

Equal Justice Under Law?

Local Justice Efficiency and Crime Deterrence

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Abstract

Efficient justice, by influencing the *certainty* of punishment, is regarded as crucial for deterring crimes. This paper assesses the impact of a reform of the criminal justice system implemented in Italy in 2012, significantly reshaping the geography of first-instance courts in the country through court mergers. We evaluate the reform's effects on justice efficiency and crime deterrence. Event study and difference-in-differences estimates reveal that the efficiency of criminal courts improved significantly as a result of the reform. This contributed to deterring property crimes and organised crimes, while violent crimes were not affected. These results support the idea that the deterrence effect of justice efficiency applies particularly to 'rational' crimes, while criminals acting under impulsive and less rational circumstances do not internalise information about justice in their decision-making.

Keywords: crime, justice, justice efficiency, deterrence, Italy.

JEL Codes: K14, K42, P43, Z18

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1 Introduction

Recent advancements in the theory of crime deterrence suggest that criminals value the likelihood of being sentenced more than the severity of the sentence (Chalfin & McCrary, 2017). This proposition finds support in a wealth of evidence revealing the limited deterrent effect of severe sanctions (Bell et al., 2014; Mueller-Smith & Schnepel, 2021; Mungan, 2021), yet direct empirical validation of the deterrent effect of certain punishment remains limited (Nagin et al., 2018). This paper contributes to address this gap by examining the impact of a significant reform within Italy's criminal justice system in 2012, which restructured the geographical distribution of first-instance courts, on justice efficiency and crime incidence in the country.

The likelihood of being sentenced crucially depends on the efficiency of the justice system. If criminals weigh the likelihood of punishment when deciding whether to commit a crime (Beccaria, 1764; Bentham, 1789; Becker, 1968), then the assurance of swift and effective justice resulting from enhanced efficiency is expected to reduce the propensity to commit crimes (Chalfin & McCrary, 2017).

Empirical evidence on this matter is scarce, mixed, and predominantly focused on procedural reforms in Latin America aiming to align with criminal procedures typical of Global North countries, such as those in Europe (Dalla Pellegrina, 2008; Zorro Medina et al., 2016; Hernández, 2019; Cattaneo et al., 2022). However, the specific needs of justice efficiency in European countries, addressed through dedicated policy interventions (European Commission for the Efficiency of Justice, 2013), and their effects on crime deterrence remain largely unexplored. This lack of evidence matters globally, given that the transformations of the justice systems in the Global North often inspire reforms worldwide.

In recent decades, numerous countries have pursued justice efficiency through court mergers, under the assumption that this process would foster economies of scale, specialisation, and cost reduction (European Commission for the Efficiency of Justice, 2013). Countries including Denmark, France, Italy, the Netherlands, Sweden, and the UK merged courts that served smaller areas and populations into larger courts (European Commission for the Efficiency of Justice, 2013; Simson Caird, 2016; Agrell et al., 2020). Despite the scale of these reforms, which have reshaped the geographical landscape of justice, the impacts of court mergers on efficiency and crime deterrence remain largely unexplored. This study aims to address this gap through an examination of the Italian case.

The so-called 'Severino reform' (from the name of the then-Minister of Justice), implemented in 2012, aimed to address longstanding weaknesses within the Italian judicial system (European Commission, 2018). This reform significantly altered the distribution of Lower Courts¹, whose spatial arrangement had remained largely unchanged since the 1860s. The reform 'rationalised' the number of Lower Courts by incorporating 26 courts and their territory of competence within 23 preexisting courts and their territory, reducing their number by 15.75%. Notably, the reform was not part of any political agenda, campaign, or discussion, and its design and implementation were particularly swift. This occurred because the reform was pushed forward by the technical government appointed in 2011 to address the sovereign debt crisis.

The spatial reorganisation of the Lower Court selected the ones to be incorporated based on size and population. Smaller courts were absorbed into the nearest larger court, expanding the jurisdiction of the incorporating courts. Our identification strategy leverages the quasi-experimental nature of this territorial reorganisation to assess whether the re-

¹Also referred to as First-Instance Courts or Ordinary Courts (*Uffici Giudiziari e Tribunali Ordinari*).

form contributed to a reduction in crime by increasing efficiency in reorganised courts.

Rationalising lower courts to serve larger areas, as enacted by the 2012 Italian reform, may yield two opposite effects on justice efficiency and crime deterrence. On the one hand, it may signal a commitment to high-quality justice and improve the efficiency of the justice system by capitalising to synergies, knowledge spillovers, and economies of scale resulting from increased judicial capacity in the same court (Voigt, 2016). On the other hand, larger courts may reduce efficiency due to higher coordination costs among larger personnel, and the closure of courts in certain areas may be perceived as reduced institutional commitment to safety and justice (Peyrache & Zago, 2016). Which effect prevails is a matter of empirical investigation.

This paper exploits the Italian justice reform to test whether court mergers have affected crime incidence and whether any efficiency-related channels were activated by the reform. First, we evaluate whether the reform influenced crime deterrence. We estimate event study and difference-in-differences models comparing crime incidence in court districts reshaped by the reform and court districts untouched by the reform. Drawing on the literature on crime deterrence, we consider three broad categories of crime: property, organised, and violent crimes. By doing so, we account for acknowledged differences in the opportunity-cost evaluation by violent and nonviolent offenders (Raskolnikov, 2020).

Second, we estimate the effect of the reform on criminal justice efficiency, comparing the efficiency of reshaped and unchanged courts, before and after the reform. Justice efficiency is computed using the 'clearance rate', the ratio of terminated cases to incoming cases (Cook, 1979), which is the established metric used by international and national institutions (European Commission for the Efficiency of Justice, 2013). Third, we verify if the changes in justice efficiency determined by the reform explain the variations in criminal activity.

Our analysis reveals that the court reorganisation imposed by the reform significantly reduced the incidence of property and organised crime in treated courts, while violent crimes remained unaffected. Additionally, we find that the reform significantly increased the clearance rate of treated courts. We demonstrate that these outcomes are not driven by self-selection among treated courts based on predetermined crime and justice characteristics and that increased court efficiency in treated courts does not correlate with low-quality sentencing quality. Lastly, we show that the efficiency gains stemming from the reform underlie the decrease in property and organised crimes. These findings suggest that by enhancing justice efficiency, the reform exerted a lasting deterrent effect on non-violent crimes, supporting rational choice theories of crime (Loughran et al., 2016) and corroborating the importance of certainty of punishment for crime deterrence (Chalfin & McCrary, 2017).

Moreover, we demonstrate that these results are not influenced by alternative mechanisms, such as the local attitude toward rule-abiding, judges turnover, the local spending on safety and justice, and changes in the propensity to report crimes.

Our crime measure is derived from reported crimes, and we account for underreporting bias by separately estimating deterrence effects for different types of crimes and including crimes with negligible underreporting rates.

To the best of our knowledge, this study is the first to assess the deterrent effect of efficient justice using counterfactual analysis, evaluating a reform reshaping the court geography of an entire country, and accounting for local heterogeneity in crime patterns. This paper extends beyond existing research, which typically adopts countries or regions as units of observation (Bun et al., 2020; Mocan et al., 2020), by conducting the analysis *within* regions and employing an empirical strategy that enables observation of the dynamic responses

of justice efficiency and crime rates over time.

This study contributes to the economic literature on crime deterrence by demonstrating that court mergers aimed at enhancing efficiency have a deterrent effect on crime. This evidence aligns with recent theoretical advancements postulating that the certainty of punishment is more important than its severity for deterrence (Chalfin & McCrary, 2017; Nagin et al., 2018). The paper also contributes to the literature on justice efficiency and court size, by adding causal evidence on criminal courts to the existing evidence on civil courts (Peyrache & Zago, 2016; Giacomelli & Menon, 2017; Ciapanna et al., 2022). Finally, the findings also add to the literature on the optimal scale of the jurisdictions of local institutions (Dalla Pellegrina, 2008; Blesse & Baskaran, 2016; Blom-Hansen et al., 2016; Di Cataldo et al., 2023), showing that merging courts was beneficial in the Italian case.

The rest of the manuscript is structured as follows. Section 2 is devoted to literature review. Section 3 describes the institutional setting and Lower-Court merger (3.1), presents the identification strategy (3.2) and details relevant characteristics of the Italian judicial system that we need to consider (3.3). Section 4 describes the data and justice efficiency measure. Section 5 presents the empirical strategy. The results are exposed in section 6, while section 7 provides some robustness checks and Section 8 concludes.

2 Crime deterrence and justice efficiency

According to the Rational Theory of Crime (or Rational Choice theory) proposed by Becker (1968), individuals make rational choices when deciding whether to engage in criminal activities. By weighting expected benefits against potential costs, which include the *certainty* and the *severity* of punishment, individuals determine whether the net utility of

committing a crime is positive or negative (Grogger, 1991; Winter, 2019). Testing this theory, Chalfin and McCrary (2017) illustrate that crime is deterred by punishment that is *certain*, rather than *severe*. Clearly, in this framework, an effective criminal justice system increases the expected cost of committing crime by making punishment more certain, hence triggering a deterrence effect.² Courts with (in)efficient case management have a (large) small backlog of pending cases relative to terminated cases, and this (decreases) increases the likelihood of cases resolution or good case management (Engel & Weinshall, 2020; Shumway & Wilson, 2022). Durkheim (1893) stresses that the cost of committing crimes increases also due to the signalling effect of efficient justice, which deters crime by spreading the information of societal commitment to law enforcement.

Albeit its crucial importance in crime theory, the empirical attention devoted to the deterrence effect of justice efficiency is little and with limited geographical scope. Recent research exploit the Latin America transition to the U.S. model of criminal procedure to estimate the impact of this reform on crime (Zorro Medina et al., 2016; Hernández, 2019; Cattaneo et al., 2022). The resulting evidence is mixed, with the reform deterring crime in Peru (Hernández, 2019) but having the opposite effect in Colombia (Zorro Medina et al., 2016) and in the city of Montevideo, Uruguay (Cattaneo et al., 2022).

Although this approach is valuable for understanding the overall impact of a specific procedural reform on crime, it fails to identify the specific mechanism behind the results, because it considers a policy that altered both the *certainty* and *severity* of punishment (Cattaneo et al., 2022). This point is emphasised by recent studies on crime deterrence, which recommend focusing on measures of certainty of punishment (like justice efficiency) to

²The deterrence effect of the certainty of punishment enabled by efficient justice dates back to seminal work in different disciplines, including works by Beccaria (1764) and Bentham (1789). The prevailing role of certainty over severity of punishment was first empirically investigated by Grogger (1991).

overcome the ambiguity inherent to approaches which confound certainty with severity (Chalfin & McCrary, 2017; Nagin et al., 2018). Furthermore, the evidence resulting from the Latin America procedural reforms cannot be generalised to the Global North, where this kind of reform was first introduced a long time ago.

Evidence on the effect of certain punishment on crime deterrence in Global North countries remains limited, and this could be due to the fact that research on this part of the world has mainly focused on the severity of punishment (Lofstrom & Raphael, 2016a; Ciacci & Sansone, 2023; Doleac, 2023).³ This evidence gap is relevant and needs to be addressed, to empirically assess the prominent role of the certainty of punishment in the rational theory of Crime (Chalfin & McCrary, 2017; Nagin et al., 2018), and to go beyond the contrasting findings on the deterrence effect of the severity of punishment.⁴

Among the few existing studies on the certainty of punishment, some authors consider delays in trials and appeals (Dalla Pellegrina, 2008), or procedural reforms, like fast-track procedures for misdemeanours in the Czech Republic (Dušek, 2015) and the exclusion from trial of any evidence obtained in violation of the Fourth Amendment in the U.S. (Atkins & Rubin, 2003). In all cases, the evidence shows that simpler and faster trial management reduces crime rates. In addition, the reforms considered in these studies have changed the choice of crimes to be prosecuted by the judicial system, rather than the actual crimes being committed - in the aftermath of reforms, police forces tend to focus on crimes that have become less expensive to investigate. Among other studies, Mocan et al.

³Other approaches have focused on the deterrence effect of police forces (Chalfin & McCrary, 2017), individual experience of offending (Bazzi et al., 2021), family background (Eriksson et al., 2016), local labour market conditions (Chalfin & McCrary, 2017; Doleac, 2023) and natural lighting (Tealde, 2022).

⁴Some empirical work supports severe punishment as effective deterrent (Dezhbakhsh & Rubin, 2011; Bell et al., 2014). Other work finds higher deterrence associated with less severe punishments like electronic monitoring (Di Tella & Schargrodsky, 2013; Henneguelle et al., 2016), probation enforcement (Nagin et al., 2018), difference sentencing depending on repeated offending (Helland & Tabarrok, 2007), monetary rewards for non-conviction (Mungan, 2021), diversion (Mueller-Smith & Schnepel, 2021).

(2020) estimate the relationship between people's propensity to commit crime and justice efficiency for 25 European countries through an instrumental variable strategy that exploits survey data to measure the propensity to commit crime. Focusing on the Australian state of New South Wales, Bun et al. (2020) show that better justice is related to a reduced willingness to commit crimes. Whilst demonstrating that effective justice might be relevant for crime deterrence, this body of work mainly adopts spatial macro-units (e.g., European countries) or focuses on specific regions within a country (Australian New South Wales), or it analyses short time spans with no evidence on the dynamic evolution of treatment effects.⁵ Furthermore, endogeneity issues are often not addressed, or identified through strategies that prevent sub-national analysis.

Over and above, there is no empirical examination to date on the impact of court amalgamation on crime deterrence. This is in spite of the widespread adoption of court amalgamation and the extensive debates surrounding this type of reform, in light of the tensions and turmoils produced by the closure of many local courts (European Commission for the Efficiency of Justice, 2013; Simson Caird, 2016; Agrell et al., 2020).

To fill this evidence gap, we assess the impact of justice efficiency on crime by exploiting the Italian reform imposing the merging of different courts. As such, we also contribute to the literature studying whether court mergers affect justice efficiency, whose evidence is mixed. Some authors claim that justice efficiency grows with the size of courts, because larger courts favour the specialisation of judges and economies of scale (Chappe & Obidzinski, 2014).⁶ In this view, larger courts allow judges to specialise in specific fields of criminal law, hence inducing better case management and higher clearance rate (The Eu-

⁵An exception is Hernández (2019), demonstrating that the introduction of a new penal code in Peru produces only temporary reductions in the crime rate of the country.

⁶Evidence on the positive impact of judge specialisation on justice efficiency is provided by Coviello et al. (2019).

ropean Commission for the Efficiency of Justice, 2012), which in turn is related to the certainty of punishment (Engel & Weinsahl, 2020). An opposing view regards larger courts resulting from the incorporation of smaller courts as potential harm to deterrence. Citizens residing in the territory of competence of smaller courts merged into larger ones may perceive the court merger as a reduced commitment toward accessibility to justice by institutions. In turn, this would reduce their willingness to report crimes (Chappe & Obidzinski, 2014). Also, larger courts might be inefficient due to bureaucratic and case congestion, as well as to high communication, control, and coordination costs (Peyrache & Zago, 2016). Hence, the prevailing effect of court mergers on judicial efficiency remains unclear, as descriptive evidence reports mixed results and causal identifications are almost nonexistent (Espinosa et al., 2017; Castro & Guccio, 2018; Agrell et al., 2020; Arcuri et al., 2023).

Any empirical investigation of the Rational Choice theory must consider that justice deterrence varies depending on the type of committed crime (Lofstrom & Raphael, 2016b; Mocan et al., 2020). Criminals charged with violent crimes (e.g. murders, arson, sexual violence, etc) do not respond to changes in sanction laws, while criminals charged with property crimes (e.g. theft, dealing with stolen goods) do.⁷ This suggests that changes in justice are likely to influence more the most 'rational' crimes, because criminals who commit these offences are more likely to internalise information about justice in their decision-making, rather than criminals acting under less rational circumstances (Loughran et al., 2016). This is why, in assessing the effect of changes in justice efficiency on crime rates, we sub-divide crimes into three categories: property crimes, organised crimes, and violent crimes. We assume that the latter category is characterised by a more irrational drive.

⁷This aligns with other evidence showing that violent offenders do not respond to sanctions according to rational behaviour, while non-violent offenders are more likely to do so (Chalfin & McCrary, 2017; Raskolnikov, 2020).

Our classification of crime categories draws on law and criminology research in criminal deterrence (Raskolnikov, 2020), and it aligns with previous empirical work (Hernández, 2019; Bun et al., 2020; Cattaneo et al., 2022).

3 Judicial reform in Italy and identification strategy

Relative to other European countries, Italy has been characterised by a long period of low efficiency in its justice system. This weakness was often speculated to hinder Italy's growth (Esposito et al., 2014; Giacomelli & Menon, 2017; Ciapanna et al., 2022), with European institutions specifically pointing to the high and fragmented number of Italian Lower Courts as a leading source of inefficacy.

Italian Lower Courts are spatially bounded judicial units that comprise Lower Ordinary Court (i.e., *Tribunali Ordinari*), related to Prosecutor's Offices and Judges for Preliminary Investigations. In 2011, following recommendations from European institutions (European Network of Councils for the Judiciary, 2012), the Italian Government started the legislative process to revise Lower Court geography, which was then implemented through an institutional reform by decree 155 in 2012.⁸

3.1 Law 148/2011 and decree 155/2012

The Italian judicial geography, which had remained substantially unchanged since the 1860s, underwent a radical transformation of the spatial distribution of Lower Courts with

⁸*Riduzione degli uffici giudiziari ordinari*: the reform also included decree 156/2012 which reorganised minor courts dedicated to misdemeanours and ruled by honorary (not professional) judges (*Giudici di Pace*). This paper focuses on felonies, hence it considers only the reform of criminal courts managed by professional judges addressed by decree 155/2012. [Online text](#).

Law 148, approved in 2011, which became operational with decree 155 approved by a technocratic government in 2012.⁹ During the 2011-2012 sovereign debt crisis, Italian lawmakers believed that 'rationalising' the justice system by relocating personnel from courts that serve small areas to bigger courts would enhance the effectiveness of judicial activities, reduce cost, and reassure investors and markets (Italian Government, 2012).

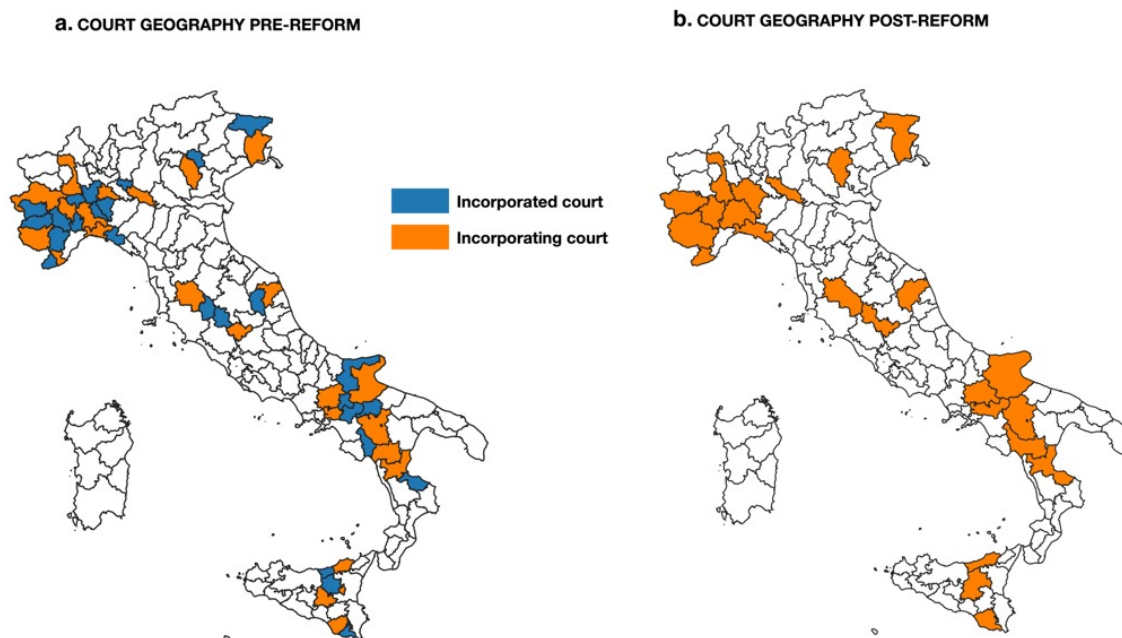
Following this rationale, decree 155/2012 ended 15.75% of Lower Courts. Before the reform, there were 165 Lower Courts. The reform suppressed by incorporation 26 Lower Courts by incorporating them into 23 preexisting Lower Courts. The 26 Lower Courts to be suppressed by incorporation were selected on the basis of objective and homogeneous criteria (Chamber of Deputies, 2012). First, incorporated Lower Courts needed not to be located in province capitals. Second, within each Court of Appeal district, Lower Courts were eligible for incorporation until no less than three Lower Courts remained in the Court of Appeal district. Third, they had to score under geographic threshold values for district population and territorial extension, whereby thresholds were determined by averaging population and size of the 103 provincial capital Lower Courts during 2006-2010.¹⁰ All Lower Courts below threshold values were incorporated into the closest and larger Lower Court.

Out of a total of 26 incorporated Lower Courts, 24 were incorporated into the Lower Court of their provincial capital. The remaining 2 Lower Courts were incorporated into sub-provincial Lower Courts because these were closer to them than the provincial capital Lower Court. Figure 1a illustrates the area of competence of Lower Courts pre-reform in blue, while the new area of competence of incorporating Lower Courts post-reform is

⁹The final decree 14/2014, approved at the beginning of 2014, assessed the operationalisation of the reform and confirmed the geography of Lower Courts drafted by Decree 155/2012 (Italian Ministry of Justice, 2014).

¹⁰Averages did not include Rome, Milan, Naples, Turin, and Palermo since these provinces have remarkably larger populations compared to the others.

Figure 1: Incorporated and incorporating Lower Courts boundaries before and after the reform



reported in Figure 1b (in orange). The full list of incorporating and incorporated courts is listed in Table A2.

Implemented in 2012, the reform automatically reallocated judges, prosecutors, and administrative staff of incorporated Lower Courts to the larger incorporating Lower Courts. To ensure the continuity of pending criminal trials and to avoid renewals of proceedings due to a different composition of the judging body, decree 155/2012 entrusted the incorporation of Lower Courts to ensure the continuation of the trial before the same judges who were assigned to them in the incorporated Lower Courts (Chamber of Deputies, 2012). New hearings were held at the merged Lower Courts. At the same time, the new geography of criminal justice allowed a 12-month buffer period to dispose of dismissed buildings

and/or to temporarily use old facilities, pending the adaptation of the new ones. The total cost savings from the reform amounted to €2,889,597 in 2012, €17,337,581 in 2013, and €31,358,999 in 2014 (Italian Government, 2012). These cost reductions were made from savings in the management and operating costs of the structures (Severino, 2013). The cost of personnel of the judicial administration was incompressible, given that positions are permanent (Italian Government, 2012). Differently from reforms pursuing merging between other institutions (Blesse & Baskaran, 2016), no incentives were assigned to incorporating courts.

Following this major streamlining, the geographic boundaries of Lower Courts that were not incorporated were assessed to check their compliance with the extension of the court territory and population size, as set by decree 155/2012. From this assessment, most Lower Courts remained completely untouched, while a small number went through extremely marginal boundary changes, at the same time keeping the same staffing plans they had before the reform (CSM, 2013). Given the peripherality of these boundary changes, in our main specification we will consider all Lower Courts that were not involved in merging as unreformed Lower Courts (Panel I in Table A1 summarizes Lower Courts key figures).¹¹

The approval of decree 155/2012 initiated the transfer of personnel from incorporated to incorporating courts. Yet, some courts to be incorporated enacted precautionary suspensions of the trade union agreements on the early mobility of workers, to slow down personnel relocation (Tribunale Sulmona, 2012). Other actions to stop the reform were taken, including court appeals to restore the extant court geography by some incorporated courts, municipalities, and bar associations (Stasio, 2012; Corte Costituzionale, 2013, 2014a). Also,

¹¹80.18% of Lower Courts (94 Lower Courts) that were not incorporated remained completely untouched (Chamber of Deputies, 2012), while the remaining 19.81% (22 Lower Courts) went through extremely marginal changes (CSM, 2013).

when the effects of the reform became manifest to the public, some local communities engaged in boycotts (protests, strikes, occupations of buildings, blockades) to stop moves of materials between courts (Mariozzi, 2012; Rotunno, 2012). A referendum proposal was then promoted by 45% of regional governments, months after the implementation of the reform (Corte Costituzionale, 2014b). The referendum and the appeals were rejected by the end of 2014, extinguishing all feasible ways to restore the previous geography. This turmoil did not take place in all the courts involved in the reorganisation, nor did it prevent the reorganisation from taking place.

3.2 Identification strategy

Our identification strategy exploits the variation in court geography induced by the judicial reform as quasi-natural experiment. Given that the criteria for mergers are based exclusively on population and size of Lower Courts, they are orthogonal to justice efficiency.¹² Furthermore, the reform happened fast and was arguably unexpected, since it resulted from the economic and political crisis that hit Italy in 2011, triggered by the sovereign debt crisis that shook the Eurozone and culminated with a technocratic government replacing the elected one. Therefore, it is unlikely that the observed individual behaviour of both criminals and court staff was affected by previous knowledge of the reform. This also alleviates concerns about bias due to population sorting.

The reform divided Lower Courts into three categories: incorporated, incorporating, and untouched. Both incorporated and incorporating Lower Courts were reformed, and hence they belong to treated courts in our setting. Incorporated Lower Courts ceased to exist in

¹²Considering merging reforms based on population size and territorial extension as a source of exogenous variation has already been done in the literature, both for municipal reforms (Blom-Hansen et al., 2016) and for school reforms (De Haan et al., 2016).

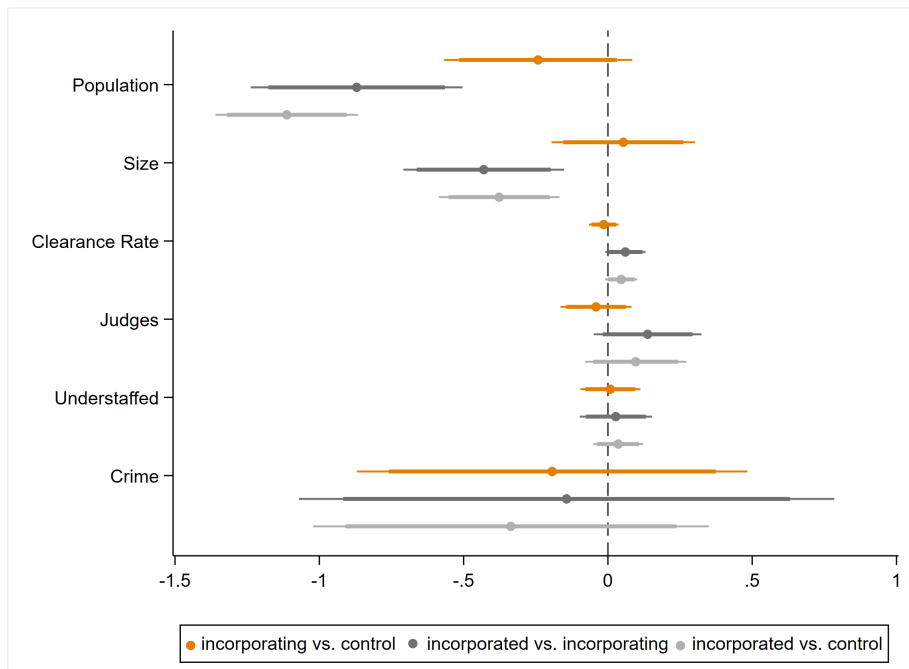
2012 as independent bodies since they were spatially and organisationally aggregated to incorporating Lower Courts. At the same time, incorporating Lower Courts changed too in 2012, as they were absorbing spatial coverage and personnel.

The streamlining of Lower Courts detailed in decree 155/2012 followed the work of several actors. A dedicated study group commissioned by the Ministry of Justice after Law 148/2011 drafted preliminary guidelines which were updated through Ministerial and Parliamentary activities.¹³ While the approved reform determined criteria for court mergers exclusively based on population and size of Lower Courts, preliminary drafts of the reform had conceived additional criteria, including judges per inhabitant and demand for justice. Therefore, it appears informative to measure what characteristics of Lower Court district, if any, would predict incorporation. We account for this through a balancing test, verifying the statistical difference across a number of relevant characteristics between three sets of Court pairs: incorporating vs. untouched (control), incorporated vs. incorporating, and incorporated vs. untouched. We look at population, size, clearance rate, judges per capita, understaffed courts, and crime rate. All these indicators are averaged for 2006-2011, i.e. the pre-reform period.

The results are illustrated in Figure 2. The only factors emerging as statistically different between incorporated Lower Courts and incorporating or untouched (control) Courts are population and territorial extension, while all other characteristics including justice efficiency (measured through the clearance rate) are not statistically significant. Incorporated Courts appear smaller in size and with fewer inhabitants in their jurisdiction as compared to incorporating and control Courts. This confirms that the reform has indeed selected

¹³The dedicated study group commissioned by the Ministry of Justice after Law 148/2011 drafted preliminary guidelines which were amended and updated through Parliamentary and Governmental activities (Chamber of Deputies, 2012; Italian Ministry of Justice, 2012).

Figure 2: Balancing test on pre-reform characteristics



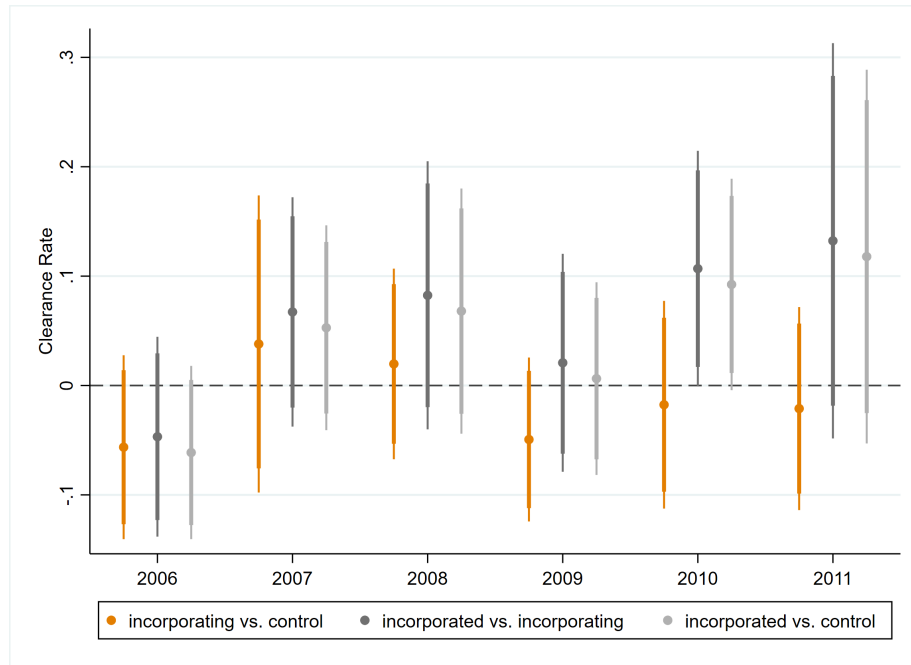
Note: Balancing test estimating the difference between incorporating, control (untouched), and incorporated courts. For each couple of court categories, it estimates a basic regression comparing a set of covariates measured in the pre-treatment (2006-2011) period. Population: log inhabitants; Size: log squared metres of land; clearance rate: indicator of court effectiveness; Judges: judges and Prosecutors per 1000 inhabitants; Understaffed: court classified as understaffed (1/0); Crime: crime rate (pre-treatment crime data at municipal level provided by ISTAT & Min. Justice). Thicker confidence intervals refer to 90% level, thinner ones to 95%. Dark orange CIs: treated vs. control courts comparison; dark grey CIs: incorporated vs. treated courts; light grey CIs: incorporated vs. control courts.

Courts to be incorporated exclusively on the basis of their size/population, while other factors such as crime rate or efficiency of the judges did not play a role.

To further investigate the evolution of judges' efficiency in the three categories of Courts in the pre-reform period, we perform a statistical test of difference in mean clearance rate by each pre-reform year. As shown in Figure 3, the pre-reform efficiency of the three categories of Courts is comparable. There is no evidence that Lower Courts were selected for incorporation due to their scarce efficiency. We also find no evidence of significant differ-

ence in the share of cases dismissed due to statutes of limitations, another feature which could influence Court performance (see Figure A1 in the Appendix).

Figure 3: Pre-reform clearance rate by year



Note: Estimated difference in clearance rate by pre-reform year between incorporating, control, and incorporated courts. For each couple of court categories, we estimated a basic regression comparing the value of clearance rate in each pre-treatment year (2006-2011). Thicker confidence intervals refer to 90% level, thinner ones to 95%. Dark orange CIs: treated vs. control courts comparison; dark grey CIs: incorporated vs. treated courts; light grey CIs: incorporated vs. control courts.

A further set of criteria was introduced in the reform relating to the specificity of the catchment area, by measuring the presence of organised crime¹⁴ and transportation infrastructures. Lower Courts that were below population and territory thresholds but were characterized by a relevant presence of organised crimes and/or lack of adequate transport in-

¹⁴The presence of organised crime was quantified using the number of court debates which concerned organised crime in the previous five years and information from the District Anti-Mafia Prosecutor's Office (Italian Government, 2012).

frastructures connecting to other Lower Courts were spared from incorporation.¹⁵ Among robustness checks, we assess the influence (if any) of these spared courts on estimates.

Given that neither incorporating nor incorporated Courts maintain the same shape across the full pre/post-reform period (the former expand, the latter disappear), in the empirical analysis we use post-merger Courts as treated units of observation. We label them ‘synthetic Lower Courts’, constructed as the aggregation of each incorporated-incorporating Lower Court pair until 2012 (reform year), and by the actual Lower Court resulting from the incorporation afterwards. In other words, we use as treated units the coloured Courts in Figure 1b, observing them before and after 2012, even if these jurisdictions only existed after 2012. This allows to compare trajectories of treated (‘synthetic’) Lower Courts to control Lower Courts. Similar approaches have been used to measure the effect of similar institutional mergers (Blesse & Baskaran, 2016; Agrell et al., 2020).

3.3 Court understaffing

In dealing with justice efficiency in Italy, available personnel is another key issue that needs to be considered (Voigt, 2016; Yang, 2016; Shumway & Wilson, 2022). While Italian criminal courts did not experience understaffing in 2005-2007, an opposite trend began in 2008, following the implementation of Law 111/2007. This law forbade the assignment of magistrates at the end of their traineeship to perform the duties of prosecutors, monocratic judges in criminal cases, judges of preliminary investigations, or judges of preliminary hearings.¹⁶ The effect of Law 111/2007 on Lower Court understaffing was evident. One

¹⁵These Lower Courts are Caltagirone and Sciacca (Sicily), Castrovillari, Lamezia Terme and Paola (Calabria) and Cassino (Lazio).

¹⁶By virtue of this provision, the filling of prosecutor and magistrate’s offices that remained vacant, due to lack of candidates, at the end of the ordinary transfer procedures can no longer be ensured by allocating young magistrates at the end of their training (as was the case before) to those offices, but only through the

year after its implementation, judicial vacant posts rose by 266%, peaking at 366% in 2009 (CSM, 2010).¹⁷ To alleviate this, the Government issued decrees 143/2008 and 193/2009 (so-called “*Sedi disagiate*” decrees) which set up a census of understaffed Lower Courts and implemented special measures to favour their staffing (Senato della Repubblica, 2008). These measures have been in place since 2008 and have involved 11.5% of Lower Courts between 2008 and 2018, with peaks of 30% in 2010, and over 20% in 2008, 2009, and 2016.

Importantly, severe understaffing was not exclusively a problem of small Lower Courts. On the contrary, understaffed Lower Courts were distributed in a similar way among all Lower Courts types identified by decree 155/2012 (incorporating Lower Courts, incorporated Lower Courts, untouched Lower Courts), as visible in Figure 2. In any case, court understaffing appears a relevant potential confounder for justice efficiency, which we will consider in our estimation.

4 Data and index of justice efficiency

4.1 Data

Our dataset has been obtained from a variety of sources.

Figures on terminated and incoming criminal cases in each Lower Court, used to compute the clearance rate of justice efficiency discussed in section 4.2, were retrieved from the Italian Ministry of Justice (Italian Ministry of Justice, 2023), which also provided data on

employment of magistrates who have accrued the minimum length of service required by law (Senato della Repubblica, 2008)

¹⁷On 31 July 2006, 86 judicial posts were vacant nationwide. After one year, these vacant judicial posts were 68. However, after one year from the implementation of law 111/2007, judicial vacant posts rose to 181, peaking at 249 in July 2009 (CSM, 2010).

the geography of Lower Courts before and after decree 155/2012. The annual number of Prosecutors and criminal judges in each Lower Court is measured through novel data that we collected reviewing official court staffing census documents from the Ministry of Justice and the Consiglio Superiore della Magistratura (CSM), the Italian magistrates' governing body (CSM, 2013; Italian Ministry of Justice, 2017). We complemented them with yearly data on understaffed Lower Courts, provided by CSM (CSM, 2023). Through this, we can measure the annual personnel availability of each court and court understaffing.

Statistics on crimes are mainly retrieved from ISTAT, and complemented with figures from the Italian Ministry of Interior to design crime measure at the Lower-Court level. ISTAT data on crime are available from 2006 onwards and measure the annual number of crimes transmitted by the police force to the criminal justice system at the province level. With this data we compute three categories of crimes, drawing on the literature on crime deterrence: property crimes, organised crime, and violent crimes (Enamorado et al., 2016; Lofstrom & Raphael, 2016a; Raskolnikov, 2020).¹⁸

Crime data are available at the municipal level between 2006 and 2009 and at the province level for 2010-2019.¹⁹ Therefore, crime data must be consolidated on the geography of Lower Courts. This is straightforward for 80 Lower Courts, since their territory of competence exactly coincides with the corresponding province. The remaining 52 Lower Courts

¹⁸We only consider crimes that did not change definition and legislation in the considered timespan (2006-2019). The following crimes had changes in legislation between 2006 and 2019 and were consequently discarded: drug, prostitution crimes, rape, menace, assault, money laundry, corruption, bribery (Law 125/2008; Law 38/2009; Law 199/2013, Law 186/2014; Law 190/2012). We also discard misdemeanours since they are minor crimes associated with lighter penalties such as community service, fines, rehabilitation, and/or probation, and they are largely administered by honorary (non-professional) judges ("*Giudice di Pace*") (EU European e-Justice, 2022). We also discard manslaughter as it has an involuntary motive, while our goal is to assess the deterrence effect of justice efficiency on the voluntary act of committing crimes.

¹⁹Unlike most of previous research (Hernández, 2019; Mocan et al., 2020), we do not rely on survey data to measure crime deterrence, avoiding measurement errors possibly induced by the fact that respondents give answers in line with what is socially acceptable rather than saying what they really think.

are located in provinces with multiple Lower Courts. In these cases, to avoid double counting, we implement two alternative approaches. In the first approach, we keep one Lower Court per province through a random selection, consolidating crime data on this court. We test the robustness of this approach by considering two alternative sets of dropped courts: we keep only the most urban court or the most peripheral court within each province. The second approach considers the average value of each court-level variable across all courts within the same province if these courts are all untreated (treated).²⁰ In both approaches, in the three provinces with both treated and untreated Lower Courts we discard the untreated Lower Courts and consolidate crime data on the remaining treated court.

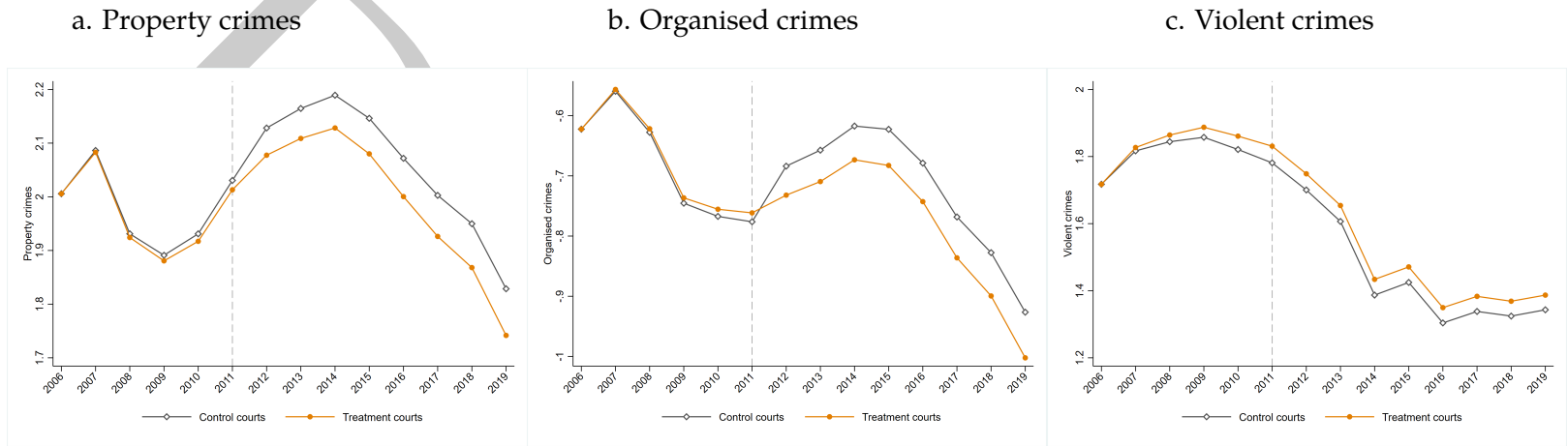
Both approaches correspond to a final estimation sample of 102 court/provinces that we use to estimate the impact of the reform on crime deterrence.²¹ 22 Lower Courts were affected by Law 155/2012 (resulting from merging) and 80 were unaffected (control courts) (Panel II in Table A1 summarizes the key figures for the Lower Courts in the estimation sample).

Figure 4 illustrates the trend in crime rates for the three categories of crime we focus on. We compare crime rates by province before and after the implementation of decree 155/2012. Provinces with courts experiencing mergers are labelled as treatment, while provinces without mergers are labelled as control. Figure 4 estimates the crime rates for treatment and control groups through a simple difference-in-differences model with linear trends and no additional control variables.

²⁰The only province that has multiple *treated* courts is Potenza, where the treated Lower Courts are Lagonegro and Potenza.

²¹We discard Sardinian courts due to a lack of microregional data on crime. All Sardinia Lower Courts were untouched by decree 155/2012, hence we do not lose any treated Lower Court.

Figure 4: Crime rates trends - treated vs. control courts



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Note: Difference-in-differences estimates with linear trends. Treatment courts: 'synthetic' merged courts; control courts: untouched courts. Dependent variables: (log) crimes per 1,000 inhabitants.

Our empirical models account for confounding elements potentially influencing crime deterrence. Lower Court-level unemployment, population density, and population are computed using statistics from ISTAT.²² Spending on safety and justice by local authorities, available from 2008 onwards, is retrieved from the Ministry of Finance. Finally, we proxy the local attitude to comply to rules by exploiting Bank of Italy data on suspicious financial transactions, following Dalla Pellegrina et al. (2020). These data is computed from annual reports from financial intermediaries, notaries public, lawyers, accountants, gold traders, gambling services, about transactions where it is known, suspected or there is reasonable grounds to suspect that money laundering is being or has been committed or attempted. These reports are mandatory, but they do not necessarily translate into police reporting and investigation.

Further details on the variables used are in Table A3 and Table A4 reports summary statistics.

4.2 Justice efficiency index

In order to measure justice efficiency we adopt the clearance rate (CR), defined as the ratio between terminated cases and incoming cases. This is the conventional indicator of justice efficiency used to evaluate court productivity (Cook, 1979; Marciano et al., 2019).²³ For Italian criminal justice, measuring efficiency through CR must account for the fact that the trial stage strongly depends on the pre-trial stage, which takes place in the same court as the trial and involves Prosecutors and Judges for the Preliminary Investigation. Criminal

²²These data are available at municipal or supra-municipal scale, allowing to design Lower-Court measures mapping these smaller scale geographies into their Lower Court.

²³Data availability prevents considering the Disposition Time, which is the other key metric for justice efficiency (European Commission for the Efficiency of Justice, 2013) and it is given by the ratio between pending cases and resolved cases.

proceedings are initiated by the Prosecutor. Clearly, for criminal courts, the Prosecutor's efficiency in keeping up with the incoming caseload influences the number of cases that are passed on to the trial stage (Voigt, 2016). The judges for the Preliminary Investigations control the actions of the Prosecutor when these activities could affect the personal rights of the suspected person. Again, the efficiency of these judges in handling cases affects the number of pending cases in the trial stage.

Hence, we measure the efficiency of the criminal justice using the following justice efficiency index:

$$JE_{Ct} = w_{Ct} \times TCR_{Ct} \quad (1)$$

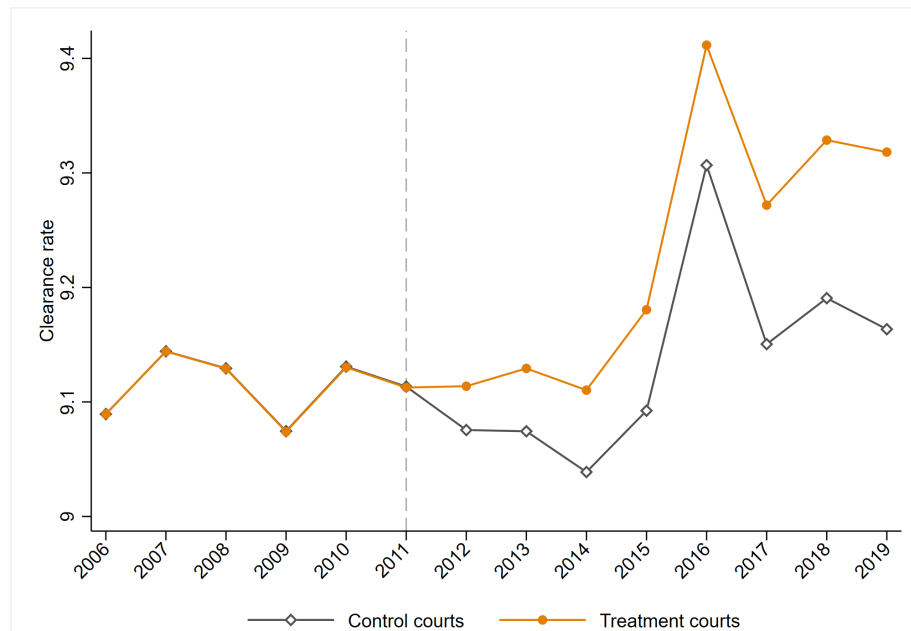
where the justice efficiency JE_{Ct} for Lower Court C at time t is equal to the criminal Trial clearance rate TCR_{Ct} , which measures the efficiency of keeping up with the incoming caseload at the trial stage in criminal Lower Court C at time t , weighted (i.e. multiplied) by the average clearance rate in the pre-trial stage, w_{Ct} . This weight is given by the average clearance rate for Prosecutors and Judges for the Preliminary Investigation in Lower Court C . Thus, JE_{Ct} accounts for both stages determining criminal Lower Court efficiency.²⁴

To measure the effect of the reform on justice efficiency, we need to compare the clearance rate of treated units with the clearance rate of control units. Treated units are 'synthetic' courts designed by aggregating incorporated-incorporating Lower Court pairs until 2012 and by the actual Lower Court resulting from incorporation afterwards. Hence, the clearance rate for treated courts until 2012 is given by the ratio between cumulative terminated

²⁴ w_{Ct} and TCR_{Ct} refer to $\frac{\text{solved}_{Ct}}{\text{incoming}_{Ct}} \times 100$, the percentage of solved over incoming cases by court C for pre-trial (w_{Ct}) and trial (TCR_{Ct}) period. Values of w_{Ct} or TCR_{Ct} above 100 indicate that more proceedings have been settled than have occurred, resulting in a reduction of the backlog. Conversely, values below 100 indicate that the number of finalised cases is less than the number of cases that have arisen and therefore there is an increase in the backlog. JE_{Ct} is the product of these two rates, and if this product is 100^2 , then $\ln JE_{Ct}$ is approximately 9.21.

cases of the incorporated Lower Court and its incorporating Lower Court and cumulative incoming cases of the incorporated Lower Court and its incorporating Lower Court.

Figure 5: Clearance rate trends - treated vs. control courts



Note: Difference-in-differences estimates with linear trends. Treatment courts: ‘synthetic’ merged courts; control courts: untouched courts. Dependent variables: (log) clearance rate.

Figure 5 outlines the trend in the clearance rate before and after the implementation of decree 155/2012, comparing the evolution of the clearance rate of synthetic Lower Courts (treated) to the clearance rate of Lower Courts untouched by the reform (control). It estimates the clearance rate for treatment and control groups through a difference-in-differences model with linear trends and no control variables. The sharp rise in the clearance rate in both treated and control Lower Courts between 2014 and 2016 is due to a reduction in new and pre-existing cases determined by the decriminalisation of several offences (insults, failure to pay social security withholdings) (ISTAT, 2020). Lower Courts dismissed all cases related to decriminalised felonies, hence reducing backlog.

While equation (1) operationalises justice efficiency considering the specific structure of the Italian criminal procedures, we will further assess the effect of the reform on efficiency by testing its specific effect on the clearance rate of the pre-trial stage (w_{Ct}) and trial stage (TCR_C). Additionally, among robustness checks, we will consider a measure for justice efficiency that weights the clearance rate by the share of criminal cases dismissed because of Statutes of Limitations (SoL) - i.e. *prescrizione*, time-based regulations that restrict legal actions - given that they can hinder judicial efficiency in various ways. First, when there is a backlog of cases, the statutes of limitations can impose additional pressure on the court, and this could worsen the management of cases. Second, the statutes of limitations may hurry the prosecution of a crime without allowing for the time needed to discover and/or present intricate or concealed evidence, damaging cases that necessitate further investigation or revelation.²⁵

5 Empirical strategy

5.1 Judicial reform and crime

First, we focus on the direct impact of the reform of Lower Courts geography on crimes. We estimate the following reduced form event study model with leads and lags dummy variables:

$$\ln Crime_{Ct} = \sum_{j=-k}^k \alpha_j D_{C,t+j} + \delta X_{Ct} + \gamma UndSta.f_{Ct} + \lambda_C + \psi_t + \epsilon_{Ct} \quad (2)$$

²⁵The duration of statutes of limitations differs based on the type of crime, ranging from 6 years to no restrictions. This prevents assessing the impact of the reform on the proportion of cases that are dismissed due to statutes of limitations, as cases dismissed for this reason after the reform was initiated prior to the reform taking place.

where $\ln Crime_{Ct}$ is the log transformation of the crime rate for a given crime category (measured as total number of crimes per 1,000 inhabitants) in Lower Court C at time t . $D_{C,t+j}$ is the lead/lag dummy variable referring to Court C in year $t + j$ before or after the reform, and α_j is the treatment effect at lead/lag j . X_{Ct} is a vector of control variables which, drawing on extant research, includes population and population density (Chalfin & McCrary, 2017), Lower Court criminal staff per 10,000 inhabitants (Peyrache & Zago, 2016) and unemployment (Doleac, 2023). This set of controls is later incremented for sensitivity checks with organised crime intensity, local authorities' spending on safety, local authorities' spending to support the judicial system and local education level (proxied by tertiary education degree holders). $UndStaff_{Ct}$ is a dummy variable taking value 1 if Lower Court C had severe understaffing in year t . λ_C are Lower Court fixed effect, and ψ_t are year fixed effect. Standard errors are clustered at the province level.

Following the literature on rational crimes, we consider three types of crime as outcomes: property crimes, organised crimes and violent crimes.

We complement equation (2) with a difference-in-differences model estimating the average treatment effect of the reform on the crime rate:

$$\ln Crime_{Ct} = \pi_0 + \pi_1 Post_t \times R_C + \pi_2 X_{Ct} + \pi_3 UndStaff_{Ct} + \chi_C + \xi_t + \epsilon_{Ct} \quad (2.1)$$

where R_C is a dummy taking value 1 if Lower Court C is treated and 0 otherwise. $Post_t \times R_C = 1$ for treated Lower Courts in the post-reform period (i.e., from 2012); $Post_t \times R_C = 0$ for untreated Lower Courts as well as for treated Lower Courts before 2012; χ_C and ξ_t are court and year fixed effects.

5.2 Judicial reform and justice efficiency

Next, to determine the effect of the judicial reform on criminal justice efficiency, we estimate the following Event Study model:

$$\ln JE_{Ct} = \sum_{j=-k}^k \beta_j D_{C,t+j} + \eta X_{Ct} + \theta UndSta_{Ct} + \mu_C + \nu_t + \varepsilon_{Ct} \quad (3)$$

where $\ln JE_{Ct}$ is the log of criminal justice efficiency in Lower Court C at time t defined as described in equation (1). $D_{C,t+j}$ is the dummy lead/lag indicator for Court C at time $t+j$ before or after the reform. and β_j is the treatment effect at lead/lag j . X_{Ct} is a vector of control variables, the same ones of equation (2). We again control for Court understaffing with the dummy $UndSta_{Ct}$. μ_C and ν_t are Lower Court and year fixed effect, respectively. Standard errors are clustered at the Lower Court level.

Again, we complement model (3) with a traditional difference-in-differences model:

$$\ln JE_{Ct} = \rho_0 + \rho_1 Post_t \times RC + \rho_2 X_{Ct} + \rho_3 UndSta_{Ct} + \kappa_C + \xi_t + u_{Ct} \quad (3.1)$$

where ρ_1 captures the average treatment effect of the reform on justice efficiency.

5.3 CF-IV model: justice efficiency as a channel for the impact of the reform on crime

To verify if the impact of the reform on the crime rate is mediated by justice efficiency, we rely on an 'Instrumented Difference-in-Differences' (DIDIV) model (Duflo, 2001; Abdulkadiroğlu et al., 2016), estimated through the Control Function (CF-IV) approach (Wooldridge,

2015).²⁶ The reform is the instrument for justice efficiency. The first stage is given by equation (3.1). The instrument is $Post_t \times R_C$, equal to 1 for treated Lower Courts from the moment of the reform until the end of the period. Then, in the second stage, we add the residuals resulting from the first stage among regressors. The second stage is given by:

$$\ln Crime_{Ct} = \alpha + \beta_1 \ln JE_{Ct} + \beta_2 u_{Ct} + \beta_3 X_{Ct} + \beta_4 UndSta_{Ct} + \eta_C + \mu_t + v_{Ct} \quad (4)$$

where u_{Ct} are the residuals obtained from equation (3.1) (first stage).²⁷ The coefficient of interest is β_1 , measuring the change in crime incidence that follows from an increase in justice efficiency. As before, X_{Ct} contains control variables, $UndSta_{Ct}$ is the dummy variable for severe understaffing, η_C and μ_t are fixed effects, and v_{Ct} is the error term. Adjustment of standard errors for the two-step estimation is achieved by bootstrapping, as suggested by Wooldridge (2015), or by clustering.

6 Results

6.1 Judicial reform and crimes - estimates

Figure 6 shows the estimation results of the model summarised by equation (2), analysing the way in which different types of crimes evolve before and after the judicial reform in treated courts, compared to untouched courts. We look at three categories of crimes: property, organised, and violent crimes. Pre-reform criminal activity appears similar in merged

²⁶The DIDIV scales a DID effect on an outcome by a DID effect on a mediating treatment variable. It appears suitable for our setting, as we test whether the DID effect of the reform on crime deterrence is scaled by justice efficiency.

²⁷Notice that this is empirically equivalent to estimating $\ln Crime_{Ct} = \alpha + \beta_1 \widehat{\ln JE_{Ct}} + \beta_2 X_{Ct} + \beta_3 UndSta_{Ct} + \eta_C + \mu_t + v_{Ct}$ where $\widehat{\ln JE_{Ct}}$ would be obtained from equation (3.1). The coefficient β_1 would be the same.

and untouched courts, as illustrated by the insignificant coefficients of leads dummy variables referring to the period preceding 2012.

Figure 6a. and b. outline a significant decrease in property and organised crimes in the aftermath of the reform in treated courts, while the evolution of violent crimes seems to be essentially unaffected. The results from the difference-in-differences model of equation 2.1 confirm these findings (Appendix Table A5, column 2; Table A6 Panel a., column 2).

Property and organised crimes reduce significantly in the aftermath of the reform. This may be explained by drawing on Becker (1968)'s rational choice theory, claiming that criminals evaluate expected costs and benefits of committing a crime considering their information set. The kick-off of the reform disrupted pre-existing information on criminal courts in treated places. Their extant knowledge of judges and prosecutors' priorities, *modus-operandi*, strengths, and weaknesses were shocked by courts' incorporation, determining information asymmetries that increased the cost of committing a crime. We can hypothesise that once information on the performance of reformed courts became available, criminals used it to adjust their cost-benefit evaluation. Figure 6 a. and b. suggest that this adjustment confirmed higher costs of committing crime due to the reform, in line with the idea of a persistent deterrent effect.

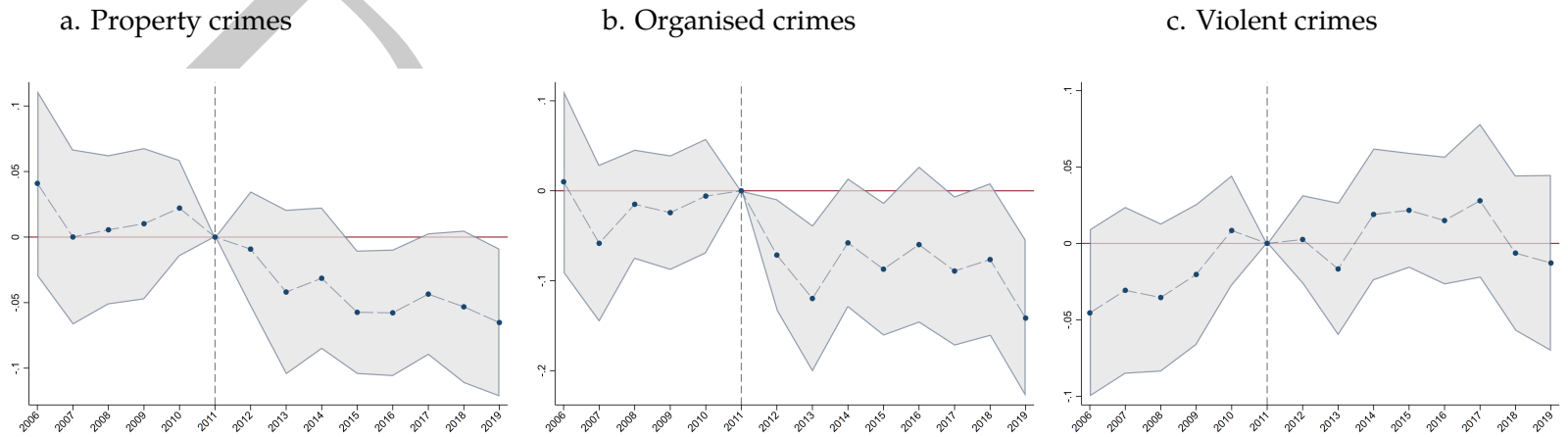
The post-reform adjustment of organised crimes appears faster as compared to property crimes. This may be due to the fact that organised crime groups expect variations in the management of justice at the moment the reform is implemented. Said differently, this may imply that organised crime members mitigate their criminal behaviour in provinces including reformed courts, in expectation of a different - possibly better - crime case management from judicial authorities. This change in criminal behaviour appears persistent in the medium-term, given that the decrease in organised crimes is visible up to 8 years

from the reform implementation date. Differently, property crime rates do not seem to be altered in the immediate post-reform period. While property crimes, relative to control provinces, are consistently trending downward in treated provinces from 2012 until the end of sample period, a significant difference in property crimes is only visible from the fourth year following the reform. This suggests that criminals committing property crimes modify their behaviour slowly. This can be due to different evaluations of the opportunity cost of committing property or organised crimes, possibly because of differences in the capacity of internalising a systematic variation in judicial management and/or more stringent needs.

Figure 6 c. shows no significant difference in violent crimes before and after the reform for provinces with treated courts, compared to control courts. Such a result confirms previous evidence, demonstrating that different types of crimes respond differently to reforms improving justice efficiency (Dušek, 2015; Hernández, 2019). This finding reinforces the idea that the observed crime patterns may be explained through the rational theory of crime, according to which violent crimes are generally less planned and more impulsive than property and organised crimes, which are, instead, mainly committed to gain money, goods, or power. The emotional and impulsive characteristics of violent crimes make them less sensitive to influence by perceived risk and, therefore, less 'deterable' (Loughran et al., 2011; Loughran et al., 2016).²⁸

²⁸ Any model assessing the determinants of crime must consider the possibility of measurement error, as victims may not be willing to report crimes. This aspect is crucial when crime is measured through police records, as in the present case. While underreporting may be high for violent crimes such as like gender violence (Denti & Iammarino, 2022) and organised crime (Di Cataldo & Mastrococco, 2022), it is likely to be limited for property crimes (Xie & Baumer, 2019; Pinotti, 2020), because reporting is mandatory for insurance claims and document replications. The strong effect of the reform on deterring property crimes alleviates concerns that the results are due to measurement bias.

Figure 6: Judicial reform and crimes - event study



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Note: Event study estimates of the effect of decree 155/2012 on Property crimes (Fig. 6a.), Organised crimes (Fig.6b.), Violent crimes (Fig. 6c.). Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate.

6.2 Judicial reform and justice efficiency - estimates

The results of the event study model estimating the effect of decree 155/2012 on Lower Court efficiency (equation 3) are reported in Figure 7. We find no evidence of pre-trends, as merged and untouched courts have similar values of efficiency prior to 2012, a result in line with the evidence presented in Figures 3 and 5. Looking at the post-2012 period, we note that the reform has had a positive impact on Lower Courts' clearance rate, our proxy for judicial efficiency. The difference in efficiency between treated and control courts is positive and significant at 90% level in 2012, turns insignificant in 2013 and keeps growing in the following years.²⁹ The increase in efficiency in treated courts in the post-reform period is confirmed by the difference-in-differences model (equation 3.1), whose results are reported in Appendix Table A6, panel b.

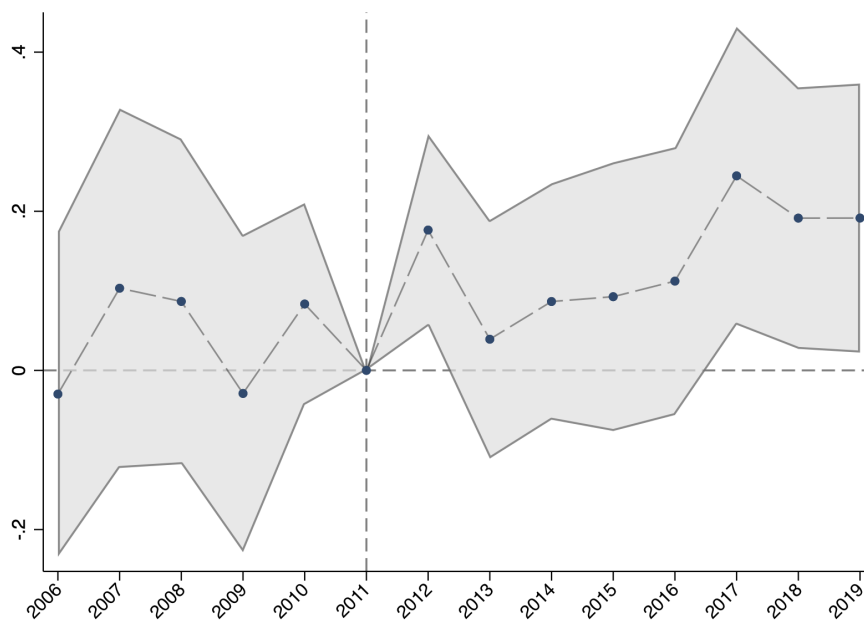
Figure 7 shows a significant difference in justice efficiency in 2012, the year in which the reform was implemented. To explain such an immediate effect of the reform we can draw on studies analysing human resources and mergers, claiming that institutional mergers can be perceived as opening new opportunities such as expanded roles and career advancement prospects (Schraeder & Self, 2003; Dao & Bauer, 2021). In the case of the justice reform, the prospect of growth and development might have motivated judges of the merged courts to perform at their best already when the merger was still at the design-stage (early 2012), as a way to signal their skills and seize these opportunities. More efficient judges have more incentives to exploit the merger to improve their position, and this could explain the sizeable growth in the clearance rate happening soon after the actual merger.³⁰

²⁹One way to explain the lack of significance in the difference between treated and control courts' efficiency in 2013 is the turmoil related to protests and legal attempts to cancel the reform that took place in some courts.

³⁰The preliminary information about the merging criteria started circulating at the beginning of 2012, after the Italian technical government came into office. By Italian law, court mergers did not lead to personnel layoffs, because court staff has permanent working contracts.

We hypothesise that the estimated improvement in court efficiency observed across the full post-reform period can be explained by the fact that the creation of merged courts has allowed for the development of economies of scale, with a higher concentration of judges specialising in the handling of specific case types (Chappe & Obidzinski, 2014; Coviello et al., 2019). Furthermore, the mergers may have unsettled pre-existing working routines and relationships, favouring the creation of new settings disrupting pre-existing inertia, and improving efficiency (Sarala et al., 2019). Finally, court mergers might have spurred efficiency by allowing employees to identify novel goals and motivations (Schraeder & Self, 2003).

Figure 7: Judicial reform and clearance rate - event study



Note: Event study estimates of the effect of decree 155/2012 on the clearance rate. Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate.

Having established that the reform has led to an improvement in justice efficiency in re-

formed courts, as measured by the clearance rate, we test whether a significant effect is visible for both different components of the clearance rate JE_{Ct} , i.e. the criminal Trial clearance rate $TCRC$, which involves the Attorney office and the Judge for Preliminary Investigation, and the pre-trial clearance rate w_{Ct} . They refer to the efficiency of keeping up with the incoming caseload at the trial stage and the pre-trial stage, respectively. The results, outlined in Figure A2 in the Appendix, show that the reform improved the clearance rate in both stages.

6.3 CF-IV estimates

To verify if justice efficiency acts as a transmission channel through which the reform affects crime deterrence we estimate a Control Function with Instrumental Variable (CF-IV) model. The first stage is given by model (3.1), estimating the impact of the reform on justice efficiency. Then, the second stage, described by equation (4), includes the residuals resulting from the first stage to explain the observed incidence of crime. We focus on property crimes and organised crimes as outcomes, because reduced-form estimates in section 6.1 have shown evidence of a significant impact of the reform on these types of crimes.

The results are reported in Table 1. The estimates in column (1) correspond to the first-stage, and they are equal to those reported in Table A6, panel b, column (3). Those in columns (2)-(3), instead, reproduce the impact of an increase in justice efficiency, induced by the judicial reform, on property crimes and organised crimes, respectively. They confirm that the increased efficiency of courts determined by the reform can be viewed as a significant mediator for the impact of the reform on the court-level crime rate, as corroborated by the significant coefficient of the first-stage residuals (Wooldridge, 2015). Relative to the pre-reform period and untouched courts, both property crimes and organised

crimes decrease significantly in the post-reform period for merged courts whose efficiency has significantly increased after 2012.

Table 1: Justice efficiency and crime deterrence - CF-IV estimates

	Clearance rate (1)	Property crimes (2)	Organised crimes (3)
Post × Reform	0.106*** (0.034) [0.037]		
Clearance rate		-0.548*** (0.147) [0.294]	-0.679** (0.205) [0.344]
1st stage residuals		0.545*** (0.137) [0.294]	0.663*** (0.200) [0.344]
Court FE	✓	✓	✓
Year FE	✓	✓	✓
Controls	✓	✓	✓
Observations	1,848	1,428	1,428
Courts/provinces	132	102	102

Note: Standard errors bootstrapped 1,000 times in round brackets: *** $p < .01$, ** $p < .05$, * $p < .1$. clustered standard errors at court level (column 1) or at province level (columns 2, 3) in squared brackets. Post × Reform takes value 1 for synthetic courts experiencing mergers since 2012, and 0 otherwise. Clearance rate: justice efficiency indicator. Controls: population (logs), understaffed court, judges per 10,000 inhabitants (logs), unemployment rate, population density, local spending in safety and justice per inhabitant (logs). Time span: 2008–2019. Results are confirmed with different sets of controls - estimates available upon request.

The point estimate of 0.106 in column 1 indicates that the judicial reform translates approximately into a 10.6% increase in the ratio of terminated vs incoming cases by court, in reformed Lower Courts relative to untouched courts, before and after the reform. Columns 2 and 3 reveal that, on average, a 1% increase in the clearance rate converts into a 0.5% and 0.7% decrease in property and organised crimes, respectively. Hence, to compute the average treatment effect of the reform on property and organised crime we can multiply the column 1 coefficient (10.6%) by the columns 2 (0.55) and 3 (0.68) coefficients. The average 10.6% increase in justice efficiency has produced an average decrease in property crimes

by 5.8%, and an average decrease in organised crimes by 7.2%, across the full post-reform period. These effects are larger than those obtained by Hernández (2019), reporting a 2% decrease in property crimes in the immediate aftermath of a judicial reform implemented in Peru. Different from this study, however, the reform's effect is not losing strength with time. Our estimated effects are persistent and fairly stable (or growing) across the full post-reform period.

7 Robustness checks

7.1 Alternative control groups and placebo tests

To validate the results reported in section 6 we perform a number of robustness checks.

To start with, we remove from sample the largest Lower Courts, which may influence the results given their size.³¹ Figure A3 in the Appendix, performing this test, confirms the results. As a second test, Figure A4 in the Appendix shows that the results are robust to the exclusion from the control group of the 6 Lower Courts which were spared from merging due to lack of adequate transport infrastructures and/or relevant presence of organised crime. As a third test, we remove the Lower Courts which had high staff turnover, since intense mobility of judges and prosecutors could influence results.³² Figure A5 in the Appendix confirms our results.

As a fourth validity check, we adopt alternative ways of building the province-level dataset

³¹The following Lower Courts are removed: Milan, Palermo, Rome, Turin. They all belong to the control group. Naples is not part of the present investigation as the reform disaggregated the Lower Court into sub-units.

³²Data on turnover are provided by 'Consiglio Superiore della Magistratura' as figures aggregated over several years (Filomeno & Rocchetti, 2018). This prevents using these data to design a control variable.

we use to estimate the impact of the justice reform on crime rates. As detailed in section 4, while crime data is available at the province level, a minority of provinces have more than one Lower Court. In our main estimation, for provinces with more than one Lower Court, we remove multiple courts within provinces through random selection. As robustness, we adopt different approaches for dropping Lower Courts in multiple-Lower Courts provinces. First, we keep the Lower Court of the largest urban area (i.e., the court in the provincial capital), discarding the peripheral one. Second, we do the opposite, keeping peripheral courts and discarding urban courts. Figure A6 in the Appendix, performing this test, confirms the results. The results in Figure A6 also suggest that the effect of the reform on crime and the clearance rate is not influenced by urban/peripherality issues. Finally, rather than dropping courts, we compute the average of court-level values for multiple Lower Courts within the same province. Again, results are confirmed (Appendix Figures A7-A8 and Table A9).

As a fifth robustness test, we perform placebo tests anticipating the treatment year to 2009 or 2010. We find no significant difference between treatment and control courts in pre-treatment years (Appendix Tables A10 and A11), while event study estimates artificially anticipating the treatment show no significant treatment effect before 2012 (Appendix Figure A9). Finally, we test the validity of the parallel trends assumption for the main specifications and all other specifications used in the robustness checks. The assumption appears to hold in all models (Appendix Table A7).

7.2 Alternative explanations

In this section, we examine a set of alternative explanations for the results discussed in section 6.

First, the reform may be related to local differences in the attitude toward rule-abiding, as places with a stronger presence of organised crime could experience specific dynamics, like attempts to bribe and/or intimidate court staff. To account for this possibility, we add a proxy for the rule-complying attitude among the set of controls. The proxy we use for organised crime presence is the local per-capita volume of suspicious financial transactions detected by all local actors engaged in financial, legal, and accounting services, gold trading, and gambling (Dalla Pellegrina et al., 2020). These are not crime records, since the suspicious transactions are reported to the Bank of Italy observatory and not to the police. This measure is available from 2008 onward, thus forcing us to restrict the time span to 2008-2019. The top panel of Figure 8 confirms the deterrence effect of the reform on property and organised crimes, and the positive effect of the reform on justice efficiency, once the pre-existing strength of organised crime is accounted for in the estimates.³³

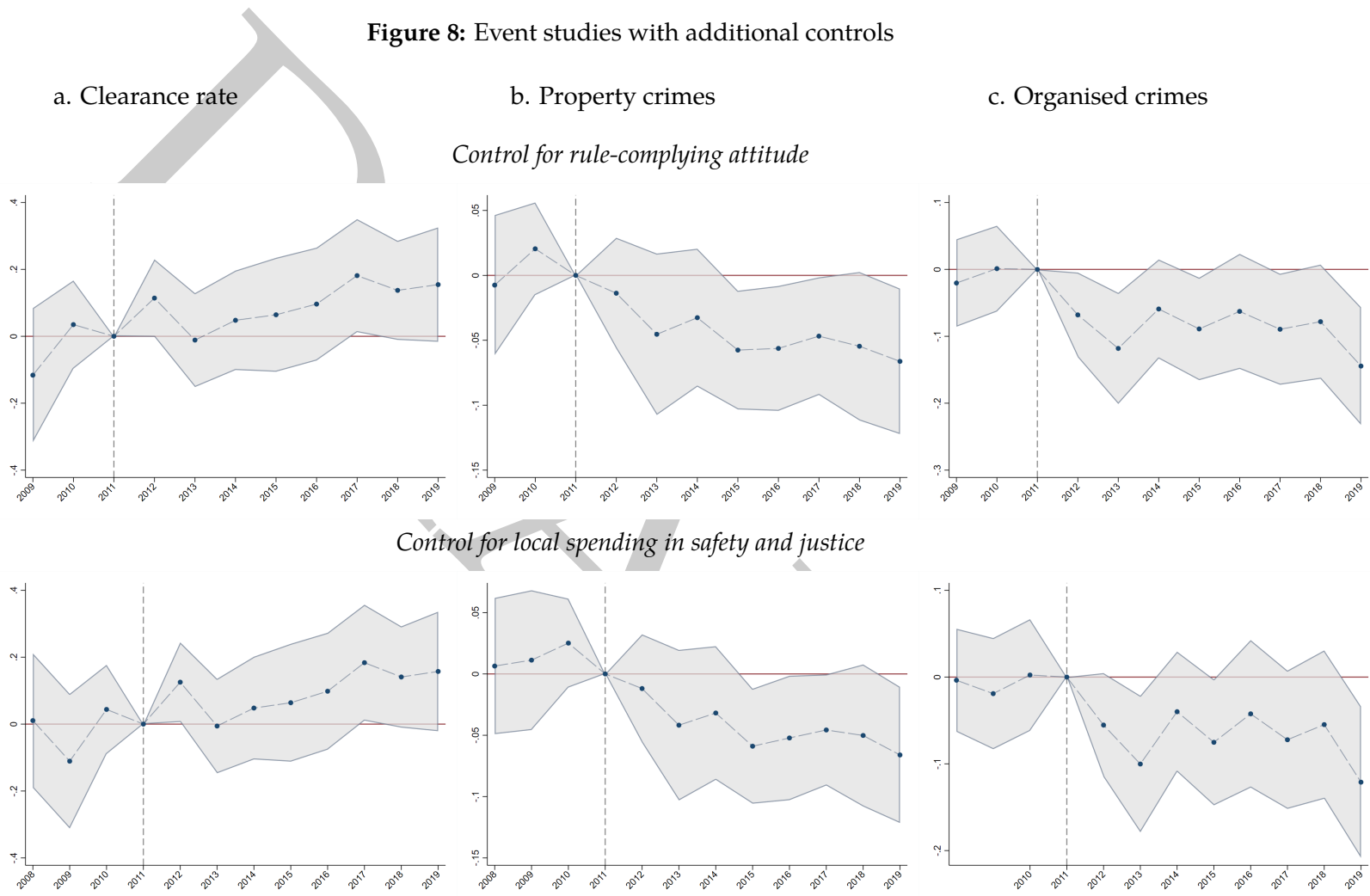
Another possible confounding element for our model is the implementation of local policies aiming at boosting safety. Places with more or less local investment in public safety could be targeted by the reform, aiming at improving efficiency. Local authorities could implement policy reinforcing the justice system, which, for Italian law, allows to contribute to expenditure for judicial facilities. We control for this by adding local spending on safety and justice to the control set. Data on this variable is available from 2008 onward, hence constraining the time span to 2008-2019. The bottom panel of Figure 8 confirms that our main results are robust to controlling for local expenditures for justice and safety.^{34,35}

³³This evidence holds also in the difference-in-difference specification, as illustrated in Appendix Table A5, column 3, and Table A6, Panel b. column 3.

³⁴The results also hold in the difference-in-difference model, as reported in Table A5 column 4 and Table A6, Panel b., column 4 in the Appendix. There are no data on local safety spending for the Aosta province, forcing us to drop this from these estimations.

³⁵The estimates' results are confirmed also if we control for the level of education. See Appendix Table A5, column 5, and Table A6, column 5.

Figure 8: Event studies with additional controls



Note: Event study estimates of the effect of decree 155/2012 on clearance rate (Fig. 8 a.), Property crimes (Fig.8 b.), Organised crimes (Fig. 8 c.). Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE event study model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate. Additional control in top figures: log rule-complying attitude; additional control in top figures: log local spending in safety and justice.

Another explanation for our results could be that places whose Lower Court was incorporated reported less crimes due to increased reporting costs resulting from higher transport costs and/or reduction in trust in institutions following the closure of the local court. If this were true, we should see post-reform differences in crime trends between the district capital, where the incorporating Lower Court is located, and the districts which were previously endowed with the incorporated court. To check for this, we compare trends in crime reporting between the district capital and the district formerly holding a local court in the pre-reform period.³⁶ Figure 9 displays these trends for property and organised crimes, suggesting parallel trends in crime reporting between district capital and the rest of the district, before and after the reform. This suggests that our results are not driven by reduced reporting from places that had their court suppressed.

Figure 9: Crime trends within provinces with merged courts



³⁶Data on crime types by district capital (*capoluogo di provincia*) and rest of district provided by ISTAT.

We are also interested in understanding whether the estimated impact of the reform on crime rates is due to criminals refraining from committing crimes, as we hypothesise, or to criminals relocating to areas unaffected by the reform. If the latter case is true, our estimates would be determined by a spillover effect inducing an increase in crime in control courts. We check for this by estimating the post-reform differences in property and organised crime between unreformed courts bordering reformed courts and unreformed courts that are more distant from treated courts. Being relocation costly, criminals are likely to reduce this cost in their decision-making. Therefore, we assume that if criminals indeed relocate as a result of the reform, this relocation would be more likely to occur from treated court jurisdictions to neighbouring court jurisdictions. The test shows no significant difference in property and organised crime during post-reform years between the two types of untreated courts (Appendix Figure A10). This suggests that the reduction in crime occurring in reformed courts does not combine with criminals moving their business to the neighbouring area.

Next, our results on the effect of the reform on court efficiency could be explained by the influence of the statutes of limitations (*prescrizione*) on case management inside the Lower Courts. In dealing with case backlog, judges and prosecutors could be hurried to prioritise cases that are soon expiring due to statutes of limitations. Alternatively, they could choose to ignore cases whose statute of limitations is approaching to avoid dealing with reduced time to collect evidence, hear witnesses and debate. Furthermore, the imposition of statutes of limitations may expedite the legal process of bringing charges against a perpetrator, potentially limiting the opportunity to uncover and present relevant evidence. We account for this by introducing a measure for justice efficiency that weights the clearance rate by the share of cases dismissed due to statutes of limitations and estimating the

effect of the reform on this measure.³⁷ Figure A11 in the Appendix confirms our main result, by showing that the reform increased Lower Court efficiency even when we account for the burden posed by statutes of limitations.

To support our underlying hypothesis that the judicial reform deterred crime because it increased court efficiency and consequently incremented the *certainty* of punishment, we must rule out the possibility that the observed rise in justice efficiency of treated courts correlates to poor decisions. We assume that low-quality decisions by Lower Courts are more likely to be *appealed*, as poor judgment increases the likelihood that a second trial could alter the outcome. This, in turn, reduces the certainty of punishment. Hence, as a proxy for the quality of work produced by the courts we refer to the proportion of sentences that have been appealed. We use data on appealed sentences in two ways, constructing two indicators proxying for the quality of court decisions (The European Commission for the Efficiency of Justice, 2016). The first is the 'appeal rate', given by the percentage of appeals against the total number of judgments at Lower Court level for criminal cases. The second is the 'held appeal rate', which is the ratio of appeals confirming the decision taken by the Lower Court.³⁸ The fact that Courts of Appeal are few across the country (26 in total, approximately one per Italian region) prevents us from replicating our event study analysis. Yet, we can descriptively compare the evolution of appeal rate and held appeal rate in the 9 Court of Appeal jurisdictions where no court mergers took place and the 17 jurisdictions in which court mergers occurred. The pre/post-treatment comparison between these two groups shows no differential trends in terms of appeal rate or held

³⁷We estimate eq.(3) and eq.(3.1) with $\ln \tilde{J}E_{Ct} = \ln(JE_{Ct}/SoL_{Ct})$ as outcome, where JE_{Ct} is the clearance rate defined by eq.(1) and SoL_{Ct} is the share of cases dismissed due to statutes of limitations in Lower Court C at time t by court judges, preliminary investigation judges and prosecutors.

³⁸Using both metrics reduces the bias that could result by depending solely on the 'appeal rate', since appeals can also be the result of tactical reasons that are unrelated to the accuracy of the decision (e.g., appeals submitted for the purpose of delaying) (The European Commission for the Efficiency of Justice, 2016).

appeal rate over the analysed period, suggesting that the reform did not alter the quality of Lower Court decisions (Appendix, Figures [A12](#) - [A13](#).)

Finally, we need to alleviate concerns that the significant reduction in property and organised crime after the reform depends on the reformed Lower Courts changing their priorities on the type of crimes to be prosecuted. Since we measure crimes with offences reported to the police by crime victims rather than with reporting by courts, it is reasonable to assume that our estimates reflect changes in criminal activity rather than changes in the decision to prosecute specific criminal types.

8 Conclusions

This paper has examined the impact of the 2012 Italian judicial reform on crime rates, shedding light on the deterrence effect of efficient justice. By analysing the spatial reorganisation of first-instance courts resulting from the reform, which streamlined the Lower Courts through mergers, the paper demonstrates that the spatial reorganisation of courts has led to an increase in justice efficiency which, in turn, has contributed to a reduction in crime.

This evidence is consistent with the idea that rational criminals internalise the changes in justice performance in the opportunity-cost evaluation of committing crimes. Following the reform, merged courts exhibited an 11% increase in efficiency, as measured by their clearance rate. We hypothesise that this result can be attributed to the fact that merging judges from many courts into one single court enables the development of scale economies, specialisation of judges in specific case management (Coviello et al., [2019](#)), new working routines (Sarala et al., [2019](#)) and personal motivations linked to new career development

(Dao & Bauer, 2021).

Efficient justice raises the costs of crime by enhancing the likelihood of conviction. In other words, crime punishment is more *certain*. This is expected to deter criminals from committing crimes in reformed territories, which is indeed what we observe. However, not all types of crimes are affected equally. Efficient justice appears to deter particularly 'instrumental' crimes (property and organised crimes) while showing less impact on emotionally driven violent crimes, such as arson or homicides (Loughran et al., 2016). In the post-reform period, property crimes (e.g. non-violent stealing of properties) are found to decrease by 5.8%, while organised crimes (e.g. criminal association, extortion) are lowered by 7.2%.

While the magnitude of the effect on property and organised crimes is relatively similar, the dynamic reaction of these two crime categories to the reform appears to differ. In particular, organised crimes display a swift response to the reform. A significant decrease in organised crime rate in provinces experiencing court mergers is visible already in the first post-reform year, remaining stable over the full post-reform period (2012-2019). Conversely, property crimes exhibit a slower, progressive decline over time. This discrepancy suggests variations in how criminals adapt to institutional changes and perceive the effectiveness of punishment. Offenders involved in property crimes may be less rapid in learning about the change in the effectiveness of punishment, through their experience or that of their peers (Glaeser et al., 1996), not perceiving an immediate increase in the opportunity cost of committing crimes and therefore changing their behaviour slowly. Differently, organised crime offenders may be more aware of the institutional framework and the potential consequences such a reform can have, and/or it might be less costly for them to 'wait and see' what happens in the reformed area.

These results support the view that the certainty of punishment crucially matters for deterring crimes (Nagin et al., 2018). While this study focuses on the evaluation of a specific reform implemented in Italy, future research may examine whether comparable reforms of the geography of criminal justice implemented in other European countries such as Denmark, Croatia, France, the Netherlands, Norway, Portugal, and the UK produce similar effects on justice efficiency and crime deterrence.

The implications of our research are significant for policy considerations. We have analysed a judicial reform implemented during a unique period, primarily aimed at rationalising public expenditures on justice. As such, our results speak to policy debates on fiscal austerity measures. We show that removing local public services - in this case, first-instance courts - does not compromise general justice efficiency and citizens' safety; rather, it enhances them. Descriptive evidence suggests that this effect estimated for provinces undergoing court mergers distributes homogeneously within the province, both to the area incorporating small courts as well as to the territory characterised by the court removal. This implies that the reform does not disproportionately disadvantage areas undergoing service reductions, a finding somehow in contrast with previous evidence documenting adverse effects on peripheral communities following the rationalisation of public expenditures, including the closure of schools, hospitals, transportation facilities, police stations, and local support services. (Holmes et al., 2006; Gibbons et al., 2018; Blesse & Diegmann, 2022; Denti & Iammarino, 2022; Di Cataldo & Romani, 2023). A potential explanation for this discrepancy lies in the role of judicial courts. Unlike 'first-stop shop' public services such as police stations or anti-violence centres, courts may be perceived as resources to be utilised at a later stage in the crime reporting process. Thus, the enhanced efficiency resulting from a spatial redistribution of judicial services seem to outweigh the drawbacks of service reductions for peripheral communities. This suggests that the benefits of improved

justice efficiency offset the costs associated with court mergers.

The certain determination of judicial issues, favoured by efficient justice, not only ensures a country's degree of security but also plays a crucial role in shaping trust, cooperation, and economic progress (Chemin, 2020; Ciapanna et al., 2022; Kondylis & Stein, 2023). Recognised by policy frameworks like the European Union's Resilience and Recovery Facility (European Commission, 2022) and the United Nations' Sustainable Development Goals (United Nations, 2021), efficient judicial reforms can significantly impact socio-economic performance. While our findings demonstrate that reforming national courts' geography may crucially influence local criminal behaviour, we have not examined the welfare effect of the observed variation in justice efficiency through economic indicators. An important task for future research is to delve into the socio-economic effects of such institutional changes, examining how they influence the community welfare and the economic performance of countries and regions.

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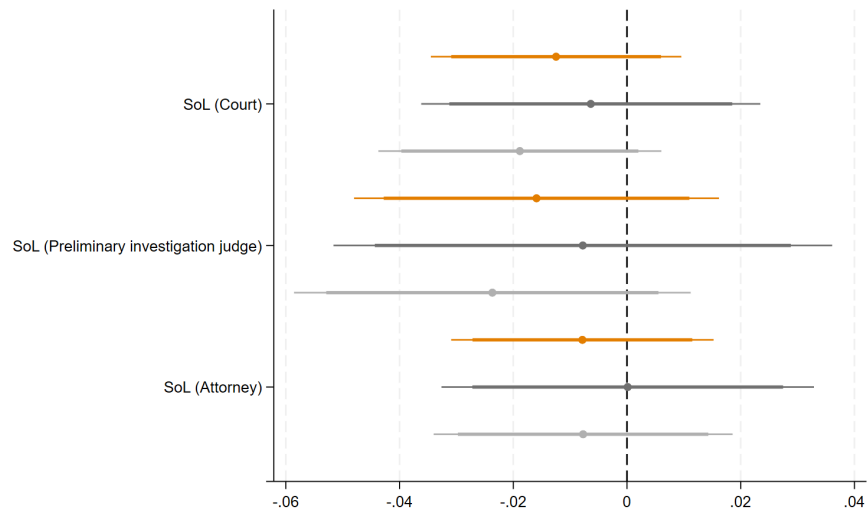
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Appendices

Data details and summary statistics

Figure A1: Balancing test on the pre-reform share of cases dropped by Statutes of Limitations (SoL)



Note: Balancing test estimating the difference between incorporating, control (untouched), and incorporated courts. For each couple of court categories, it estimates a basic regression comparing the share of criminal cases which were dismissed due to Statutes of Limitations (SoL) measured in the pre-treatment (2006-2011) period in the Prosecutors office, the Judges for Preliminary Investigation office and the Trial stage. Thicker confidence intervals refer to 90% level, thinner ones to 95%. Dark orange CIs: treated vs. control courts comparison; dark grey CIs: incorporated vs. treated courts; light grey CIs: incorporated vs. control courts.

Table A1: Treated and Untreated Lower Courts

Panel I. All Italy				
Pre-reform total	Treated			Untreated
164	49			115
	<i>Incorporated</i>	<i>Incorporating</i>	<i>Small boundary changes</i>	<i>No boundary changes</i>
	26	23	20	95
Panel II. Estimation panel. Lower Court level				
a. Pre-reform panel	Treated			Untreated
158	49			109
	<i>Incorporated</i>	<i>Incorporating</i>	<i>Small boundary changes</i>	<i>No boundary changes</i>
	26	23	20	89
b. Panel total	Synthetic treated			Untreated
132	23			109
Panel III. Estimation panel. Province level.				
a. Pre-reform panel	Treated			Untreated
127	47			80
	<i>Incorporated</i>	<i>Incorporating</i>	<i>Small boundary changes</i>	<i>No boundary changes</i>
	25	22	13	67
b. Panel total	Synthetic treated			Untreated
102	22			80

Note: Panels I-III exclude Napoli Lower Court which the reform split in two courts. **Panel I** details, for Italy, (i) the pre-reform number of Lower Courts, (ii) the number of Lower Courts treated by the reform with figures of the incorporated (incorporating) courts, (iii) the number of untreated Lower Courts with figures of courts with small (no) boundary change. **Panel II.a** shows (i) the Lower Courts sample considered in the estimation at Lower Court level, which excludes the 6 Sardinia courts (all untreated), due to lack of spatial data on crime, (ii) the number of treated Lower Courts with figures of the incorporated (incorporating) courts, (iii) the number of untreated Lower Courts with figures of courts with small (no) boundary change. **Panel II.b** shows the observations used in estimation at Lower Court level. **Synthetic treated** are the aggregation of each incorporated-incorporating Lower Court bundle until 2012 (reform year), and by the actual Lower Court resulting from the incorporation afterwards. 20 synthetic Lower Courts refer to an incorporated-incorporating court pair. 3 synthetic Lower Courts refer to an incorporating court absorbing two incorporated courts. **Panel III.a** shows (i) the sample of Lower Courts considered in the province-level estimation, again excluding Sardinia provinces. For provinces with multiple courts, data are consolidated, to avoid double-counting since crime data are at province level. Two alternative consolidation approaches are used. The first drops 30 Untreated Lower Courts that are in provinces with multiple Lower Courts. For the same reason, it discards 2 Treated Lower Court, which merged together and belong to the same province of other treated Lower Court. For the 3 provinces with a treated and untreated Lower Courts, this approach keeps the treated one and discards the other courts. The alternative approach averages the value of each court-level variable across all courts in the same province if these courts are all untreated (treated). For the 3 provinces with a treated and untreated Lower Courts, values referring to the treated (untreated) court are kept (discarded). **Panel III.b** shows that both approaches provide an estimation panel of 102 province/courts. Details on the incorporated-incorporating Lower Court bundles are in Table A2.

Table A2: List of incorporated and incorporating Lower Courts resulting from Law 148/2011 and Decree 155/2012

<i>Incorporated Lower Courts</i>		<i>Incorporated by:</i>	
Court name	code	Court name	code
ACQUI TERME	16001	ALESSANDRIA	16004
ALBA	16003	ASTI	16010
ARIANO IRPINO	16008	BENEVENTO	16017
BASSANO DEL GRAPPA	16015	VICENZA	16161
CAMERINO	16028	MACERATA	16073
CASALE MONFERRATO	16030	VERCELLI	16158
CHIAVARI	16035	GENOVA	16052
CREMA	16040	CREMONA	16041
LUCERA	16072	FOGGIA	16048
MELFI	16078	POTENZA	16110
MISTRETTA	16081	PATTI	16100
MODICA	16083	RAGUSA	16112
MONDOVÍ	16084	CUNEO	16043
MONTEPULCIANO	16085	SIENA	16131
NICOSIA	16088	ENNA	16044
ORVIETO	16094	TERNI	16140
PINEROLO	16106	TORINO	16142
ROSSANO	16119	CASTROVILLARI	16032
SALA CONSILINA	16122	LAGONEGRO	16060
SALUZZO	16124	CUNEO	16043
SANREMO	16125	IMPERIA	16055
SANT'ANGELO DEI LOMBARDI	16126	AVELLINO	16011
TOLMEZZO	16141	UDINE	16150
TORTONA	16144	ALESSANDRIA	16004
VIGEVANO	16162	PAVIA	16101
VOGHERA	16164	PAVIA	16101

Table A3: Variables description

Variable	Description	Source
<i>Criminal First-Instance Courts characteristics</i>		
Clearance rate	Court ratio between terminated cases and incoming cases weighted by the Prosecutor Office & Judge for the Preliminary Investigation ratio between terminated cases and incoming case (logs)	Ministry of Justice
Understaffed Court	Dummy equal 1 for courts whose percentage of Judges and Prosecutors' vacancies is greater than 20 % of allocation (0 otherwise)	CSM
Judges	Judges and Prosecutors allocated to the Court per 10,000 inhabitants (logs)	Ministry of Justice
FIC administrative boundaries	Court geographic coverage	Ministry of Justice
<i>Crime</i>		
Property crimes	Stealing crimes per 1,000 inhabitants (logs)	ISTAT-Ministry of Interior
Organised crimes	Criminal association, extortion, kidnapping, usury, dealing in stolen properties per 1,000 inhabitants (logs)	ISTAT-Ministry of Interior
Violent crimes	All robberies, arson, vandalism per 1,000 inhabitants (logs)	ISTAT-Ministry of Interior
<i>Controls</i>		
Population	Resident population (logs)	ISTAT
Unemployment	Unemployment rate	ISTAT
Territory	Lower Court administrative boundaries	Ministry of Justice
Population density	Population per km sq	ISTAT
Local authorities' safety spending	Local authorities' budget for safety per inhabitant (logs)	Ministry of Finance
Local authorities' justice spending	Local authorities' budget for justice per inhabitant (logs)	Ministry of Finance
Rule-complying attitude	Compulsory reports done by intermediaries to the Central Bank on suspected money laundry per 100 000 inhabitants (logs)	Bank of Italy
Human capital	Percentage of population with tertiary degree	ISTAT
<i>Other variables</i>		
Clearance rate pre-trial stage	Ratio between terminated cases and incoming cases in the pre-trial stage (Prosecutor Office and Judge for Preliminary Investigation) (logs)	Ministry of Justice
Clearance rate trial stage	Ratio between terminated cases and incoming cases in the trial stage (logs)	Ministry of Justice
Clearance rate weighted by the Statute of Limitations	clearance rate weighted by the share of cases dropped due to Statute of Limitations (logs)	Ministry of Justice

Table A4: Summary statistics

Variables	N	Mean	St.dev	Min	Max
<i>Panel a. Courts consolidated into provinces for crime estimation</i>					
<i>Criminal First-Instance Courts characteristics</i>					
Clearance rate (logs)	1,428	9.140	0.266	7.934	10.67
Understaffed Court	1,428	0.0791	0.270	0	1
Judges (logs)	1,428	-0.449	0.410	-1.438	1.004
<i>Crimes</i>					
Property crimes (logs)	1,428	2.016	0.462	0.674	3.377
Organised crimes (logs)	1,428	-0.713	0.388	-2.213	0.920
Violent crimes (logs)	1,428	1.598	0.419	0.390	2.943
<i>Controls</i>					
Population (logs)	1,428	12.76	0.687	11.29	14.75
Population density	1,428	297.4	361.2	37.98	2,494
Unemployment rate	1,428	9.750	5.406	0.732	31.46
Local authorities' justice spending (logs)	1,224	2.529	1.717	-0.693	5.347
Local authorities' safety spending (logs)	1,212	3.603	0.953	-0.676	5.025
Rule-complying attitude (logs)	1,117	4.650	0.782	1.503	7.512
Human capital (%)	1,070	57.053	7.688	36.3	75.7
<i>Other variables</i>					
Clearance rate pre-trial stage (logs)	1,428	4.591	0.136	3.801	5.224
Clearance rate trial stage (logs)	1,428	4.549	0.221	3.448	6.188
Clearance rate with Statute of Limitations (logs)	1,428	8.975	0.287	7.766	10.306
<i>Panel b. All Courts</i>					
<i>Criminal First-Instance Courts characteristics</i>					
Clearance rate (logs)	1,848	9.136	0.274	7.934	10.67
Understaffed Court	1,848	0.0958	0.294	0	1
Judges (logs)	1,848	-0.394	0.446	-1.438	1.004
<i>Controls</i>					
Population (logs)	1,848	12.67	0.722	11.11	14.85
Population density	1,848	315.7	429.8	37.98	2,494
Unemployment rate	1,848	10.56	5.628	0.732	31.46
Local authorities' justice spending (logs)	1,584	2.564	1.606	-0.693	5.347
Local authorities' safety spending (logs)	1,570	3.352	1.255	-0.676	5.025
Rule-complying attitude (logs)	1,447	4.844	0.907	1.503	7.973
Human capital (%)	1,370	57.258	7.814	36.3	75.7
<i>Other variables</i>					
Clearance rate pre-trial stage (logs)	1,848	4.591	0.139	3.801	5.388
Clearance rate trial stage (logs)	1,848	4.545	0.235	3.416	6.189
Clearance rate with Statute of Limitations (logs)	1,848	8.972	0.297	7.766	10.306

Note: Local authorities' budget spending in safety and justice available from 2008 onward. Local authorities' budget spending in safety not available for Aosta province. Rule-complying attitude available from 2009 onward. Human capital available between 2009 and 2018.

Detailed estimates, robustness, and falsification tests

DRAFT

Table A5: Effect of the reform on property crimes and organised crimes - Difference-in-Difference estimates for several model specifications

	(1)	(2)	(3)	(4)	(5)
<i>Panel a. Dep. var.: property crimes</i>					
Post × Reform	-0.0609* (0.0316)	-0.0581* (0.0312)	-0.0510** (0.0247)	-0.0555** (0.0272)	-0.0545** (0.0259)
population	-0.0260 (0.0820)	-0.139** (0.0653)	-0.0824 (0.0651)	-0.0837 (0.0683)	-0.099* (0.056)
understaffed court	0.0403*** (0.0139)	0.0369*** (0.0139)	0.0293** (0.0145)	0.0314** (0.0139)	0.0367** (0.0129)
judges	-0.0483 (0.0764)	-0.0525 (0.0715)	-0.0138 (0.0670)	-0.0452 (0.0703)	-0.0404 (0.0545)
unemployment		-0.00657 (0.00399)	-0.00233 (0.00466)	-0.00418 (0.00425)	-0.0034 (0.0045)
population density		0.000596*** (7.01e-05)	0.000488*** (4.93e-05)	0.000554*** (4.75e-05)	0.0005*** (0.00004)
rule-complying attitude			-0.0243 (0.0182)		
local authorities' justice spending				0.0217** (0.0100)	
local authorities' safety spending				0.0395** (0.0158)	
human capital					0.001 (0.0029)
Observations	1,428	1,428	1,117	1,212	1,010
Court FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
<i>Panel b. Dep. var.: organised crimes</i>					
Post × Reform	-0.0727** (0.0365)	-0.0722* (0.0365)	-0.0822** (0.0360)	-0.0650* (0.0349)	-0.0708** (0.0338)
population	0.0701 (0.116)	0.0844 (0.127)	0.0163 (0.100)	0.0177 (0.114)	-0.0168 (0.101)
understaffed court	0.00738 (0.0178)	0.00667 (0.0187)	0.0156 (0.0198)	-0.00127 (0.0214)	0.021 (0.020)
judges	-0.0796 (0.106)	-0.0800 (0.105)	-0.152 (0.0990)	-0.0936 (0.108)	-0.110 (0.105)
unemployment		-0.000737 (0.00557)	-0.00276 (0.00517)	-0.00308 (0.00546)	-0.002 (0.005)
population density		-7.54e-05 (0.000334)	8.61e-05 (9.90e-05)	-3.67e-06 (0.000224)	0.0001* (0.00008)
rule-complying attitude			0.0502* (0.0268)		
local authorities' justice spending				0.0160 (0.0128)	
local authorities' safety spending				0.00485 (0.0186)	
human capital					0.003 (0.003)
Observations	1,428	1,428	1,117	1,212	1,010
Court FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

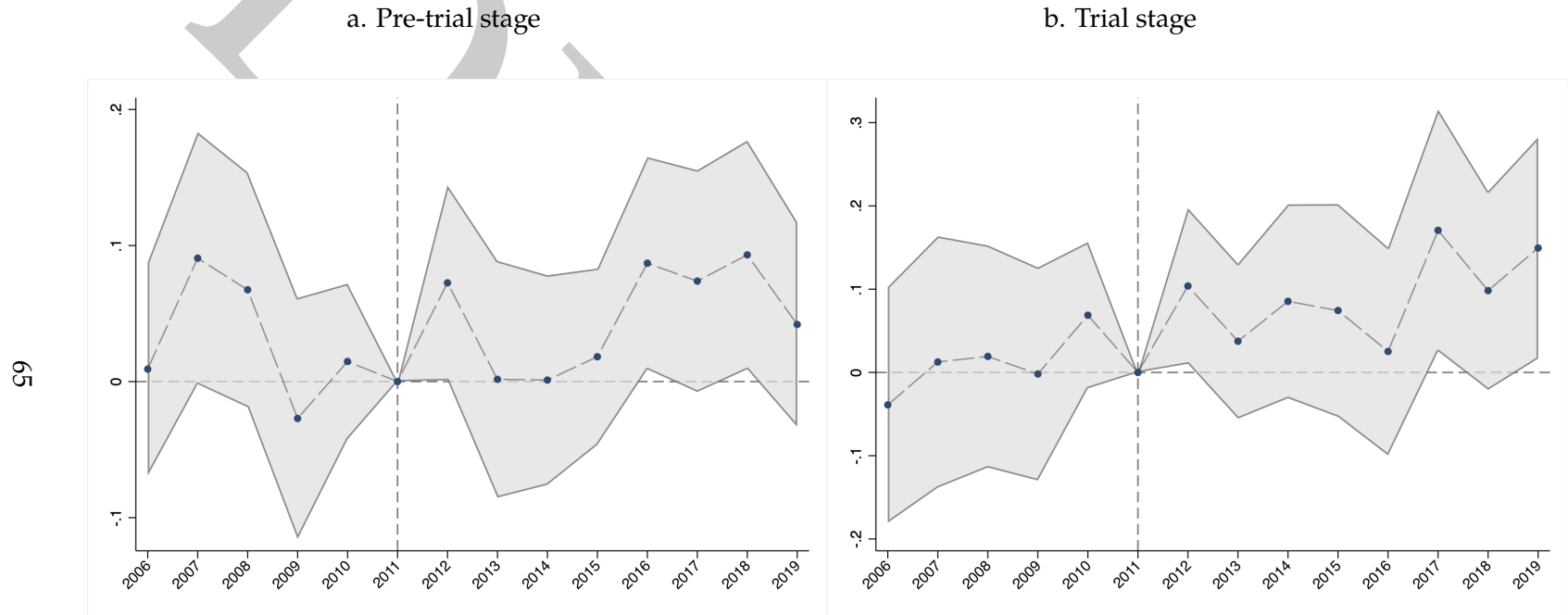
Note: Clustered standard errors at province level in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Post × Reform takes value 1 for synthetic courts experiencing mergers since 2012, and 0 otherwise.

Table A6: Effect of the reform on violent crimes and clearance rate - Difference-in-Difference estimates for several model specifications

	(1)	(2)	(3)	(4)	(5)
<i>Panel a. Dep. var.: violent crimes</i>					
Post × Reform	0.0236 (0.0228)	0.0269 (0.0220)	0.00225 (0.0187)	0.0130 (0.0199)	0.0099 (0.018)
population	-0.0651 (0.0776)	-0.0270 (0.0975)	-0.0808 (0.0722)	-0.0231 (0.0734)	-0.041 (0.064)
understaffed court	-0.00841 (0.0124)	-0.0135 (0.0124)	-0.0142 (0.0127)	-0.0120 (0.0137)	-0.011 (0.011)
judges	0.0529 (0.0683)	0.0494 (0.0691)	0.0633 (0.0666)	0.0421 (0.0628)	0.074 (0.062)
unemployment		-0.00633** (0.00300)	-0.00318 (0.00354)	-0.00446 (0.00334)	-0.003 (0.003)
population density		-0.000201 (0.000426)	-8.69e-06 (6.93e-05)	-0.000208 (0.000227)	-0.00007* (0.00004)
rule-complying attitude			-0.0192 (0.0147)		
local authorities' justice spending				-0.0174* (0.00901)	
local authorities' safety spending				-0.0139 (0.0127)	
human capital					0.004** (0.0017)
Observations	1,428	1,428	1,117	1,212	1,010
Court FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
<i>Panel b. Dep. var.: clearance rate</i>					
Post × Reform	0.101*** (0.0368)	0.106*** (0.0366)	0.121*** (0.0429)	0.108*** (0.0412)	0.107** (0.0435)
population	-0.167 (0.126)	-0.246* (0.128)	-0.191 (0.166)	-0.252 (0.159)	-0.144 (0.183)
understaffed court	0.00240 (0.0316)	0.00420 (0.0310)	0.0248 (0.0337)	0.0172 (0.0318)	0.0307 (0.0334)
judges	-0.0296 (0.143)	-0.0346 (0.141)	-0.0560 (0.142)	-0.0303 (0.140)	-0.0643 (0.156)
unemployment		0.00202 (0.00505)	0.0110* (0.00611)	0.00563 (0.00566)	0.00893 (0.00643)
population density		0.000166* (8.52e-05)	-9.49e-05 (0.000151)	8.22e-05 (0.000126)	-9.37e-05 (0.000178)
rule-complying attitude			-0.0656* (0.0335)		
local authorities' justice spending				0.00554 (0.00897)	
local authorities' safety spending				-0.00164 (0.0134)	
human capital					-0.00416 (0.00398)
Observations	1,848	1,848	1,447	1,570	1,310
Court FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Note: Clustered standard errors at province level in panel a; Clustered standard errors at court level in parentheses in panel b; *** p<0.01, ** p<0.05, * p<0.1. Post × Reform takes value 1 for synthetic courts experiencing mergers since 2012, and 0 otherwise. Clearance rate: justice efficiency indicator.

Figure A2: Judicial reform and clearance rate in the different stages of the Lower Criminal Court



Note: Event study estimates of the effect of decree 155/2012 on clearance rate in the Pre-trial Stage (Fig. A2 a.) and Trial Stage (Fig. A2 b.). The Pre-trial stage involves the Attorney Office and the Judge for Preliminary Investigation. It is a compulsory stage for criminal prosecution in the Italian law. The clearance rate of the Pre-trial stage is summarised by w_{Ct} in eq.(1). The Trial clearance rate is summarised by TRC_{Ct} in eq.(1). Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE event study model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate.

Table A7: Parallel trend test between treated and control courts

H_0 : linear pre-trends are parallel			
	Property crimes	Organised crimes	Clearance Rate
	(1)	(2)	(3)
<i>basic controls</i>			
F-stat	0.20	0.05	0.29
Prob > F	0.652	0.830	0.589
<i>additional controls</i>			
F-stat	0.18	0.05	0.30
Prob > F	0.672	0.829	0.587
<i>rule-complying attitude included among controls</i>			
F-stat	0.05	0.27	0.93
Prob > F	0.818	0.606	0.337
<i>local spending in safety and justice included among controls</i>			
F-stat	0.00	0.07	0.09
Prob > F	0.9657	0.7904	0.7653

Note: This test augments the model with with two more terms: one captures the differences in slopes between treatment group and control group in pre-treatment periods and the other captures the differences in slopes between treatment group and control group in post-treatment periods. Then, it performs a Wald test of the coefficient of the differences in slopes between treatment group and control group in pre-treatment periods against 0 to assess whether the linear trends are parallel before treatment. The null hypothesis of this test is that the linear trends are parallel. The top panel includes basic controls: population (logs), judges and prosecutors allocated to the Court per 10,000 inhabitants (logs), and understaffed court (dummy). Additional controls (second panel) are population density, unemployment (%). The third panel adds rule-complying attitude (logs); the fourth panel adds local spending in safety (logs), local spending in justice (logs).

Table A8: Effect of the reform on clearance rate. Difference-in-Difference estimates for all Lower Courts and several model specifications

	(1)	(2)	(3)	(4)	(5)
<i>Dep. var.: clearance rate</i>					
Post × Reform	0.101*** (0.0368)	0.106*** (0.0366)	0.121*** (0.0429)	0.108*** (0.0412)	
population	-0.167 (0.126)	-0.246* (0.128)	-0.191 (0.166)	-0.252 (0.159)	
understaffed court	0.00240 (0.0316)	0.00420 (0.0310)	0.0248 (0.0337)	0.0172 (0.0318)	
judges	-0.0296 (0.143)	-0.0346 (0.141)	-0.0560 (0.142)	-0.0303 (0.140)	
unemployment		0.00202 (0.00505)	0.0110* (0.00611)	0.00563 (0.00566)	
population density		0.000166* (8.52e-05)	-9.49e-05 (0.000151)	8.22e-05 (0.000126)	
rule-complying attitude			-0.0656* (0.0335)		
local authorities' justice spending				0.00554 (0.00897)	
local authorities' safety spending				-0.00164 (0.0134)	
Observations	1,848	1,848	1,447	1,570	
Court FE	✓	✓	✓	✓	
Year FE	✓	✓	✓	✓	

Note: Clustered standard errors at court level in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Post × Reform takes value 1 for synthetic courts experiencing mergers since 2012, and 0 otherwise.

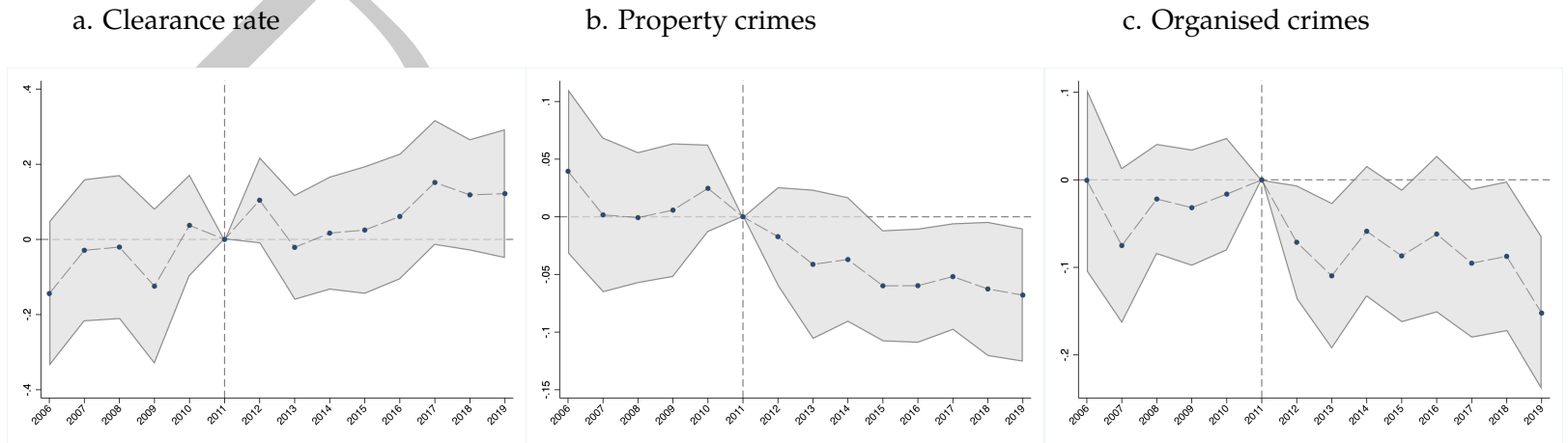
Figure A3: Event studies excluding largest Lower Courts



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Note: Event study estimates of the effect of decree 155/2012 on clearance rate (Fig. A3 a.), Property crimes (Fig. A3 b.), Organised crimes (Fig. A3 c.), excluding the largest courts (Milan, Palermo, Rome, Turin). Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE event study model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate.

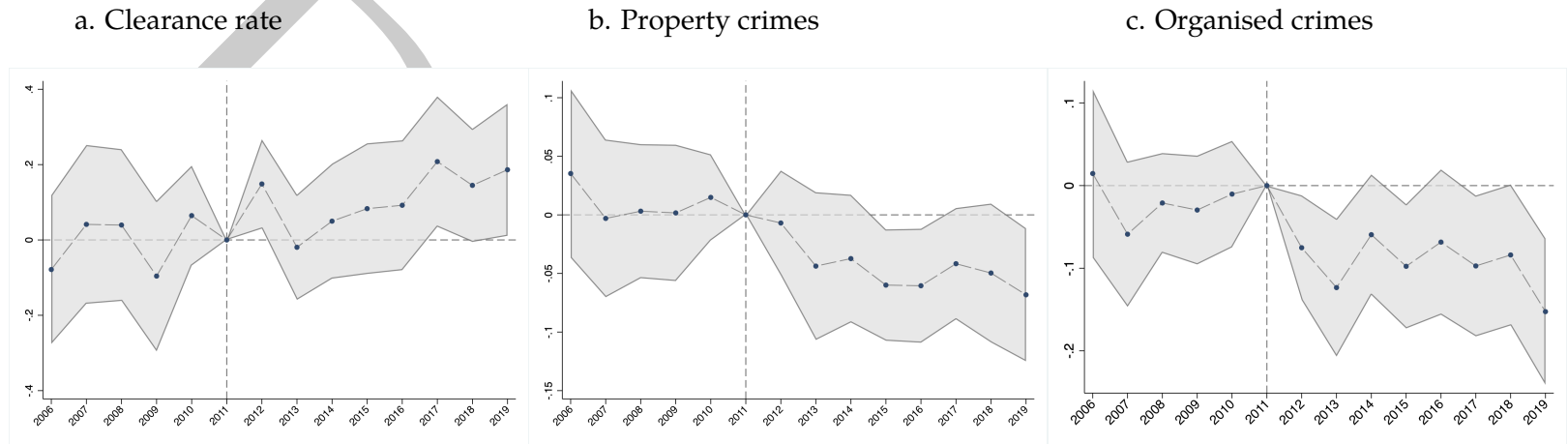
Figure A4: Event studies excluding almost treated Lower Courts



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Note: Event study estimates of the effect of decree 155/2012 on clearance rate (Fig. A4 a.), Property crimes (Fig. A4 b.), Organised crimes (Fig. A4 c.), excluding the 6 almost-treated courts. Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE event study model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate.

Figure A5: Event studies excluding Lower Courts with high turnover



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Note: Event study estimates of the effect of decree 155/2012 on clearance rate (Fig. A5 a.), Property crimes (Fig. A5 b.), Organised crimes (Fig. A5 c.), excluding the Lower Courts with high turnover. Excluded Lower Courts are: Barcellona Pozzo di Gotto, Caltagirone, Crotone, Fermo, Gela, Lagonegro, Lanciano, Paola, Rieti, Sciacca, Vibo Valentia. Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE event study model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate.

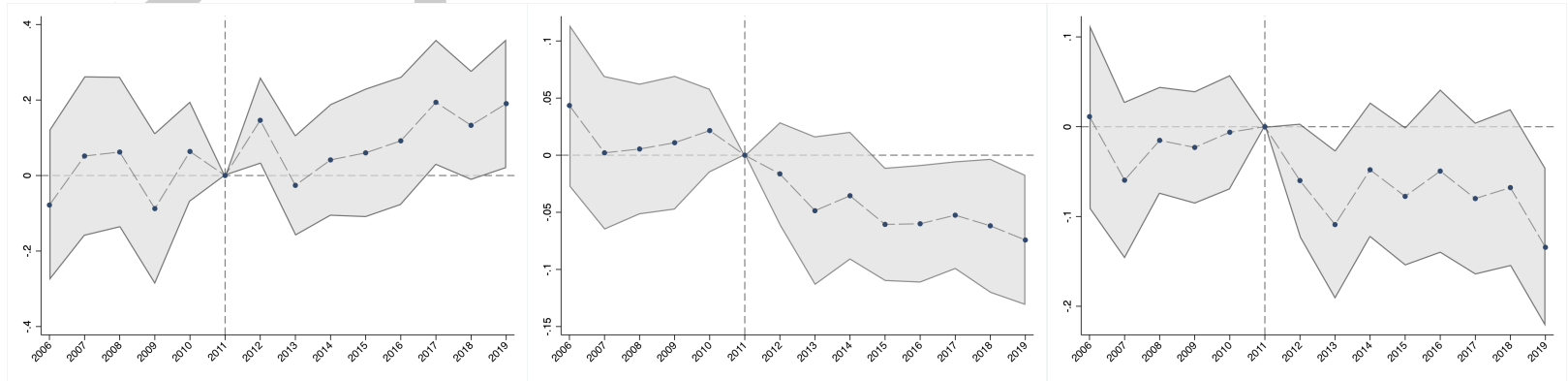
Figure A6: Event studies with different control groups

a. Clearance rate

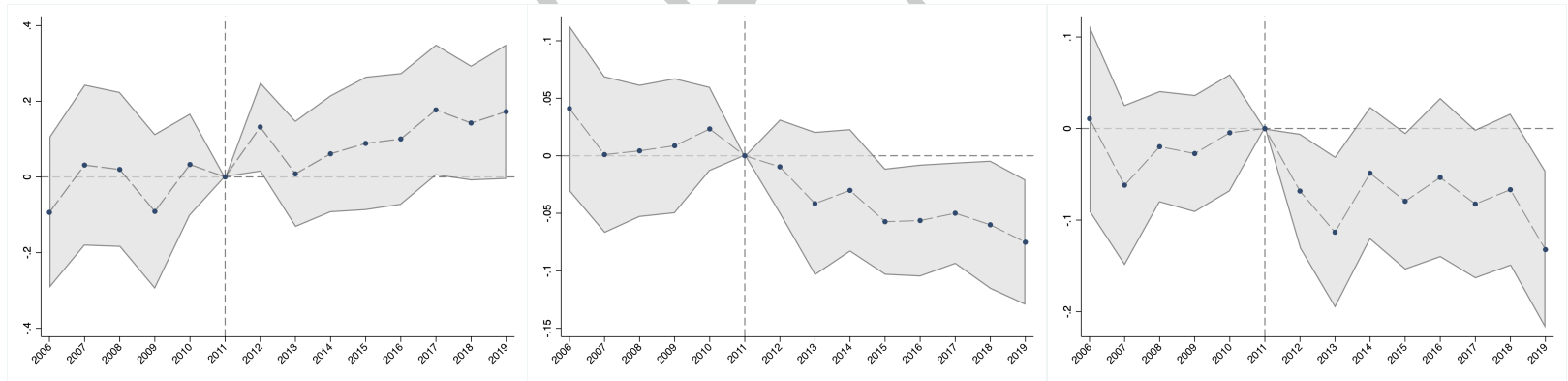
b. Property crimes

c. Organised crimes

Dropping the peripheral court if multiple courts in the same province



Dropping the urban court if multiple courts in the same province



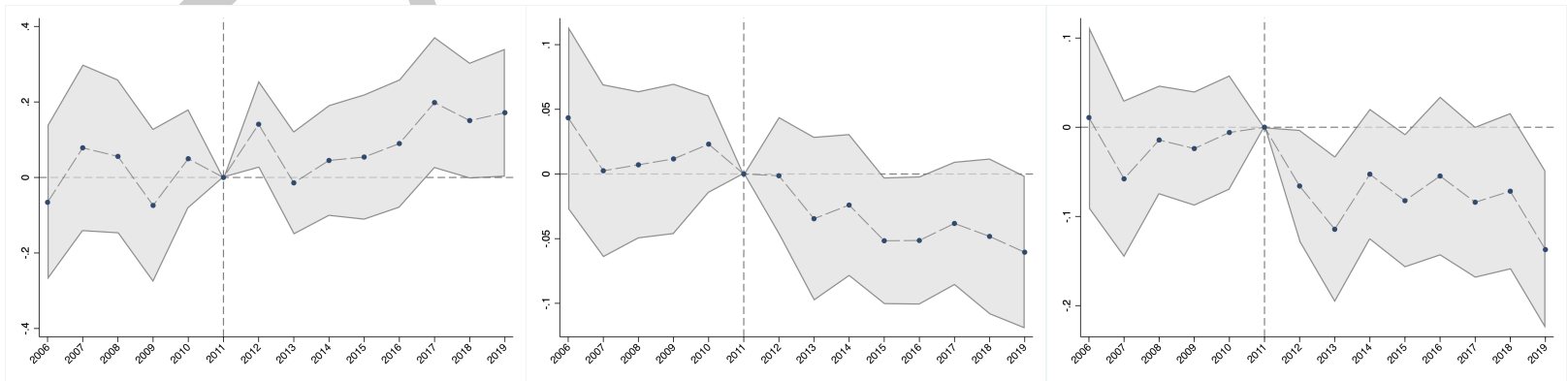
Note: Event study estimates of the effect of decree 155/2012 on clearance rate (Fig. A6 a.), Property crimes (Fig. A6 b.), Organised crimes (Fig. A6 c.), modifying the type of duplicate court discarded from the control group. Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE event study model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate.

Figure A7: Event studies with alternative approach to consolidate multiple courts in the same province

a. Clearance rate

b. Property crimes

c. Organised crimes



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Note: Event study estimates of the effect of decree 155/2012 on clearance rate (Fig. A7 a.), Property crimes (Fig. A7 b.), Organised crimes (Fig. A7 c.), modifying the approach to deal with multiple untreated courts in the same province. Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE event study model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate. Panel is province. Crime is measured at province level. For provinces with multiple untreated (treated) courts, the remaining variables are measured by averaging court-level values across the courts in the province. The 3 provinces with treated and untreated courts, are consolidated on the treated court.

Table A9: Justice efficiency and crime deterrence - CF-IV estimates with alternative approach to consolidate multiple courts in the same province

	Clearance rate	Property crimes	Organised crimes
	(1)	(2)	(3)
Post × Reform	0.106*** (0.035)		
Clearance rate		-0.330*** (0.124)	-0.684*** (0.175)
1st stage residuals		0.328*** (0.125)	0.6695*** (0.182)
Court FE	✓	✓	✓
Year FE	✓	✓	✓
Controls	✓	✓	✓
Observations	1,848	1,428	1,428
Courts/provinces	132	102	102

Note: *** p<.01, ** p<.05, * p<.1; Instrumental variable (IV) Difference-in-difference estimation. Standard errors bootstrapped 1,000 times in round brackets. Post × Reform takes value 1 for synthetic courts experiencing mergers since 2012, and 0 otherwise. Clearance rate: justice efficiency indicator. Controls: population (logs), understaffed court, judges per 10,000 inhabitants (logs), unemployment (%), population density, local spending in safety and justice per inhabitant (logs). Time span: 2008–2019. Second stage models use court-level data consolidated at province level, with the exception of the 3 provinces with treated and untreated courts, which are consolidated on the treated court. Results are confirmed with different sets of controls - estimates available upon request.

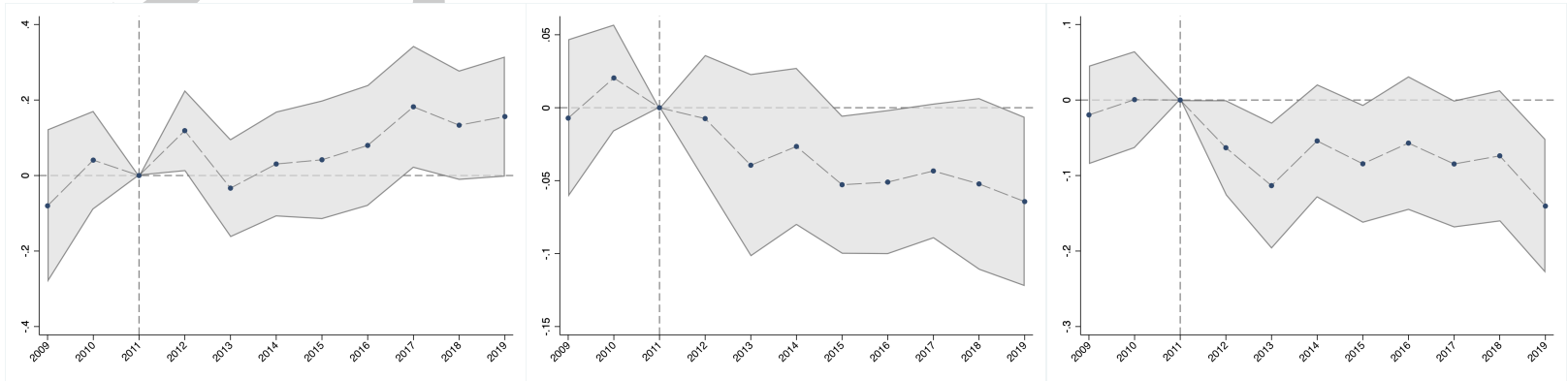
Figure A8: Event studies with alternative approach to consolidate multiple courts within the same province

a. Clearance rate

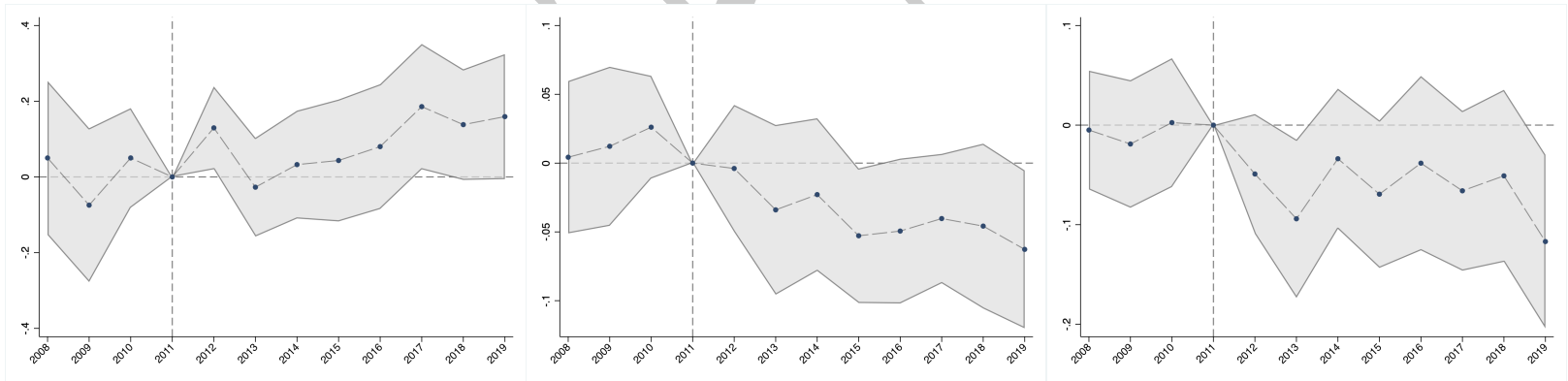
b. Property crimes

c. Organised crimes

Control for rule-complying attitude

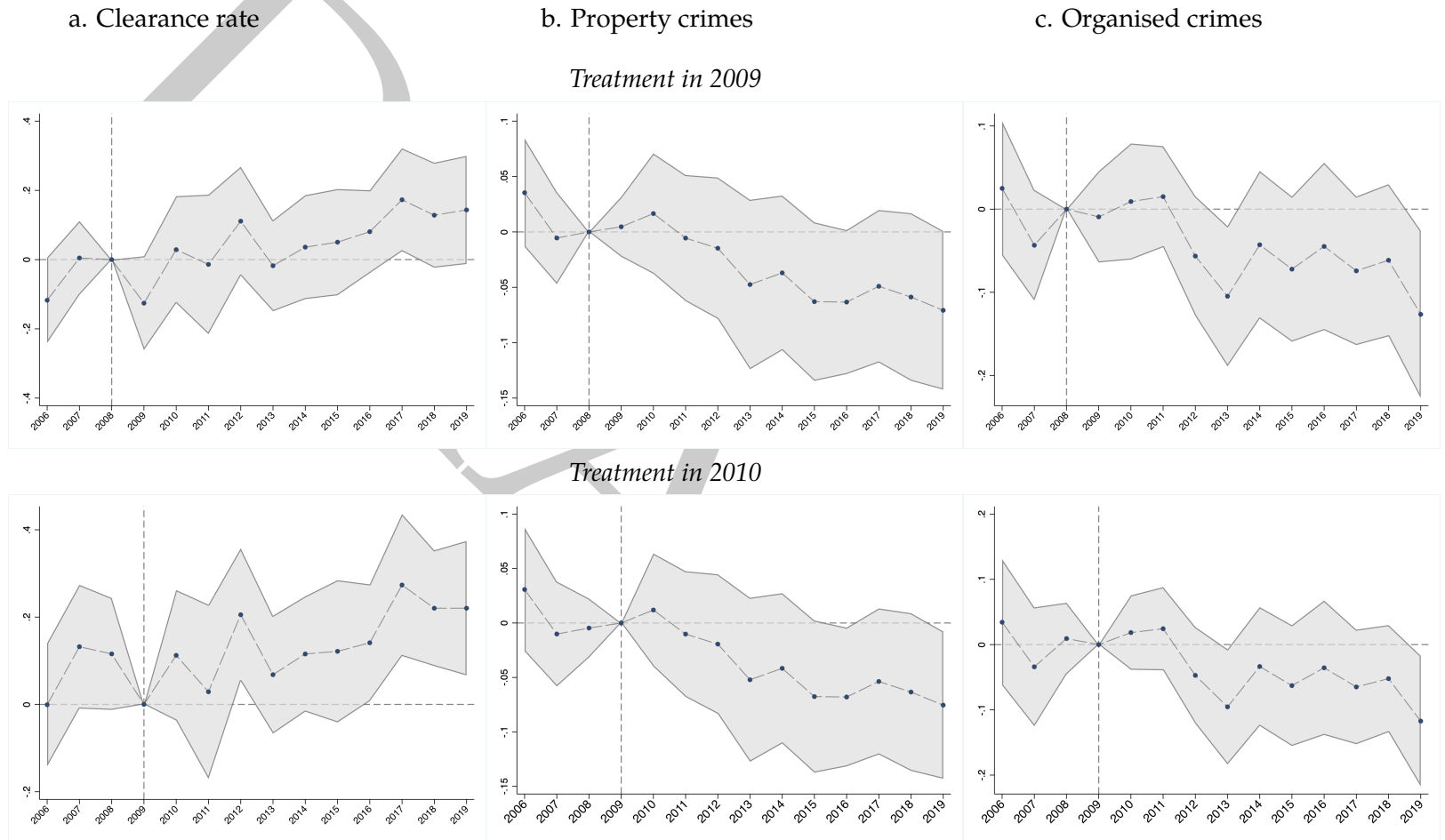


Control for local spending in safety and justice



Note: Event study estimates of the effect of decree 155/2012 on clearance rate (Fig. A8 a.), Property crimes (Fig. A8 b.), Organised crimes (Fig. A8 c.), modifying the approach to deal with multiple untreated courts in the same province. Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE event study model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate. Additional control in top figures: log rule-complying attitude; additional control in top figures: log local spending in safety and justice. Panel is province. Crime is measured at province level. For provinces with multiple untreated (treated) courts, the remaining variables are measured by averaging court-level values across the courts in the province. The 3 provinces with treated and untreated courts, are consolidated on the treated court.

Figure A9: Event studies with anticipation of treatment



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Note: Event study estimates of the effect of decree 155/2012 on clearance rate (Fig. A9 a.), Property crimes (Fig. A9 b.), Organised crimes (Fig. A9 c.), modifying the reference year. Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE event study model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate.

Table A10: Placebo experiment: fake merger prior to 2012

	Property crimes (1)	Organised crimes (2)	Clearance rate (3)
<i>Treatment in 2009</i>			
judicial reform	-0.0451 (0.0360)	-0.0451 (0.0443)	0.0907 (0.0607)
population	-0.147** (0.0658)	0.0746 (0.137)	-0.226 (0.199)
understaffed court	0.0371*** (0.0136)	0.00661 (0.0195)	0.0243 (0.0374)
judges	-0.0533 (0.0676)	-0.0796 (0.102)	-0.140 (0.166)
unemployment	-0.00684* (0.00407)	-0.00113 (0.00557)	-0.000259 (0.00648)
population density	0.001*** (7.52e-05)	-7.77e-05 (0.000326)	0.00016 (0.00015)
Observations	1,428	1,428	1,428
Court FE	✓	✓	✓
Year FE	✓	✓	✓
<i>Treatment in 2010</i>			
judicial reform	-0.0478 (0.0360)	-0.0486 (0.0402)	0.0882 (0.0625)
population	-0.145** (0.0653)	0.0770 (0.136)	-0.233* (0.127)
understaffed court	0.0369*** (0.0137)	0.00649 (0.0191)	0.00494 (0.0311)
judges	-0.0524 (0.0687)	-0.0788 (0.104)	-0.0370 (0.136)
unemployment	-0.00676* (0.00405)	-0.00104 (0.00556)	0.00199 (0.00503)
population density	0.0006*** (7.35e-05)	-7.77e-05 (0.000328)	0.00015* (8.74e-05)
Observations	1,428	1,428	1,848
Court FE	✓	✓	✓
Year FE	✓	✓	✓

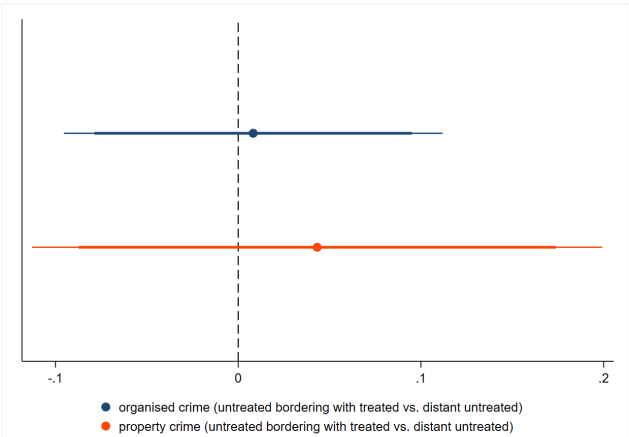
Note: Clustered standard errors at province level in parentheses in columns 1 and 2; Clustered standard errors at court level in parentheses in column 3; *** p<0.01, ** p<0.05, * p<0.1

Table A11: Granger test for anticipation of treatment

H_0 : No effect in anticipation of treatment			
	Property crimes	Organised crimes	Clearance rate
	(1)	(2)	(3)
<i>basic controls</i>			
F-stat	0.89	0.74	1.33
Prob > F	0.4920	0.595	0.257
<i>additional controls</i>			
F-stat	0.89	0.74	1.33
Prob > F	0.4918	0.596	0.258
<i>rule-complying attitude included among controls</i>			
F-stat	0.74	0.23	1.49
Prob > F	0.477	0.795	0.229
<i>local spending in safety and justice included among controls</i>			
F-stat	0.48	0.17	1.31
Prob > F	0.696	0.916	0.276

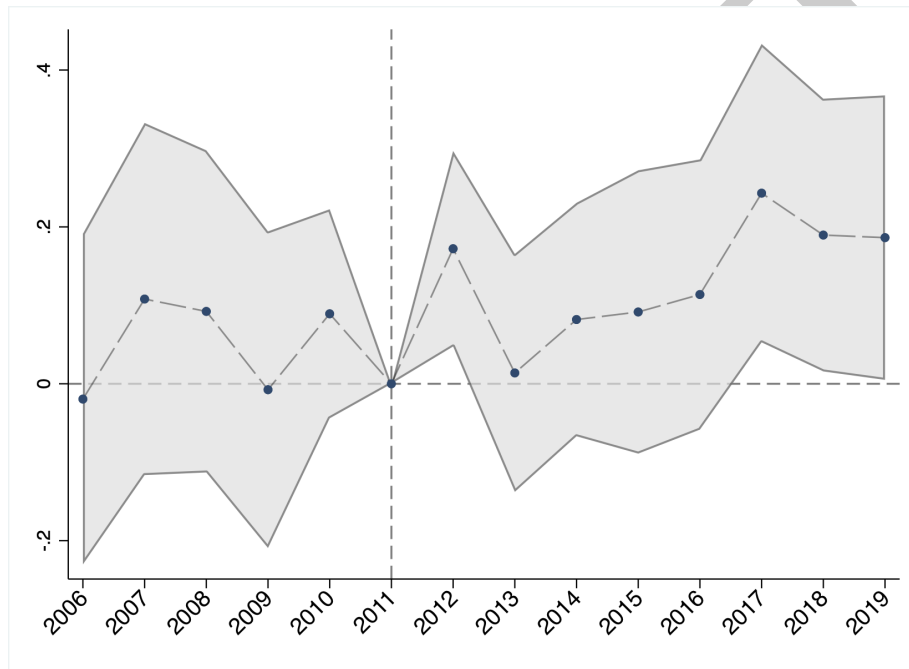
Note: The Granger-type test augments the model with counterfactual treatment-time indicators for time periods prior to the treatment to capture any potential anticipatory treatment effects. The top panel includes basic controls: population (logs), judges and prosecutors allocated to the Court per 10,000 inhabitants (logs), and understaffed court (dummy). Additional controls (second panel) are population density, unemployment (%). The third panel adds rule-complying attitude (logs); the fourth panel adds local spending in safety (logs), local spending in justice (logs).

Figure A10: Post-reform trends in property and organised crime in untreated neighbouring courts and untreated distant courts



Note: Estimates of the post-treatment difference in property and organised crime between untreated courts which border with a treated court and untreated courts which are distant. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Figure A11: Judicial reform, clearance rate and Statutes of Limitations - event study



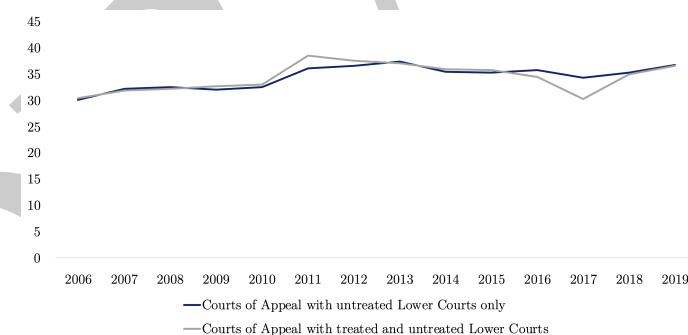
Note: Event study estimates of the effect of decree 155/2012 on the clearance rate weighted by the share of cases dismissed due to statutes of limitations. Blue dots: point estimates; gray bands: 90% confidence intervals, clustered standard errors. TWFE model controlling for log population, log judges and prosecutors allocated to the Court per 10,000 inhabitants, understaffed court, population density, unemployment rate.

Criminal justice quality pre-post reform

We use data from the Ministry of Justice to analyse the 'held appeal rate' and the 'appeal rate' for the 26 Italian Courts of Appeals. Courts of Appeals comprise several Lower Courts, and it is possible to classify between those containing both treated and untreated Lower Courts, and those with untreated Lower Courts only.³⁹ This allows to compare the 'appeal rate' and the 'held appeal rate' before and after 2012 between the two groups of Courts of Appeal to check whether there are visible changes after the introduction of the reform.

Figure A12 shows that the held appeal rate does not display relevant differences between the two types of Courts of Appeal between 2006 and 2019. The absence of relevant changes after 2012, when the reform was implemented, suggests that the revision of Lower Court geography did not lower the quality of decisions.

Figure A12: Held appeal rate of criminal cases - % sentences confirmed by Court of Appeal



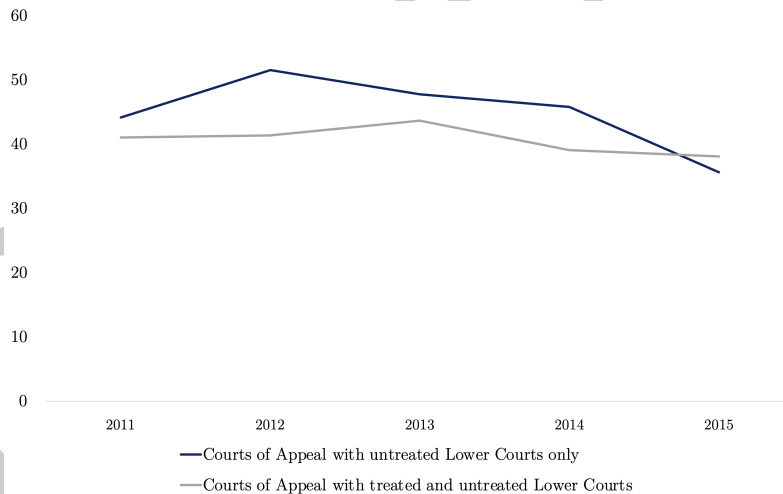
Note: Held appeal rate: percentage of appeals that confirms the decision taken by the Lower Courts. Data are from the Italian Ministry of Justice.

³⁹There are 9 Courts of Appeal containing untreated courts only: Bologna, Campobasso, L'Aquila, Lecce, Palermo, Reggio Calabria, Roma, Salerno, Trento. The remaining 17 Courts of Appeal contain both treated and untreated Lower Courts. No Court of Appeal contains treated Lower Courts only

Figure A13 shows that the appeal rate in districts with treated Lower Court remained fairly stable in the aftermath of the reform, and gradually decreased after 2013. This trend suggests that decisions to appeal the Lower Court decisions did not increase after 2012 in the areas impacted by the reform. Hence, it seems that the reform did not stimulate potential appellants to appeal, since the outcome was highly foreseeable and with a higher probability of losing the case. This suggests that the reform did not reduce the quality of Lower Court decisions and supports its deterrence effect channelled through more efficient and not-less effective justice. Since data on the 'appeal rate' are only available at the Court of Appeal level, we cannot investigate further whether the observed decrease in the appeal rate is determined by the reformed Lower Courts.

Taken together, figures A13 and A12 support the hypothesis that the deterrence effect of the reform occurred through the efficiency channel, with no effect on effectiveness.

Figure A13: Appeal rate of criminal cases



Note: Appeal rate: percentage of appealed judgments at Lower Court level for criminal cases. Data are from the Italian Ministry of Justice.

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