University of Southampton

Economics Department
University of Southampton
Southampton SO17 1BJ, UK

Discussion Papers in
Economics and Econometrics

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Alessandra Foresta, University of Southampton

Rigissa Megalokonomou, Monash University

Michael Vlassopoulos, University of Southampton

No. 2503

This paper is available on our website
http://www.southampton.ac.uk/socsci/economics/research/papers

ISSN 0966-4246

Crisis Narratives and Judicial Enforcement: Evidence from the Greek Fiscal Crisis

Alessandra Foresta[1] Rigissa Megalokonomou[2] Michael Vlassopoulos[3]

March 2026

Abstract

This paper investigates whether crisis narratives affect how the judiciary handles tax evasion. We study this question in the context of the Greek debt crisis, in which tax evasion was publicly blamed for the fiscal collapse, and judges themselves experienced substantial salary cuts as part of the resulting austerity programme. Using a novel dataset compiled from Greek Supreme Court rulings between 2006 and 2014, we compare tax evasion appeals with appeals in other serious crimes not directly related to the fiscal crisis, such as homicide and rape, in a difference-in-differences framework. We find that the probability that the Supreme Court rejects tax-evasion appeals increases by about 25 percentage points relative to these control offences after January 2010—about a 43% increase relative to the pre-crisis baseline. Effects are larger in months with greater public attention to tax evasion, as measured by Google Trends, suggesting a role for salience. Our findings suggest that crisis narratives, particularly when coupled with personal economic shocks to judges, can influence the judicial treatment of tax offences.

Keywords: economic narratives, judicial decision-making, tax evasion, financial crisis, legal institutions, difference-in-differences
JEL Classifications: D91, P16, K40, K42, H26, Z13

[1]Alessandra Foresta: University of Southampton, UK. Email: a.foresta@soton.ac.uk.; Rigissa Megalokonomou: Monash University, Australia, CEPR, and IZA. Email: rigissa.megalokonomou@monash.edu; Michael Vlassopoulos: University of Southampton, UK, IFS, and IZA. Email: m.vlassopoulos@soton.ac.uk. Rigissa Megalokonomou acknowledges financial support from the Monash Establishment Grant (No. 1755774). We thank Elias Papaioannou and seminar participants in Southampton for very useful discussions and comments.

1 Introduction

Economic crises trigger blame. Politicians, the media, and international institutions converge on narratives regarding who is responsible: corrupt politicians, reckless bankers, or tax evaders. These narratives serve political purposes: they justify austerity, legitimise policy change, and allocate the burden of adjustment (Shiller 2017, 2020). But blame narratives may do more than justify policy. If officials within state institutions internalise the prevailing narrative, this may shape how they apply existing rules to behaviours that become focal during the crisis. Whether this happens is difficult to establish, because explicit policy changes and narrative-driven behavioural shifts typically occur together.

We address this question in the context of the Greek debt crisis, in which a dominant narrative attributed the country's fiscal collapse to widespread tax evasion. The setting offers several advantages for studying the question at hand. First, the narrative was sharp and specific: tax evasion was identified as a key culprit in public debate, academic analyses, and official documentation (Commission (2010, 2012), Kaplanoglou and Rapanos (2013), and Katsimi and Moutos (2010)). Second, the Greek Supreme Court provides an institution with strong formal safeguards—constitutional tenure, random case assignment, and review limited to questions of law—that should, in principle, insulate its decisions from external pressure. Third, and critically, judges themselves bore the costs of the crisis; salaries were cut by 12 to 38% between 2010 and 2013 (Hnitidou (2016)) as part of the austerity programme that was publicly justified, in part, by discourse that emphasised tax compliance and enforcement. The officials who adjudicated tax-evasion appeals had experienced personal financial harm in a setting in which tax evasion was repeatedly singled out in public debate. If enforcement shifts even here—in an insulated apex court, without legislative change, against a specifically blamed offence—it suggests that crisis narratives are not merely reflections of public opinion or political strategy, but may also shape judicial outcomes.

We compile a novel dataset of Supreme Court criminal appeals from 2006 to 2014 and hand-code case characteristics, judicial panels, and outcomes from written decisions. Using a difference-in-differences (DiD) design, we compare rulings in tax-evasion cases with rulings in homicide and rape cases—serious crimes with no connection to the crisis narrative. The introduction of austerity and the crystallisation of the tax-evasion story provide the temporal break in 2010. Our specifications include rich fixed effects for time, court type, and individual judges. Identifying variation arises from within-judge changes in the relative treatment of tax-evasion versus control cases before and after crisis onset. Our identification assumption is that, absent the crisis narrative and

associated austerity, trends in rejection rates for tax evasion and control crimes would have evolved similarly, conditional on fixed effects.

We find a large and specific effect. After 2010, rejection rates for tax-evasion appeals—the probability that the Supreme Court upholds the lower-court conviction—rose by 25 percentage points relative to control cases. Given a baseline rejection rate of 58%, this represents a 43% increase. Event-study analysis confirms parallel pre-trends, with sharp divergence beginning in 2010. The result survives alternative control groups, exclusion of outlier judges, and permutation inference. Placebo tests using other crime types show no comparable shift.

We probe the mechanisms behind our main results. We find that the effect is larger in months when Google searches for tax evasion were elevated, suggesting that salience may shape judicial decisions. We find no evidence that results are driven by compositional changes in cases or defendants, shifts in legal representation, or changes in judicial effort. We cannot fully disentangle the narrative channel from judges' personal financial grievances, because the crisis bundled these together. Still, the offence-specificity of the effect and its association with contemporaneous salience are difficult to reconcile with a generalised post-crisis shift toward harsher judicial behaviour.

Research in narrative economics has documented that crisis stories shape household expectations (Andre et al. 2022), asset prices (Goetzmann et al. 2022), and political attitudes (Alesina et al. 2018). Whether such narratives also shape how fiscal crises change the state's treatment of tax offences has received less attention. Work on fiscal capacity typically treats courts as fixed constraints: the endpoint at which enforcement either succeeds or fails (Besley and Persson (2009, 2010)). Our results suggest otherwise; judicial treatment of tax offences responds to crisis conditions, which means that effective enforcement intensity can shift beyond any changes in statutes, prosecutorial effort, or policing. For institutional design, the implication is that formal safeguards—tenure, random assignment, salary protections—may be necessary but not sufficient when crises generate blame stories that are both publicly salient and personally consequential for those within the institution.

Our paper also contributes to a large empirical literature that shows that judicial decisions respond to external shocks, such as terrorist attacks (Brodeur and Wright 2019; McConnell and Rasul 2021); media coverage (Ash and Poyker 2024; Spirig 2023); and even weather and sports results (Eren and Mocan 2018).[4] The consistent finding is that judges are not fully insulated from their environment. Our contribution is to study a setting in which judges

[4]Related work documents responses to electoral incentives and political pressures (Abrams et al. 2023; Amaral-Garcia et al. 2009; Berdejo and Daniel L Chen 2017; Dippel and Poyker 2021; Garoupa et al. 2013); emotional and contextual shocks such as sports outcomes and local norms (Abrams et al. 2022; Daniel L Chen and Philippe 2023; Clark et al. 2018); personal experience and decision order (Daniel L. Chen et al. 2016; Nguyen et al. 2025); and broader media and salience effects (Canes-Wrone et al. 2018; Hanemaaijer et al. 2024; Lim 2015; Lim et al. 2015; Philippe and Ouss 2018).

bear substantial costs, and those costs are publicly attributed to the group before them. In our setting, judges belong to the broader group of public employees who faced substantial income losses, and the crisis narrative repeatedly singled out tax evaders as responsible for those losses. This combination—collective harm, public attribution to a specific behaviour, and affected officials adjudicating cases that involve the blamed group—has not been studied directly. For instance, in the 9/11 studies, judges were not victims of terrorism (Brodeur and Wright 2019; McConnell and Rasul 2021). In Spirig (2023)'s setting, Swiss judges bore no personal costs from asylum inflows. Whether the configuration we study matters for judicial behaviour is, ex ante, an open question. We show that it does; rejection rates in tax-evasion appeals increase sharply relative to otherwise similar serious crimes.

The paper proceeds as follows. Section 2 develops a conceptual framework to guide our empirical analysis and aid interpretation. Section 3 describes the institutional setting and data. Section 4 introduces our empirical strategy and discusses identification. Section 5 reports our main results and robustness checks, and Section 6 explores mechanisms. Section 7 concludes.

2 Conceptual Framework

Consider a Supreme Court judge who decides whether to uphold or overturn a criminal conviction on appeal. In a court of cassation, such as the Greek Supreme Court, the judge reviews the legal soundness of the conviction rather than reassessing factual guilt. We therefore let the true legal status of the conviction be denoted by $\theta \in \{0, 1\}$, where $\theta = 1$ indicates that the conviction is legally sound (no reversible error) and $\theta = 0$ that it contains reversible legal error. The judge observes the case and forms a posterior belief $p \in [0, 1]$, which represents the perceived probability that the conviction is legally sound.

The judge chooses a decision $d \in \{0, 1\}$, where $d = 1$ denotes rejection of the appeal (i.e., upholding the conviction), and $d = 0$ denotes acceptance (i.e., overturning the conviction). Incorrect decisions impose a disutility cost; if the judge rejects when $\theta = 0$ (upholding a conviction that in fact contains reversible error) or accepts when $\theta = 1$ (overturning a legally sound conviction), she incurs a loss. Let $C_j > 0$ denote judge j's personal or professional cost of legal inaccuracy.

We introduce a narrative utility benefit, denoted $\lambda_j \geq 0$, which applies in narrative-salient cases if the judge rejects the appeal. This term captures possible reputational, psychological, or career-related gains from aligning with a dominant societal narrative. It may reflect genuine belief updating; if the crisis revealed that tax evasion imposes higher social costs than previously understood, judges might rationally lower their threshold for upholding

convictions. Alternatively, it may capture psychological or reputational benefits from aligning decisions with prevailing public sentiment, independent of any informational content. Let $n_i \in \{0, 1\}$ be an indicator equal to 1 if case i is narrative-salient (for example, a tax-evasion case during the crisis period).

The judge's utility from decision d in a case with true state θ is

$$U_j(d, \theta, n_i) = -C_j \cdot \mathbb{I}[d \neq \theta] + \lambda_j \cdot \mathbb{I}[d = 1] \cdot n_i, \tag{1}$$

where $\mathbb{I}[\cdot]$ is the indicator function. The judge maximises expected utility by comparing the two actions.

The expected utility from rejecting the appeal ($d = 1$) is

$$EU_j(1) = -C_j(1 - p) + \lambda_j \cdot n_i, \tag{2}$$

while the expected utility from accepting the appeal ($d = 0$) is

$$EU_j(0) = -C_j p. \tag{3}$$

The judge rejects if $EU_j(1) > EU_j(0)$, which simplifies to

$$p > \frac{1}{2} - \frac{\lambda_j}{2C_j} \cdot n_i. \tag{4}$$

This expression shows that the threshold probability above which the judge rejects the appeal is lowered in narrative-salient cases. The effect is increasing in λ_j and decreasing in C_j, which means that judges with lower costs of legal error or higher narrative alignment are more susceptible to narrative influence. The model thus highlights an implicit trade-off between legal accuracy and narrative conformity. When $n_i = 1$, judges with sufficiently high λ_j/C_j will reject appeals even at lower posterior beliefs, which implies that narrative salience can shift the threshold at which judges reject appeals, even holding constant the legal merits of the case.

This simple model provides a basis for interpreting shifts in observed behaviour as changes in the underlying trade-off. In particular, if public narratives intensify following the crisis and increase λ_j for a subset of cases—such as tax evasion—we expect a discrete shift in the rejection threshold for those cases. Rejection rates should rise in narrative-salient offences relative to other offences, even in the absence of changes to the legal code or formal

standards of review. In the empirical analysis, we therefore examine whether rejection rates in tax-evasion cases rose after the onset of the crisis relative to serious offences not targeted by the fiscal narrative.[5]

3 Institutional Background and Data

3.1 The Greek Supreme Court

The Greek justice system follows a civil law tradition and has a multi-tiered court structure. Cases are first tried in the local court of first instance, where factual evidence is presented and a verdict rendered. These decisions may then be appealed to the Court of Appeal, which can review both the facts and the law. Final appeals are brought before the Supreme Court of Greece, known as Areios Pagos, which serves as the highest court for civil and criminal matters. The Supreme Court's jurisdiction is limited to points of law; it examines whether legal principles were correctly interpreted and applied by lower courts, but does not reassess the factual record or witness testimony.

Supreme Court cases are typically decided by a panel of five judges, drawn from a pool of roughly 60 sitting justices. A small minority of cases are reviewed by a three-judge panel (only 8%, as indicated in Table 2). Case allocation follows a randomised process designed to safeguard impartiality. Within each panel, one judge serves as the presiding member, who is responsible for chairing the hearing, and another serves as the rapporteur, who is responsible for preparing and presenting the legal memorandum. After oral and written submissions, the panel deliberates and issues a written decision, which either upholds or annuls the appellate court's ruling. These decisions are published in full and serve as precedent and guidance for lower courts.

Judges are appointed to the Supreme Court from within the judiciary, typically after decades of service in lower courts. Promotions and appointments are governed by the Supreme Judicial Council, composed of senior judges, and formalised by presidential decree. Judges enjoy life tenure and constitutional protections of independence, as set out in Article 88 of the Greek Constitution and Law 1756/1988 on the Organisation of Courts and the Status of Judges. These institutional safeguards suggest that the Supreme Court is unlikely to be influenced by political pressures, which renders it a demanding setting in which to detect the possible influence of crisis narratives.

[5]The model describes an individual judge's decision rule and abstracts from the collective decision-making of the judicial panel. Because panel rulings aggregate individual choices, changes in individual decision thresholds can plausibly translate into changes in collective outcomes.

3.2 Key Dates of the Greek Debt Crisis

The Greek financial crisis began in October 2009, when the newly elected government revised the estimated budget deficit sharply upward to 12.7% of GDP and triggered a collapse in market confidence in the sustainability of Greece's public finances. On 23 April 2010, the government formally requested financial assistance from the European Union and the International Monetary Fund (Commission 2010).

On 2 May 2010, the first bailout agreement was signed, which provided Greece with €110 billion in emergency loans, conditional on a wide-ranging fiscal consolidation program. The measures included public sector wage cuts, pension freezes, and increases in indirect taxation. Judicial salaries were reduced through Law 3833/2010, which imposed a 12% cut across all public sector earnings, including those of Supreme Court judges (European Court of Human Rights 2014; Hnitidou 2016).[6]

In July 2011, a second bailout programme was agreed. This included €130 billion in official support and introduced the Private Sector Involvement (PSI) plan, which restructured Greek sovereign debt held by private creditors (Zettelmeyer et al. 2013). The reform agenda was broadened to cover tax enforcement and judicial administration (Commission 2012). In 2012 and 2013, judicial earnings were further reduced by up to 38% relative to pre-crisis levels, through Law 4093/2012. These reductions prompted protests from judicial associations, which denounced the measures as unconstitutional and damaging to judicial independence. In September 2012, judges initiated a work stoppage and held public demonstrations to express their opposition (Reuters 2012). A 2014 decision by the Council of State ruled that the salary cuts imposed under Law 4093/2012 violated constitutional guarantees protecting the independence and remuneration of the judiciary, which led to partial salary restorations (Eurofound 2014).

Table 1 provides a timeline of the key crisis-related events.

[TABLE 1 APPROXIMATELY HERE]

3.3 Tax Evasion, Fiscal Capacity, and the Crisis Narrative

Greece has long struggled with tax collection, which has historically lagged behind that of other OECD countries. As illustrated in Figure 1, based on OECD data,[7] prior to 2010 the tax-to-GDP ratio in Greece was consistently lower than the OECD average. Greece's situation in the early 2000s was somewhat puzzling, as noted by Kaplanoglou

[6] Law 3833/2010 introduced broad austerity measures in Greece, which included reductions in public-sector compensation, and applied to members of the judiciary. Judges experienced an exogenous reduction in pay beginning in 2010, which generated a common income shock across courts.

[7] Our elaboration is based on OECD data. Source: OECD Data Archive.

and Rapanos (2013). On the one hand, the tax-to-GDP ratio had increased since 1965; however, it remained lower than that of other developed countries. The composition of tax revenue was also unusual, with a significant share derived from consumption taxes; in contrast, income taxes played a smaller role than in comparable nations. Previous research suggests that part of this imbalance can be attributed to tax evasion, as it is generally more difficult to evade indirect taxes than direct taxes (Anastasiou et al. 2020).

[FIGURE 1 APPROXIMATELY HERE]

While theoretical work and policy accounts had long emphasised tax evasion as a structural weakness of the Greek state (Commission 2010, 2012; Kaplanoglou and Rapanos 2013; Katsimi and Moutos 2010), it was only during the crisis that tax evasion moved to the centre of public discourse as a primary culprit of fiscal collapse. To assess the discussion surrounding tax evasion during the crisis, we use Google Trends to analyse how searches for "tax evasion" evolved over time and to compare these trends with those for other salient issues—such as immigration and corruption—during the same period. The data are monthly, normalised, and illustrated in Figure 2.

The trends in Figure 2 show a sharp increase in attention to tax evasion in Greece after 2010. By contrast, the two other topics remain relatively stable before and after the onset of the crisis. This pattern is consistent with tax evasion becoming a focal element of the blame narrative; the crisis is repeatedly framed as the consequence of widespread tax cheating, and austerity as the necessary response. This pattern suggests that tax evasion became a salient and persistent object of public discussion during the crisis.

[FIGURE 2 APPROXIMATELY HERE]

3.4 Data and Descriptive Statistics

We use a novel and rich case-level dataset drawn from the Supreme Court of Greece's written decisions, which are publicly available in its online archives.[8] The dataset covers appeals decided between 2006 and 2014, drawn directly from the Court's official website. The dataset contains detailed case-level information for each appeal, including key characteristics such as case type, appeal filing date, and trial date. Cases are then classified into types, such as homicide, rape, etc. We provide a list of those classifications in Appendix Table A1. The primary outcome recorded is whether the Supreme Court upheld the lower court's decision and rejected the appeal. The dataset

[8]The website of the Supreme Court in Greece is at: http://areiospagos.gr/en/INDEX.htm.

includes rich information on all parties involved—judges, defendants, plaintiffs, lawyers, and other participants. This includes the number of litigants, their names, and the roles of individuals such as the secretary, introducer, and district attorney. It also records whether the plaintiff is an individual, a government entity, a bank, or an organisation. The dataset comprises cases from 364 local courts and provides broad geographic coverage and institutional diversity.

All information was extracted directly from the Court's written decisions, which renders this a unique and comprehensive dataset for our analysis. Figure 3 presents an illustrative example of a published decision.

[FIGURE 3 APPROXIMATELY HERE]

We use these records to construct variables for our empirical analysis, including indicators for tax evasion, homicide, and rape, as well as measures of case complexity (e.g., the number of defendants) and characteristics of the judicial panel. Our main analysis focuses on tax evasion cases as well as homicide and rape cases, which serve as our primary control group given the severity of these crimes and their lack of direct connection to fiscal policy debates.

Summary statistics for the estimation sample are presented in Table 2. The sample contains 448 appeals, of which roughly 63% are tax-evasion cases, and the remainder are homicide and rape. The baseline rejection rate is 0.58, so a clear majority of convictions are upheld on cassation. Around 40% of trials occur after 2010, and the mean delay for a Supreme Court decision is about 77 days. Cases typically involve few defendants (an average of 1.4), who are predominantly male, and are heard by large panels (composed of 5 judges). Judicial and prosecutorial roles are also heavily male-dominated; about three-quarters of rapporteurs, 85% of heads of panels, and 96% of district attorneys are men. These patterns indicate that we are studying a relatively small but high-stakes set of serious appeals, in a context in which upholding lower-court convictions is more likely than not.

[TABLE 2 APPROXIMATELY HERE]

Table 3 reports rejection rates by offence type before and after 2010. Tax-evasion appeals become noticeably more likely to be rejected in the post-crisis period, with a sizeable increase in the share of cases in which the Supreme Court upholds the conviction: from 0.48 before 2010 to 0.70 afterwards —a 22–percentage–point rise that is statistically significant at the 1% level. By contrast, our main control offences, homicide and rape, show much smaller and statistically insignificant changes over the same period: rejection rates rise modestly, from 0.57 to 0.62 for homicide and from 0.69 to 0.73 for rape. Several other offence categories display substantial pre/post

shifts in rejection rates, including some financial and documentation crimes that are plausibly linked to the broader economic environment. As shown below, we exclude these offences from the main control group because they may have been directly affected by the crisis and because their raw pre/post changes raise concerns about differential trends. Overall, Table 3 suggests that tax evasion stands out as one of the few offences with a large post-2010 increase in rejection rates, while our chosen control offences provide a comparatively stable benchmark. These comparisons are purely descriptive; in the next Section, we formalise them within a DiD framework that conditions on case and judge characteristics and common time shocks.

[TABLE 3 APPROXIMATELY HERE]

4 Empirical Strategy

4.1 Baseline DiD Specification

Our empirical strategy exploits the timing of the Greek debt crisis to create variation in the environment surrounding judicial decisions. We use a DiD design to compare changes in appeal rejection rates for tax evasion cases (the treatment group) with those for homicide and rape cases (the control group) before and after January 1, 2010. Formally, we estimate

$$\begin{aligned} Rejection_i = \alpha_0 + \alpha_1 TaxEvasion_i + \alpha_2 \left(TaxEvasion_i \times Post_i\right) + \alpha_3 X_i \\ + \gamma_y + \delta_c + \zeta_s + \eta_d + \theta_h + \kappa_r + \varepsilon_i \end{aligned} \tag{5}$$

where $Rejection_i$ is an indicator equal to 1 if the Supreme Court panel rejects the appeal in case i, and $TaxEvasion_i$ is an indicator equal to 1 if the case is a tax evasion case, i.e., a treatment case. The indicator $Post_i$ equals 1 if the Supreme Court hearing in case i took place on or after January 1, 2010 and 0 otherwise. The vector X_i includes case-level and defendant-level covariates. We include year fixed effects (γ_y), court-type fixed effects (δ_c), semester-of-year fixed effects (ζ_s), day-of-week fixed effects (η_d), head judge fixed effects (θ_h), and rapporteur fixed effects (κ_r). The semester-of-year fixed effects capture seasonal patterns in case disposition (first versus second half of the calendar year) that are common across years. Because we include year fixed effects, the main effect of the post-2010 indicator is absorbed. Identification of α_2 comes from the differential change in rejection rates for tax evasion cases relative to control cases within each year after 2010, conditional on these fixed effects and controls.

Throughout, we estimate linear probability models and report heteroscedasticity-robust standard errors. In Section 5.2, we show that our main result is robust to clustering standard errors by introducer (rapporteur) judge and by the origin court of the appeal (see Table 8). For our main coefficient of interest, we also implement a permutation test that repeatedly reassigns the treatment indicator and re-estimates the DiD; the observed interaction lies in the extreme right tail of this randomisation distribution (see Figure 7).

4.2 Control Group and Identification

The choice of control group is crucial for the validity of our design. Our baseline control group consists of homicide and rape cases. These offences are serious, carry high stakes for defendants, and are not directly connected to the fiscal crisis or to tax-enforcement reforms. They therefore provide a natural benchmark for whether tax-evasion appeals were treated differently after the crisis. We arrive at this benchmark after excluding offences that are either directly tied to economic activity or display little meaningful variation in appeal outcomes.

Homicide and rape share key features with tax evasion that make them particularly suitable controls: they are grave offences with substantial stakes for defendants, so judges are likely to treat them seriously regardless of macroeconomic conditions. At the same time, they were not the subject of public narratives that linked them to the fiscal crisis and are unlikely to have been directly affected by crisis-driven tax-enforcement reforms or fiscal-policy rhetoric. We further examine the robustness of this choice in Section 5.2, where we show that our main results are similar when we (i) include additional serious offences that pass the pre-trend and pre/post-shift screens, and (ii) restrict the sample to crimes whose lower court case occurred before 2010.

For the resulting estimates to support a causal interpretation, it is essential that, in the absence of the crisis, the difference in rejection rates between the treatment and control groups would have evolved in parallel over time, conditional on covariates and fixed effects. Below, we provide event-study evidence consistent with parallel pre-trends, and in Section 5.2 we show that placebo DiD estimates using other offences, such as drug cases or calumniation, are small and statistically insignificant (see Table 6).

In Figure 4, we present event-study estimates using homicide and rape cases as the control group. We see that prior to 2010, the differences in rejection rates between tax-evasion appeals and the control offences are small and statistically indistinguishable from zero. At the bottom of Figure 4, we also present the results of the pre-trend test, which indicates that the joint test for the pre-2010 coefficients (with 2010 as the omitted year) is not significant.

[FIGURE 4 APPROXIMATELY HERE]

To further assess the reliability of the parallel trends assumption, we also estimate event studies using broader control groups of crimes (homicide, rape, immigration, injuries, environmental, and misconduct) as shown in Figures A1, A2, A3, and A4 in Section A4 of the Online Appendix. The results in Figures A1 and A2 do not exhibit convincing pre-treatment patterns. By contrast, the patterns in Figures A3 and A4 exhibit flatter pre-trends and a post-2010 break, similar to those in our baseline specification.

In our main analysis, we therefore adopt homicide and rape as our baseline control group. However, the results for the last two broader control groups (corresponding to Figures A3 and A4) are reported in Table 8 of Section 5.2, which shows effect sizes and significance levels similar to the baseline estimates.

4.3 Selection of Appeals and Case Composition

A further concern for our identification strategy is that the composition of cases that reach the Supreme Court may have changed around the crisis. Appeals are not randomly drawn from the universe of lower-court convictions: defendants and lawyers decide whether to file, and procedural rules govern which appeals are admissible. If the crisis or the tax-evasion narrative altered which tax-evasion cases were appealed—for example, if only more serious or more legally clear-cut cases reached the Supreme Court after 2010—our DiD estimates would combine changes in judicial behaviour with changes in the underlying pool of cases.

We address this concern in several ways. First, access to the cassation review is governed by national procedural rules that apply across offences. To the extent the legal environment evolved during our sample period, our design requires that any such changes do not differentially affect tax-evasion appeals relative to homicide and rape. Second, our baseline specifications include rich controls for observable case characteristics and judge fixed effects; identification, therefore, comes from within-judge changes in the relative treatment of tax-evasion versus control cases among observably similar appeals. Third, we formally test for differential shifts in observable composition by re-estimating equation (5) with various case and defendant characteristics as dependent variables. Table 5 shows that the interaction between *Tax Evasion* and *After 2010* is small and statistically insignificant for nearly all characteristics we observe, and our main results are unchanged when we control for the one marginally significant covariate.[9]

Fourth, we conduct a more stringent test by restricting the sample to cases in which the lower court decision occurred before 2010—that is, prior to the bailout, judicial salary cuts, and the peak of the tax-evasion narrative

[9]The interaction with *Company/Public Org.* in Table 5 is significant at the 10% level; the magnitude is small, and the main results are robust to its inclusion.

(see Column 5 in Table 8). In this subsample, all convictions under review were adjudicated before the crisis; only the Supreme Court's disposition occurred afterward. The estimated interaction coefficient is 0.55 (SE = 0.30, p = 0.072, N = 212), which is larger in magnitude than our baseline estimate, but less precisely estimated due to the smaller sample size. This result is difficult to reconcile with an explanation based on a legal change. The lower courts applied pre-crisis law, and the Supreme Court evaluates whether those courts committed reversible errors under the legal standards that existed at the time. What changed after 2010 is not the law under review but rather the environment in which judges apply it.

More fundamentally, for compositional change to account for our DiD estimate, the selection process must have shifted differentially for tax evasion relative to homicide and rape. Any common post-crisis change in which defendants appeal—driven, for instance, by economic hardship that reduced access to legal counsel or by general changes in court processing times—would affect all crime categories similarly and be absorbed by time fixed effects. The identifying variation arises from relative changes in tax-evasion versus control cases, so a compositional explanation requires that the crisis or the tax-evasion narrative specifically altered which tax-evasion cases reached the Supreme Court in ways that did not apply to violent crimes.

We find such stories difficult to rationalise. If the hostile climate toward tax evaders deterred marginal appeals, the surviving pool should contain defendants with stronger legal positions. Since the Supreme Court reviews only questions of law rather than factual guilt, cases with stronger legal grounds should be harder to reject, biasing us against finding increased rejection rates. If resource-constrained defendants differentially dropped tax-evasion appeals, one must explain why similar defendants facing homicide or rape convictions—equally serious charges with comparably high stakes—would not respond similarly to the same economic pressures affecting the broader population. The most natural selection stories either predict bias in the opposite direction or lack a plausible mechanism for differential effects across crime types.

We cannot definitively rule out selection on unobservable case attributes, and our estimates should therefore be interpreted as reduced-form effects on the set of appeals that reach the Supreme Court. However, three facts make a compositional explanation less persuasive: observable case characteristics remain broadly stable, the results are similar when attention is restricted to cases with pre-2010 lower-court decisions, and any selection story must explain why the composition of tax-evasion appeals changed differentially relative to homicide and rape. Taken together, these patterns make it difficult to attribute the main estimates solely to changes in which cases were appealed rather than to changes in how the Court disposed of those appeals.

5 Results

5.1 Main Effect on Rejection Rates

[TABLE 4 APPROXIMATELY HERE]

Table 4 reports our main results: estimates of equation (5), comparing tax-evasion appeals with homicide and rape appeals before and after January 1, 2010. Across all specifications, the coefficient on the interaction term *Tax Evasion* × *Post* is positive and statistically significant. In our preferred specification with full fixed effects and controls (Column (3)), the estimated coefficient is approximately 0.25. This implies that the post-crisis probability that the Supreme Court rejects a tax-evasion appeal increases by 25 percentage points relative to the control offences. Given a pre-crisis rejection rate of 0.585, this corresponds to roughly a 43% increase in the probability that a tax-evasion conviction is upheld on cassation. In an institutional setting in which the formal legal standard and access rules did not change, this 25-percentage-point increase in the probability that tax-evasion convictions are upheld suggests a substantial change in how judges apply the law in the subset of cases publicly blamed for the fiscal crisis.

The main effect is remarkably stable across specifications. Adding case and defendant-level controls has little impact on the point estimates or their precision, and the interaction term remains statistically significant when we vary the fixed-effects structure or alter the set of control offences (see Table 8 in Section 5.2). The pattern is also apparent in the raw data: rejection rates for tax-evasion appeals evolve similarly to those for homicide and rape before 2010 and then diverge sharply afterward, rising by about 22 percentage points (from 0.48 to 0.70), while the control offences exhibit only modest and statistically insignificant changes (see Table 3). Our DiD estimate of roughly 25 percentage points is slightly larger but very close in magnitude and captures this shift net of any common changes that affect homicide and rape, conditional on case and judge characteristics. Applied to the 110 post-2010 tax-evasion appeals in our sample (Table 3), it corresponds to roughly 28 additional rejections relative to a counterfactual in which tax-evasion appeals moved in parallel with homicide and rape.

The estimated magnitude is large, but not unprecedented in related work. For example, Anwar et al. (2012) finds a 16-percentage-point conviction difference between all-white juries and juries where there is at least one black person in the jury pool in two counties in Florida (USA). Similarly, Shayo and Zussman (2011) documents judicial in-group bias in Israel, showing that a claim is roughly 17% to 20% more likely to be accepted when the judge and the plaintiff share the same ethnic background. While these studies examine different institutional

settings and outcomes, they indicate that adjudicatory decisions can move substantially in response to salient social context.

As shown in Section 4, the event-study version of equation (5) (Figure 4) shows no evidence of differential pre-trends between tax evasion and the control offences. Coefficients on *Tax Evasion* interacted with pre-2010 year dummies are small and statistically indistinguishable from zero, whereas the 2010 and post-2010 coefficients jump upward and remain elevated thereafter. This supports the key identifying assumption that, absent the crisis and its associated narrative, rejection rates in tax-evasion and control cases would have followed parallel trends.

A potential concern regarding our identification strategy is that the characteristics of trials in our sample may have changed before and after 2010. Table 5 reports "balance" regressions for case and defendant characteristics. Consistent with the discussion in Section 4, we do not find evidence of systematic differential changes in observable composition for tax-evasion versus control appeals after 2010, with the exception of one characteristic that is significant at the 10% level.

[TABLE 5 APPROXIMATELY HERE]

5.2 Robustness and Falsification

We next assess whether the main estimated effect is robust to a wide range of specification checks. Placebo DiD estimates that replace tax evasion with offences not central to the crisis narrative (calumniation, drugs) are small and statistically insignificant, which suggests that the post-crisis change in rejection rates is specific to tax evasion rather than a general tightening across all crime categories (Table 6). The results are also insensitive to alternative choices of the crisis date (Table 7), to changes in the definition of the control group (Table 8), and to trimming outliers or using randomisation inference (Table 9).

Placebo Test In Table 3, we observe multiple cases with statistically significant differences between the periods before and after 2010. To address the possibility that our findings in Table 4 are influenced by a specific effect on tax evasion, we replicate the analysis using calumniation and drug-related offences as placebo tests for tax evasion and present the results in Table 6. The selection of these cases is based on the statistically significant difference found in Table 3 combined with the fact that the pre-trend joint test for these two variables is (almost) always non-significant, with only one minor exception in Column (4) of Table 6 (bottom panel). Conversely, the pre-trend joint test reveals statistically significant effects for fraud and forgery, which raise concerns about violation of parallel trends; hence, we omit these offences from the placebo analysis.

If the crisis had induced a general shift in Supreme Court harshness across serious crimes, we would expect sizeable and statistically significant interaction terms for these placebo offences as well. Instead, the interaction terms for calumniation and drugs are small and statistically insignificant across all specifications in Table 6. This pattern is consistent with the post-crisis change in rejection rates being specific to tax-evasion appeals, rather than reflecting a broad tightening of cassation outcomes across all offence categories.[10]

[TABLE 6 APPROXIMATELY HERE]

Different Dates In Table 4, we use January 1, 2010, as the baseline threshold date, which reflects the onset of the Greek sovereign debt crisis and precedes the implementation of major austerity measures that affect the public sector, including the judiciary. As detailed in Section 3.2, the crisis unfolded through a sequence of distinct political, fiscal, and institutional events rather than at a single point in time. To evaluate the sensitivity of our results to the choice of threshold, we replicate the analysis using three alternative dates that capture different stages of this process. January 1, 2009, serves as a pre-crisis benchmark before the public disclosure of Greece's fiscal imbalances. October 4, 2009, corresponds to the parliamentary elections and the subsequent revision of the budget deficit, which triggered a sharp deterioration in market conditions. Finally, May 2, 2010, marks the signing of the first EU–IMF bailout agreement, which introduced substantial public-sector wage cuts; these include reductions in judicial compensation under Law 3833/2010. The results of these robustness exercises are reported in Table 7.[11]

[TABLE 7 APPROXIMATELY HERE]

The results shown in Columns (2) and (3) of Table 7 indicate that before 2010, there were no evident effects of the financial crisis on tax evasion, since the interaction coefficients were positive but not statistically significant. In contrast, the interaction term after the first bailout (as seen in Column (4) of Table 7) is both positive and statistically significant, and its magnitude is very similar to our baseline estimate. Also, the pre-trend p-values presented in the bottom panel of Table 7 are consistently non-significant, which supports the view that pre-crisis differences between tax-evasion and control offences are flat and that the main break occurs around the 2010 bailout and associated austerity measures.

[10] Appendix Table A3 presents the corresponding specifications with the full set of controls included.
[11] Appendix Table A4 presents the corresponding specifications with the full set of controls included.

Alternative Specifications, Outliers, and Permutation Test We next assess the robustness of the findings presented in Table 4 along four dimensions: alternative specifications (Table 8), outlier trims (Table 9), leave-one-out sensitivity to influential judges (Figures 5 and 6), and permutation-based inference (Figure 7).

First, we examine sensitivity to control-group choice, fixed effects, sample restrictions, and inference in Table 8. Following the event-study evidence, we replicate the analysis using broader control groups. Column (2) adds immigration cases to homicide and rape, while Column (3) further adds injuries. In Column (4), we use judge fixed effects rather than head-judge and introducer fixed effects. In Column (5), we restrict the sample to cases in which the lower-court decision occurred before 2010; this check is necessarily limited to offences for which lower-court dates are observed, which in our data are homicide and tax evasion. Columns (6) and (7) report alternative inference by clustering standard errors at the lower-court and introducer (rapporteur) levels, respectively.[12]

[TABLE 8 APPROXIMATELY HERE]

The interaction between tax evasion and the period after 2010, as shown in Table 8, is positive in every column and remains statistically significant under broader control groups and under alternative clustering schemes. Precision is broadly stable across specifications, with the only material loss occurring in Column (5), where the restriction to cases with pre-2010 lower-court decisions sharply reduces N; in that reduced sample, the estimate remains positive but is less precisely estimated. The pre-trend test values in the bottom panel are consistently non-significant. This indicates that our main findings are robust to the choice of control group, the fixed-effects structure, the sample restriction based on lower-court timing, and alternative inference procedures.

Second, we assess sensitivity to outliers by examining the influence of extreme values in judges' and trial characteristics, as shown in Table 9. To do this, we removed the top and bottom 1% of each variable and replicated the main analysis.[13] Overall, the findings remained positive and statistically significant across all specifications, which indicates that the presence of specific outliers does not affect our conclusions.

[TABLE 9 APPROXIMATELY HERE]

A natural concern in a small sample is that our results might be driven by a single particularly harsh or lenient judge. To address this, we conduct a leave-one-out sensitivity analysis in which we iteratively re-estimate the main specification, excluding, one at a time, each head judge and each introducer (rapporteur) judge. Figures 5 and 6

[12] In Appendix Table A5, we present the corresponding results, including the full set of controls.
[13] Results with all controls included are presented in Appendix Table A6.

plot the resulting coefficients on *Tax Evasion*×*After 2010*. Point estimates remain tightly clustered around the baseline value, and in all but two cases the coefficient remains statistically significant at conventional levels in Figure 5. In those two cases, the point estimate remains close to the baseline, and the loss of significance reflects a modest reduction in precision rather than a meaningful change in magnitude. This pattern indicates that the post-crisis shift in the treatment of tax-evasion appeals is a general feature of the data rather than an artefact of a single influential judge.

[FIGURE 5 and FIGURE 6 APPROXIMATELY HERE]

Finally, we assess the statistical significance of the estimated interaction under placebo assignments using a permutation test (Figure 7). We randomly reassign the "tax evasion after 2010" treatment indicator within the sample and re-estimate the interaction coefficient under these placebo assignments over 1,000 iterations.[14] This randomisation inference test for the benchmark model (as shown in Column (3) of Table 4) reveals that the estimated coefficient lies in the extreme right tail of the resulting randomisation distribution, as illustrated in Figure 7.

[FIGURE 7 APPROXIMATELY HERE]

Overall, the placebo tests, alternative timing assumptions, specification changes, outlier trims, leave-one-out sensitivity, and permutation inference all point in the same direction: the roughly 25-percentage-point increase in rejection rates for tax-evasion appeals appears to be a stable and statistically robust feature of the data.

6 Potential Mechanisms

In this section, we explore three potential mechanisms that may explain our findings. First, we examine whether periods of greater public attention to tax evasion are associated with stronger differential effects on tax-evasion appeals, using Google Trends data. Second, we test whether changes in judges' effort could account for our results, using trial duration as a proxy for such changes. Third, we investigate whether changes in lawyers' characteristics may account for the findings.

Taken together, the evidence is most consistent with a salience-based narrative channel, and provides little support for alternative explanations based on changes in judicial effort or legal representation.

[14] For this analysis, we use the *ritest* command introduced by Heß (2017).

6.1 Narrative on Tax Evasion

To investigate the potential impact of the tax-evasion narrative, we analyse whether there is a positive correlation between Google Trends searches for tax evasion and the rejection rates in tax evasion cases within our sample. We estimate a version of equation (5) on the post-2010 sample, including Google Trends for "tax evasion" and its interaction with the tax evasion indicator. The interaction coefficient captures whether the gap in rejection rates between tax-evasion and control cases widens in months when search intensity is higher.

We use Google Trends as a high-frequency proxy for public attention to tax evasion, following the logic of Choi and Varian (2012).[15] This analysis focuses on the post-2010 period, since the Google Trends data show minimal activity prior to that year, as shown in Figure 2.[16] We use Google Trends data for tax evasion from the month before the Supreme Court hearing, the month of the hearing, and the month after the hearing. Our findings are presented in Table 10.[17]

[TABLE 10 APPROXIMATELY HERE]

The differential effect on rejection rates is statistically significant only when Google searches for tax evasion are measured contemporaneously with the hearing month, while the lead estimates are close to zero and the lagged estimates are positive but imprecisely estimated. Quantitatively, the contemporaneous interaction coefficient is about 0.006 (Columns (4)–(6)), which implies that a 10-point increase in the Google Trends index is associated with roughly a 6-percentage-point higher rejection rate for tax evasion relative to control offences. In the bottom panel, we test equality between the contemporaneous interaction and the lead and lag interactions. We do not reject equality between the contemporaneous and lagged coefficients. We also do not reject equality between the contemporaneous and lead coefficients at conventional levels, although in one specification the difference is marginal ($p = 0.063$). This pattern is consistent with higher salience of tax evasion being associated with harsher treatment of tax-evasion appeals.

Although this evidence is necessarily correlational and not definitive, it is difficult to reconcile with a purely mechanical post-2010 shift in the treatment of tax-evasion cases. Instead, it points to a role for the intensity of public discussion of tax evasion in shaping judicial outcomes during the crisis period. An alternative explanation based on crisis severity alone, for instance, worsening economic conditions driving harsher judicial behaviour

[15] Search intensity has been used as a proxy for attention and attitudes in a range of contexts (e.g., Corbi and Picchetti (2020), Stephens-Davidowitz (2014), Stow and Bason (2021), and Vitt et al. (2018)).

[16] We provide results for the entire sample in Appendix Table A8.

[17] In Appendix Table A7, we include results with all controls displayed.

across the board, cannot account for the offence-specificity of the pattern, since control offences adjudicated in the same months show no comparable shift.

6.2 Difference in Judges' Effort

One alternative interpretation is that the crisis and the resulting wage cuts affected how judges processed tax-evasion appeals, even without changing their substantive orientation toward these cases. If these cases became more complex, sensitive, or time-consuming after the bailout, judges may have given them less attention than other types of cases. This could lead to more automated or default rejection decisions, even without any change in underlying preferences. Although this mechanism is different from the narrative channel presented in Section 6.1, the two channels are not mutually exclusive.

To explore this possibility, we analyse two measures of trial duration as imperfect proxies for judicial effort and attentiveness across different case types before and after the crisis. Results are presented in Table 11.[18] Across specifications, the interaction terms are generally not statistically significant, with one exception in Column (2), which is only marginally significant. This suggests that there are no systematic differences in trial length for tax-evasion versus control cases after 2010 that could plausibly account for the large change in rejection rates.

[TABLE 11 APPROXIMATELY HERE]

While we cannot rule out more subtle shifts in how judges process tax-evasion appeals, the absence of clear changes in observable timing patterns provides little support for an explanation based on reduced effort in tax-evasion cases after the crisis.

6.3 Differences in Lawyers' Characteristics

The financial crisis may have affected defendants' ability to hire multiple lawyers or prompted changes to their legal teams. Research indicates that lawyers' characteristics can influence their behaviour (or judges' behaviours) during trials and thereby affect trial outcomes. For instance, some evidence suggests that female attorneys in the United States are more likely to be interrupted earlier and face longer speeches from U.S. Supreme Court justices (Patton and Smith 2017). Other studies, particularly those that focus on public defenders in the U.S., show that a higher workload may contribute to increased incarceration rates (Gottlieb and Arnold 2021). In this section, we examine whether lawyers' characteristics may help explain our main findings.

[18] In Appendix Table A9, we provide results with all controls included.

In Table 12, we examine the impact of tax evasion during the crisis on the number of lawyers (Columns (1)-(3)), the average number of lawyers per defendant (Columns (4)-(6)), and the share of male lawyers (Columns (7)-(9)). The total number of lawyers and the average number of lawyers per defendant can serve as imperfect proxies for the income available to defendants and the case workload for different lawyers. We exclude from this analysis trials in which no lawyers are reported.[19]

Overall, the interaction terms are generally not statistically significant, which suggests that the number of lawyers or the gender distribution of lawyers does not appear to influence our findings significantly.

[TABLE 12 APPROXIMATELY HERE]

Combined with the evidence on judicial effort, these results suggest that our main effects are unlikely to be driven by crisis-induced changes in case processing or defence quality. The only mechanism for which we find consistent suggestive support is variation in the salience of the tax-evasion, as captured by contemporaneous Google search intensity.

7 Conclusion

This paper examines whether crisis-era blame narratives can influence how courts enforce tax law, even in an apex court with strong formal safeguards against political pressure. We study this question in the context of the Greek debt crisis (2009–2018), when tax evasion became a focal object of public blame for the fiscal collapse and judges, like other public servants, faced substantial wage cuts under austerity. We find that, following the bailout and wage cuts, the probability that the Supreme Court rejects a tax-evasion appeal increases by about 25 percentage points, that is, roughly 43% relative to the pre-crisis baseline, while rejection rates for homicide and rape remain stable.

Our findings document a shift in appellate outcomes, not a deviation from legally correct decision-making. The evidence is consistent with judges updating beliefs about social costs, correcting prior leniency, or responding to narrative salience. Our research design identifies the shift but cannot determine which interpretation is correct. What the results establish is that judicial outcomes are responsive to crisis conditions in ways that formal institutional safeguards do not fully prevent.

Our findings emerge from a setting in which crisis narratives were sharp, judges personally bore the costs attributed to the blamed group, and strong formal safeguards were in place. This makes the setting informative:

[19] In Table A10 in Section A3, we present the results with all controls included.

if judicial decisions concerning the treatment of tax evasion shift even in an apex court designed for insulation, settings with weaker protections would plausibly show larger effects. Which features of the Greek configuration are necessary, including whether personal stakes without a public narrative would suffice, or vice versa, remains an open question. But the broader implication is clear: formal institutional design may protect against explicit political interference yet remain permeable to subtler influences that operate through shared narratives and common material experiences.

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8 Tables

Table 1: Key Dates of the Greek Debt Crisis and Judicial Salary Cuts

Date	Event
Oct 2009	Greece revises budget deficit to 12.7% of GDP, triggering market panic.
Apr 23, 2010	Greece formally requests financial assistance from EU and IMF.
May 2, 2010	First Bailout Agreement (€110 billion) signed. Judicial salaries cut under legislation (Law 3833/2010).
July 21, 2011	Second Bailout (€130 billion) includes private-sector debt restructuring.
2012–2013	Additional wage cuts for judges enacted; protests by judicial associations.
Jan 25, 2015	Syriza elected on anti-austerity platform.
July 13, 2015	Third Bailout (€86 billion) agreed after referendum.
Aug 20, 2018	Greece exits formal bailout programs. Fiscal oversight continues.

Notes: The table summarises major events of the Greek financial crisis and highlights key legislative episodes during which judicial salaries were reduced. This includes the legislation related to Law 3833/2010 and subsequent austerity measures enacted in 2012–2013.

Table 2: Summary Statistics for the Main Sample

	N	Mean	SD	Min	Max
Trial Characteristics					
Rejections	448	0.58	0.49	0	1
Tax Evasion	448	0.63	0.48	0	1
Homicide	448	0.30	0.46	0	1
Rape	448	0.06	0.25	0	1
After 2010	448	0.40	0.49	0	1
After 2009	448	0.54	0.50	0	1
After Election 2009	448	0.44	0.50	0	1
After Bailout 2010	448	0.34	0.47	0	1
Delay Days	448	77.28	80.14	0	506
Male District Attorney	448	0.96	0.21	0	1
Number Lawyers	448	0.92	0.63	0	7
Share Male Lawyers	448	0.77	0.42	0	1
Share Female Lawyers	448	0.06	0.23	0	1
Judges' Characteristics					
Large Committee	448	0.92	0.27	0	1
Share Female Judges	448	0.28	0.33	0	1
Share Male Judges	448	0.72	0.33	0	1
Average Judges Experience	448	2.00	1.18	0	6
Male Introducer Judge	448	0.75	0.43	0	1
Head Experience	448	3.27	2.08	0	9
Male Head Judge	448	0.85	0.35	0	1
Introducer Experience	448	1.92	1.55	0	9
Head is Introducer	448	0.00	0.07	0	1
Defendants' Characteristics					
Company/Public Org.	448	0.05	0.21	0	1
Number Defendants	448	1.38	1.13	0	10
Share Male Defendants	448	0.84	0.35	0	1
Share Female Defendants	448	0.13	0.32	0	1
Missing Defendants	448	0.00	0.07	0	1
Types of crimes:					
Tax Evasion	✓	✓	✓	✓	✓
Homicide	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓

Notes: The bottom part of the table shows the sample's composition by crime type. *Large Committee* is a binary indicator that takes the value 1 if the trial was heard by a committee of five judges; *Delay Days* is measured as the number of days between the hearing date and the issuance of the decision; *Average Judges' Experience* is the average experience of the judging committee, measured in years; *Share* indicates the proportion; *Introducer* is the rapporteur judge and *Head* is the judge head of the committee; *Missing Defendants* is the proportion of defendants for which we have no information about gender.

Table 3: Appeal Rejection Rates by Offence Type Before and After 2010

	Before 2010		After 2010		
	Nr. of cases	Avg. Rejection	Nr. of cases	Avg. Rejection	Differences
	(1)	(2)	(3)	(4)	(2)-(4)
Control Group:					
Homicide	82	0.57	66	0.62	-0.048
Rape	16	0.69	15	0.73	-0.046
Injuries	46	0.57	58	0.64	-0.073
Drugs	85	0.72	48	0.52	0.197**
Environmental	42	0.55	54	0.61	-0.063
Misconduct	13	0.46	36	0.44	0.017
Calumniation	93	0.46	64	0.66	-0.194**
Immigration	20	0.75	32	0.75	0.000
Relevance	33	0.03	53	0.02	0.011
Guns	164	1.00	80	1.00	0.000
Not Appear	280	0.57	174	0.60	-0.026
Bad Check	109	0.61	60	0.58	0.022
Financially Related	36	0.44	29	0.48	-0.038
Fraud	73	0.78	54	0.61	0.170**
Forgery	116	0.74	74	0.53	0.214***
Theft	87	0.77	78	0.69	0.078
International Transfer	28	0.64	27	0.70	-0.061
Miscellaneous	201	0.87	200	0.83	0.031
Treated Group:					
Tax Evasion	184	0.48	110	0.70	-0.222***

Notes: The table reports average appeal rejection rates before and after 2010 across different types of crimes. Columns (1) and (2) show the number of cases and the average rejection rate for cases before 2010, while Columns (3) and (4) report the corresponding figures for cases after 2010. The last column reports the difference in average rejection rates between the pre- and post-2010 periods, computed as Column (2) minus Column (4), along with p-values from two-sided t-tests of equality of means. The treated group consists of tax evasion cases; all other crime categories serve as the control group. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Crime classifications follow the definitions in Section 3.4 and Table A1 of the Online Appendix.

Table 4: Baseline DiD Estimates of Appeal Rejection Rates

	Rejection		
	(1)	(2)	(3)
Tax Evasion	-0.048	-0.035	-0.042
	(0.10)	(0.11)	(0.12)
Tax Evasion X After 2010	0.167*	0.238**	0.250**
	(0.10)	(0.11)	(0.11)
Observations	448	448	448
DV mean	0.585	0.585	0.585
INDV mean	0.632	0.632	0.632
Pre-trend p-value	0.534	0.563	0.482
Control Var.			✓
Control groups:			
Homicide	✓	✓	✓
Rape	✓	✓	✓
Fixed effects:			
Year FE	✓	✓	✓
Court Type FE	✓	✓	✓
Semester FE	✓	✓	✓
Week day FE	✓	✓	✓
Head Judge FE		✓	✓
Introducer FE		✓	✓

Notes: The table reports DiD estimates examining treatment–control differences in appeal rejection rates before and after 2010. The unit of observation is the case. The dependent variable is a binary indicator equal to 1 if the appeal is rejected. *Tax Evasion* is a binary indicator for cases involving tax evasion, and *After 2010* is a binary indicator equal to 1 if the hearing is held after January 1, 2010. The interaction term *Tax Evasion* × *After 2010* captures the differential change in rejection rates for tax-evasion cases relative to the control offences after 2010. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, and day-of-week fixed effects. Columns (2) and (3) additionally include head judge and introducer fixed effects. Control variables, included where indicated, comprise committee size, average judicial experience of the committee, indicators for companies or public organisations as plaintiffs, the number of defendants, the gender composition of defendants, the share of male judges, and indicators for whether the head judge is also the introducer. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. Pre-trend p-value reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Balance Tests of Judge and Trial Characteristics Before and After 2010

	Tax Evasion		Tax Evasion X After 2010				Control Groups:	
	b	se	b	se	Obs.	Fixed	Homicide	Rape
	(1)	(2)	(3)	(4)	(5)	Effects		
Judges' Characteristics								
Large Committee	0.141***	(0.05)	-0.043	(0.03)	448	✓	✓	✓
Share Male Judges	-0.047	(0.06)	0.010	(0.05)	448	✓	✓	✓
Share Female Judges	0.047	(0.06)	-0.010	(0.05)	448	✓	✓	✓
Male Head Judge	0.072	(0.06)	-0.043	(0.06)	448	✓	✓	✓
Male Introducer Judge	-0.017	(0.07)	0.056	(0.08)	448	✓	✓	✓
Head Experience	-0.110	(0.30)	0.219	(0.29)	448	✓	✓	✓
Introducer Experience	-0.135	(0.27)	0.415	(0.30)	448	✓	✓	✓
Average Judges Experience	0.173	(0.18)	-0.307	(0.19)	448	✓	✓	✓
Trial Characteristics								
Male District Attorney	0.056	(0.05)	-0.061	(0.04)	448	✓	✓	✓
Number Defendants	-0.351	(0.37)	0.056	(0.29)	448	✓	✓	✓
Share Male Defendants	-0.180***	(0.06)	0.087	(0.07)	448	✓	✓	✓
Share Female Defendants	0.173***	(0.05)	-0.100	(0.06)	448	✓	✓	✓
Company/Public Org.	0.117**	(0.05)	-0.064*	(0.04)	448	✓	✓	✓

Notes: The table reports balance tests for judicial and trial characteristics across crime types and time periods. Each row corresponds to a separate OLS regression in which the listed characteristic is the dependent variable. Columns (1) and (2) report the estimated coefficients and robust standard errors for the indicator *Tax Evasion*, while Columns (3) and (4) report the coefficients and standard errors for the interaction *Tax Evasion* × *After 2010*. All regressions are estimated at the case level. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, and day-of-week fixed effects. Control groups include homicide and rape cases, as indicated. The number of observations is reported in Column (5). *Large Committee* is a binary indicator equal to 1 for trials heard by a five-judge panel, and *Experience* is measured in years. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Placebo Tests Based on Alternative Crime Types

	Rejection					
	(1)	(2)	(3)	(4)	(5)	(6)
Calumniation	-0.111	-0.031	-0.002			
	(0.08)	(0.10)	(0.11)			
Calumniation X After 2010	0.154	0.010	-0.006			
	(0.11)	(0.13)	(0.13)			
Drugs				0.052	0.063	0.058
				(0.13)	(0.15)	(0.16)
Drugs X After 2010				-0.216	-0.221	-0.265
				(0.13)	(0.16)	(0.17)
Observations	316	316	316	287	287	287
DV mean	0.576	0.576	0.576	0.624	0.624	0.624
INDV mean	0.475	0.475	0.475	0.436	0.436	0.436
Pre-trend p-value	0.794	0.492	0.388	0.0867	0.123	0.141
Control Var.			✓			✓
Control groups:						
Homicide	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓
Fixed effects:						
Year FE	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓
Head Judge FE		✓	✓		✓	✓
Introducer FE		✓	✓		✓	✓

Notes: The table presents placebo exercises in which we use *Calumniation* (Columns 1–3) and *Drugs* (Columns 4–6) cases as the treatment group. Each column reports estimates from OLS regressions at the case level. The dependent variable is *Rejection*; a binary indicator that takes the value of 1 if the appeal is rejected. *Calumniation* and *Drugs* are binary indicators for the corresponding crime types, and *After 2010* is a binary indicator that takes the value of 1 if the hearing takes place after January 1, 2010. The interaction terms *Calumniation* × *After 2010* and *Drugs* × *After 2010* capture differential changes in rejection rates for these placebo treatment groups after 2010. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, and day-of-week fixed effects. Columns (2), (3), (5), and (6) additionally include head judge and introducer fixed effects. Control variables, included where indicated, include committee size, average judicial experience of the committee, indicators for companies or public organisations as plaintiffs, the number of defendants, the gender composition of defendants, the share of male judges, an indicator for a male district attorney, and an indicator for whether the head judge is also the introducer. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Calumniation* and *Drugs* in the sample. Pre-trend p-value reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Sensitivity to Alternative Crisis Cutoff Dates

	Rejection			
	(1)	(2)	(3)	(4)
Tax Evasion	-0.042	0.027	0.005	-0.035
	(0.12)	(0.13)	(0.12)	(0.12)
After Election 2009			0.350	
			(0.27)	
After Bailout 2010				-0.248
				(0.20)
Tax Evasion X After 2009		0.116		
		(0.12)		
Tax Evasion X After Election 2009			0.161	
			(0.11)	
Tax Evasion X After 2010	0.250**			
	(0.11)			
Tax Evasion X After Bailout 2010				0.264**
				(0.12)
Observations	448	448	448	448
DV mean	0.585	0.585	0.585	0.585
INDV mean	0.632	0.632	0.632	0.632
Pre-trend p-value	0.482	0.591	0.615	0.615
Control Var.	✓	✓	✓	✓
Control groups:				
Homicide	✓	✓	✓	✓
Rape	✓	✓	✓	✓
Fixed effects:				
Year FE	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓
Head Judge FE	✓	✓	✓	✓
Introducer FE	✓	✓	✓	✓

Notes: The table reports OLS estimates of treatment–control differences in appeal rejection rates using alternative cutoff dates related to the Greek sovereign debt crisis. *After 2009* is a dummy equal to 1 if the hearing is held after January 1, 2009. *After Election 2009* is a binary indicator that takes the value 1 if the hearing is held after October 4, 2009, following the parliamentary elections and the revision of the budget deficit. *After 2010* is the baseline time definition and captures a binary indicator that takes the value one if the hearing is held after January 1, 2010. *After Bailout 2010* is a dummy equal to 1 if the hearing is held after May 2, 2010. The interaction terms between *Tax Evasion* and each post-period indicator capture differential changes in rejection rates for tax-evasion cases relative to other crime categories after the corresponding cutoff date. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, and day-of-week fixed effects, as well as head judge and introducer fixed effects. Control variables, included where indicated, comprise committee size, average judicial experience of the committee, indicators for companies or public organisations as plaintiffs, the number of defendants, the gender composition of defendants, the share of male judges, an indicator for a male district attorney, and an indicator for whether the head judge is also the introducer. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. Pre-trend p-value reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Robustness to Alternative Specifications, Samples, and Inference

	Rejection						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tax Evasion	-0.042	-0.049	-0.064	-0.043	0.065	-0.042	-0.042
	(0.12)	(0.10)	(0.08)	(0.13)	(0.25)	(0.13)	(0.14)
Tax Evasion X After 2010	0.250**	0.220**	0.193**	0.188*	0.545*	0.250*	0.250**
	(0.11)	(0.10)	(0.09)	(0.11)	(0.30)	(0.14)	(0.11)
Male Introducer Judge				0.191			
				(0.15)			
Head Experience				-0.015			
				(0.07)			
Male Head Judge				0.051			
				(0.32)			
Introducer Experience				0.031			
				(0.04)			
Observations	448	500	602	441	212	448	448
DV mean	0.585	0.600	0.595	0.587	0.519	0.585	0.585
INDV mean	0.632	0.572	0.477	0.628	0.802	0.632	0.632
Pre-trend p-value	0.482	0.253	0.224	0.891	0.188	0.493	0.401
Control Var.	✓	✓	✓	✓	✓	✓	✓
Control groups:							
Homicide	✓	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓		✓	✓
Injuries			✓				
Immigration		✓	✓				
Lower-court decision before 2010 (restricted sample)					✓		
Cluster S.E.						✓	✓
Nr. Cluster.						135	47
Fixed effects:							
Year FE	✓	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓	✓
Head Judge FE	✓	✓	✓		✓	✓	✓
Introducer FE	✓	✓	✓		✓	✓	✓
Judge FE				✓			

Notes: Column (1) presents the benchmark specification. Columns (2) and (3) extend the control group to include additional crime categories. Column (4) augments the specification with judge-level fixed effects. Column (5) restricts the sample to cases decided before 2010 at the lower-court level. Columns (6) and (7) replicate the benchmark specification with alternative clustering of standard errors, at the lower-court level and at the introducer level, respectively. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, day-of-week fixed effects, and fixed effects for the head judge and the introducer, as indicated. *Control Var.* denotes the inclusion of additional controls: committee size, average judicial experience of the committee, indicators for companies or public organisations as plaintiffs, number of defendants, gender composition of defendants, share of male judges, an indicator for a male district attorney, and an indicator for whether the head judge is also the introducer. *After 2010* is a binary indicator equal to 1 if the hearing is held after January 1, 2010. *Rejection* is a dummy equal to 1 if the appeal is rejected. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. *Pre-trend p-value* reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses, except in Columns (6) and (7), where standard errors are clustered at the lower-court level and at the introducer level, respectively. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 9: Robustness to Excluding Extreme Case and Judge Characteristics

	Rejection									
	Benchmark	Remove extreme delay-day values			Remove extreme Share of male judges			Remove extreme avg. experience judges		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Tax Evasion	-0.042	-0.034	-0.186	-0.201	-0.075	-0.033	-0.065	-0.043	-0.037	-0.037
	(0.12)	(0.12)	(0.23)	(0.27)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
Tax Evasion X After 2010	0.250**	0.259**	0.366**	0.421**	0.257**	0.242**	0.247**	0.251**	0.245**	0.245**
	(0.11)	(0.11)	(0.18)	(0.20)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)	(0.12)
Observations	448	401	232	183	441	441	434	443	443	438
DV mean	0.585	0.569	0.599	0.563	0.583	0.587	0.585	0.587	0.585	0.587
INDV mean	0.632	0.646	0.647	0.689	0.637	0.635	0.641	0.628	0.632	0.628
Pre-trend p-value	0.482	0.284	0.360	0.212	0.412	0.476	0.420	0.488	0.435	0.441
Control Var.	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Removed from the sample		TOP	BOTTOM	BOTH	TOP	BOTTOM	BOTH	TOP	BOTTOM	BOTH
Control groups:										
Homicide	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed effects:										
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Head Judge FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Introducer FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Column (1) presents the benchmark specification. Columns (2)–(4) exclude observations with extreme values of delay days (top percentile, bottom percentile, and both tails, respectively). Columns (5)–(7) exclude observations with extreme values of the share of male judges. Columns (8)–(10) exclude observations with extreme values of average judicial experience. In each case, "TOP" indicates removal of the top 1st percentile, "BOTTOM" indicates removal of the bottom 1st percentile, and "BOTH" indicates removal of both the top and bottom 1st percentiles. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, day-of-week fixed effects, and fixed effects for the head judge and the introducer, as indicated. *Control Var.* denotes the inclusion of additional controls: committee size, average judicial experience of the committee, indicators for companies or public organisations as plaintiffs, number of defendants, gender composition of defendants, share of male judges, an indicator for a male district attorney, and an indicator for whether the head judge is also the introducer. *After 2010* is a binary indicator equal to 1 if the hearing is held after January 1, 2010. *Rejection* is a dummy equal to 1 if the appeal is rejected. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. *Pre-trend p-value* reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10: Appeal Rejection and Public Attention to Tax Evasion (Google Trends)

	Rejection								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tax Evasion	0.222*	0.331**	0.366**	0.172	0.290**	0.311**	0.169	0.248*	0.228
	(0.116)	(0.147)	(0.148)	(0.110)	(0.128)	(0.140)	(0.125)	(0.139)	(0.150)
F1 Google Trends (Tax Evasion)	0.003	0.001	0.002						
	(0.003)	(0.004)	(0.004)						
Tax Evasion X F1 Google Trends (Tax Evasion)	0.001	0.001	-0.001						
	(0.003)	(0.003)	(0.003)						
Google Trends (Tax Evasion)				-0.003	-0.004	-0.005			
				(0.002)	(0.004)	(0.004)			
Tax Evasion X Google Trends (Tax Evasion)				0.006**	0.006*	0.006**			
				(0.003)	(0.003)	(0.003)			
L1 Google Trends (Tax Evasion)							-0.003	-0.005	-0.003
							(0.004)	(0.005)	(0.004)
Tax Evasion X L1 Google Trends (Tax Evasion)							0.006	0.008	0.009
							(0.004)	(0.006)	(0.006)
Observations	206.000	190.000	189.000	210.000	194.000	193.000	210.000	194.000	193.000
DV mean	0.694	0.700	0.698	0.690	0.696	0.694	0.690	0.696	0.694
INDV1 mean	0.563	0.568	0.566	0.552	0.557	0.554	0.552	0.557	0.554
INDV2 mean	17.073	16.189	16.275	18.400	18.784	18.881	15.243	15.258	15.337
Control Var.			✓			✓			✓
Control groups:									
Homicide	✓	✓	✓	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed effects:									
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Head Judge FE		✓	✓		✓	✓		✓	✓
Introducer FE		✓	✓		✓	✓		✓	✓
H0: Lagged = Contemporaneous									
p-value	0.920	0.262	0.652						
Lead = Contemporaneous									
p-value	0.206	0.468	0.063						

Notes: Google Trends (Tax Evasion) measures search intensity for the term "tax evasion". *F1 Google Trends (Tax Evasion)* refers to the lead search intensity during the month after the Supreme Court hearing, while *L1 Google Trends (Tax Evasion)* refers to search intensity measured in the month before the hearing. Interaction terms between Tax Evasion and the Google Trends variables capture whether the association between public attention and rejection rates differs for tax evasion cases relative to other crimes. Columns (1)–(3) include lead Google Trends measures; Columns (4)–(6) use Google Trends measures in the same month as the hearing; and Columns (7)–(9) use lagged Google Trends measures. All specifications are estimated on post-2010 cases only. All regressions include year fixed effects, court-type fixed effects, month fixed effects, day-of-week fixed effects, and fixed effects for the head judge and the introducer, as indicated. *Control Var.* denotes the inclusion of additional controls: committee size, average judicial experience of the committee, indicators for companies or public organisations as plaintiffs, number of defendants, gender composition of defendants, share of male judges, an indicator for a male district attorney, and an indicator for whether the head judge is also the introducer. The bottom panel reports p-values from tests of equality between coefficients on contemporaneous and lagged Google Trends measures. *After 2010* is a binary indicator equal to one if the hearing is held after January 1, 2010. *Rejection* is a dummy equal to one if the appeal is rejected. *DV mean* reports the mean of the dependent variable in the sample. *INDV1 mean* reports the mean of *Tax Evasion* in the sample, while *INDV2 mean* reports the means of *Google Trends* in the sample. *Pre-trend p-value* reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 11: DiD Estimates of Decision Delays (Levels and Logs)

	Delay Days			Delay Days (Log)		
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Evasion	-2.754	-10.128	-6.547	-0.024	0.031	0.087
	(15.16)	(14.15)	(14.49)	(0.25)	(0.20)	(0.22)
Tax Evasion X After 2010	22.014	20.517*	19.491	0.365	0.340	0.289
	(14.76)	(11.98)	(12.00)	(0.30)	(0.26)	(0.25)
Observations	448	448	448	448	448	448
DV mean	77.28	77.28	77.28	3.906	3.906	3.906
INDV mean	0.632	0.632	0.632	0.632	0.632	0.632
Pre-trend p-value	0.366	0.840	0.779	0.712	0.983	0.991
Control Variables			✓			✓
Control groups:						
Homicide	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓
Fixed effects:						
Year FE	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓
Head Judge FE		✓	✓		✓	✓
Introducer FE		✓	✓		✓	✓

Notes: The table reports treatment–control differences in decision delays before and after 2010. Interaction terms between Tax Evasion and After 2010 capture differential changes in decision delays for tax evasion cases relative to other crime categories after 2010. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, and day-of-week fixed effects, as well as fixed effects for the head judge and the introducer, as indicated. *Control Var.* denotes the inclusion of additional controls: committee size, average judicial experience of the committee, indicators for companies or public organisations as plaintiffs, number of defendants, gender composition of defendants, share of male judges, an indicator for a male district attorney, and an indicator for whether the head judge is also the introducer. *After 2010* is a binary indicator equal to 1 if the hearing is held after January 1, 2010. *Delay Days* is measured as the number of days between the hearing date and the final decision, and *Delay Days (Log)* is defined as the natural logarithm of delay days +0.0001. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. *Pre-trend p-value* reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

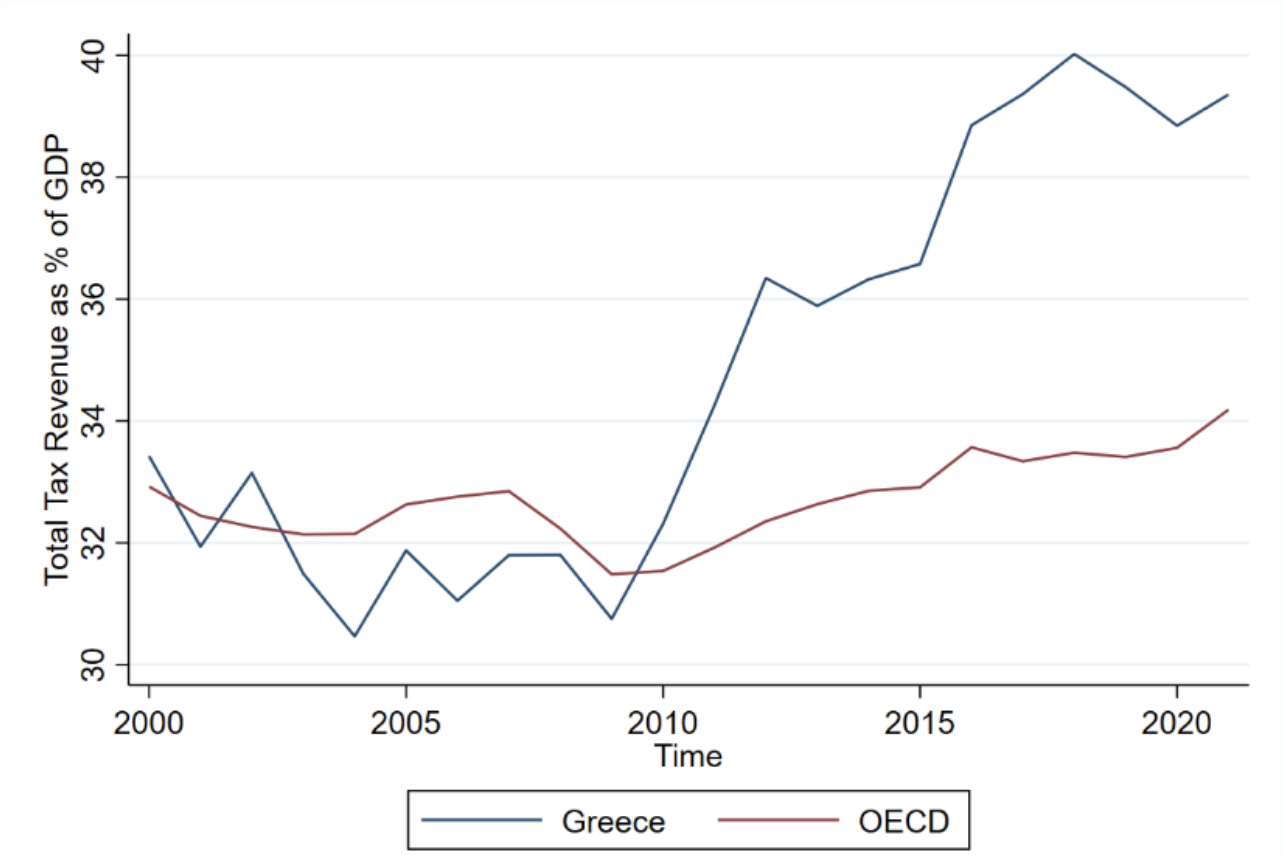
Table 12: DiD Estimates of Legal Representation Outcomes

	Nr. Lawyers			Avg. Nr. Lawyers			Share of Male Lawyers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tax Evasion	-0.450***	-0.363**	-0.396**	0.060	0.065	0.045	-0.015	-0.046	-0.079
	(0.16)	(0.18)	(0.20)	(0.08)	(0.09)	(0.08)	(0.08)	(0.11)	(0.11)
Tax Evasion X After 2010	0.056	0.153	0.195	-0.015	0.043	0.027	0.038	0.085	0.113
	(0.12)	(0.13)	(0.13)	(0.08)	(0.08)	(0.08)	(0.06)	(0.07)	(0.08)
Observations	370	369	369	370	369	369	370	369	369
DV mean	1.108	1.108	1.108	0.950	0.950	0.950	0.927	0.927	0.927
INDV mean	0.659	0.661	0.661	0.659	0.661	0.661	0.659	0.661	0.661
Pre-trend p-value	0.164	0.283	0.274	0.318	0.100	0.0782	0.229	0.645	0.599
Control Variables			✓			✓			✓
Control groups:									
Homicide	✓	✓	✓	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed effects:									
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Head Judge FE		✓	✓		✓	✓		✓	✓
Introducer FE		✓	✓		✓	✓		✓	✓

Notes: The dependent variables are *Nr. Lawyers*, the total number of lawyers representing the parties in the appeal; *Avg. Nr. Lawyers*, defined as the ratio of the total number of lawyers to the number of defendants; and *Share of Male Lawyers*, defined as the share of male lawyers among all lawyers involved in the case. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, day-of-week fixed effects, and fixed effects for the head judge and the introducer, as indicated. *Control Var.* denotes the inclusion of additional controls: committee size, average judicial experience of the committee, indicators for companies or public organizations as plaintiffs, gender composition of defendants, an indicator for missing defendants, share of male judges, an indicator for a male district attorney, and an indicator for whether the head judge is also the introducer. *After 2010* is a binary indicator equal to 1 if the hearing is held after January 1, 2010. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. *Pre-trend p-value* reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

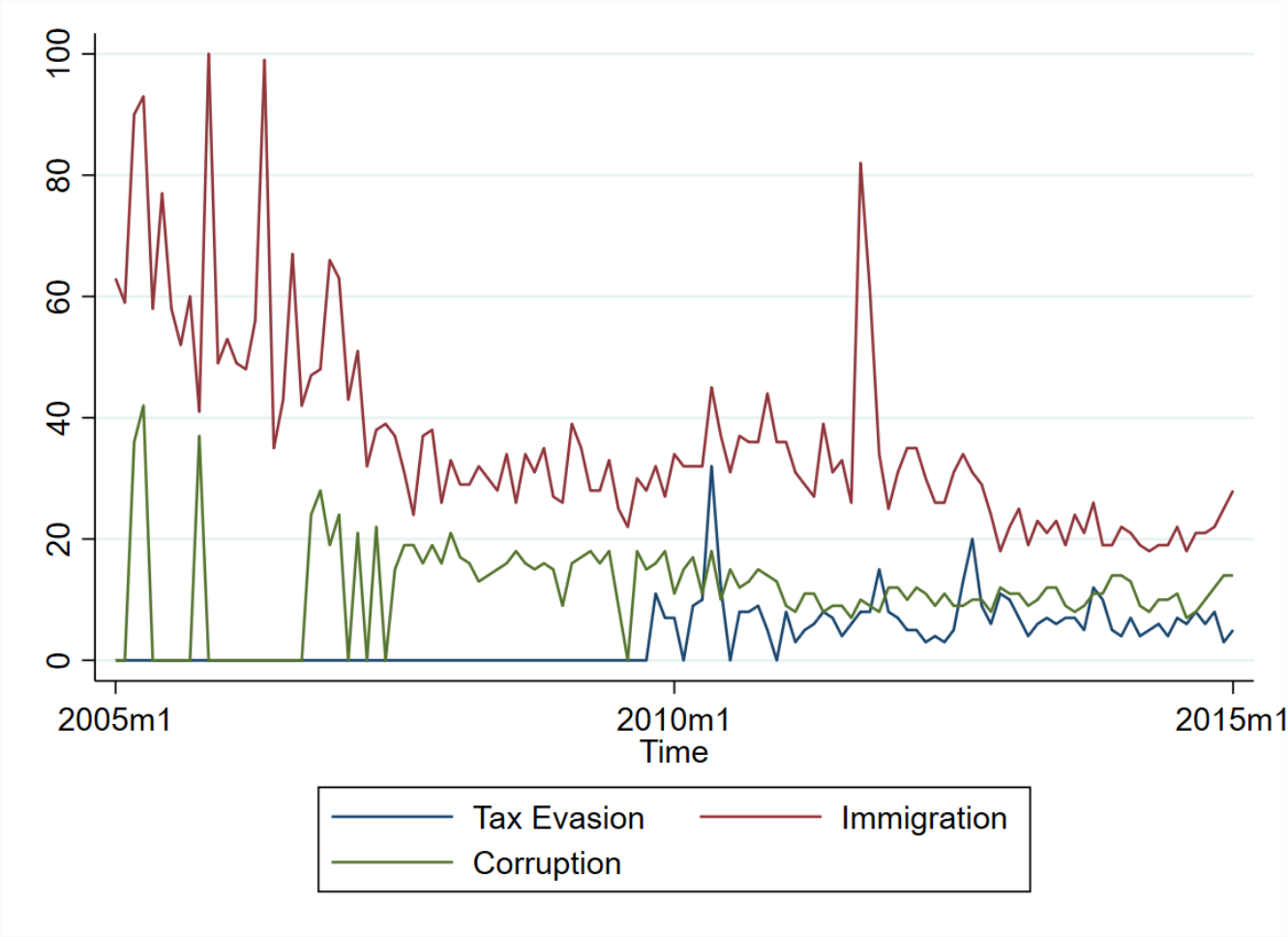
9 Figures

Figure 1: Tax Revenue as a Share of GDP: Greece and the OECD



Note: The figure plots total tax revenue as a percentage of GDP for Greece and the OECD average over the period 2000–2021. OECD values correspond to the unweighted OECD average. The series illustrates the evolution of tax revenue before and after the sovereign debt crisis, highlighting the divergence between Greece and the OECD beginning in the early 2010s. Data are expressed as shares of GDP and are comparable across countries. Source: OECD Revenue Statistics.

Figure 2: Google Trends for Tax Evasion, Immigration, and Corruption



Note: Our elaboration is based on Google Trends data.

Figure 3: Illustrative Supreme Court Appeal Decision

Απόφαση **1763 / 2017** (Ζ, ΠΟΙΝΙΚΕΣ)

ΑΡΙΘΜΟΣ 1763/2017

ΤΟ ΔΙΚΑΣΤΗΡΙΟ ΤΟΥ ΑΡΕΙΟΥ ΠΑΓΟΥ

Ζ' ΠΟΙΝΙΚΟ ΤΜΗΜΑ - ΣΕ ΣΥΜΒΟΥΛΙΟ

Συγκροτήθηκε από τους Δικαστές: Αγγελική Αλειφεροπούλου, Αντιπρόεδρο Αρείου Πάγου, Δημήτριο Γεώργα, Δημήτριο Τζιούβα, Μαρία Γκανιάτσου και Μαρία Παπασωτηρίου - Εισηγήτρια, Αρεοπαγίτες.

Με την παρουσία και της Αντεισαγγελέως του Αρείου Πάγου Ελένης Μετσοβίτου - Φλουρή (γιατί κωλύεται η Εισαγγελέας) και της Γραμματέως Αικατερίνης Σιταρά.

Συνήλθε σε Συμβούλιο στο Κατάστημά του στις 4 Οκτωβρίου 2017, προκειμένου να αποφανθεί για την αίτηση του αναιρεσείοντος- κατηγορουμένου Ι. Π. του Γ. κατοίκου ..., που δεν παρέστη στο συμβούλιο, για αναίρεση της υπ' αριθμ. 1552/2016 αποφάσεως του Τριμελούς Πλημμελειοδικείου Χαλκιδικής. Με πολιτικώς ενάγοντα τον Κ. Μ. του Γ., κάτοικο ....

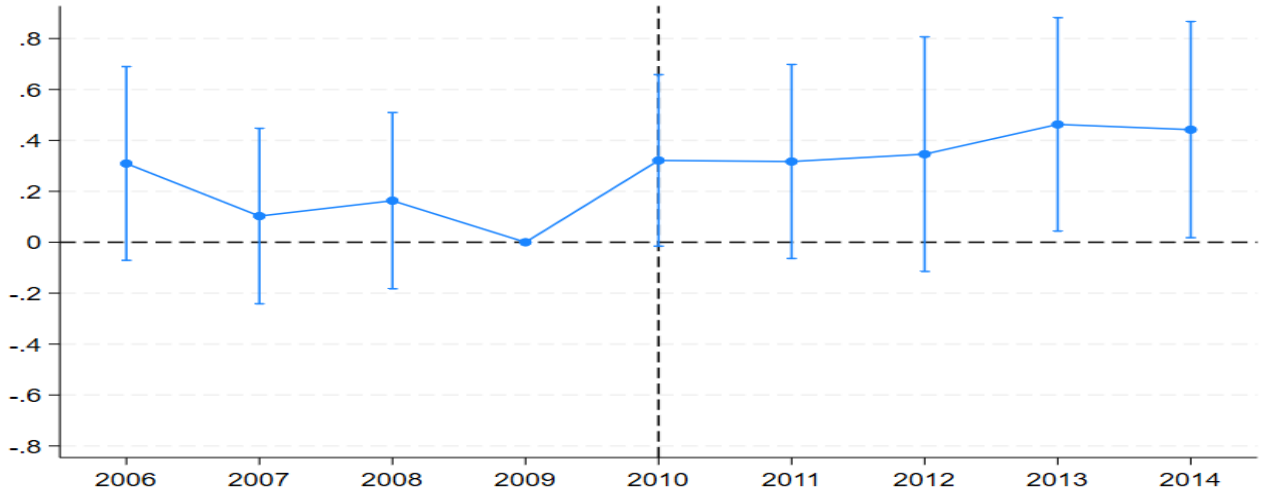
Το Τριμελές Πλημμελειοδικείο Χαλκιδικής, με την ως άνω απόφασή του διέταξε όσα λεπτομερώς αναφέρονται σ' αυτή, και ο αναιρεσείων-κατηγορούμενος ζητεί τώρα την αναίρεση της απόφασης αυτής, για τους λόγους που αναφέρονται στην από 17 Φεβρουαρίου 2017 αίτησή του, η οποία καταχωρίστηκε στο οικείο πινάκιο με τον αριθμό ...2017.

Έπειτα η Αντεισαγγελέας του Αρείου Πάγου Ελένη Μετσοβίτου - Φλουρή, εισήγαγε για κρίση στο Συμβούλιο τη σχετική δικογραφία με την πρόταση του Αντεισαγγελέα του Αρείου Πάγου Αθανάσιου Κατσιρώδη με αριθμό ...-4-2017, στην οποία αναφέρονται τα ακόλουθα: "Ι. Εισάγω στο Δικαστήριο Σας σε Συμβούλιο, σύμφωνα με τα άρθρα 473 παρ. 2 και 476 παρ. 1 του Κ.Π.Δ. όπως το τελευταίο αντ. από το άρθρο 2 παρ. 18 του Ν. 3160/2003 , την ...2-2017 αίτηση αναιρέσεως του Ι. Γ. Π. κατοίκου ... κατά της 1552/2016 αποφάσεως του Τριμελούς Πλημμελειοδικείου Χαλκιδικής, με την οποία καταδικάστηκε σε φυλάκιση έξη μηνών για πλαστογραφία με τριετή αναστολή, και εκθέτω τα ακόλουθα: ΙΙ. Σύμφωνα με το άρθρο 473 παρ. 2 και 3 του Κ.Π.Δ., η αναίρεση κατά της τελεσίδικης καταδικαστικής απόφασης μπορεί να ασκηθεί από εκείνον που κρίθηκε ένοχος και με δήλωση του στον Γραμματέα του δικαστηρίου που εξέδωσε την προσβαλλόμενη απόφαση εντός προθεσμίας δέκα ημερών από την ημέρα που η απόφαση αυτή καταχωρήθηκε καθαρογραμμένη στο ειδικό βιβλίο που προβλέπεται από την παρ. 3 του άρθρου αυτού. Η ημέρα καταχώρησης δεν υπολογίζεται στην προθεσμία αυτή δηλαδή η προθεσμία αυτή αρχίζει από την επόμενη της καταχώρησης (ΑΠ 1095/1985, ΑΠ 426/1984). Σε περίπτωση που η αίτηση αναιρέσεως ασκηθεί εκπρόθεσμα αυτή, χωρίς να αναφέρονται σε αυτή οι λόγοι ανωτέρας βίας που δικαιολογούν την εκπρόθεσμη άσκηση της αναιρέσεως , πρέπει αυτή, σύμφωνα με το άρθρο 476 παρ. 1 του Κ.Π.Δ. ίδιου Κώδικα, να απορριφθεί ως απαράδεκτη από το Δικαστήριο του Αρείου Πάγου σε Συμβούλιο, ύστερα από πρόταση του Εισαγγελέα και αφού ακούσει τους διαδίκους που εμφανιστούν, ενώ ταυτόχρονα αυτό: α) διατάσσει την εκτέλεση της απόφασης που έχει προσβληθεί και β) καταδικάζει τον αναιρεσείοντα στα σχετικά δικαστικά έξοδα σύμφωνα με το άρθρο 583 παρ. 1 του Κ.Π.Δ. (ΑΠ 738/2016, ΑΠ 614/2015, ΑΠ 29/2013, ΑΠ 76/2011, ΑΠ 1639/2010). ΙΙΙ. Η παραπάνω αίτηση αναιρέσεως ασκήθηκε στις 17-2-2017 με δήλωση του αναιρεσείοντα στον Γραμματέα Πλημμελειοδικών Χαλκιδικής και η καταδικαστική απόφαση κατά της οποίας στρέφεται καταχωρήθηκε καθαρογραμμένη στο ειδικό βιβλίο στις 2-2-2017, ενώ έπρεπε η αναίρεση αυτή, σύμφωνα με όσα παραπάνω εκθέσαμε, να ασκηθεί μέχρι και τις 13-2-2017 ημέρα Δευτέρα. Επειδή η αίτηση αυτή αναιρέσεως ασκήθηκε εκπρόθεσμα, χωρίς επίκληση λόγων ανωτέρας βίας για την εκπρόθεσμη αυτή άσκηση, πρέπει το Δικαστήριο Σας σε Συμβούλιο, σύμφωνα με τα άρθρα 476 παρ.1 και 583 παρ. 1 του Κ.Π.Δ. όπως αντ. από το άρθρο 55 παρ. 1 του Ν. 3160/2003 και το άρθρο 3 του Ν. 663/77 όπως αντ. από το άρθρο 18 του Ν. 969/79 και τις 134423/1992, 58553/2006 και 123827/23-12-2010 Α.Υ. Οικονομικών και Δικαιοσύνης: α) να απορρίψει την αίτηση αυτή αναιρέσεως ως απαράδεκτη β) να διατάξει την εκτέλεση της προσβαλλόμενης απόφασης και γ) να επιβάλλει σ' αυτόν τα σχετικά δικαστικά έξοδα.

ΓΙΑ ΤΟΥΣ ΛΟΓΟΥΣ ΑΥΤΟΥΣ Προτείνω: Α) Να απορριφθεί ως απαράδεκτη η ...2-2017 αίτηση αναιρέσεως του Ι. Γ. Π. κατοίκου ... κατά της 1552/2016 αποφάσεως του Τριμελούς Πλημμελειοδικείου Χαλκιδικής, με την οποία καταδικάστηκε σε φυλάκιση έξη μηνών για πλαστογραφία Β) Να διαταχθεί η εκτέλεση της προσβαλλομένης αποφάσεως και Γ) Να καταδικασθεί ο αναιρεσείων στα δικαστικά έξοδα από 250 Ευρώ. Ο Αντεισαγγελέας του Αρείου Πάγου Αθανάσιος Κ. Κατσιρώδης".

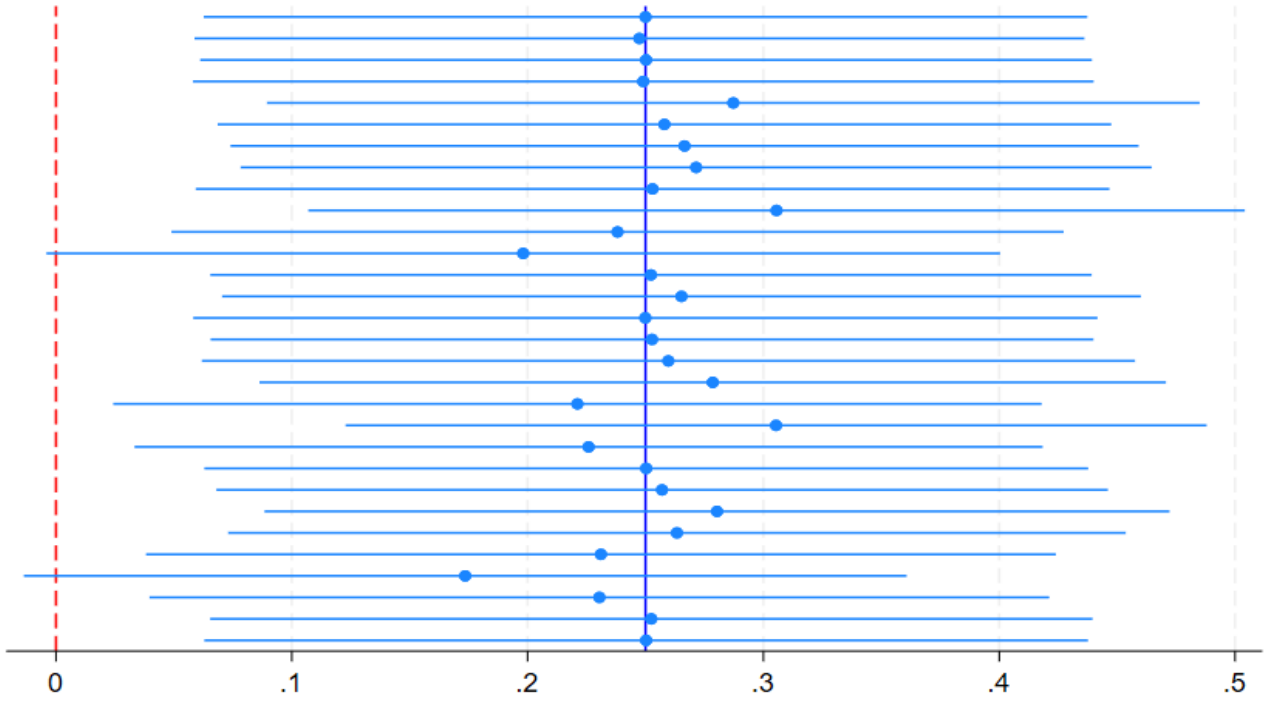
Αφού άκουσε

Figure 4: Event Study Estimates: Tax Evasion × Year



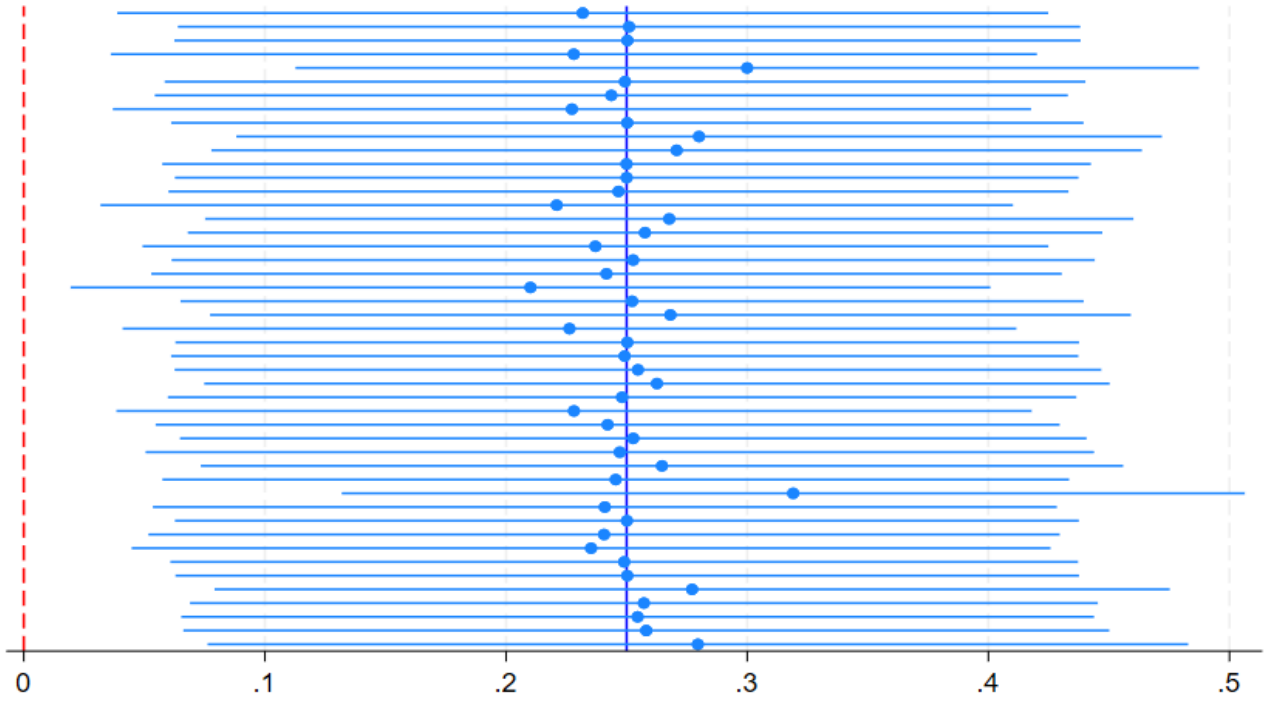
Notes: The figure reports the coefficients from an event-study specification estimating the differential evolution of tax-evasion cases relative to the control crimes of homicide and rape. Each point corresponds to the coefficient on the interaction between the tax evasion indicator and a year indicator. The omitted reference year is 2010, marked by the vertical dashed line. The specification includes fixed effects and the full set of control variables used in the baseline regressions. Vertical bars denote 90% confidence intervals based on heteroskedasticity-robust standard errors. The p-value for the joint test that all pre-2010 coefficients equal 0 is 0.482 .

Figure 5: Leave-One-Out Estimates Excluding Each Head Judge



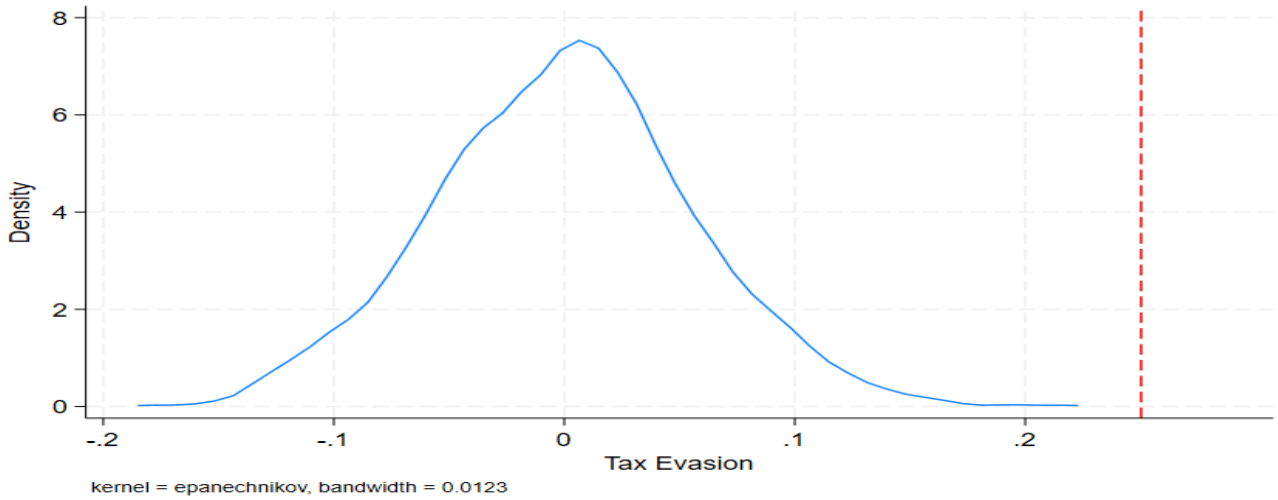
Notes: The figure reports leave-one-head-judge-out estimates. Each row corresponds to re-estimating the baseline specification after excluding one head judge from the sample. The head judges excluded (from top to bottom) are identified by the following IDs: 5, 7, 10, 16, 38, 39, 45, 61, 72, 76, 78, 80, 86, 87, 89, 102, 107, 109, 139, 151, 173, 183, 184, and 186. Blue markers denote the resulting coefficient estimates, and horizontal bars denote 90% confidence intervals. The vertical line indicates the full-sample estimate reported in Column (3) of Table 4.

Figure 6: Leave-One-Out Estimates Excluding Each Introducer Judge



Notes: The figure reports leave-one-introducer-out estimates. Each row corresponds to re-estimating the baseline specification after excluding one introducer judge from the sample. The introducer judges excluded (from top to bottom) are identified by the following IDs: 1, 3, 12, 14, 20, 24, 26, 29, 32, 33, 35, 38, 39, 40, 41, 47, 48, 50, 56, 57, 60, 62, 63, 64, 65, 67, 68, 69, 71, 72, 73, 74, 81, 82, 84, 85, 86, 88, 90, 91, 92, 93, 94, 95, 98, 99, 100, 101, 102, 103, 107, 110, 114, 116, 119, 121, 130, 136, 147, 150, 152, 153, 159, 162, and 173. Blue markers denote the resulting coefficient estimates, and horizontal bars denote 90% confidence intervals. The vertical line indicates the full-sample estimate reported in Column (3) of Table 4. The stability of the estimates across exclusions indicates that the main results are not driven by any single introducer judge.

Figure 7: Permutation Test for the Baseline Treatment Effect



Notes: The figure presents the distribution of coefficient estimates obtained from a randomisation (permutation) inference procedure based on 1,000 replications. In each replication, the tax evasion treatment is randomly reassigned across cases, and the baseline specification is re-estimated. The kernel density plot displays the resulting distribution of simulated estimates. The red vertical line indicates the benchmark estimate from the baseline specification reported in Column (3) of Table 4. The position of the benchmark estimate relative to the simulated distribution provides evidence on whether the observed effect could arise by chance.

Appendix: Tables

Table A1: Data Dictionary for Types of Supreme Court Cases

Offense	Keywords
Homicide	Any Homicide
Injuries	Injury, Violence, Injury, Traffic, Human Trafficking
Rape	Rape, Seduction, Indecency, Sexual Abuse
Tax Evasion	Tax evasion, Delay in Payment/Non-payment of Debts/Taxes/Employer Contributions, Fictitious Invoices
Bad Check	Uncovered Check
Financial Related	Smuggling, Usury, Dubious Coins, Forgery, Misuse of Money, Money Laundering, Delay/Non-payment of Earnings
Drugs	Anything Related to Drugs
Misconduct	Violation of Duty, Abuse/Misuse of Power, Bribery
Fraud	Deception, Violation, Fraud
Forgery	Forgery, Perjury, False Certification, False Document, Document Exportation
Calumniation	Slander, False Accusation, Moral Harm, Distortion of Facts
Theft	Theft, Robbery, Embezzlement
Environmental	Pollution, Illegal Logging, Unauthorized Construction, Illegal Building, Arson, Illegal Dumping, Environmental Degradation, Urban Planning Violations
Immigration	Transfer/Promotion of Foreigners, Illegal Immigration, Illegal Entry
Relevance	Jurisdiction Regulation
International Transfer Defendant	Extradition, Extradition of Foreigners
Guns	Gun Possession, Carrying Firearms, Firearm Use
Did Not Appear	Desertion of the Appellant
Miscellaneous	Anything that cannot be grouped in the above categories and still provides some type
Missing Type	No information at all about the case type

Notes: The table provides information about case types along with the most relevant keywords that were searched for in the data classification process. These classifications were used as a protocol during the data collection process.

A1 Appendix: Main Findings

Table A2: Baseline Regressions with Full Controls Reported

	Rejection		
	(1)	(2)	(3)
Tax Evasion	-0.048	-0.035	-0.042
	(0.10)	(0.11)	(0.12)
Tax Evasion X After 2010	0.167*	0.238**	0.250**
	(0.10)	(0.11)	(0.11)
Large Committee			-0.163
			(0.17)
Average Committee Experience			-0.148*
			(0.08)
Company/Public Org.			0.007
			(0.14)
Number Defendants			-0.018
			(0.04)
Share Male Defendants			0.055
			(0.27)
Share Female Defendants			-0.114
			(0.26)
Missing Defendants			0.178
			(0.34)
Share Male Judges			-0.267*
			(0.14)
Male District Attorney			0.133
			(0.14)
Head is Introducer			-0.326
			(0.26)
Constant	0.575***	0.551***	1.092**
	(0.06)	(0.07)	(0.46)
Observations	448	448	448
DV mean	0.585	0.585	0.585
INDV mean	0.632	0.632	0.632
Pre-trend p-value	0.534	0.563	0.482
Control groups:			
Homicide	✓	✓	✓
Rape	✓	✓	✓
Fixed effects:			
Year FE	✓	✓	✓
Court Type FE	✓	✓	✓
Semester FE	✓	✓	✓
Week day FE	✓	✓	✓
Head Judge FE		✓	✓
Introducer FE		✓	✓

Notes: The table reports DiD estimates examining treatment–control differences in appeal rejection rates before and after 2010. The unit of observation is the case. The dependent variable is a binary indicator equal to 1 if the appeal is rejected. *Tax Evasion* is a binary indicator for cases involving tax evasion, and *After 2010* is a binary indicator equal to 1 if the hearing takes place after January 1, 2010. The interaction term *Tax Evasion* × *After 2010* captures the differential change in rejection rates for tax-evasion cases relative to other crime categories after 2010. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, and day-of-week fixed effects. Columns (2) and (3) additionally include head judge and introducer fixed effects. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. Pre-trend p-value reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Placebo Tests using Calumniation and Drug Cases

	Rejection					
	(1)	(2)	(3)	(4)	(5)	(6)
Calumniation	-0.111	-0.031	-0.002			
	(0.08)	(0.10)	(0.11)			
Calumniation X After 2010	0.154	0.010	-0.006			
	(0.11)	(0.13)	(0.13)			
Drugs				0.052	0.063	0.058
				(0.13)	(0.15)	(0.16)
Drugs X After 2010				-0.216	-0.221	-0.265
				(0.13)	(0.16)	(0.17)
Large Committee			-0.059			-0.059
			(0.18)			(0.20)
Average Committee Experience			-0.016			-0.120
			(0.11)			(0.12)
Company/Public Org.			-0.194			-0.740*
			(0.22)			(0.42)
Number Defendants			-0.047			-0.039
			(0.03)			(0.05)
Share Male Defendants			-0.406			-0.432
			(0.46)			(0.37)
Share Female Defendants			-0.455			-0.578
			(0.46)			(0.41)
Missing Defendants			0.072			-0.094
			(0.56)			(0.46)
Share Male Judges			-0.009			-0.190
			(0.18)			(0.18)
Male District Attorney			0.213			0.056
			(0.18)			(0.18)
Head is Introducer			-0.569			
			(0.39)			
Constant	0.598***	0.589***	0.949	0.636***	0.632***	1.548**
	(0.04)	(0.04)	(0.59)	(0.05)	(0.06)	(0.60)
Observations	316	316	316	287	287	287
DV mean	0.576	0.576	0.576	0.624	0.624	0.624
INDV mean	0.475	0.475	0.475	0.436	0.436	0.436
Pre-trend p-value	0.794	0.492	0.388	0.0867	0.123	0.141
Control groups:						
Homicide	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓
Fixed effects:						
Year FE	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓
Head Judge FE		✓	✓		✓	✓
Introducer FE		✓	✓		✓	✓

Notes: The table presents placebo exercises in which we use *Calumniation* (Columns 1–3) and *Drugs* (Columns 4–6) cases as the treatment group. Each column reports estimates from OLS regressions at the case level. The dependent variable is *Rejection*; a binary indicator that takes the value of 1 if the appeal is rejected. *Calumniation* and *Drugs* are binary indicators for the corresponding crime types, and *After 2010* is a binary indicator that takes the value of 1 if the hearing takes place after January 1, 2010. The interaction terms *Calumniation* × *After 2010* and *Drugs* × *After 2010* capture differential changes in rejection rates for these placebo treatment groups after 2010. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, and day-of-week fixed effects. Columns (2), (3), (5), and (6) additionally include head judge and introducer fixed effects. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Calumniation* and *Drugs* in the sample. Pre-trend p-value reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A4: Sensitivity to Alternative Crisis Dates

	Rejection			
	(1)	(2)	(3)	(4)
Tax Evasion	-0.042	0.027	0.005	-0.035
	(0.12)	(0.13)	(0.12)	(0.12)
After Election 2009			0.350	
			(0.27)	
After Bailout 2010				-0.248
				(0.20)
Tax Evasion X After 2010	0.250**			
	(0.11)			
Tax Evasion X After 2009		0.116		
		(0.12)		
Tax Evasion X After Election 2009			0.161	
			(0.11)	
Tax Evasion X After Bailout 2010				0.264**
				(0.12)
Large Committee	-0.163	-0.177	-0.104	-0.094
	(0.17)	(0.17)	(0.16)	(0.17)
Average Committee Experience	-0.148*	-0.148*	-0.109	-0.145*
	(0.08)	(0.08)	(0.08)	(0.08)
Company/Public Org.	0.007	-0.001	0.008	0.011
	(0.14)	(0.14)	(0.14)	(0.14)
Number Defendants	-0.018	-0.013	-0.016	-0.011
	(0.04)	(0.03)	(0.03)	(0.03)
Share Male Defendants	0.055	0.099	0.082	0.082
	(0.27)	(0.26)	(0.26)	(0.26)
Share Female Defendants	-0.114	-0.078	-0.092	-0.088
	(0.26)	(0.26)	(0.26)	(0.26)
Missing Defendants	0.178	0.255	0.192	0.216
	(0.34)	(0.33)	(0.32)	(0.34)
Share Male Judges	-0.267*	-0.267*	-0.219	-0.277*
	(0.14)	(0.14)	(0.14)	(0.15)
Male District Attorney	0.133	0.116	0.141	0.144
	(0.14)	(0.14)	(0.14)	(0.14)
Head is Introducer	-0.326	-0.293	-0.340	-0.300
	(0.26)	(0.26)	(0.27)	(0.25)
Constant	1.092**	1.050**	0.717	1.070**
	(0.46)	(0.45)	(0.49)	(0.47)
Observations	448	448	448	448
DV mean	0.585	0.585	0.585	0.585
INDV mean	0.632	0.632	0.632	0.632
Pre-trend p-value	0.482	0.591	0.615	0.615
Control groups:				
Homicide	✓	✓	✓	✓
Rape	✓	✓	✓	✓
Fixed effects:				
Year FE	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓
Head Judge FE	✓	✓	✓	✓
Introducer FE	✓	✓	✓	✓

Notes: The table reports regressions of appeal rejection on tax evasion cases using alternative definitions of the post-treatment period. Column (1) uses the baseline specification where the post indicator equals one for hearings held after January 1, 2010. Column (2) defines the post period as hearings held after 2009. Column (3) defines the post period as hearings held after the October 2009 national election, and Column (4) defines the post period as hearings held after the 2010 bailout agreement. The interaction terms between *Tax Evasion* and the respective post indicators capture differential changes in rejection rates for tax evasion cases relative to the control crime categories following each cutoff date. All specifications include year, court-type, semester, and day-of-week fixed effects, as well as head judge and introducer fixed effects. Control groups consist of homicide and rape cases. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. Pre-trend p-value reports the p-value from a test of parallel pre-treatment trends using the corresponding cutoff year as the reference period. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A2 Appendix: Robustness Checks

Table A5: Robustness to Alternative Specifications

	Rejection						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tax Evasion	-0.042	-0.049	-0.064	-0.043	0.065	-0.042	-0.042
	(0.12)	(0.10)	(0.08)	(0.13)	(0.25)	(0.13)	(0.14)
Tax Evasion X After 2010	0.250**	0.220**	0.193**	0.188*	0.545*	0.250*	0.250**
	(0.11)	(0.10)	(0.09)	(0.11)	(0.30)	(0.14)	(0.11)
Large Committee	-0.163	-0.073	0.016	0.729		-0.163	-0.163
	(0.17)	(0.16)	(0.14)	(0.96)		(0.18)	(0.15)
Average Committee Experience	-0.148*	-0.096	-0.014		-0.428	-0.148**	-0.148*
	(0.08)	(0.08)	(0.07)		(0.39)	(0.07)	(0.08)
Company/Public Org.	0.007	0.003	-0.045	-0.020	0.150	0.007	0.007
	(0.14)	(0.13)	(0.12)	(0.14)	(0.23)	(0.17)	(0.16)
Number Defendants	-0.018	-0.021	-0.029	-0.014	-0.108	-0.018	-0.018
	(0.04)	(0.03)	(0.03)	(0.03)	(0.09)	(0.04)	(0.03)
Share Male Defendants	0.055	0.072	0.070	-0.015	-0.744	0.055	0.055
	(0.27)	(0.24)	(0.21)	(0.28)	(0.60)	(0.27)	(0.25)
Share Female Defendants	-0.114	-0.127	-0.147	-0.151	-0.974	-0.114	-0.114
	(0.26)	(0.24)	(0.21)	(0.28)	(0.60)	(0.26)	(0.22)
Missing Defendants	0.178	0.211	0.228	-0.199	-0.873	0.178	0.178
	(0.34)	(0.34)	(0.30)	(0.34)	(0.89)	(0.37)	(0.28)
Share Male Judges	-0.267*	-0.195	-0.123	0.518	-1.013*	-0.267*	-0.267**
	(0.14)	(0.13)	(0.12)	(0.32)	(0.56)	(0.14)	(0.12)
Male District Attorney	0.133	0.088	0.028	0.233*	0.014	0.133	0.133
	(0.14)	(0.12)	(0.11)	(0.13)	(0.29)	(0.16)	(0.14)
Head is Introducer	-0.326	-0.287	-0.625**	-0.435		-0.326	-0.326
	(0.26)	(0.28)	(0.30)	(0.37)		(0.24)	(0.26)
Average Judges Experience				0.051			
				(0.10)			
Male Introducer Judge				0.191			
				(0.15)			
Head Experience				-0.015			
				(0.07)			
Male Head Judge				0.051			
				(0.32)			
Introducer Experience				0.031			
				(0.04)			
Constant	1.092**	0.904**	0.674*	-0.946	2.859**	1.092**	1.092**
	(0.46)	(0.42)	(0.37)	(0.92)	(1.14)	(0.47)	(0.43)
Observations	448	500	602	441	212	448	448
DV mean	0.585	0.600	0.595	0.587	0.519	0.585	0.585
INDV mean	0.632	0.572	0.477	0.628	0.802	0.632	0.632
Pre-trend p-value	0.482	0.253	0.224	0.891	0.188	0.493	0.401
Control groups:							
Homicide	✓	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓		✓	✓
Injuries			✓				
Immigration		✓	✓				
Lower-court decision before 2010 (restricted sample)					✓		
Cluster S.E.						✓	✓
Nr. Cluster.						135	47
Fixed effects:							
Year FE	✓	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓	✓
Head Judge FE	✓	✓	✓		✓	✓	✓
Introducer FE	✓	✓	✓		✓	✓	✓
Judge FE				✓			

Notes: The table reports robustness checks of the baseline specification, estimating the effect of tax-evasion cases on appeal rejection. The interaction term *Tax Evasion* × *After 2010* captures the differential change in rejection rates for tax evasion cases relative to the control crime categories following 2010. Column (1) presents the benchmark specification. Columns (2) and (3) extend the control group to include additional crime categories. Column (4) augments the specification with judge-level fixed effects. Column (5) restricts the sample to cases decided before 2010 at the lower-court level. Columns (6) and (7) replicate the benchmark specification with alternative clustering of standard errors, at the lower-court level and at the introducer level, respectively. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, day-of-week fixed effects, and fixed effects for the head judge and the introducer, as indicated. *After 2010* is a binary indicator equal to 1 if the hearing is held after January 1, 2010. *Rejection* is a dummy equal to 1 if the appeal is rejected. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. *Pre-trend p-value* reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses, except in Columns (6) and (7), where standard errors are clustered at the lower-court level and at the introducer level, respectively. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: Robustness to Excluding Extreme Case and Judge Characteristics

	Rejection									
	Benchmark	Remove: Delay Days Extreme			Remove: Share of Male Judges Extreme			Remove: Avg. Experience judges Extreme		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Tax Evasion	-0.042	-0.034	-0.186	-0.201	-0.075	-0.033	-0.065	-0.043	-0.037	-0.037
	(0.12)	(0.12)	(0.23)	(0.27)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
Tax Evasion X After 2010	0.250**	0.259**	0.366**	0.421**	0.257**	0.242**	0.247**	0.251**	0.245**	0.245**
	(0.11)	(0.11)	(0.18)	(0.20)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)	(0.12)
Large Committee	-0.163	-0.399**	0.473		-0.145	-0.167	-0.150	-0.162	-0.202	-0.201
	(0.17)	(0.19)	(0.40)		(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)
Average Committee Experience	-0.148*	-0.102	0.028	-0.295	-0.175**	-0.156**	-0.183**	-0.147*	-0.115	-0.112
	(0.08)	(0.11)	(0.20)	(0.28)	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)	(0.09)
Company/Public Org.	0.007	0.070	0.257	0.369	0.010	-0.007	-0.004	0.007	0.014	0.015
	(0.14)	(0.15)	(0.19)	(0.22)	(0.15)	(0.14)	(0.15)	(0.14)	(0.14)	(0.14)
Number Defendants	-0.018	0.011	-0.043	-0.056	-0.022	-0.018	-0.022	-0.018	-0.017	-0.017
	(0.04)	(0.04)	(0.06)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
Share Male Defendants	0.055	0.227	-0.513	-0.728	0.038	0.064	0.046	0.059	0.118	0.123
	(0.27)	(0.29)	(0.45)	(0.70)	(0.28)	(0.27)	(0.28)	(0.27)	(0.27)	(0.27)
Share Female Defendants	-0.114	0.042	-0.639	-0.908	-0.123	-0.135	-0.145	-0.114	-0.050	-0.048
	(0.26)	(0.28)	(0.44)	(0.68)	(0.27)	(0.27)	(0.28)	(0.26)	(0.27)	(0.27)
Missing Defendants	0.178	0.254	-0.648	-1.078	0.145	0.177	0.143	0.183	0.221	0.227
	(0.34)	(0.35)	(0.62)	(0.83)	(0.35)	(0.34)	(0.35)	(0.34)	(0.35)	(0.35)
Share Male Judges	-0.267*	-0.680***	-0.487	-0.563	-0.244	-0.230	-0.204	-0.274*	-0.273*	-0.282*
	(0.14)	(0.22)	(0.41)	(0.61)	(0.15)	(0.14)	(0.15)	(0.15)	(0.14)	(0.15)
Male District Attorney	0.133	0.179	0.003	0.001	0.119	0.125	0.111	0.133	0.134	0.134
	(0.14)	(0.15)	(0.25)	(0.37)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
Head is Introducer	-0.326	-0.921**	-0.260		-0.264	-0.340	-0.276	-0.330	0.006	0.008
	(0.26)	(0.45)	(0.30)		(0.26)	(0.26)	(0.26)	(0.27)	(0.19)	(0.19)
Constant	1.092**	1.291**	0.863	2.419*	1.169**	1.090**	1.168**	1.096**	0.987**	0.989**
	(0.46)	(0.53)	(1.03)	(1.28)	(0.47)	(0.46)	(0.47)	(0.46)	(0.47)	(0.48)
Observations	448	401	232	183	441	441	434	443	443	438
DV mean	0.585	0.569	0.599	0.563	0.583	0.587	0.585	0.587	0.585	0.587
INDV mean	0.632	0.646	0.647	0.689	0.637	0.635	0.641	0.628	0.632	0.628
Pre-trend p-value	0.482	0.284	0.360	0.212	0.412	0.476	0.420	0.488	0.435	0.441
Removed from the sample		TOP	BOTTOM	BOTH	TOP	BOTTOM	BOTH	TOP	BOTTOM	BOTH
Control groups:										
Homicide	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed effects:										
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Head Judge FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Introducer FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Column (1) presents the benchmark specification. Columns (2)–(4) exclude observations with extreme values of delay days (top percentile, bottom percentile, and both tails, respectively). Columns (5)–(7) exclude observations with extreme values of the share of male judges. Columns (8)–(10) exclude observations with extreme values of average judicial experience. In each case, "TOP" indicates removal of the top 1st percentile, "BOTTOM" indicates removal of the bottom 1st percentile, and "BOTH" indicates removal of both the top and bottom 1st percentiles. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, day-of-week fixed effects, and fixed effects for the head judge and the introducer, as indicated. *After 2010* is a binary indicator equal to 1 if the hearing is held after January 1st, 2010. *Rejection* is a dummy equal to 1 if the appeal is rejected. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. *Pre-trend p-value* reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors are reported in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A3 Appendix: Potential Mechanisms

Table A7: Appeal Rejection and Google Trends (Post-2010 Sample)

	Rejection								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tax Evasion	0.222*	0.331**	0.366**	0.172	0.290**	0.311**	0.169	0.248*	0.228
	(0.116)	(0.147)	(0.148)	(0.110)	(0.128)	(0.140)	(0.125)	(0.139)	(0.150)
F1 Google Trends (Tax Evasion)	0.003	0.001	0.002						
	(0.003)	(0.004)	(0.004)						
Tax Evasion X F1 Google Trends (Tax Evasion)	0.001	0.001	-0.001						
	(0.003)	(0.003)	(0.003)						
Google Trends (Tax Evasion)				-0.003	-0.004	-0.005			
				(0.002)	(0.004)	(0.004)			
Tax Evasion X Google Trends (Tax Evasion)				0.006**	0.006*	0.006**			
				(0.003)	(0.003)	(0.003)			
L1 Google Trends (Tax Evasion)							-0.003	-0.005	-0.003
							(0.004)	(0.005)	(0.004)
Tax Evasion X L1 Google Trends (Tax Evasion)							0.006	0.008	0.009
							(0.004)	(0.006)	(0.006)
Large committee			-0.077			-0.032			0.007
			(0.328)			(0.301)			(0.306)
Average committee experience			-0.003			0.058			0.012
			(0.164)			(0.166)			(0.160)
Company/Public Org.			0.052			-0.156			-0.192
			(0.210)			(0.196)			(0.191)
Number Defendants			-0.036			-0.069			-0.068
			(0.048)			(0.043)			(0.045)
Share male defendants			-0.032			-0.162			-0.178
			(0.354)			(0.305)			(0.305)
Share female defendants			-0.305			-0.395			-0.408
			(0.361)			(0.315)			(0.319)
Missing defendants			-0.062			-0.226			-0.168
			(0.443)			(0.384)			(0.395)
Share male judges			-0.084			0.100			0.360
			(1.236)			(1.218)			(1.226)
Share female judges			0.192			0.380			0.629
			(1.290)			(1.270)			(1.281)
Male District Attorney			0.458**			0.494**			0.472**
			(0.207)			(0.196)			(0.206)
Head is introducer			-0.108			-0.105			-0.156
			(0.230)			(0.236)			(0.240)
Constant	0.501***	0.496***	0.196	0.602***	0.545***	0.028	0.602***	0.573***	-0.108
	(0.085)	(0.100)	(1.152)	(0.079)	(0.086)	(1.071)	(0.090)	(0.099)	(1.092)
Observations	206	190	189	210	194	193	210	194	193
DV mean	0.694	0.700	0.698	0.690	0.696	0.694	0.690	0.696	0.694
INDV1 mean	0.563	0.568	0.566	0.552	0.557	0.554	0.552	0.557	0.554
INDV2 mean	17.07	16.19	16.28	18.40	18.78	18.88	15.24	15.26	15.34
Control groups:									
Homicide	✓	✓	✓	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed effects:									
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Head Judge FE		✓	✓		✓	✓		✓	✓
Introducer FE		✓	✓		✓	✓		✓	✓
H0: Lagged = Contemporaneous									
p-value	0.920	0.262	0.652						
Lead = Contemporaneous									
p-value	0.206	0.468	0.063						

Notes: The table reports regressions of appeal rejection on tax evasion cases and Google Trends measures related to tax evasion using the subsample of trials after 2010. Columns (1)–(3) include the lead Google Trends index for tax evasion searches, Columns (4)–(6) include the contemporaneous Google Trends index, and Columns (7)–(9) include the lagged Google Trends index. Interaction terms between Tax Evasion and the Google Trends measures capture whether changes in search intensity for tax evasion are associated with differential rejection rates for tax evasion cases relative to the control crime categories. All specifications include year, court-type, month, and day-of-week fixed effects, as well as head judge and introducer fixed effects. Control groups consist of homicide and rape cases. *DV mean* reports the mean of the dependent variable in the sample. *INDV1 mean* reports the mean of the Tax Evasion indicator in the sample. *INDV2 mean* reports the mean of the Google Trends index for tax evasion searches. The rows "Lagged = Contemporaneous" and "Lead = Contemporaneous" report p-values from tests comparing the lagged and lead coefficients with the contemporaneous specification. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Appeal Rejection and Google Trends (Full Sample)

	Rejection								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tax Evasion	-0.045	-0.062	-0.054	-0.049	-0.044	-0.025	-0.049	-0.079	-0.081
	(0.09)	(0.10)	(0.11)	(0.09)	(0.10)	(0.10)	(0.09)	(0.10)	(0.11)
F1 Google Trends (Tax Evasion)	-0.000	-0.002	-0.002						
	(0.00)	(0.00)	(0.00)						
Tax Evasion X F1 Google Trends (Tax Evasion)	0.006**	0.007***	0.007***						
	(0.00)	(0.00)	(0.00)						
Google Trends (Tax Evasion)				-0.002	-0.003	-0.005			
				(0.00)	(0.00)	(0.00)			
Tax Evasion X Google Trends (Tax Evasion)				0.007***	0.007***	0.007**			
				(0.00)	(0.00)	(0.00)			
L1 Google Trends (Tax Evasion)							-0.005	-0.007	-0.006
							(0.00)	(0.00)	(0.00)
Tax Evasion X L1 Google Trends (Tax Evasion)							0.008**	0.012***	0.012**
							(0.00)	(0.00)	(0.00)
Large committee			-0.166			-0.099			-0.088
			(0.15)			(0.15)			(0.15)
Average committee experience			-0.112			-0.093			-0.130
			(0.08)			(0.08)			(0.08)
Company/Public Org.			0.039			-0.040			-0.028
			(0.13)			(0.12)			(0.12)
Number Defendants			-0.021			-0.027			-0.025
			(0.03)			(0.03)			(0.03)
Share male defendants			0.080			0.027			-0.007
			(0.26)			(0.24)			(0.24)
Share female defendants			-0.071			-0.126			-0.173
			(0.26)			(0.25)			(0.25)
Missing defendants			0.341			0.271			0.259
			(0.33)			(0.31)			(0.31)
Share male judges			0.711			1.024			1.059
			(0.74)			(0.75)			(0.73)
Share female judges			0.947			1.284			1.295*
			(0.78)			(0.79)			(0.76)
Male District Attorney			0.182			0.192			0.197
			(0.13)			(0.14)			(0.14)
Head is introducer			-0.401			-0.418			-0.349
			(0.29)			(0.30)			(0.26)
Constant	0.600***	0.621***	0.035	0.606***	0.618***	-0.330	0.630***	0.645***	-0.231
	(0.06)	(0.07)	(0.82)	(0.06)	(0.06)	(0.80)	(0.06)	(0.07)	(0.78)
Observations	509	484	482	513	488	486	503	477	475
DV mean	0.593	0.595	0.595	0.593	0.594	0.595	0.592	0.593	0.594
INDV1 mean	0.609	0.616	0.614	0.604	0.611	0.609	0.608	0.614	0.613
INDV2 mean	6.910	6.663	6.691	7.532	7.525	7.556	6.364	6.249	6.276
Control groups:									
Homicide	✓	✓	✓	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed effects:									
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Head Judge FE		✓	✓		✓	✓		✓	✓
Introducer FE		✓	✓		✓	✓		✓	✓
H0: Lagged = Contemporaneous									
p-value	0.595	0.650	0.224						
Lead = Contemporaneous									
p-value	0.849	0.176	0.971						

Notes: The table reports regressions of appeal rejection on tax-evasion cases and Google Trends measures related to tax evasion using the full sample. Columns (1)–(3) include the lead Google Trends for tax evasion searches, Columns (4)–(6) include the contemporaneous Google Trends index, and Columns (7)–(9) include the lagged Google Trends index. Interaction terms between Tax Evasion and the Google Trends measures capture whether changes in search intensity for tax evasion are associated with differential rejection rates for tax-evasion cases relative to the control crime categories. All specifications include year, court-type, month, and day-of-week fixed effects, as well as head judge and introducer fixed effects. Control groups consist of homicide and rape cases. *DV mean* reports the mean of the dependent variable in the sample. *INDV1 mean* reports the mean of the Tax Evasion indicator in the sample. *INDV2 mean* reports the mean of the Google Trends index for tax evasion searches. The rows "Lagged = Contemporaneous" and "Lead = Contemporaneous" report p-values from tests comparing the lagged and lead coefficients with the contemporaneous specification. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: Decision Delays as Outcomes: Levels and Logs

	Delay days			Delay days (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Evasion	-2.754	-10.128	-6.547	-0.024	0.031	0.087
	(15.16)	(14.15)	(14.49)	(0.25)	(0.20)	(0.22)
Tax Evasion X After 2010	22.014	20.517*	19.491	0.365	0.340	0.289
	(14.76)	(11.98)	(12.00)	(0.30)	(0.26)	(0.25)
Large Committee			-33.852*			-0.691**
			(17.57)			(0.27)
Average Committee Experience			-30.264**			-0.545*
			(14.23)			(0.32)
Company/Public Org.			-14.769			-0.287
			(14.86)			(0.20)
Number Defendants			1.559			-0.011
			(4.28)			(0.06)
Share Male Defendants			29.470			0.292
			(30.63)			(0.48)
Share Female Defendants			28.441			0.332
			(29.62)			(0.43)
Missing Defendants			84.990**			0.406
			(42.18)			(0.71)
Share Male Judges			10.498			-0.254
			(22.20)			(0.35)
Male District Attorney			28.352			-0.268
			(22.24)			(0.37)
Head is Introducer			-19.713			-0.290
			(29.96)			(0.71)
Constant	73.812***	78.825***	110.839**	3.835***	3.806***	5.817***
	(8.66)	(7.77)	(55.11)	(0.13)	(0.13)	(1.42)
Observations	448	448	448	448	448	448
DV mean	77.28	77.28	77.28	3.906	3.906	3.906
INDV mean	0.632	0.632	0.632	0.632	0.632	0.632
Pre-trend p-value	0.366	0.840	0.779	0.712	0.983	0.991
Control groups:						
Homicide	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓
Fixed effects:						
Year FE	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓
Head Judge FE		✓	✓		✓	✓
Introducer FE		✓	✓		✓	✓

Notes: The table reports treatment–control differences in decision delays before and after 2010. Interaction terms between Tax Evasion and After 2010 capture differential changes in decision delays for tax evasion cases relative to other crime categories after 2010. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, and day-of-week fixed effects, as well as fixed effects for the head judge and the introducer, as indicated. *After 2010* is a binary indicator equal to 1 if the hearing is held after January 1, 2010. *Delay Days* is measured as the number of days between the trial date and the final decision, and *Delay Days (Log)* is defined as the natural logarithm of delay days +0.0001. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. *Pre-trend p-value* reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: Baseline Specification with Legal Representation Outcomes

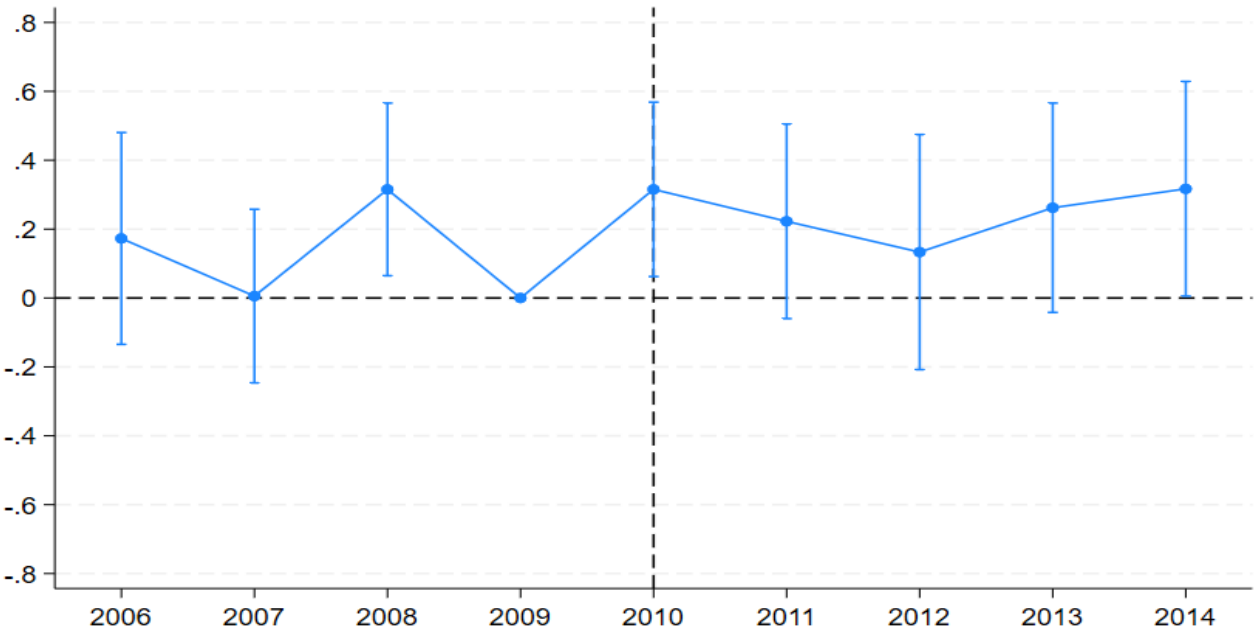
	Number of Lawyers			Average Lawyers per Defendant			Share of Male Lawyers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tax Evasion	-0.450***	-0.363**	-0.396**	0.060	0.065	0.045	-0.015	-0.046	-0.079
	(0.16)	(0.18)	(0.20)	(0.08)	(0.09)	(0.08)	(0.08)	(0.11)	(0.11)
Tax Evasion X After 2010	0.056	0.153	0.195	-0.015	0.043	0.027	0.038	0.085	0.113
	(0.12)	(0.13)	(0.13)	(0.08)	(0.08)	(0.08)	(0.06)	(0.07)	(0.08)
Large Committee			-0.187			0.057			-0.102
			(0.22)			(0.09)			(0.12)
Company/Public Org.			0.322			0.001			0.113*
			(0.20)			(0.07)			(0.06)
Share Male Defendants			0.043			0.929***			0.104
			(0.10)			(0.11)			(0.22)
Share Female Defendants			0.069			0.929***			0.088
			(0.10)			(0.10)			(0.22)
Share Male Judges			0.071			0.029			-0.137
			(0.17)			(0.10)			(0.13)
Average Judges Experience			-0.055			-0.052			-0.036
			(0.07)			(0.04)			(0.04)
Head is Introducer			-0.035			0.322**			-0.079
			(0.19)			(0.15)			(0.11)
Male District Attorney			0.061			-0.027			0.049
			(0.09)			(0.06)			(0.08)
Constant	1.392***	1.312***	1.445***	0.914***	0.897***	0.051	0.928***	0.937***	1.068***
	(0.12)	(0.11)	(0.32)	(0.05)	(0.05)	(0.16)	(0.05)	(0.07)	(0.24)
Observations	370	369	369	370	369	369	370	369	369
DV mean	1.108	1.108	1.108	0.950	0.950	0.950	0.927	0.927	0.927
INDV mean	0.659	0.661	0.661	0.659	0.661	0.661	0.659	0.661	0.661
Pre-trend p-value	0.164	0.283	0.274	0.318	0.100	0.0782	0.229	0.645	0.599
Control groups:									
Homicide	✓	✓	✓	✓	✓	✓	✓	✓	✓
Rape	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed effects:									
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Court Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Semester FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Week day FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Head Judge FE		✓	✓		✓	✓		✓	✓
Introducer FE		✓	✓		✓	✓		✓	✓

Notes: The dependent variables are *Nr. Lawyers*, the total number of lawyers representing the parties in the appeal; *Avg. Nr. Lawyers*, defined as the ratio of the total number of lawyers to the number of defendants; and *Share of Male Lawyers*, defined as the share of male lawyers among all lawyers involved in the case. All specifications include year fixed effects, court-type fixed effects, semester fixed effects, day-of-week fixed effects, and fixed effects for the head judge and the introducer, as indicated. *After 2010* is a binary indicator equal to 1 if the hearing is held after January 1, 2010. *DV mean* reports the mean of the dependent variable in the sample. *INDV mean* reports the mean of *Tax Evasion* in the sample. *Pre-trend p-value* reports the p-value from a test of parallel pre-treatment trends using 2010 as the reference year. Robust standard errors in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix: Figures

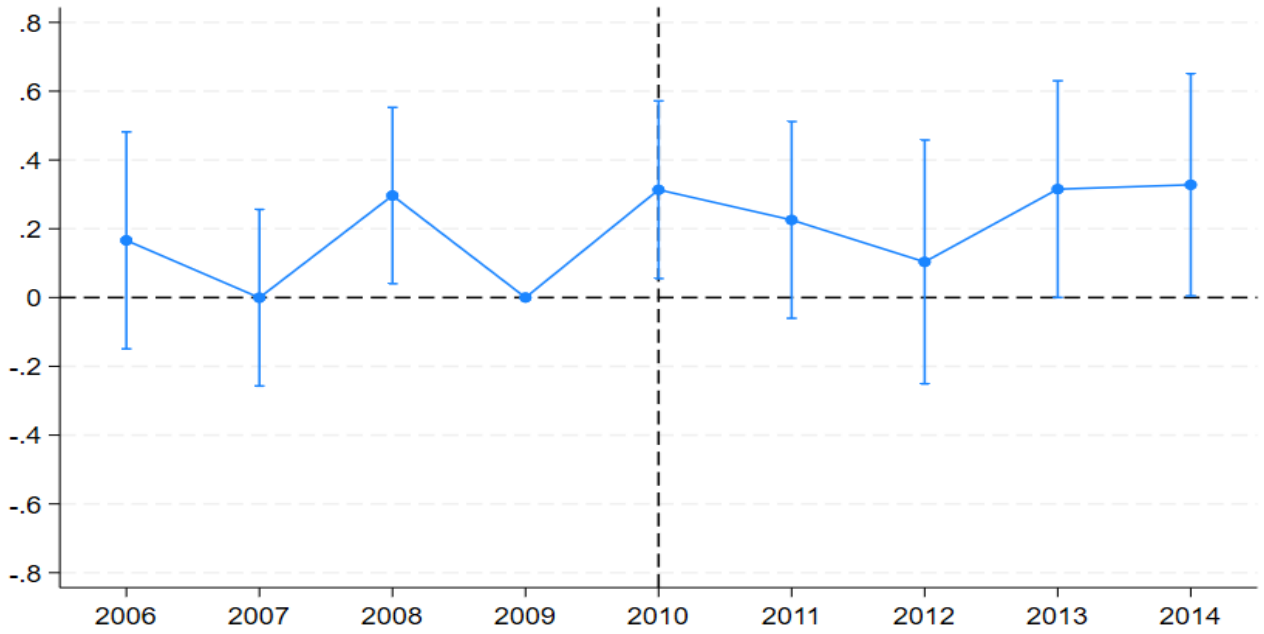
A4 Appendix: Event Study with Different Control Groups

Figure A1: Event-Study Estimates with Alternative Control Group 1



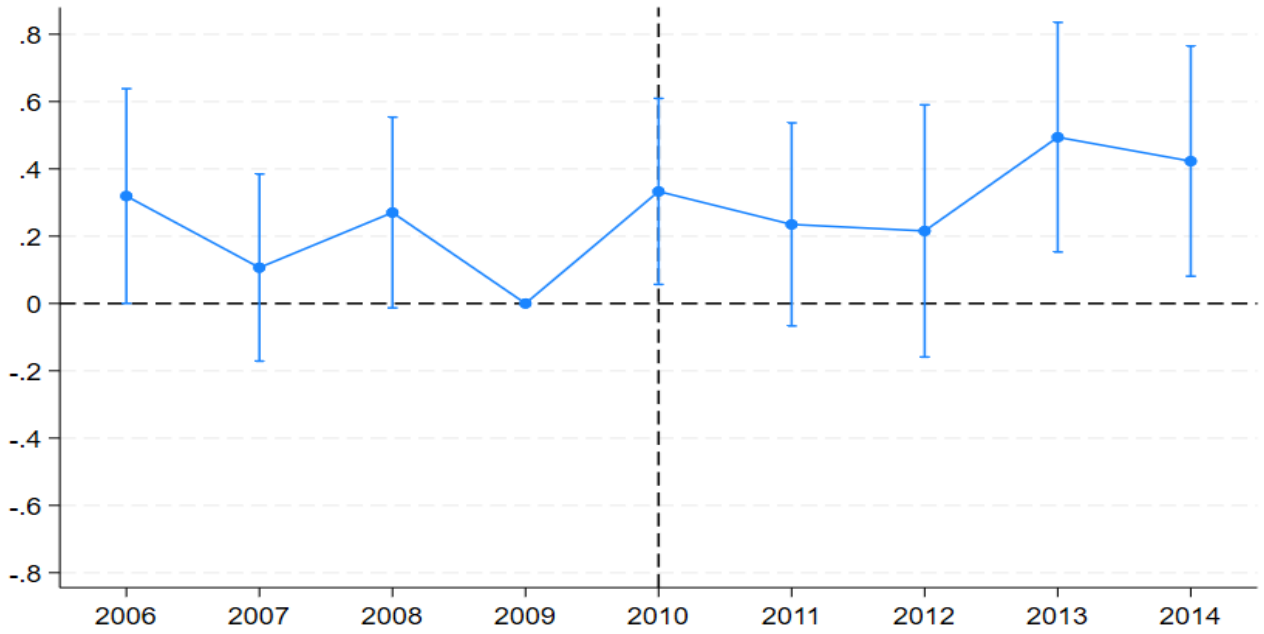
Notes: The figure reports the coefficients on the interaction between tax evasion and year indicators from the event-study specification. The omitted reference year is 2010 (indicated by the vertical dashed line). All regressions include fixed effects, and the control group includes the following case types: homicide, rape, immigration, injuries, environmental offences, and misconduct. The dashed horizontal line denotes zero. Error bars represent 90% confidence intervals. The p-value of the joint test of pre-treatment coefficients (relative to 2010) is 0.062 .

Figure A2: Event-Study Estimates with Alternative Control Group 2



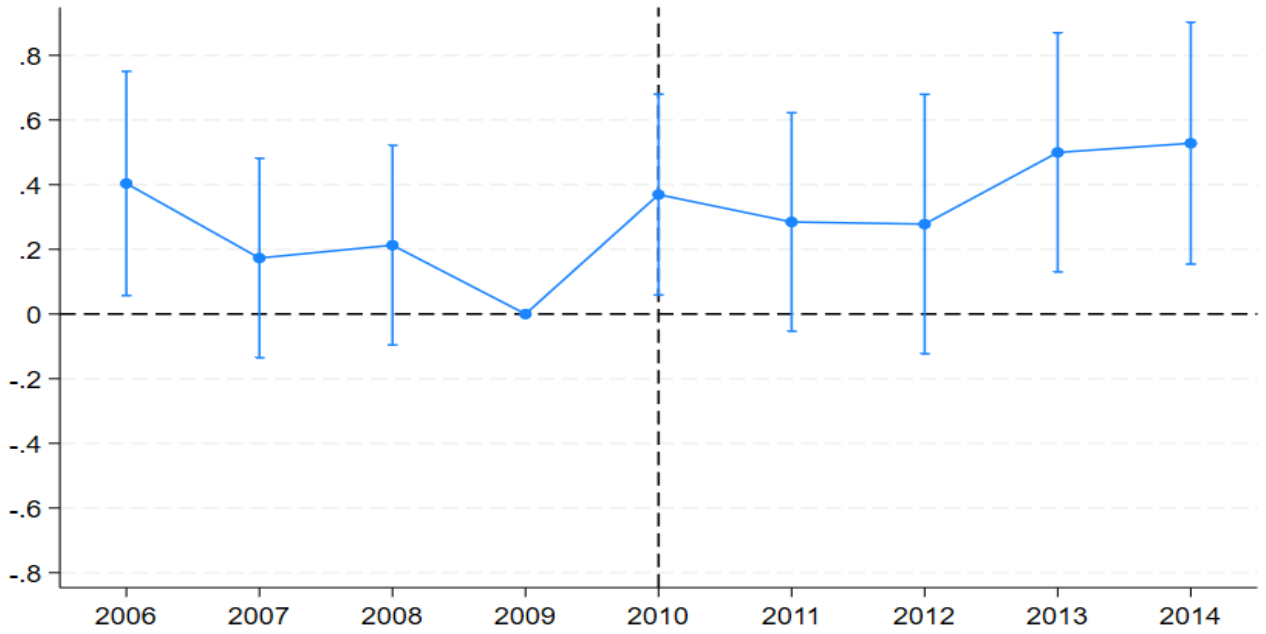
Notes: The figure reports the coefficients on the interaction between tax evasion and year indicators from the event-study specification. The omitted reference year is 2010 (indicated by the vertical dashed line). All regressions include fixed effects and the control group includes the following case types: homicide, rape, immigration, injuries, and environmental offences. The dashed horizontal line denotes zero. Error bars represent 90% confidence intervals. The p-value of the joint test of pre-treatment coefficients (relative to 2010) is 0.085 .

Figure A3: Event-Study Estimates with Alternative Control Group 3



Notes: The figure reports the coefficients on the interaction between tax evasion and year indicators from the event-study specification. The omitted reference year is 2010 (indicated by the vertical dashed line). All regressions include fixed effects, and the control group includes the following case types: homicide, rape, immigration, and injuries. The dashed horizontal line denotes zero. Error bars represent 90% confidence intervals. The p-value of the joint test of pre-treatment coefficients (relative to 2010) is 0.224 .

Figure A4: Event-Study Estimates with Alternative Control Group 4



Notes: The figure reports the coefficients on the interaction between tax evasion and year indicators from the event-study specification. The omitted reference year is 2010 (indicated by the vertical dashed line). All regressions include fixed effects and the control group includes the following case types: homicide, rape, and immigration. The dashed horizontal line denotes zero. Error bars represent 90% confidence intervals. The p-value of the joint test of pre-treatment coefficients (relative to 2010) is 0.253 .