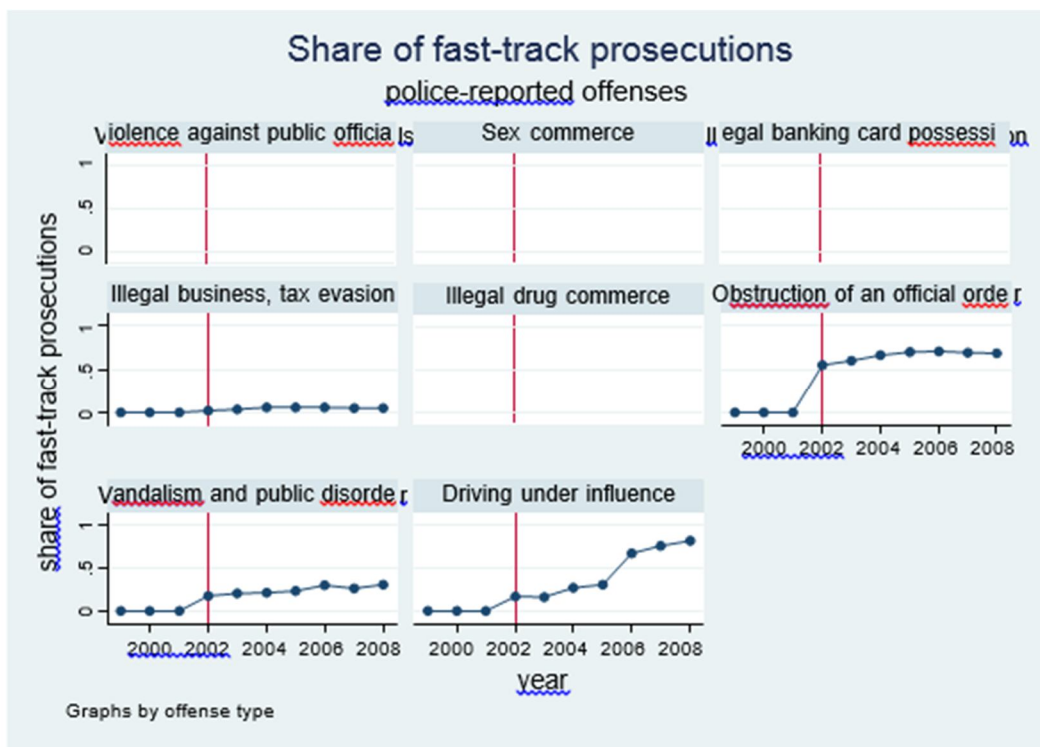


Figure 4: Gradual adoption of the fast track

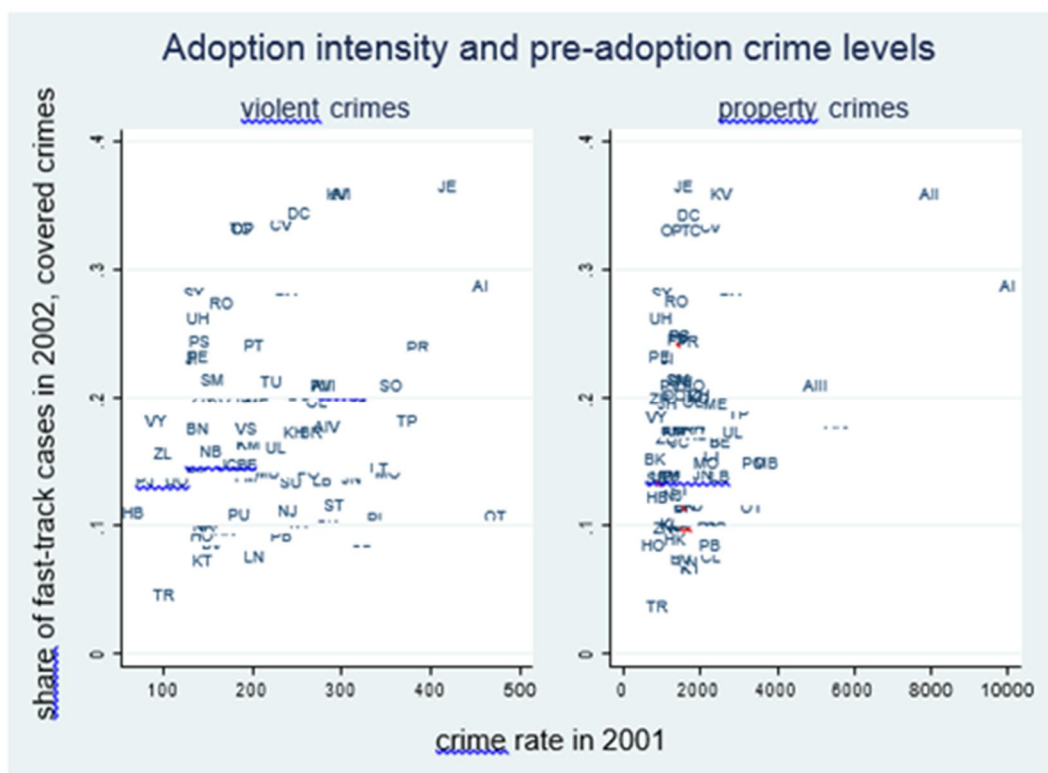


Figure 5: Endogeneity of fast-track adoption: crime levels

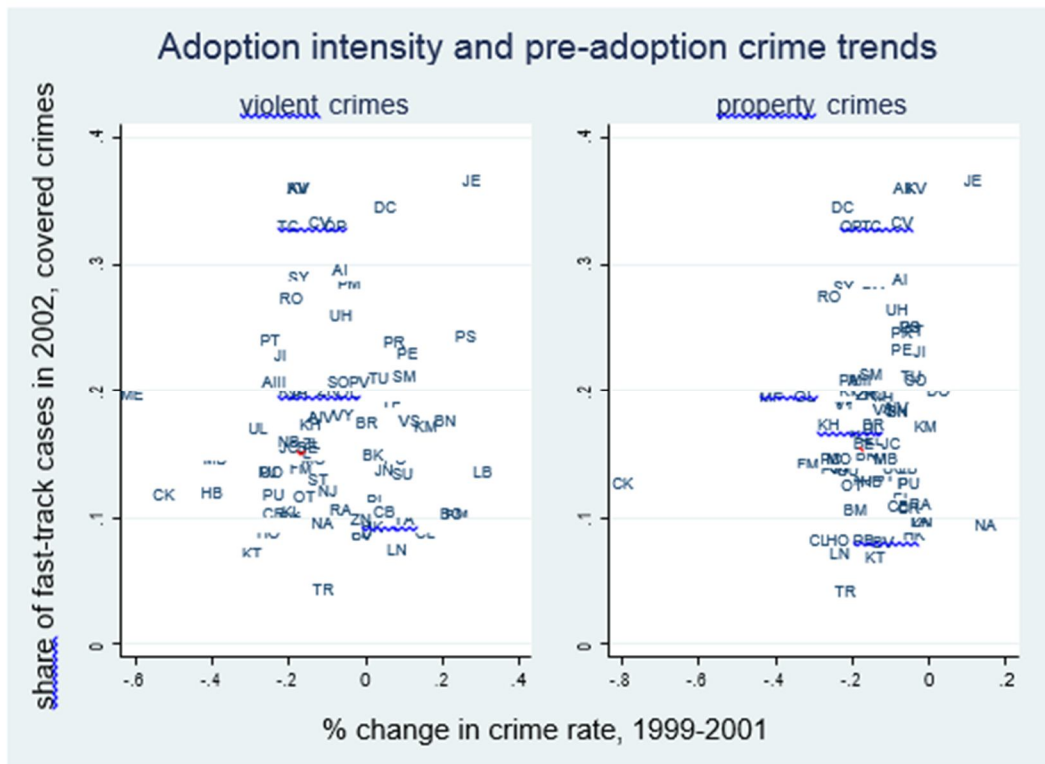


Figure 6: Endogeneity of fast-track adoption: crime trends

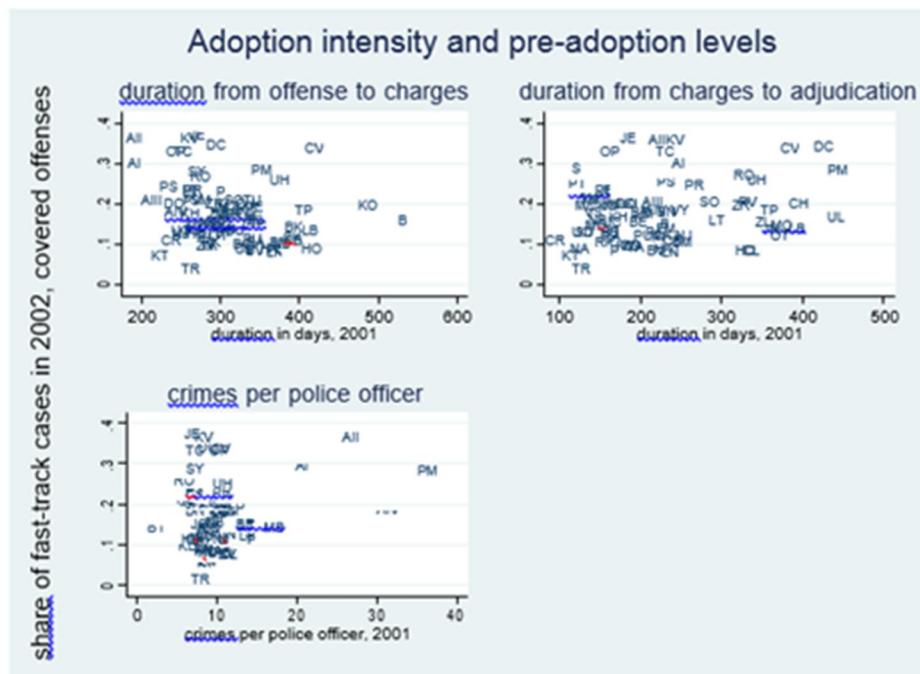


Figure 7: Endogeneity of fast-track adoption: levels of case durations and caseload

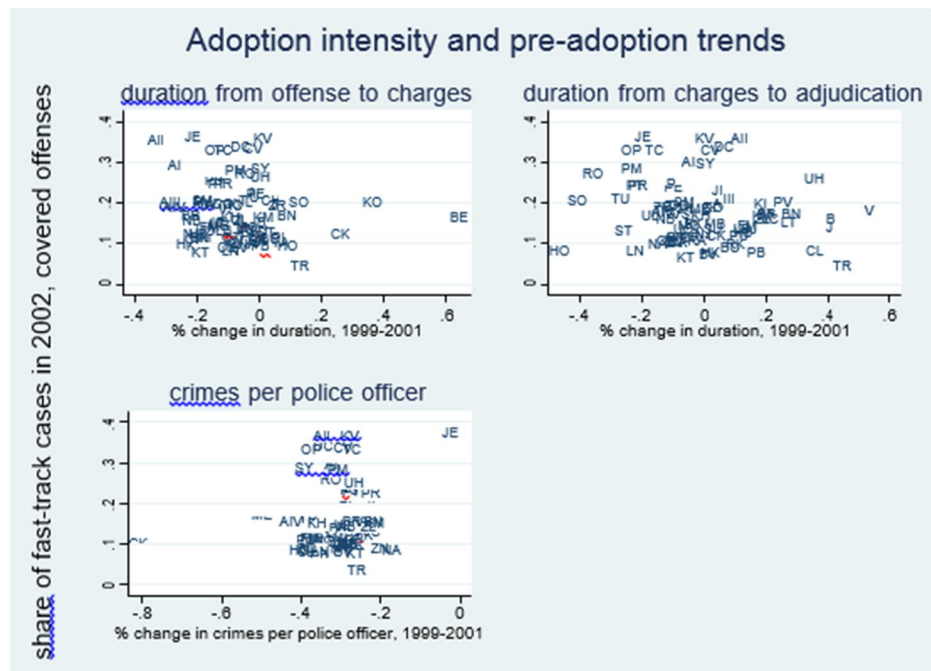


Figure 8: Endogeneity of adoption: pre-adoption trends in case duration and caseload

Table 1: Classification of offenses

BROAD CRIME CATEGORY	OFFENSE TYPE	POLICE-REPORTED OR VICTIM-REPORTED	COVERED OFFENSE
VIOLENT	ROBBERY INTENTIONAL INJURY RAPE OTHER VIOLENT OFFENSES OTHER SEX OFFENSES	VICTIM-REPORTED VICTIM- REPORTED VICTIM-REPORTED VICTIM-REPORTED VICTIM- REPORTED	NO NO NO NO
	AGGREGATED ASSAULT TRESPASS	VICTIM-REPORTED VICTIM- REPORTED	YES YES
	SEX COMMERCE	POLICE-REPORTED	NO
	VIOLENCE AGAINST PUBLIC OFFICIALS	POLICE-REPORTED	YES
	BURGLARY THEFT OTHER PROPERTY OFFENSES	VICTIM-REPORTED VICTIM- REPORTED VICTIM-REPORTED	YES YES YES
WHITE-COLLAR	ILLEGAL BANKING CARD POSSESSION	POLICE-REPORTED	YES
	FRAUD OTHER WHITE-COLLAR	VICTIM-REPORTED VICTIM- REPORTED	NO NO
	EMBEZZLEMENT	VICTIM-REPORTED	YES
OTHER	ILLEGAL BUSINESS, TAX EVASION	POLICE-REPORTED	NO
	FAILURE TO SUPPORT	VICTIM-REPORTED	NO
	NEGLIGENT ACCIDENTS AND INJURIES MISCELLANEOUS	VICTIM-REPORTED VICTIM- REPORTED	YES YES
	ILLEGAL DRUG COMMERCE	POLICE-REPORTED	NO
	OBSTRUCTION OF OFFICIAL ORDER DRIVING UNDER INFLUENCE	POLICE-REPORTED POLICE- REPORTED	YES YES

Table 2: VARIATION IN THE USE OF FAST-TRACK PROCEDURE ACROSS DISTRICTS SHARE OF FAST-TRACK PROSECUTIONS IN 2002 (%)

OFFENSE TYPE	MEAN	S.D.	5TH PERCENTILE	95TH PERCENTILE	CRIME RATE
AGGREGATED ASSAULT	20	17	0	57	27
TRESPASS	24	15	4	53	34
VIOLENCE AGAINST PUBLIC OFFICIALS	14	19	0	56	12
BURGLARY	9	6	1	20	704
THEFT	21	9	7	39	1600
ILLEGAL BANKING CARD POSSESSION	17	21	0	60	23
OTHER PROPERTY	19	15	0	45	96

EMBEZZLEMENT	6	7	0	21	78
OBSTRUCTION OF AN OFFICIAL ORDER	55	16	27	77	81
DRIVING UNDER INFLUENCE	17	22	0	62	7
VANDALISM AND PUBLIC DISORDER	19	14	0	43	54
NEGLIGENT ACCIDENTS AND INJURIES	1	5	0	6	79
MISCELLANEOUS	7	7	0	20	60

Table 3: SHARE OF FAST-TRACK PROSECUTIONS IN 2008 (%)

OFFENSE TYPE	MEAN	S.D.	5TH PERCENTILE	95TH PERCENTILE	CRIME RATE
AGGREG ASSAULT	33	24	0	71	17
TRESPASS	40	21	10	78	24
VIOLENCE AGAINST PUBLIC OFFICIALS	15	19	0	43	9
BURGLARY	15	10	3	35	510
THEFT	34	11	15	52	1410
ILLEGAL BANKING CARD POSSESSION	17	20	0	50	75
OTHER PROPERTY	28	16	0	51	122
EMBEZZLEMENT	11	9	0	30	44
OBSTRUCTION OF AN OFFICIAL ORDER	54	26	8	93	51
DRIVING UNDER INFLUENCE	81	15	38	96	110
VANDALISM AND PUBLIC DISORDER	30	18	6	60	67
NEGLIGENT ACCIDENTS AND INJURIES	18	19	0	53	107
MISCELLANEOUS	7	10	0	25	42

Table 4: IV (DURATION INSTRUMENTED BY THE SHARE OF FAST-TRACK CASES), COVERED VICTIM-REPORTED CRIMES

	(1) AGGREG ASSAULT	(2) TRESPASS	(3) BURGLARY	(4) THEFT	(5) OTHER PROPERTY	(6) EMBEZZL.	(7) NEGLIGENT ACCIDENTS	(8) MISC.
IV: 2ND STAGE								
LOG DURATION	0.00784 (0.131)	-0.143 (0.176)	0.0432 (0.0752)	-0.0654 (0.0573)	-0.176 (0.122)	0.206 (0.309)	-0.00343 (0.0886)	0.0697 (0.141)
LOG CLEARANCE (LAGGED)	0.0868 (0.180)	-0.0725 (0.0967)	-0.158*** (0.0424)	-0.224** (0.0914)	-0.0712* (0.0407)	0.168 (0.126)	0.146 (0.154)	-0.157* (0.0841)
OBS	649	697	697	700	698	700	314	700
R-SQUARED	0.193	0.138	0.553	0.313	0.532	0.560	0.522	0.380
IV: 1ST STAGE								
FAST-TRACK SHARE	-1.045*** (0.191)	-0.782*** (0.130)	-0.829*** (0.171)	-1.331*** (0.251)	-0.859*** (0.177)	-0.537*** (0.156)	-0.693*** (0.212)	-1.045*** (0.128)
PARTIAL R2	0.0976	0.118	0.102	0.244	0.0735	0.0342	0.0664	0.131
F-TEST	30.14	36.33	23.61	28.28	23.71	11.87	10.70	66.80
HAUSMAN χ^2	0.00272	-2.385	0.0000	-5.749	1.212	-1.674	0.0002	1.932
OLS:								
LOG DURATION	0.0359 (0.0319)	0.0252 (0.0611)	0.0438* (0.0237)	0.0563** (0.0234)	0.0264 (0.0299)	-0.198*** (0.0686)	0.000109 (0.0243)	-0.0999** (0.0464)

THE DEPENDANT VARIABLE IS THE LOGARITHM OF THE CRIME RATE. ROBUST STANDARD ERRORS IN PARENTHESES.

REGRESSIONS INCLUDE SOCIO-ECONOMIC CONTROLS, YEAR AND DISTRICT DUMMIES

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

Table 5: IV (DURATION INSTRUMENTED BY THE SHARE OF FAST-TRACK CASES), COVERED POLICE-REPORTED CRIMES

	(1) VIOLENCE AGAINST PUBLIC OFFICIALS	(2) BANKING CARD POSSESSION	(3) OBSTRUCTION OF OFFICIAL ORDER	(4) VANDALISM	(5) DRIVING UNDER INFLUENCE
IV: 2ND STAGE					
LOG DURATION	-0.454 (0.429)	0.0934 (0.178)	-0.243** (0.106)	-0.219 (0.185)	-0.956** (0.390)
LOG CLEARANCE (LAGGED)	-0.0370 (0.0767)	-0.189*** (0.0471)	-0.739 (0.740)	-0.106 (0.0867)	-0.318 (0.277)
OBS	652	548	700	696	655
R-SQUARED	-0.117	0.781	0.488	0.290	0.835
IV: 1ST STAGE					
FAST-TRACK SHARE	-0.362** (0.152)	-0.733*** (0.148)	-1.607*** (0.260)	-0.958*** (0.140)	-0.638*** (0.0783)
PARTIAL R2	0.0163	0.0715	0.303	0.131	0.121
F -TEST	5.689	24.66	38.15	46.82	75.11
HAUSMAN χ^2	5.605	0.0549	-0.0621	0.0638	7.541
OLS:					
LOG DURATION	0.00473 (0.0438)	0.0188 (0.0487)	-0.179*** (0.0461)	-0.0931 (0.0617)	-0.188 (0.121)

THE DEPENDANT VARIABLE IS THE LOGARITHM OF THE CRIME RATE. ROBUST STANDARD ERRORS IN PARENTHESES.

REGRESSIONS INCLUDE SOCIO-ECONOMIC CONTROLS, YEAR AND DISTRICT DUMMIES.

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

Table 5: Difference-in-differences (reduced-form), covered victim-reported crimes

log (crime rate)	(1) aggravated assault	(2) trespass	(3) burglary	(4) theft	(5) other property	(6) embezzlement	(7) negligent accidents	(8) miscellaneous
share of fast-track prosecutions	-0.0665 (0.123)	-0.180 (0.114)	-0.320* (0.184)	-0.109 (0.0886)	0.0277 (0.0710)	-0.692** (0.276)	0.121 (0.162)	0.0202 (0.225)
lagged clearancerate(log)	0.246 (0.210)	-0.0656 (0.100)	-0.153*** (0.0435)	-0.221** (0.0908)	-0.0575 (0.0391)	0.183 (0.118)	0.104 (0.132)	-0.171* (0.0904)
observations	708	711	711	711	711	711	711	711
R-squared	0.717	0.661	0.914	0.959	0.922	0.749	0.696	0.750
district fe	yes	yes	yes	yes	yes	yes	yes	yes
year fe	yes	yes	yes	yes	yes	yes	yes	yes

Robust standard errors in parentheses. Regressions include socio-economic control variables.

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

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