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1 Introduction

The rule of law is at the foundation of well-functioning institutions (Hayek, 1960). But laws can function as a rule only if they are "accessible, intelligible, clear and predictable" (Bingham, 2010).[1] Despite these requirements, legislative actors—whether elected officials or civil servants—often enact legislation hastily, prioritizing political compromise, personal career concerns, and short-term visibility over substantive legal quality. As a result, laws are often poorly drafted and, in many cases, unintentionally ambiguous.[2] Ambiguous laws, in turn, create uncertainty about rights and obligations (D'Amato, 1983), discouraging investment, trade, and other economic activities that require legal protection—a concern already emphasized by Emile Durkheim and Max Weber back in the 19th century (Trubek, 2004). While economic uncertainty has been widely studied (e.g., Dixit and Pindyck, 1994; Guiso and Parigi, 1999; Bloom, 2009), little is known about how agents respond to the legal uncertainty caused by poorly drafted statutes. In this paper, we develop a novel measure of such uncertainty and estimate its economic costs.

Laws may admit multiple interpretations and be ambiguous for several reasons. In this paper, we focus exclusively on legal ambiguity arising from poor drafting quality, setting aside deliberate ambiguity present to preserve generality or allow for judicial discretion.[3] To isolate this component, we apply text analysis to the full corpus of Italian legislation—more than 75,000 statutes comprising roughly 97 million words. We measure drafting quality (orthogonal to substantive complexity) using objective stylistic indicators drawn from law-drafting manuals (Cassese, 1993; Butt and Castle, 2006), alongside measures derived from Large Language Models (LLMs). There is substantial evidence that Italian laws are poorly drafted. For instance, 85% of sentences in Italian legislation exceed the 25-word maximum that linguists recommend to ensure readability (Fioritto, 1997; Cortellazzo and Pellegrino, 2003; Alfieri et al., 2011); Italian laws also frequently rely on complex subordinate clauses, dense cross-references to other laws, and archaic terms, all of which have been shown to unduly reduce comprehensibility (Doherty, 2020; Martinez et al., 2022, 2023; OECD, 2024).

An ambiguous law can lead two judges to rule differently on the same case, even when presented with identical information. Greater statutory ambiguity increases the likelihood of judicial disagreement and makes the legal consequences of individuals' actions less pre-

[1] The principle that laws must be clear and predictable—to ensure citizens are governed by known rules rather than arbitrary discretion—is widely accepted. For instance, the European Commission (2016) states that legal acts must be "clear, easy to understand and unambiguous; simple and concise, avoiding unnecessary elements; precise, leaving no uncertainty in the mind of the reader."

[2] See Gratton et al. (2021) for a model and evidence on the political dynamics behind these outcomes.

[3] This distinction is consistent with recent experimental evidence showing that the difficulty of understanding legal documents often stems from poor writing (Martinez et al., 2021) and that even legal experts prefer documents written in clear, plain language (Martinez et al., 2023).

dictable. In short, it increases *legal uncertainty*. To measure the effect of drafting clarity on legal uncertainty, we leverage a key institutional feature of the Italian legal system, inherited from the French Revolution and introduced during the Napoleonic invasion. The Italian judicial system includes several inferior courts (hereafter, lower courts) and a Supreme Court of Cassation (hereafter, Court of Cassation), which serves as the court of last resort. The Court of Cassation has a *nomophylactic* mandate, meaning that it ensures the uniform interpretation of laws across the country over time (Art. 65, RD January 30, 1941, n. 12)—resolving each case on its merits without creating binding precedent. Appeals to the Court of Cassation are brought only on the grounds of an alleged error in legal interpretation, and no additional evidence can be introduced. Because disagreements between a lower court and the Court of Cassation arise solely from differing interpretations, the system closely resembles an ideal experiment in which drafting quality varies across cases, and two judges independently interpret the same law in each case.[4]

We process all judgments by the Court of Cassation between 2004 and 2017 (approximately half a million cases). We assign to each law cited in a judgment our previously constructed measure of drafting clarity.[5] We then estimate how the clarity of these laws affects the likelihood that the lower court and the Court of Cassation reach different conclusions, which we term the *reversal probability*. Controlling for a comprehensive set of case-specific characteristics, we find strong evidence that poor drafting increases the reversal probability. In line with Orwell (1946), who argued that the debasement of language is unnecessary regardless of the subject matter, and consistent with the plain language movement in legal writing initiated by Mellinkoff (1963), we show that the variation in drafting clarity that drives the effect on the reversal probability is not an artifact of the underlying complexity of a law's subject matter. Rather, it reflects distortions in the legislative process during periods of political emergency or instability.

We extend the reversal probability model by interacting the measure of law-drafting clarity with a full set of dummy variables for the lower court whose case is appealed to the Court of Cassation. The interaction coefficients reveal systematic differences across lower courts, reflecting variation in how the same laws are interpreted across Italy, a phenomenon already noted in the literature (Ichino, Polo, and Rettore, 2003; Pais and Marcolin, 2024). This implies that firms subject to different courts' jurisdictions face different degrees of legal uncertainty due to statutory ambiguity. Differences across lower courts reflect variation in judicial alignment with the Court of Cassation, influenced by enduring local interpreta-

[4]Another important institutional feature brings the design close to the ideal experiment: unlike in the US, France, or Germany, appeals to the Court of Cassation in Italy are relatively common and inexpensive, with approximately 32% of lower-court decisions subsequently reviewed by the Court of Cassation .

[5]According to Article 111 of the Constitution and Article 132 of the Code of Civil Procedure, the Court of Cassation should motivate its judgments by citing the laws that justify its decisions.

tive doctrines, judicial practices, and jurisprudential traditions.[6] Our estimates align with anecdotal evidence on differences across lower courts and indicate that judges from lower courts who disagree less frequently with the Court of Cassation are significantly more likely to be promoted to it, suggesting that the two judicial bodies share some legal traits.

We study the universe of Italian limited liability companies, assigning to each firm-year the corresponding measure of legal uncertainty based on the jurisdiction of its headquarters. To identify a shock to legal uncertainty, we exploit the 2012 court consolidation that abolished some district courts and mechanically reassigned firms to other courts, generating exogenous variation in their legal uncertainty. The consolidation was exogenous to local economic conditions, as it was primarily motivated by the pursuit of economies of scale in the judiciary (Mocetti, Pesenti, and Roma, 2025), and courts selected for closure were chosen largely for administrative reasons unrelated to the local economy (Pezone, 2022). We exploit this variation to estimate the effects of legal uncertainty using a continuous-treatment difference-in-differences regression. We find no evidence of pre-trends before the 2012 reform, supporting a causal interpretation of our estimated coefficients. Legal uncertainty significantly reduces the firm's production growth rate and investment rate (investment-to-capital ratio). All else equal, in the five years following the reform, a one standard deviation increase in the legal uncertainty shock reduces the firm's annual growth rate by 1.2 percentage points (26% of the sample median), while the investment rate declines by around 1 percentage point (approximately 10% of the sample median). We also find some evidence that firms respond to increased legal uncertainty by accumulating precautionary provisions, consistent with the hypothesis that formal insurance markets do not fully cover risks arising from ambiguous laws (Dietz and Niehorster, 2019, p. 4). Estimates are robust to alternative control groups and estimators for continuous treatment effects, addressing concerns that legal uncertainty may exhibit non-random changes due to mean reversion (de Chaisemartin and Lei, 2024).

We conduct several additional checks. To ensure that our legal uncertainty shock is not confounded by changes in other court characteristics, we account for changes in court size, magistrates' experience, trial length, the average reversal probability of appeals originating from the court, and firm-court distance. We also control for the introduction of specialized business courts—another potential local shock—and construct alternative uncertainty

[6]These distinct local traditions are sometimes referred to as "Scuole giuridiche." The literature documents, among others, a Bolognese tradition (Franzoni, 2017; de Vergottini and Zanobetti, 2024), a Neapolitan tradition (Tessitore, 1979; Monticelli, 2006), a Turin tradition (Ciatti, 2017), a Genoese tradition (Benedetti, 2025), and a Messina tradition (Trimarchi, 2017). These "Scuole" are not schools in the narrow sense of a unified theoretical manifesto or a codified methodology; rather, they capture persistent differences in interpretative philosophies, approaches to legal reasoning and jurisprudential practices. Similar differences across courts are also present in the US; see Church (1985) and Ostrom et al. (2007).

shock measures unaffected by any civil procedure changes introduced by the 2012 reform. The results also remain unchanged when the analysis is restricted to treated firms only (i.e., those previously under the jurisdiction of an absorbed court). In this subsample, identification relies exclusively on cross-municipality differences in the size of the uncertainty shock, rendering the estimates immune to any reform-induced changes that might have differentially affected treated and untreated firms.

We quantify the long-run output costs by expressing steady-state GDP per capita as a function of the average firm growth rate, the business creation rate, the firm exit rate, and firm size at entry. We estimate the separate effects of legal drafting clarity on each margin. We conclude that if all Italian laws were drafted with the same clarity as the fundamental principles of the Italian Constitution—which roughly corresponds to the top quartile of the cross-sectional distribution of drafting quality across all Italian laws—, current GDP per capita would be 4.9 percent higher. Around two-thirds of the losses have materialized over the past two decades, due to a marked deterioration in the drafting clarity of Italian laws.

Relation to the literature A large body of work has emphasized that property rights (Alchian 1965; Alchian and Demsetz 1973) and institutional quality (Williamson 1985; North 1990; Acemoglu, Johnson, and Robinson 2005) are key determinants of economic performance. This view is supported by extensive empirical evidence (La Porta et al. 1998, 2008). We contribute to this literature by providing the first empirical assessment of the economic importance of legal clarity, which we view as a foundational element of the rule of law.

The effects of uncertainty are well understood theoretically (Dixit and Pindyck, 1994) and extensively documented empirically, with a particular focus on firm investment responses to unpredictable fluctuations in uncertainty about demand, productivity (e.g., Guiso and Parigi 1999; Bloom 2009; Bloom, Bond, and Van Reenen 2007), or prices (e.g., Kellogg 2014). A related literature has also developed measures of uncertainty using various sources, including the frequency of uncertainty-related terms in IMF reports (Ahir, Bloom, and Furceri 2018) or media content used to construct a policy-related uncertainty index (Baker, Bloom, and Davis 2016). We are the first to provide a measure of legal uncertainty caused by the quality of legal drafting and to study its economic effects.

The legal uncertainty we identify arises from heterogeneity in judges' interpretations of laws, aligning closely with Knight (1921) original concept of uncertainty. The notion that multiple interpretations of a law generate "legal uncertainty" (Holmes 1897; Farago 1980; D'Amato 1983) or "legal ambiguity" (Edelman et al. 1991; Poscher 2012) is widely recognized among legal scholars and seen as a fundamental threat for the rule of law. This idea also underpins the U.S. Supreme Court's "void-for-vagueness" doctrine, originally introduced by Amsterdam (1960). To our knowledge, we provide the first quantification of

how the interpretative ambiguity due to poor drafting affects judicial predictability.

Recent literature has begun to directly measure legal risk and its economic impact. Ryu (2025) uses text analysis of firms' earnings call transcripts and shows that legal risk concerns are priced by financial markets. Lee, Schoenherr, and Starmans (2024) find that reducing legal uncertainty—stemming from randomness in judge assignment or changes in legal rules—improves credit access, stimulates investment, and facilitates debt restructuring. Complementing this work, we identify a distinct source of legal uncertainty arising from poor law drafting. This form of uncertainty merits special attention not only because it may unduly undermine the rule of law—imposing the high costs found in this paper—but also because it may be addressed through relatively simple and cost-effective interventions to improve legislative drafting (see Wiener 1950, chap. 7, for further discussion).

The rest of the paper is organized as follows. Section 2 outlines the institutional context and the identification framework. Section 3 describes the data. Section 4 examines how poor drafting affects legal uncertainty. Section 5 analyzes the effects on firms. Section 6 quantifies the impact on Italian GDP. Section 7 concludes.

2 Institutional background and identification

We first describe the Italian institutional environment. Then we discuss identification and the econometric specification.

2.1 Institutional background

The Italian judicial system is composed of district courts, which comprise trial courts and, in some cases, courts of appeal, and the Court of Cassation.[7] The 2012 reform mandated the closure of 30 of the 165 previously existing district courts.[8] Each district court has jurisdiction over multiple municipalities, defining its legal district. Final decisions by a

[7] All district courts include trial courts, and 29 of them also host a court of appeal. District courts may additionally house specialized courts, including Justices of the Peace, who handle minor civil, administrative, and criminal cases; Juvenile Courts, which deal with cases involving individuals under 18; Courts of Freedom, which review precautionary measures such as pre-trial detention; Supervisory Courts, which oversee the execution of criminal sentences; and Judges for Preliminary Investigations, who supervise the pre-trial phase of criminal proceedings.

[8] The originally planned number of closures was 31, not 30, since the list also included the Court of Urbino, whose suppression was immediately declared unconstitutional by the Constitutional Court (Judgment No. 237, 3-24 July 2013). The reform also led to the creation of a new court—Napoli Nord, seated in Aversa—which we exclude from the analysis because no pre-reform data are available.

district court can ultimately reach the Court of Cassation.[9] The Court of Cassation is divided into 13 specialized sections: 6 handle civil cases and 7 handle criminal cases. Important cases requiring broad expertise are heard jointly by all civil or criminal sections—the so-called United Sections. Decisions of the Court of Cassation are influential but, unlike in common law systems, are not legally binding on future rulings of lower courts.[10] The role of the Court of Cassation is to ensure consistent interpretation of laws across the country over time—the already mentioned nomophylactic mandate. This means that, in each appeal, the Court of Cassation (i) makes its decision based on the same evidence and information considered by the lower court; (ii) reviews only whether the lower court correctly interpreted the law. The Court of Cassation can either uphold or reverse the lower court's decision. A reversal indicates that the Court of Cassation and the lower court differed in their interpretation of the relevant laws in the case at hand. If the Court reverses the decision, it may amend the ruling or annul it entirely and send the case back to the lower court for reexamination. If the Court upholds the decision, the ruling becomes final and cannot be appealed further.

2.2 Identification framework

We identify the effects of law drafting clarity on legal uncertainty and firm outcomes using two key equations. The first is an *uncertainty equation*, which links legal uncertainty to the quality of law drafting. The second is a *firm-outcome equation*, which relates firm growth, investment, and other business decisions to legal uncertainty. Causal identification comes from the 2012 court consolidation, which generated exogenous time variation in the level of legal uncertainty faced by some firms.

We denote by $p \geq 0$ the degree of poor drafting of a law, with $p = 0$ indicating optimal drafting. Poor drafting makes a law ambiguous, increasing the risk that two judges interpret

[9] Appeals to the Court of Cassation from a district court can originate from its trial court, its Court of Appeal (if applicable), or its specialized courts (if applicable). Rulings from a trial court can reach the Court of Cassation directly when both parties agree to bypass the Court of Appeal—a special procedure called *per saltum*, regulated by *Codice di Procedura Penale* (CPP) article 569 and *Codice di Procedura Civile* (CPC) article 360—or when the trial court rules on an appeal from a Justice of the Peace.

[10] The Italian and US judicial systems are grounded in distinct civil and common law traditions and differ in important ways. Lower courts in Italy are not formally bound by past rulings of the Court of Cassation, whereas the US Supreme Court sets binding precedent for lower courts under the principle of *stare decisis*. In Italy, any party can appeal to the Court of Cassation by claiming that a lower court misinterpreted the law, while the US Supreme Court exercises significant discretion in choosing which cases to hear. As a result, the Italian Court of Cassation handles a much larger caseload and has many more judges: in 2018, it heard over 35,000 cases managed by more than 100 judges. By contrast, the US Supreme Court heard only 70 cases, handled by just 9 judges. Finally, Italy also has a separate Constitutional Court that reviews the constitutionality of laws, whereas the US Supreme Court combines the functions of both the Court of Cassation and the Constitutional Court.

it differently. Legal ambiguity may gradually vanish over time. Accordingly, we assume that the ambiguity of a law with drafting clarity p and age τ is:

$$a = (1 - \delta)^{\tau} p \tag{1}$$

where δ measures the rate at which a widely accepted interpretation of a new law gradually emerges over time through the cumulative mutual contributions of lower courts, the Court of Cassation, doctrine (academics and other legal scholars), and administrative authorities.[11]

Let r_{ijt} be an indicator variable taking value 1 if case i, initially judged by lower court j, is reversed by the Court of Cassation at time t. Let a_{ijt} denote the average ambiguity of all laws relevant to case i, calculated as in (1). There are three possible reasons why lower courts and the Court of Cassation might disagree on case i, even if the evidence is the same. First, it could be due to *legal ambiguity* caused by poor drafting, measured by higher a_{ijt}. Second, it could be due to the *fundamental complexity* of case i, which makes courts interpret the facts differently even if laws are drafted perfectly ($a = 0$). Finally, there may be random variation over time in judges' interpretation of the evidence, due to personal or external conditions. As a result, the reversal probability r_{ijt} is characterized by the following *Uncertainty Equation*:

$$r_{ijt} = \lambda_j \times a_{ij} + s_j + c_i + \xi_{ijt} \tag{UE}$$

where λ_j measures the *misalignment* of court j with the Court of Cassation in interpreting laws with ambiguity a_{ij}. As discussed above and analyzed further below, the variation in λ_j across lower courts is shaped by enduring local interpretative doctrines, judicial practices, and jurisprudential traditions. In (UE), the fixed effect s_j measures the average reversal rate of cases originating from court j, which depends on the court's characteristics. The term c_i represents proxies for case-i heterogeneity, including the case topic, the length of the Court of Cassation's judgment, and additional fixed effects described below. Finally, ξ_{ijt} is a random shock arising because the lower court and the Court of Cassation deliberate on the case at different points in time.

The level of legal uncertainty *due to ambiguous laws* faced by firms subject to the jurisdiction of court j is equal to

$$u_j \equiv \lambda_j \times \bar{a} \tag{2}$$

[11] These actors influence one another through ongoing interactions. Lower courts interpret laws by applying them to individual cases. The Court of Cassation issues rulings that—though not legally binding as in common law systems—guide interpretation by signaling the Court's preferred reading of certain laws. Legal scholars shape statutory interpretation by providing systematic analyses and proposing developments in legal reasoning. Administrative agencies in specialized areas such as tax, banking, labor, or environmental law may issue interpretive guidelines to clarify the application of specific provisions.

where $\bar{a} = E_j\left(a_{ijt}|\, c_i, s_j\right)$ is the average ambiguity of laws in legal cases. We assume that, after controlling for case complexity c_i and court fixed effects s_j, $\bar{a}$ is the same across courts j—an assumption we validate in Appendix A. Firms in districts with higher u_j face greater legal uncertainty due to poor drafting. A higher u_j implies that the legal consequences of firm decisions are more unpredictable and that the lower court is more likely to misjudge firm behavior, resulting in financial and reputational costs.[12] As u_j increases, firms may respond by reducing capital investment, and any business activity exposed to legal risk, ultimately hindering firm growth, which we take as a summary measure of the overall effect of legal uncertainty due to poor drafting on firm performance.

Let $Y_{n\ell t}$ denote an outcome variable (growth, investment, precautionary savings, etc.) of firm n, in location ℓ, at time t. Firms in location ℓ at t are under the jurisdiction of court $j(\ell, t)$. We are interested in evaluating how $Y_{n\ell t}$ reacts to changes in the legal uncertainty caused by ambiguous laws $u_{j(\ell,t)}$, while controlling for other sources of legal uncertainty, as measured by $E_\ell\left(r_{ij(\ell,t)t}\right) - u_{j(\ell,t)}$. We consider the following specification for $Y_{n\ell t}$:

$$Y_{n\ell t} = \bar{Y}_n + \beta_Y \times u_{j(\ell,t)} + \phi_Y\left[E_n\left(r_{ij(\ell,t)t}\right) - u_{j(\ell,t)}\right] + \gamma X_{n\ell t} + \varsigma_{n\ell t}$$

$\bar{Y}_n$ captures the growth (investment) opportunities of firm n. $X_{n\ell t}$ denotes firm controls, including time effects. β_Y measures the effect of the legal uncertainty due to ambiguous laws on outcome Y. Equation (UE) yields the following expression for firm outcome Y:

$$Y_{n\ell t} = d_n + \beta_Y \bar{a}\lambda_{j(\ell,t)} + \phi_Y s_{j(\ell,t)} + \gamma X_{n\ell t} + \varsigma_{n\ell t} \qquad \text{(OE)}$$

where $d_n \equiv \bar{Y}_n + \phi E_n\left(c_i\right)$ is a firm-n fixed effect that controls for firm opportunities $\bar{Y}_n$ and the complexity of the firm's legal cases, $E_n\left(c_i\right)$. The coefficient of interest is β_Y, which captures the effect of legal uncertainty due to poor drafting on firm outcome Y.

2.3 Econometric framework

To separately identify the contribution of legal uncertainty due to poor drafting, $\beta_Y u_{jt}$ in (OE), and the firm fixed effect d_n, we need exogenous time variation in $u_{j(\ell,t)}$. This variation arises because at t_0 (year 2012), firms in some locations ℓ are reassigned to a different court. We then proceed in two steps. First, we use (UE) to estimate the following

[12] See Appendix B for a microfoundation explaining why a higher u_j increases legal risk and thereby discourages firm investment. The intuition is that a higher u_j increases the probability that a firm is found liable at some point during judicial proceedings reviewing the legality of its investment. When condemnation entails reputational costs (even if the investment is ultimately upheld as lawful), a higher u_j reduces the investment's expected return by increasing legal risk.

OLS regression:

$$r_{ijt} = \hat{\lambda}_j a_{ijt} + \hat{s}_j + \hat{\gamma}' X_{ijt} + \hat{\xi}_{ijt}, \tag{3}$$

where the court-j fixed effect $\hat{s}_j$ absorbs cross-court differences in case complexity and the vector X_{ijt} captures additional case-level heterogeneity. Second, given the estimated values $\hat{\lambda}_j$ and $\hat{s}_j$ from (3), we run the regression in (OE). Because the λ_j's are generated regressors, we compute standard errors via bootstrapping, combining information from both equations (UE) and (OE); see Appendix I for details. Under standard orthogonality conditions and since $u_j \equiv \lambda_j \times \bar{a}$ (see 2), the coefficient on $\lambda_{j(\ell,t)}$ in (OE), denoted $\hat{\beta}_Y$, consistently estimates $\beta_Y \times \bar{a}$:

$$\text{plim } \hat{\beta}_Y = \beta_Y \times \bar{a}. \tag{4}$$

3 Data and drafting clarity measures

We briefly describe the data sources. We then discuss how we measure the drafting clarity of laws and how we identify the depreciation rate parameter δ in (1). Appendix C contains exhaustive details on data construction, sources, and descriptive statistics.

3.1 Data

We use four main data sets: one for Court of Cassation judgments, one for the text of laws, one for firms, and one for the characteristics of lower courts. Table 1 reports descriptive statistics.

We consider the universe of all judgments by the Court of Cassation over the period 2004-2017 (Wolters Kluwer, 2019). The judgments report the Court of Cassation's decision (whether it upholds the decision of the lower court or reverses it); the location of the lower court whose decision was appealed; all laws cited by either the lower court or the Court of Cassation; the date of the decision; the nature of the case (civil or criminal); the section of the Court of Cassation that handled the case (see Table A1 in the Appendix); and the name of the president of the section of the Court at the time of the decision. A legal norm may refer to a formal law issued by Parliament, one of the legal codes (Civil, Criminal, Civil Procedure, Criminal Procedure), or a government decree. On average, judgments refer to 0.8 laws, 4 code articles, and 0.7 decrees. We refer to all these norms simply as *laws*. We include all judgments that cite at least one law (around 93% of all Court of Cassation judgments during the period). Our final sample includes 456,770 judgments: 188,068 for civil cases and 268,686 for criminal cases. The average reversal probability is 35%, slightly

Table 1: Descriptive statistics

	Average	Median	Standard deviation	N. obs.
A) Judgments by Supreme Court				
All cases				
Reversal probability	.351	0	.477	456,770
N. words (no stop words)	919.659	703	935.918	456,770
N. laws cited	.788	0	1.284	456,770
N. codes cited	3.952	3	4.032	456,770
N. decrees cited	.708	0	1.175	456,770
Ambiguity of laws, a, baseline	2.525	1.873	2.559	456,770
Ambiguity of laws, a, GPT measure	5.348	4.502	3.878	273,734
Civil cases				
Reversal probability	.387	0	.487	188,068
N. words (no stop words)	1087.098	911	725.124	188,068
N. laws citede	1.288	1	1.628	188,068
N. codes cited	4.447	3	4.596	188,068
N. decrees cited	1.040	1	1.500	188,068
Ambiguity of laws, a, baseline	2.245	1.317	2.711	188,068
Criminal cases				
Reversal probability	.326	0	.469	268,686
N. words (no stop words)	802.437	580	1042.904	268,686
N. laws cited	.439	0	.806	268,686
N. codes cite	3.605	3	3.544	268,686
N. decrees cited	.476	0	.801	268,686
Ambiguity of laws, a, baseline	2.720	2.101	2.428	268,686
B) Firms				
Balance sheets data				
Firm growth rate	.087	0	.644	3,833,047
Investment/Capital	.316	.068	.762	2,888,559
Provisions/Assets	.012	0	.047	3,639,150
Future sales/Assets	1.237	.998	1.148	3,639,150
Sales (log)	6.262	6.246	1.837	3,833,047
Lower courts data				
Size (no. judges)	99.529	42	118.092	3,833,047
Judges tenure	21.373	21.764	4.225	3,833,047
Staff shortage	8.982	4.333	9.159	3,833,047
Trial length	407.856	362.068	142.888	3,833,047

Notes: Panel A covers all Court of Cassation judgments for 2004-2017. Reversal probability is the probability that the Court of Cassation reverses a lower court ruling. Ambiguity of laws is the average ambiguity of the laws cited in a judgment. Panel B reports firm-level statistics for 2008-2017 using balance-sheet data and information on firms' lower courts of reference. Court size and staff shortage are measured by the number of judges, while judge tenure is in years. The GPT measure could not be applied to the four codes (Civil, Criminal, Civil Procedure, Criminal Procedure) and three long laws.

higher for civil than for criminal cases. Reversal probabilities have remained stable over the sample period; see Appendix C.

We draw laws from Normattiva (2016), collecting the full body of Italian legislation. We measure drafting clarity for each law, as discussed below. We also leverage a distinctive feature of the Italian legislative process—Articles 87 and 90 of the Constitution, which require laws to be countersigned by the relevant ministers—to classify laws into the 15

topic categories reported in Appendix C.

We use data on the universe of limited liability companies from the Company Accounts Data Service (CADS). CADS reports the balance sheet and the income statement of all Italian limited liability companies. It also provides relevant firm demographics, including the ATECO industry classification, and the municipality of the firm's headquarter which we use to assign to each firm and year the measure of legal uncertainty λ of the court that rules in the municipality. Given the focus on the 2012 reform, we restrict the sample to the years 2008-2017, covering five years before and five years after the reform. In some specifications, to account for aggregate cyclical and financial conditions, we include regional GDP growth (across 20 regions) interacted with ninety-five sector dummies, and the firm's estimated probability of being credit rationed—constructed from the Bank of Italy Survey of Industrial and Service Firms—as a proxy for local credit supply conditions.

We obtained data on district court and Court of Cassation characteristics through formal requests to the High Council of the Judiciary (*Consiglio Superiore della Magistratura*)—the institution responsible for judicial administration—and to the National Association of Magistrates (*Associazione Nazionale Magistrati*)—the body representing magistrates' interests (see ANMA, 2023). For each district court j, we observe the number of Court of Cassation magistrates originating from that court during 2010-2015. We also collect information on (i) planned and actual numbers of magistrates; (ii) average age; (iii) average years of experience at the court and in the judiciary; and (iv) the share of pending cases unresolved for more than three years, which we use as a proxy for average case duration. Finally, we create dummies for the presence of a specialized business court in the jurisdiction in a given year and calculate firm-court distances as the geodesic distance between each firm and its court of competence in that year.

3.2 Drafting clarity

We consider two measures of drafting clarity. The first builds on established drafting manuals (Thornton, 1979; Cassese, 1993; Butt and Castle, 2006), while the second leverages Large Language Models (LLMs).

We start considering ten indicators to assess both the drafting quality of a law and the extent to which it is self-contained or dependent on references to multiple other laws—a feature we refer to as dependency ambiguity. Appendix D provides detailed references to the specific excerpts from legal drafting manuals that support the rationale for each indicator. Drafting quality is measured using eight indicators: average phrase length (in characters); the number of gerunds per word (multiplied by 1,000); average word length; average root

length; the number of modal verbs per word; the number of demonstrative adjectives per word; the number of pronouns per word; and the number of embedding and contingency clauses per word. We measure dependency ambiguity using two indicators: (i) the presence of a preamble, which often contains numerous legal references presented in an unstructured or opaque manner; and (ii) the number of references to other laws in the main text, scaled by total word count. Style manuals emphasize brevity and clarity, warning against long sentences that are more prone to ambiguous interpretation. For example, Italian linguists widely agree that sentences should not exceed 25 words to remain easily understandable (Fioritto, 1997; Cortellazzo and Pellegrino, 2003; Alfieri et al., 2011).[13] Gerunds, especially in Italian, are also discouraged, as they obscure the subject of the sentence and compress too much information, thereby increasing ambiguity (Cassese, 1993; Cortellazzo, 2002). Using plain, common language rather than dense legal terminology ("legalese") improves comprehension, even among legal professionals (Martinez et al., 2023). In general, common words tend to be shorter due to an optimization for efficient communication (Piantadosi et al., 2011). Indeed, widely used readability indexes such as Flesch-Kincaid and Gulpease are linear functions of sentence and word length. Syntactic complexity also matters. Sentences with contingency clauses placed in the middle—a structure known as center-embedding—are difficult and cognitively costly to process (Chomsky and Miller, 1963; Gibson, 1998). Finally, laws that contain numerous cross-references and lengthy, unstructured preambles are less self-contained, more difficult to interpret and more likely to admit multiple, potentially conflicting interpretations.

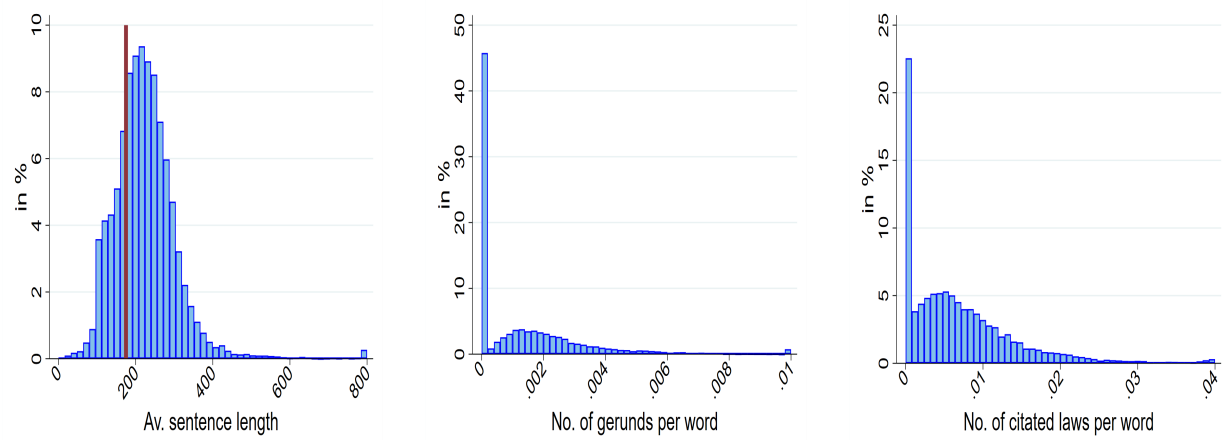
Figure 1 considers the union of all laws cited in Court of Cassation judgments and plots the cross-sectional distribution of sentence length (panel a), number of gerunds per word (panel b), and number of references to other laws per word (panel c). The vertical red line in panel (a) marks sentences of approximately 25 words. More than 80 percent of the laws cited by the Court of Cassation contain excessively long sentences. Forty-five percent of sentences contain no gerunds, and more than twenty percent of laws cite none. However, there is also a thick right tail of laws with an unusually high incidence of gerunds and legal citations—exceeding 1 gerund per 100 words and more than 4 cited laws per 100 words.

We also leveraged the capabilities of Large Language Models (LLMs) and processed each law in Normattiva (2016) to assess why it might be difficult to interpret; Appendix F describes the methodology in detail. We rely on `gpt-4.1-mini` (hereafter GPT) because, relative to other state-of-the-art models, it offers a large context window—allowing it to process substantial amounts of text at once—and delivers results quickly.[14] Since LLMs

[13] See also OECD (2024) for individual-level survey evidence showing that people fully understand only short simple sentences.

[14] In practice, we drop the four codes (Civil, Criminal, Civil Procedure, Criminal Procedure) as well as

Figure 1: Some drafting clarity indicators



(a) Length of sentences **(b) Gerunds per word** **(c) Cited laws per word**

Notes: The sample includes all laws cited in Court of Cassation judgments over 2004-2017. Panel (a) reports average sentence length, measured as characters per sentence; the vertical red line corresponds to 25 words (175 characters). Panel (b) reports the number of gerunds per 1,000 words in the law's main text. Panel (c) reports the ratio of total cited laws to total words in the law's main text.

have documented weaknesses in numerical reasoning (Levy, 2024), we do not ask GPT to assign explicit numerical clarity scores, as this would require condensing diverse features of a law into a single, possibly opaque score. Instead, we instruct GPT to identify all qualitative features that render the processed law unclear, independent of its subject matter, using the following prompt (translated into English):

> `You are a GPT tasked with evaluating the clarity of Italian legal texts, identifying all characteristics that compromise clarity. By ''clarity'' is meant the linguistic and syntactic transparency of the text, the degree of comprehensibility of the content, and the absence of interpretive ambiguity at the lexical level, regardless of the subject matter treated.`
>
> `Provide exclusively a complete and detailed list of such characteristics in JSON format. Do not include explanations or additional text.`

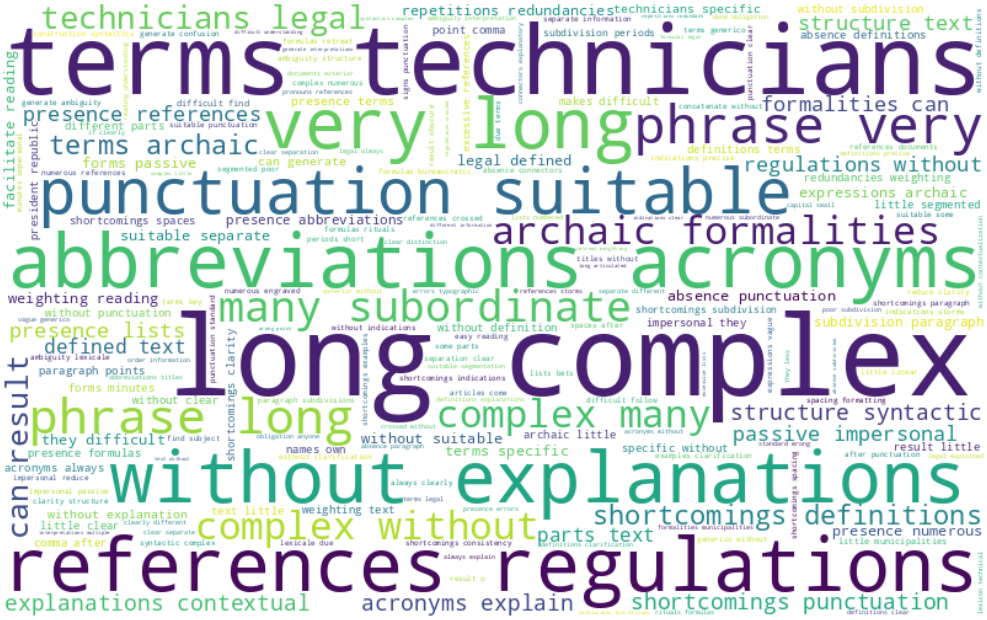
Example output:

```
{"features_impeding_clarity": ["Overly long sentences", "Use of
   archaic legal terms", "Many subordinates", "Feature 4", ...]}
```

the three long laws, each exceeding five million characters, listed in Appendix F.

As a measure of poor drafting, we use the number of features reported by GPT. Figure 2 shows an English-translated word cloud of the main features that undermine the clarity of Italian laws, as identified by the GPT analysis. Many prominent features correspond closely to our previous stylistic indicators, including excessively long sentences, numerous subordinate clauses, extensive referencing, archaic formalities, technical terms, etc. Across the full sample of laws processed by GPT, the first principal component of our ten previously defined indicators of poor drafting has a strongly positive correlation of 0.60 with the GPT measure.

Figure 2: Word cloud of features undermining the clarity of laws



Notes: The figure displays the English-translated word cloud of the main features that undermine the clarity of Italian laws, as identified by the GPT-based analysis.

We assess the robustness of results to both drafting-quality measures, taking the measure based on drafting-manual indicators as the baseline due to some well-known biases inherent in LLMs. First, seven laws (three long laws plus the four codes) could not be processed by GPT due to text-length limits. Second, LLMs are subject to look-ahead bias (Glasserman and Lin, 2023; Levy, 2024; Lopez-Lira and Tang, 2023), as they rely on information available today rather than at the time a law was enacted, potentially underestimating the poor drafting quality of laws at inception. Third, this look-ahead bias is compounded by frequency (or long-tail) bias in LLM training data (Kiyoshi et al., 2024; Li et al., 2024), implying that highly ambiguous laws—being more frequently discussed online—are systematically more underestimated.

3.3 Discount rate

The parameter δ in (1) is the rate at which legal ambiguity is resolved over time. For any $\delta \in (0, 1)$, younger laws are more likely to trigger a reversal than older ones. Then, to identify δ, we examine how the reversal probability r_{ijt} varies with the age of laws by estimating the following regression:

$$r_{ijt} = \sum_{\tau} \beta^{\tau} Law_Age_{ijt}^{\tau} + s_j + \gamma' X_{ijt} + \xi_{ijt} \tag{5}$$

where $Law_Age_{ijt}^{\tau}$ is a dummy variable equal to one if the Court of Cassation judgment cites at least one law that is τ years old. In practice, we group laws into five age categories: 0-5 years, 6-10 years, 11-15 years, 16-20 years, and older than 20 years. The regression includes the lower-court average reversal rate, as measured by the s_j dummies, and the vector of controls X_{ijt} specified in the legend of Table 2, controlling for the topic and complexity of the cases as well as individual judge characteristics. Over the first 20 years of a law's existence, the estimated coefficients β^{τ} in Table 2 are consistent with an average decline of roughly 9 percent per year.[15] We also used alternative methods to estimate δ—either via the reversal equation in (3) or based on the number of times a law is cited in Court of Cassation judgments—obtaining quantitatively similar estimates of the depreciation rate of poor drafting quality.[16] Consequently, we set $\delta = 0.09$.

We discount the 10 indicators of drafting clarity based on text manuals, summarizing them with their first principal component, which accounts for approximately three-fourths of the total variance in the indicators. We set the number of gerunds per 1,000 words as the unit of measurement, normalizing its factor loading to one. The loadings are all positive and similar in magnitude, implying that each drafting clarity indicator contributes roughly equally (see Appendix E for further details). Panel A of Table 1 reports summary statistics for the resulting baseline measure of legal ambiguity a: there is substantial variability in the ambiguity of laws cited across different judgments, with its mean roughly equal to its standard deviation. The GPT measure of drafting clarity, constructed analogously using the same δ, displays a similar property (see Table 1).

[15] Combining the estimates in Table 2 for the 0-5 and 16-20 year age brackets, and noting that each bracket spans five years, we obtain that δ should approximately satisfy $(1-\delta)^{15} \times 0.065 = 0.017$, implying $\delta = 0.09$.

[16] First, we estimated δ nonlinearly, searching for the value that maximizes the R^2 of the reversal equation in (3). Figure A5 in the appendix shows that the resulting estimate is around 11 percent, very close to the previously discussed estimate. Second, we followed laws enacted during our 2004-2017 sample period over their life cycle and examined how the effect of poor drafting quality on reversals declines with the number of prior citations in Court of Cassation judgments. This approach yields again an implied δ of just below nine percent, close to our baseline estimate.

Table 2: Reversal probability and the age of laws

	Dependent variable: Reversal probability
Law age 0-5 years	0.065***
	(0.010)
Law age 6-10 years	0.063***
	(0.008)
Law age 11-15 years	0.046***
	(0.008)
Law age 16-20 years	0.017**
	(0.009)
Law age greater than 20 years	-0.003
	(0.011)
N. observations	456,770
Adjusted R^2	0.080

Notes: Results from estimating regression (5). The sample includes all judgments by the Court of Cassation that cite at least one law over the period 2004-2017. The variables "Law age 0-5/6-10/11-15/16-20/greater than 20 years" are dummy variables indicating whether the Court of Cassation judgments cites at least one law in the corresponding age bracket. The regression includes the lower court average reversal rate, as measured by the s_j dummies, and the following vector of controls X_{ijt}: a full set of dummies for the section of the Court of Cassation which examines the case; dummies for the identity of the president of the section at the time of the decision; time dummies for the year-quarter t of the Court of Cassation decision; time dummies for the year-quarter t of the district court decision; a dummy for whether the appeal is on a decision by a Justice of the Peace; a dummy for whether the appeal was on a decision by some other courts (Juvenile Court, Court of Freedom, Supervisory Court, Judge for Preliminary Investigations). Robust standard errors clustered at the president judge level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4 Estimating the legal uncertainty equation

We begin by estimating the average effect of ambiguous laws on reversal probabilities using the uncertainty equation in (UE). We then examine variation across courts.

4.1 Uncertainty equation

We estimate the uncertainty equation in (UE) using judgments from the Court of Cassation:

$$r_{ijt} = \lambda \times a_{ijt} + s_j + \gamma' X_{ijt} + \xi_{ijt} \tag{6}$$

where i denotes the legal case heard by the Court of Cassation, j represents the lower court whose ruling was appealed, and t is the date of the Court of Cassation judgment to either

reverse the lower court's decision ($r_{ijt} = 1$) or uphold it ($r_{ijt} = 0$). a_{ijt} denotes the measure of ambiguity of the laws cited by the Court of Cassation in its judgment. s_j indicates one of the 160 dummies for the lower court whose decision was appealed to the Court of Cassation.[17] X_{ijt} is the vector of variables described in the legend of Table 3, controlling for the topic and complexity of the cases as well as individual judge characteristics. Lastly, ξ_{ijt} is an error term. We cluster standard errors by the president of the Court of Cassation section presiding over the appeal. The key parameter in (6) is λ, which measures how legal ambiguity from drafting clarity a_{ijt} affects reversal probabilities r_{ijt}.

Table 3 reports the estimates of λ considering different samples or measures of ambiguity. Columns 1-4 use the baseline measure of drafting clarity; column 5 uses the GPT measure. Results are shown for all judgments (columns 1 and 5), civil cases only (column 2), criminal cases only (column 3), and criminal cases with risk of imprisonment (column 4). Across all specifications, legal ambiguity significantly increases reversal probabilities, indicating that poorly drafted laws generate legal uncertainty. The estimated effect of legal ambiguity on the reversal probability for all cases is $\lambda = 0.012$ (column 1). This implies that a one-standard deviation decline in drafting clarity increases the reversal probability by 3

Table 3: Uncertainty equation: average effects

	Dep. var.: Reversal probability				
	All judgements	Civil only	Criminal only	With imprisonment	All with GPT measure
	(1)	(2)	(3)	(4)	(5)
Legal ambiguity	0.012***	0.016***	0.007***	0.009***	0.003***
	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)
N. observations	456,770	188,068	268,686	88,670	273,734
Adjusted R^2	0.085	0.123	0.070	0.087	0.096

Notes: Estimates of λ from regression (6) using Court of Cassation judgments over 2004-2017. Columns 1-4 use the baseline measure of legal ambiguity a; column 5 uses the GPT measure in a sample that excludes judgments that cite only the four codes and three long laws. Columns 1 and 5 use the full sample; column 2, civil cases only; column 3, criminal cases only; and column 4, criminal cases with a risk of imprisonment. All regressions include 160 lower-court j dummies s_j, and the control vector X_{ijt}, which comprises: dummies for the Court of Cassation section examining the case; dummies for the identity of the president of the section; time dummies for the year-quarter t of both the Court of Cassation and district court decisions; length (in characters) of the Court of Cassation judgment; number of laws cited in the judgment; a dummy for appeals from Justice of the Peace decisions; a dummy for appeals from other special courts (Juvenile Court, Court of Freedom, Supervisory Court, Judge for Preliminary Investigations); and 16 indicators for the topic of the laws relevant to the case. Robust standard errors clustered at the president judge level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[17]The number of estimated λ_j coefficients is 160, as 5 of the 165 district court locations existing before 2012 (La Spezia, Sant'Angelo dei Lombardi, Santa Maria Capua Vetere, Tempio Pausania, and Termini Imerese) are dropped due to the absence of Court of Cassation judgments on cases originating from them.

percentage points, or roughly 10% of the mean reversal probability. Similar results are obtained using the GPT-measure (column 5).

A legitimate concern is that the correlation between drafting quality and the reversal probability in Table 3 simply reflects the underlying complexity of a law's subject matter, which may be correlated with our drafting quality measure. In practice, the drafting indicators capture widely accepted standards of writing quality, not substantive complexity. As emphasized by the plain language movement in legal drafting, pioneered by Mellinkoff (1963) in the US and Cassese (1993) in Italy, drafting quality is not inherently constrained by subject matter.[18] Regardless of the complexity of the legal domain, rules can be drafted in plain language, following established readability guidelines. There is no intrinsic reason why legislation on complex topics must rely on excessively long or convoluted sentences, dense subordinate clauses, intricate embedding structures, layered cross-references, or archaic terminology. These features reflect stylistic choices, not unavoidable consequences of substantive complexity.

To validate this claim, in Appendix J we perform two exercises. In the first, we draw on the fact that finance and tax law are highly complex technical domains. If our drafting quality measure merely reflects subject-matter complexity, laws in these areas should exhibit systematically lower drafting quality than other laws cited by the Supreme Court. However, Table A7 shows no evidence that these laws are less well drafted; if anything, they appear marginally better than other ones.

In the second, we exploit plausibly exogenous variation in drafting quality arising from the political and institutional context at the time of enactment. While a law's subject matter reflects the policy issues salient at enactment, drafting quality tends to deteriorate when legislators operate under political emergency or instability—conditions that distort legislators' incentives (Gratton et al., 2021), compress deliberation time, and ultimately reduce drafting clarity. We exploit this distinction and, holding all controls fixed, instrument legal ambiguity, a, in the reversal equation reported in columns 1 and 5 of Table 3 using an indicator for whether the law originated from an emergency decree—a legislative act issued by the government in cases of necessity and urgency under Article 77 of the Italian Constitution. Decree legislation is issued in response to urgent circumstances (e.g., a pandemic, a financial crisis, or a cyberattack) and may cover subject matters of varying underlying complexity (e.g., public health, financial regulation, taxation, or national security). Because urgency limits the time available for drafting and may distort legislators' incentives, decree legislation has lower drafting clarity and thus provides a valid instrument to identify the effect of legal ambiguity in the reversal equation. Table A8 in the Appendix shows that

[18] Gopen and Swan (1990) make a similar point for scientific writing.

the instrument strongly predicts lower drafting quality (F-statistic = 69.11 when using the baseline measure). The IV estimates indicate a sizable positive effect of legal ambiguity from poor drafting, a, on the reversal probability. This confirms that the effects reported in Table 3 are driven by variation in drafting quality tied to the political and institutional context at enactment, rather than by the inherent complexity of the law's subject matter.

Another potential concern is that cases appealed to the Court of Cassation may not constitute a random sample of all legal cases in Italy. Poorly drafted, ambiguous laws could incentivize the losing party to appeal in the hope of reversal, potentially generating a positive selection effect that may upwardly bias the estimate of λ in equation (6). Four considerations indicate that selection issues are unlikely to be relevant. First, in Italy, appeals to the Court of Cassation are relatively inexpensive, accessible, and common—approximately 32% of cases from lower courts reach the Court of Cassation. Second, legal ambiguity a exhibits substantial identifying variation when estimating the regression in (6): its standard deviation among cited laws is roughly twice that in the full population. Third, we note that in criminal cases involving a defendant's personal freedom, self-selection is less problematic than in other criminal cases, because the stakes for the defendant are high and authorities are obliged to prosecute.[19] We find no evidence that the estimate of λ for criminal cases involving a risk of imprisonment (column 4 of Table 3) is lower than the corresponding estimate for other criminal cases (column 3), which indicates that self-selection is unlikely to materially bias our results.

Finally, and most importantly, even if self-selection were to bias the estimate of λ, this bias is immaterial for our empirical results, because identification relies exclusively on cross-sectional differences across lower courts, $\Delta\lambda$, rather than on the level of λ. Self-selection bias would only be a concern if it varied systematically across lower courts, which the evidence suggests is unlikely. Appendix A shows that the average legal ambiguity of cases reviewed by the Court of Cassation, $\bar{a}$ in (2), does not systematically vary across lower courts of origin. If self-selection bias were systematically related to the lower court of origin, the same forces would also generate systematic differences in legal ambiguity $\bar{a}$, which we do not observe, indicating that self-selection bias is unlikely to be a concern.

[19] This obligation is enshrined in Article 112 of the Italian Constitution, which establishes the principle of mandatory criminal prosecution.

4.2 Variation across courts

We now allow the coefficient on legal ambiguity, λ, to differ by lower court, estimating the uncertainty equation (6), as follows:

$$r_{ijt} = \lambda_j \times a_{ijt} + s_j + \gamma' X_{ijt} + \xi_{ijt} \tag{7}$$

This specification yields a total of 160 court-specific coefficients λ_j, which capture heterogeneity in alignment with the Court of Cassation. The lower-court dummy, s_j, accounts for systematic differences across courts—including average case complexity—, which can cause disagreement with the Court of Cassation even when laws are optimally drafted ($a_{ijt} = 0$). The control variables X_{ijt} are the same as those described in the legend of Table 3.

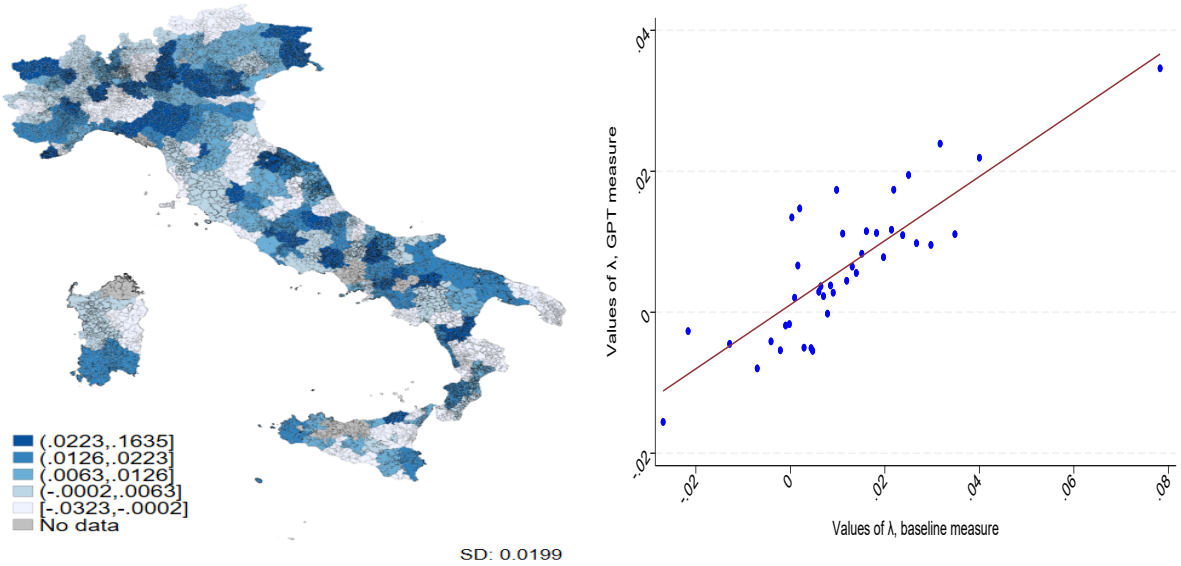
Panel (a) of Figure 3 shows the geographical distribution of the estimated coefficients λ_j using the baseline measure of ambiguity a. Darker areas correspond to higher values of λ_j. An F-test for the equality of the λ_j coefficients across courts strongly rejects the null hypothesis, implying that legal uncertainty due to ambiguous laws differs systematically across courts.[20] The map reveals substantial heterogeneity in the estimated λ_j across court jurisdictions. The correlation of the estimated λ_j with court size is weak (0.04) and statistically insignificant, and there are no evident North-South differences across jurisdictions. Panel (b) of Figure 3 presents a binned scatter plot relating the 160 court-specific coefficients λ_j estimated using the baseline measure (x-axis) to those obtained using the GPT-measure (y-axis). The correlation between the λ_j coefficients obtained using the baseline and GPT-measures is high (58%), indicating that the two drafting clarity measures identify highly consistent geographical patterns.

Our estimates of court misalignment with the Court of Cassation, λ_j, are consistent with anecdotal evidence. For example, the Court of Milan is widely regarded as a central reference point for Italian law, especially in commercial matters—analogous to the Delaware courts in the US.[21] Among the courts in the ten largest Italian cities (Bari, Bologna, Catania, Firenze, Genova, Milano, Napoli, Palermo, Roma, Torino), it exhibits the third-lowest value of λ, at approximately 0.7%, about one third of the level observed in the two most misaligned courts, Genova and Bari (around 2%). Similarly, the Court of Bologna, with a long-standing labor-law tradition (Franzoni 2017; de Vergottini and Zanobetti 2024),

[20] Of the 160 estimated λ_j coefficients, 120 are positive and 64 are statistically different from zero. We also find that the correlation between λ_j and s_j is low and negative (-0.2), consistent with the interpretation that these coefficients capture distinct characteristics of lower court j.

[21] The Court of Milan's jurisprudence is so authoritative that even the Court of Cassation has, at times, followed its lead. For example, in 2011 the Court of Cassation explicitly recognized the Milan Tables for calculating non-pecuniary damages as the national benchmark, from which lower courts may deviate only with adequate justification (Corte di Cassazione Civile, Sezione III, 7 June 2011, n. 12408).

Figure 3: Court-specific coefficients on legal ambiguity, λ_j



(a) Court-specific λ-coefficients **(b) λ's from baseline vs. GPT measure**

displays the fourth-lowest value of λ (0.8%). By contrast, the Court of Naples—rooted in a legal tradition strongly influenced by a substantive-oriented approach, distinct from the Court of Cassation's more formal approach (Tessitore 1979; Monticelli 2006)—ranks among the top three courts in terms of misalignment, with λ equal to 1.3%, roughly twice the level observed in Milan.

Because differences in λ_j capture variation in judicial alignment with the Court of Cassation—shaped by local doctrines, judicial practices, and jurisprudential traditions—lower courts that share relevant legal, cultural, and intellectual traits with the Court of Cassation tend to exhibit lower λ_j. Consistent with this characterization, Appendix G shows that judges from courts with lower λ_j are more likely to be promoted to the Court of Cassation.

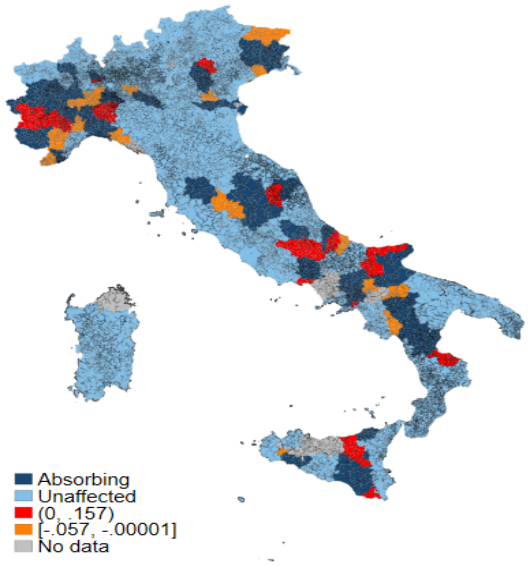
5 Firm responses to legal uncertainty

As discussed in Section 2, we exploit the 2012 court consolidation reform to estimate the effects of legal uncertainty on firm outcomes. We first describe the reform and then use it to estimate the effect of legal uncertainty from poor drafting on firm growth, using the specification in (OE). We subsequently extend the analysis to firm investment and precautionary provisions and conduct several robustness checks. In Section 6, we broaden the scope to examine effects on business creation, firm exit, and firm size at entry.

5.1 The court consolidation

In 2012, which we conventionally identify as as $t = 0$, a reform mandated the closure of 30 district courts (see Appendix C for the list and further discussion). Firms previously under the jurisdiction of a closed court were automatically reassigned to a nearby district court, with court personnel transferred accordingly.[22] As a result, the court consolidation generated three types of municipalities: *treated* municipalities, whose firms were assigned to a different court $j(\ell, -1) \neq j(\ell, 0)$; *absorbing* municipalities, under a district court that absorbed one or more closed courts; and the remaining municipalities *unaffected* by the court consolidation. Since $j(\ell, -1) \neq j(\ell, 0)$, firms in treated municipalities experienced a change in legal uncertainty of size $\Delta\lambda$.[23] Firms in absorbing or unaffected municipalities remained under the same court, so $j(\ell, -1) = j(\ell, 0)$. Figure 4 shows the geographic distribution of four types of municipalities. Municipalities unaffected by the court consolidation are shown in light blue; absorbing municipalities in dark blue; treated municipalities with an increase in legal uncertainty ($\Delta\lambda > 0$) in red; and those with a decrease in legal uncertainty ($\Delta\lambda < 0$) in orange. The reform affected 1,235 out of 8,093 municipalities spread across northern, central, and southern Italy, with a slightly higher concentration

Figure 4: Geographical distribution of legal uncertainty shocks $\Delta\lambda$



Notes: Geographical distribution of changes in legal uncertainty, $\Delta\lambda_j$, using baseline measure. The standard deviation of $\Delta\lambda_j$ is 0.043.

[22] Court jurisdiction is strictly determined by the firm's registered office; firms have no discretion, ensuring exogenous variation in the assignment of courts.

[23] Italian firms are highly exposed to judicial processes and are knowledgeable about the characteristics of their local court environment: two-thirds of firms maintain a stable and trusting relationship with a professional lawyer, 27% have engaged in formal civil litigation at least once during their lifetime, and 10.2% have faced criminal or tax litigation (Format Research, 2024).

in the northwest. There are 115,609 treated firms (6.1 percent of the sample). Of these, 51.6 percent experienced an increase in legal uncertainty, while the remainder experienced a decrease or close to no change. The distribution of $\Delta\lambda$ among treated firms is centered around zero and exhibits substantial dispersion (equal to 0.043). For treated municipalities, the correlation between the change in λ just described (calculated using the baseline measure) and the change in λ obtained with the GPT measure is high, at 67 percent.

Table 4 presents descriptive statistics for the year prior to the reform (2011), separately for treated, absorbing, and unaffected districts. Panel A shows firm-level statistics, while Panel B shows court-level statistics. Firm-level characteristics were strikingly similar across treated, absorbing, and unaffected municipalities, with virtually identical growth rates, investment rates, provisions, and sales-to-assets ratios. Since the goal of the reform was to close smaller courts to achieve economies of scale, closed courts had a median of 6.7 judges, compared to 12.7 in absorbing courts and 20.7 in unaffected courts.

Table 4: Descriptive statistics by type of municipality, year 2011

	Treated municipalities			Absorbing municipalities			Unaffected municipalities		
	Average	Median	SD	Average	Median	SD	Average	Median	SD
A) Firms									
Firm growth rate	.118	.017	.674	.119	.019	.669	.110	.013	.674
Investment/Capital	.290	.066	.722	.305	.066	.754	.300	.062	.751
Provisions/Assets	.010	0	.044	.011	0	.046	.011	0	.047
Future sales/Assets	1.06	.811	1.14	1.07	.843	1.12	1.10	.852	1.17
Z score	4.52	5	2.48	4.57	5	2.46	4.59	5	2.49
Sales (log)	6.12	6.17	1.89	6.13	6.18	1.92	6.08	6.115	1.92
B) Lower courts									
Size (N. judges)	7.28	6.66	2.37	20.51	12.66	24.81	35.85	20.66	54.18
Judges tenure	16.41	15.84	3.80	18.93	20.23	5.26	18.83	18.61	4.96
Staff shortage	1.54	1.66	.92	2.81	2.66	2.71	4.29	3	4.67

Notes: All statistics are calculated in 2011. The statistics in panel A are at the firm level; those in panel B are at the district level. In 2011 there were 39,241 firms in treated districts, 97,026 firms in absorbing districts and 490,854 in unaffected districts.

Two features support our empirical design to identify the effects of the legal uncertainty shock, $\Delta\lambda$, on firm-level outcomes. First, the consolidation can be considered plausibly exogenous to firm-level outcomes, since it was motivated by the pursuit of economies of scale in the judiciary (Mocetti, Pesenti, and Roma, 2025), and courts selected for closure were chosen largely for administrative reasons unrelated to local economic conditions (Pezone, 2022). Second, given the substantial lag between initial lower-court decisions and subsequent Court of Cassation rulings, our reform-induced shocks to legal uncertainty, $\Delta\lambda$, are

constructed using legal cases that originated almost exclusively prior to the 2012 court consolidation reform. We use the full sample only to maximize estimation efficiency. Results remain unchanged when $\Delta\lambda$ is constructed using only pre-2012 cases; see Appendix H.[24]

5.2 Firm results

Let $Y_{n\ell t}$ denote a generic outcome variable of firm n in municipality ℓ in year t (its growth rate, investment rate, or precautionary provisions). The effects of legal uncertainty on firm outcomes is measured by (OE) equations of the form:

$$Y_{n\ell t} = \beta_Y \lambda_{n\ell t} + f_n + f_t + \gamma' X_{n\ell t} + u_{n\ell t} \tag{8}$$

where $\lambda_{n\ell t}$ denotes the level of legal uncertainty due to ambiguous laws prevailing in the court with jurisdiction over municipality ℓ in year t. $\lambda_{n\ell t}$ varies over time for municipalities treated by the 2012 reform. f_n and f_t denote firm n and time t fixed effects, respectively; $X_{n\ell t}$ is a vector of additional controls; and $v_{n\ell t}$ is an error term. We start by estimating a simple two-way fixed effects specification that includes only firm and time dummies. In the baseline specification, we then control for potential confounding shocks by including $X_{n\ell t}$, which comprises: regional GDP growth interacted separately with sector and time dummies to capture heterogeneous business-cycle effects across sectors and regions; firm size (measured as the percentile of total sales prior to the reform) interacted with the firm's Z-credit score; and time fixed effects interacted with the indicator for credit rationing probability, both of which account for time-varying credit market conditions.[25] In some specifications, we also include the lower-court average reversal rate, as captured by the s_j dummies from regression (7), along with several court-level controls. The effect of legal uncertainty due to ambiguous laws on variable Y is measured by β_Y.

To formalize the econometric specification, let T_n denote a dummy equal to one if firm n is located in an absorbed municipality (hereafter, a treated firm). Let Post_t denote a dummy equal to one in the post-reform period. Let $\lambda^-_{n\ell}$ denote the value of $\lambda_{n\ell t}$ in the pre-reform period, and let $\Delta\lambda_{n\ell}$ be the change in $\lambda_{n\ell t}$ induced by the reform. By construction, $\Delta\lambda_{n\ell} = 0$ for untreated firms ($T_n = 0$) and $\Delta\lambda_{n\ell} \neq 0$ for treated firms ($T_n = 1$). With this

[24] Appendix H formally shows that the λ coefficients are statistically indistinguishable between the pre-2012 (2004-2012) and post-2012 (2013-2017) periods, and that the estimated effects of the uncertainty shock are virtually unchanged when $\Delta\lambda$ is constructed using only pre-2012 cases.

[25] When $Y_{n\ell t}$ is the investment rate or precautionary provisions, $X_{n\ell t}$ also includes next year's sales scaled by firm total assets, in line with standard accelerator models of investment (Harrod, 1936).

notation, the regression in (8) can be rewritten as:

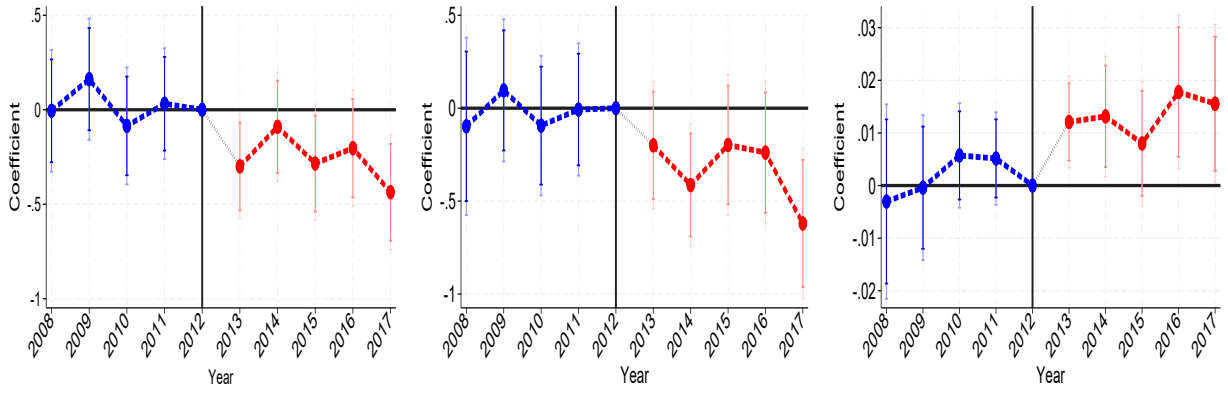
$$Y_{n\ell t} = \beta_Y \Delta\lambda_{n\ell} \times T_n \times \text{Post}_t + \tilde{f}_n + f_t + \gamma' X_{n\ell t} + v_{n\ell t} \tag{9}$$

where $\tilde{f}_n = f_n + \beta_Y \lambda^-_{n\ell}$ is a firm fixed effect that also absorbs the effect of pre-reform legal uncertainty, $\beta_Y \lambda^-_{n\ell}$. Equation (9) corresponds to a difference-in-differences regression with a continuous treatment, as studied by Callaway, Goodman-Bacon, and Sant'Anna (2024). In this setting, the parameter β_Y is identified solely from the legal uncertainty shock $\Delta\lambda$ induced by the reform for firms in treated municipalities. As shown by Callaway et al. (2021), the two-way fixed effects estimator of β_Y in equation (9) identifies the average causal response to a treatment of size $\Delta\lambda$ (with unit-sum positive weights for all treated units) only if, in the absence of treatment, outcome trends for units with different treatment intensities would have evolved in parallel over time. This strong parallel trends assumption may be violated if legal uncertainty tends to revert toward its mean and changes in legal uncertainty are not random. We begin our analysis with a standard two-way fixed effects estimator. There is prima facie evidence that variation in $\Delta\lambda_{n\ell}$ is plausibly random: the distribution of changes is approximately symmetric around zero with substantial dispersion, and Table 4 shows no systematic correlation between treatment status and observable firm characteristics. We then address residual concerns about nonrandom changes in $\Delta\lambda_{n\ell}$ using the estimator proposed by de Chaisemartin and Lei (2024). Their approach proceeds in two steps: first, the change in legal uncertainty $\Delta\lambda_{n\ell}$ is regressed nonparametrically on its pre-treatment level $\lambda^-_{n\ell}$; second, the main equation in (9) is estimated by replacing $\Delta\lambda_{n\ell}$ with its residual from the first stage, $\Delta\lambda_{n\ell} - E(\Delta\lambda_{n\ell})$, and controlling for the predicted change $E(\Delta\lambda_{n\ell})$.

Firm growth Table 5 reports the estimate of β_Y in (9), with bootstrapped standard errors shown in square brackets. Panel A reports the plain two-way fixed effects estimates, while Panel B reports the estimates including the baseline controls $X_{n\ell t}$, as detailed in the legend. In each panel, columns 1-3 present estimates of the impact on firm growth, using different control groups. Column 1 reports results using firms in unaffected municipalities as the control group. Column 2 uses firms in absorbing municipalities, which are geographically close to treated areas and may better account for common time-varying shocks. Column 3 broadens the control group to include both absorbing and unaffected municipalities. Across specifications, the results robustly show that legal uncertainty has a negative and precisely estimated effect on firm growth. Economically, a one-standard-deviation increase in legal uncertainty ($\Delta\lambda = 0.043$) reduces firm growth by 1.2 percentage points, equivalent to 14% of the sample mean and 26% of the median growth rate.

Panel (a) of Figure 5 plots year-by-year estimates of treated firms' growth rate responses to $\Delta\lambda$ relative to control firms over a 10-year window (five years before and five years after the reform), with the 2012 coefficient normalized to zero. These estimates are obtained from equation (9) including the baseline controls, where the dependent variable is firm output growth and the coefficient on $\Delta\lambda_{n\ell} \times T_n$ is interacted with a full set of pre- and post-reform time dummies. The control group consists of all firms located in districts unaffected by the court consolidation. As Figure 5 shows, there is no evidence of pre-trends: in all pre-reform years, the effect of legal uncertainty on growth is estimated precisely at zero, and the F-test of the null hypothesis that all pre-reform effects are zero yields a p-value of 0.60.[26] In the years following the reform, the coefficient becomes significantly negative, consistent with treated firms slowing their growth in response to increased legal uncertainty.

Figure 5: Parallel trends



(a) Growth rate **(b) Investment rate** **(c) Precautionary savings**

Notes: Estimates from (9) of the coefficients on $\Delta\lambda_{n\ell} \times T_n$, interacted with a full set of pre- and post-reform time dummies, with the 2012 coefficient normalized to zero. The λ's are obtained using the baseline measure of drafting clarity. Controls $X_{n\ell t}$ are as in panel B of Table 5. The control group includes all firms in municipalities unaffected by the court consolidation. The F-test for zero pre-reform effects yields p-values of 0.60, 0.86, and 0.48 for growth rate, investment rate, and precautionary savings.

Investment and precautionary provisions To further understand how legal uncertainty affects firm growth, we examine its impact on capital accumulation and precautionary provisions. Because investment is often irreversible, uncertainty tends to reduce capital accumulation (see, e.g., Abel and Eberly 1996; Guiso and Parigi 1999; Bloom 2009; Bloom, Bond, and Van Reenen 2007). Firms may also respond to higher risk by increasing precautionary provisions to buffer potential future costs.

[26] As noted, difference-in-differences models with continuous treatments require the stronger assumption that the absence of pre-trends holds at all treatment levels. To test this, we estimate the model separately for firms experiencing positive and negative change to legal uncertainty $\Delta\lambda$. In both cases, we fail to reject the null hypothesis of zero pre-treatment effects (p-values of 0.26 and 0.58, respectively).

Table 5: Effect of legal uncertainty shock on firm growth, investment and precautionary savings

Dep. var.:	Growth rate			Investments rate			Precautionary savings		
Control group:	Unaffected	Absorbing	All districts	Unaffected	Absorbing	All districts	Unaffected	Absorbing	All districts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Plain TWFE									
$\Delta\lambda\times$Post	-0.292***	-0.322***	-0.305***	-0.215**	-0.197*	-0.199**	0.012**	0.007	0.012**
	(0.082)	(0.081)	(0.081)	(0.101)	(0.101)	(0.100)	(0.005)	(0.004)	(0.005)
	[0.081]	[0.082]	[0.081]	[0.101]	[0.101]	[0.100]	[0.006]	[0.006]	[0.006]
N. observations	3,994,686	1,007,820	4,735,728	3,216,510	836,584	3,825,691	4,709,548	1,184,643	5,579,560
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other FE	No	No	No	No	No	No	No	No	No
Panel B: Baseline specification									
$\Delta\lambda\times$Post	-0.272***	-0.288***	-0.268***	-0.304***	-0.254**	-0.283***	0.012**	0.012**	0.012**
	(0.085)	(0.085)	(0.085)	(0.110)	(0.110)	(0.109)	(0.005)	(0.005)	(0.005)
	[0.088]	[0.089]	[0.088]	[0.106]	[0.106]	[0.105]	[0.006]	[0.006]	[0.006]
N. observations	3,833,047	968,290	4,546,829	2,888,559	754,162	3,439,438	3,639,150	923,854	4,319,816
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results from estimating (9). Panel A reports estimates from a plain two-way fixed effects specification, while Panel B adds the baseline controls $X_{n\ell t}$ described below. The dependent variable is the firm growth rate in columns 1-3, the investment rate in columns 4-6, and the precautionary savings rate in columns 7-9. The variable $\Delta\lambda \times$ Post measures the effect of changes in legal uncertainty in the post-reform period for firms in treated municipalities. The λ are estimated using the baseline measure of drafting clarity. The vector of controls $X_{n\ell t}$ includes: a full set of firm and time fixed effects; sector fixed effects interacted with regional GDP growth; time fixed effects interacted with both the firm's credit-rationing probability and regional GDP growth; and firm size (pre-reform sales percentile) interacted with the firm's Z-credit score. In columns 4-9, controls also include next year's sales scaled by firm total assets. Robust standard errors clustered at the firm level are reported in round brackets, while bootstrapped standard errors are reported in square brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panels (b) and (c) of Figure 5 show the year-by-year coefficients of the effect of the legal uncertainty shock, $\Delta\lambda$, on treated firms, with panel (b) presenting results for the firm investment rate and panel (c) for precautionary provisions (scaled by total assets). These panels are analogous to panel (a), which reports results for firm growth rates. For both investment and precautionary provisions, treated and untreated firms follow similar trajectories prior to the reform, with divergence emerging only afterward. This pattern is consistent with treated firms responding to heightened legal uncertainty by reducing investment and increasing precautionary provisions.

The last six columns of Table 5 report the average post-reform effects of the uncertainty shock, $\Delta\lambda$, on firms' investment rates (Columns 4-6) and precautionary provisions (Columns 7-9). In response to increased legal uncertainty, investment declines, with estimated coefficients similar across control groups. A one standard deviation increase in $\Delta\lambda$ (0.043) reduces the investment rate by between 1 and 1.3 percentage points, equivalent to approximately 9-12 percent of the median firm's investment rate. For precautionary provisions, a one standard deviation increase in the legal uncertainty shock raises firm precautionary savings by 4 percent of the sample mean.

GPT measure Table 6 reports estimates of the effects of changes in legal uncertainty, $\Delta\lambda$, calculated using the GPT measure of drafting clarity. These estimates are obtained from equation (9) including the baseline controls. The control group consists of firms located in municipalities unaffected by the court consolidation. Column 1 report results for firm growth, column 2 for investment and column 3 for precautionary provisions. Consistent

Table 6: Firm outcome effects using the GPT measure

Dep. var.:	Growth rate	Investments rate	Precautionary savings
	(1)	(2)	(3)
$\Delta\lambda\times$Post	-0.373***	-0.379**	0.007
	(0.120)	(0.151)	(0.008)
N. observations	3,833,047	2,888,559	3,639,150
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Other FE	Yes	Yes	Yes

Notes: Results from estimating (9) using the GPT-measure of drafting clarity. The standard deviation of $\Delta\lambda$ calculated with the GPT measure is 0.027. The dependent variable is the firm growth rate in column 1, the investment rate in column 2, and precautionary savings in column 3. The control group includes all firms in municipalities unaffected by the court consolidation. The controls are the same as in panel B of Table 5. Robust standard errors clustered at the firm level are reported in round brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

with previous results, we find a negative and statistically significant coefficient of legal uncertainty, λ, on firm growth. The effects on firm growth are quantitatively similar to those obtained using the baseline drafting clarity measure: the point estimate is roughly one-third larger, while the standard deviation of the uncertainty shock calculated using the GPT measure, $\Delta\lambda$, is about one-third smaller (0.027 compared with 0.043 under the baseline measure). A similar pattern emerges for investment, whereas the effects on precautionary provisions remain positive but lose statistical significance.

5.3 Robustness

We next present robustness checks on the effects of the legal uncertainty shock, $\Delta\lambda$, on firm growth, investment, and precautionary provisions. All specifications are variations of equation (9), based on the baseline measure of drafting clarity, including the baseline controls from Panel B of Table 5, and using firms in municipalities unaffected by the court consolidation as the control group.

Our identification strategy requires that the local legal uncertainty shock $\Delta\lambda$ differentially affects treated firms and that we adequately control for other local changes potentially correlated with this shock. The reassignment of firms to different courts may have induced changes in court-related characteristics beyond legal uncertainty due to poor drafting. Hence, we control for several other (time varying) court attributes, including court size, magistrates' experience, trial length, firm distance to the court, and the average reversal probability on appeals originated from the court s_j (as estimated using (7)). We also control, using dummy variables, for the staggered introduction of specialized business courts in selected jurisdictions induced by the 2012 reform. We refer to this as the *court-control* specification.[27]

The 2012 reform also included civil procedure changes aimed at promoting preliminary mediation and curbing delaying tactics. Time fixed effects (along with a broad set of regional controls) absorb all reform-induced changes common across firms. To ensure that our estimates are not influenced by reform-induced changes affecting treated and untreated firms differently, we re-estimate regression (9) using only the treated sample (i.e., firms in absorbed municipalities). In this subsample, identification relies solely on cross-municipality differences in the size of the uncertainty shock, making the estimates robust to differential effects of the reform. We also show that our results remain robust when the legal uncertainty shocks $\Delta\lambda$ are measured using only criminal cases, which, by construction, are

[27] All court controls are introduced simultaneously to save space; Table A9 in Appendix J shows that the results are robust to introducing them separately.

orthogonal to the civil procedure changes introduced by the reform. Focusing solely on criminal cases also provides a robustness check, ensuring that self-selection into cases appealed to the Court of Cassation does not drive our results. As discussed, such self-selection concern is likely to be less pronounced in criminal cases, since the stakes for the defendant are arguably higher and prosecuting authorities are obliged to act under the principle of mandatory criminal prosecution.

Finally, because we are using an unbalanced panel of firms whose composition may have been affected by the reform through changes in business turnover—via exit or entry, as documented in Section 6—we also re-estimate regression (9) on the balanced panel of firms present continuously throughout the ten-year sample period.

Table 7 presents the robustness checks for firm growth (columns 1-4), investment (columns 5-8), and precautionary provisions (columns 9-12). Within each set of four columns, the first reports results for the *court-control* specification; the second uses the balanced panel of firms; the third uses the uncertainty shock $\Delta\lambda$ constructed from criminal cases only; and the fourth restricts the sample to treated firms. Table A10 in Appendix J reports the estimated coefficients of the new court-level regressors included in the *court-control* specification.[28]

Across the specifications in Table 7, the estimated effect of the uncertainty shock $\Delta\lambda$ on firm growth (columns 1-4) remains similar in both magnitude and significance to the baseline estimate. The estimated coefficients are remarkably stable across specifications, and, if anything, the point estimates are slightly larger when the shock is constructed using only criminal cases. The firm growth results are broadly confirmed when examining the effects of the uncertainty shock on firm investment (columns 5-8 of Table 7) or precautionary provisions (columns 9-12 of Table 7). These patterns indicate that the baseline results are not driven by changes in other court characteristics, by civil procedure changes introduced by the 2012 reform, by sample attrition due to business turnover, by self-selection into cases appealed to the Court of Cassation, or by differential effects of the reform between treated and untreated firms, except for those induced by the uncertainty shock.

[28]The average experience of judges in a court increases firm growth, while greater distance to the court reduces firm growth and investment, consistent with Mocetti, Pesenti, and Roma (2025), who document that proximity to courts affects how agents make use of the judicial system. The court's average reversal rate in levels, s_j, is negatively associated with firm growth and investment, although the coefficient is statistically significant only for the investment rate. The presence of a business court has a positive, though not statistically significant, effect on firm growth.

Table 7: Effect of legal uncertainty shock, Robust analysis

Dep. var.:	Growth rate				Investment rate				Precautionary savings			
	With court controls	Firms entire period	$\Delta\lambda$ from criminal cases	Only treated firms	With court controls	Firms entire period	$\Delta\lambda$ from criminal cases	Only treated firms	With court controls	Firms entire period	$\Delta\lambda$ from criminal cases	Only treated firms
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\Delta\lambda\times$ Post	-0.260***	-0.280***	-0.470***	-0.281***	-0.270**	-0.311***	-0.329***	-0.286**	0.011**	0.011**	0.003	0.0116**
	(0.086)	(0.084)	(0.095)	(0.0866)	(0.112)	(0.110)	(0.119)	(0.112)	(0.005)	(0.005)	(0.006)	(0.00508)
N. obs.	3,737,118	3,437,521	3,783,713	256,017	2,814,415	2,664,352	2,852,235	204,580	3,547,844	3,331,578	3,593,972	244,557
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents results from estimating (9). The dependent variable is the firm growth rate in columns 1-4, the investment rate in columns 5-8, and the precautionary savings rate in columns 9-12. The uncertainty shock $\Delta\lambda$ is calculated using the baseline measure of drafting clarity. All specifications include the baseline control variables used in panel B of Table 5, as described in the corresponding legend. Columns 1, 4, and 7 add to the baseline controls the following set of time varying court-level variables: district court size, average judicial tenure, staff shortages, trial length, the lower-court average reversal rate, as captured by the s_j dummies from regression (7), a dummy for the presence of a business court in the year, and the firm distance to the court. Columns 2, 5, and 8 restrict the analysis to the subsample of firms continuously present throughout the entire sample period. Columns 3, 7, and 11 use the uncertainty shock $\Delta\lambda$ calculated using only criminal cases. Columns 4, 8, and 12 restrict the analysis to the subsample of treated firms. Except in Columns 4, 8, and 12, the control group consists of all firms in municipalities unaffected by the court consolidation. Robust standard errors clustered at the firm level are reported in round brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8 presents results using the two-step estimator of de Chaisemartin and Lei (2024), which accounts for potential dependence between the uncertainty shock $\Delta\lambda_{n\ell}$ and the pre-reform level of legal uncertainty, $\lambda^{-}_{n\ell}$. Panel A reports the first-stage results, in which the change in legal uncertainty, $\Delta\lambda_{n\ell}$, is conditioned on its pre-treatment level, $\lambda^{-}_{n\ell}$, using a quadratic B-spline, chosen with Stata's default settings[29] Panel B reports the second-stage estimates of the effects of the legal uncertainty shock, using the first-stage residuals $\Delta\lambda_{n\ell} - E(\Delta\lambda_{n\ell})$, as the independent variable, while controlling for the predicted change, $E(\Delta\lambda_{n\ell})$. Columns 1-3 report results for firm growth rates, columns 4-6 for investment rates, and columns 7-9 for precautionary provisions. For each outcome, we report results for the three control groups discussed earlier: unaffected municipalities, absorbing municipalities, or both. Bootstrapped standard errors in the second stage regressions are shown in square brackets.

The first-stage estimates indicate a negative and statistically significant relationship between the change in legal uncertainty induced by the reform and its pre-reform level, which validates the mean reversion concerns raised by de Chaisemartin and Lei (2024). The second-stage results are broadly consistent with those reported in Table 5. For example, the point estimates for firm growth across the three subsamples are -0.312, -0.255, and -0.275, compared with -0.272, -0.288, and -0.268 in Table 5. There is some evidence that the estimated effect of the legal uncertainty shock on firm investment is slightly larger in magnitude, while the estimated effect on precautionary savings is smaller and not statistically significant in two out of the three specifications.

[29] In the second stage, we collapsed the panel into two periods: the five years preceding the reform and the five years following it. To address the unbalanced nature of the panel, we first remove year and firm fixed effects from all variables before aggregating the data within each period.

Table 8: De Chaisemartin and Lei two-stage estimator

Dep. var.:	Growth rate			Investments rate			Precautionary savings		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Control group	Unaffected	Absorbing	All districts	Unaffected	Absorbing	All districts	Unaffected	Absorbing	All districts
Panel A: First stage									
Effect of λ_{nl}^{-}	-0.113***	-0.485***	-0.129***	-0.118***	-0.485***	-0.131***	-0.112***	-0.483***	-0.128***
	(0.004)	(0.004)	(0.001)	(0.005)	(0.005)	(0.001)	(0.004)	(0.004)	(0.001)
Panel B: Second stage									
$\Delta\lambda_{nl} - \mathbb{E}_n[\Delta\lambda_{nl}\|\lambda_{nl}^{-}]$	-0.255**	-0.312**	-0.275**	-0.372**	-0.467***	-0.349**	0.009*	0.002	0.006
	(0.120)	(0.125)	(0.118)	(0.145)	(0.151)	(0.142)	(0.005)	(0.005)	(0.005)
	[0.114]	[0.117]	[0.120]	[0.137]	[0.136]	[0.144]	[0.005]	[0.004]	[0.004]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of D.V.	-0.041	-0.040	-0.040	-0.003	-0.004	-0.003	0.000	0.000	0.000
N. observations	356584	90024	424892	286072	74148	341868	340826	86243	406228

Notes: Panel A presents the first-stage results from regressing the change in legal uncertainty post-reform, $\Delta\lambda_{n\ell}$, on the pre-treatment legal uncertainty level, $\lambda_{n\ell}^{-}$. To fully capture potential non-linearities in this relationship, we employ a quadratic B-spline selected according to Stataâ€™s default settings. Panel B reports the results of estimating equation (9), using as the key independent variable the residuals from the first-stage regression, $\Delta\lambda_{n\ell} - E(\Delta\lambda_{n\ell})$, and controlling for the predicted change $E(\Delta\lambda_{n\ell})$. The data are collapsed into two periods: the five years preceding the reform and the five years following it. Before collapsing, we partial out year and firm fixed effects from all variables. The control variables are the same as those used in the baseline specifications of Table 5. Bootstrapped standard errors for the second-stage regressions are reported in square brackets. Robust standard errors clustered at the firm level are reported in round brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Output costs

Higher legal uncertainty due to poor drafting, a, can affect long-run aggregate GDP per capita through the following channels: (i) the firm growth rate, g (as estimated above), (ii) the rate of business creation, b, (iii) the business exit rate, e, and (iv) the initial size at entry, y_0. We next estimate the effects of a on each margin and quantify its impact on Italian GDP per capita. We first present the theoretical framework and then assess the associated output costs.

6.1 GDP decomposition

For simplicity we work in continuous time. Suppose firms are born with output y_0 and grow over time at rate g.[30] Firms die at Poisson arrival rate e. The number of new businesses per capita is b. Then, steady-state aggregate GDP per capita is given by

$$y = \frac{b}{e-g} y_0,$$

which in logs becomes

$$\ln y = \ln b - \ln (e-g) + \ln y_0. \tag{10}$$

A change in legal ambiguity due to poor drafting, a, alters legal uncertainty, $u = \overline{\lambda} a$, which in turn could affect all components of logged GDP per capita in (10). Then, a one percent increase in a leads to a long-run percentage change in output per capita equal to:

$$\mathbf{H} \equiv \frac{d \ln y}{d \ln a} = \left[\frac{d \ln b}{du} + \frac{d \ln y_0}{du} - \frac{e}{e-g} \cdot \frac{d \ln e}{du} + \frac{1}{e-g} \cdot \frac{dg}{du} \right] \times a \times \frac{du}{da}. \tag{11}$$

Let a_t denote the level of ambiguity arising from poor drafting in the average Italian law at time t, and let L_t denote the stock of legislation at time t, measured by the total number of words in all laws. We assume that L_t grows at a constant rate l, so that $\dot{L}_t = l L_t$. New legislation is drafted with an average degree of poor drafting, p, and existing ambiguity is resolved over time at rate $\delta = 0.09$. Then, legal ambiguity a_t evolves according to

$$\dot{a}_t = pl - (\delta + l) a_t, \tag{12}$$

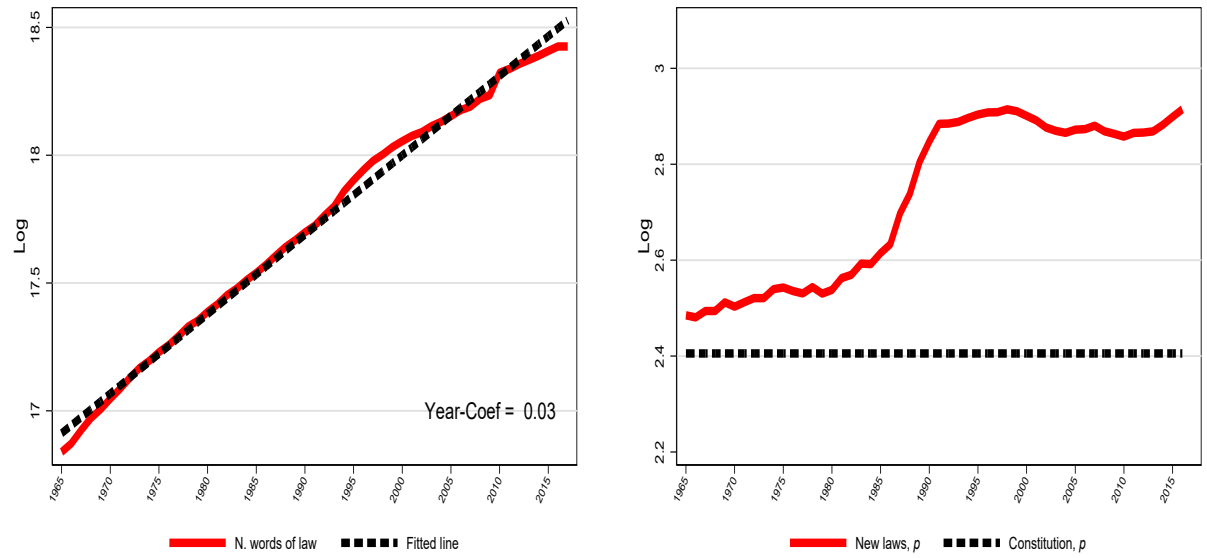
[30] In practice, we assume that firm output is described by the Cobb-Douglas production function $\tilde{y} = e^z k^\eta$, where z denotes the firm's total factor productivity (TFP), k is the firm's capital stock, and $\eta \in (0,1)$ is the output elasticity of capital. Capital evolves according to $\dot{k}_t = -\tilde{\delta} k_t + i_t$, where $\tilde{\delta}$ is the capital depreciation rate and i_t is firm investment. Then, the growth rate of firm output is $g = g_z + \eta g_k$, equal to the sum of the TFP growth rate, g_z, and the capital growth rate, g_k, with the latter weighted by the capital share, η.

which implies that the steady-state level of legal ambiguity, in logs, is equal to

$$\ln a = \ln\left(\frac{l}{l+\delta}\right) + \ln p. \tag{13}$$

Panel (a) of Figure 6 shows the annual total word count of Italian legislation in logs, $\ln L_t$, since the mid-1960s. The solid red line shows the actual series, and the black dotted line shows its fitted linear trend. The trend implies that the volume of legislation has increased at a remarkably stable rate of about 3 percent per year, $l = 0.03$, suggesting that the first term on the right-hand side of equation (13) has remained approximately constant over time. Panel (b) shows the average degree of poor drafting of all new laws enacted in a given year in logs, $\ln p_t$ (red solid line), calculated using our principal component with the factor loadings reported in Table A3. The horizontal black dotted line represents the (log) degree of poor drafting of the fundamental principles of the Italian Constitution, which approximately corresponds to the top quartile of the cross-sectional distribution of drafting quality across all Italian laws over the full period. Figure 6 indicates that the

Figure 6: New legislation word count and degree of poor drafting over time



(a) N. Words of laws, $\ln L$ **(b) Degree of poor drafting, $\ln p$**

Notes: Panel (a) presents the total number of words in Italian legislation each year, $\ln L_t$. The solid red line shows the actual series; the black dotted line its fitted linear trend. In panel (b), the black dashed line indicates the degree of poor drafting, $\ln p$, of the fundamental principles of the Italian Constitution. The red solid line shows the average degree of poor drafting of all new laws enacted that year. The degree of poor drafting is calculated as a five-year rolling average. All variables are in logs.

drafting clarity of Italian laws has declined by approximately 40% since the late 1990s.[31] Relative to the average newly enacted laws in the 2000s, the drafting clarity of the Italian Constitution is about 60% higher.

6.2 Quantification of costs

To evaluate $\mathbf{H}$ in (11), we exploit variation across municipalities and over time in firm creation, $b_{\ell t}$, firm exit rates, $e_{\ell t}$, average firm size at entry, $y_{0\ell t}$, and reform-induced changes in legal uncertainty, λ, where $b_{\ell t}$ is the number of new firms, $e_{\ell t}$ is the fraction of firms exiting in a year, and $y_{0\ell t}$ is the production value in the first year after entry. For each $z_{\ell t} = \ln b_{\ell t}, \ln e_{\ell t}, \ln y_{0\ell t}$, we estimate the following simple two-way fixed effects model:

$$z_{\ell t} = \beta_z \Delta\lambda_{\ell t} \times \text{Post}_t + f_\ell + f_t + u_{\ell t} \tag{14}$$

Table 9 reports the results from estimating equation (14): column (1) shows results for business creation, $\ln b_{\ell t}$; column (2), for the business exit rate, $\ln e_{\ell t}$; and column (3), for the initial size at entry, $\ln y_{0\ell t}$. The estimates indicate that an increase in legal uncertainty raises business turnover: firm exit increases, which in turn stimulates new business creation. Additionally, legal uncertainty reduces firm size at entry, consistent with the empirical evidence on investment discussed in Section 5.

To evaluate $\mathbf{H} \equiv \frac{d\ln y}{d\ln a}$ in (11), we set the steady-state growth rate of GDP per capita to $g = 0.02$, and the average business exit rate to $e = 0.07$, approximately equal to their sample averages. Combining these values with the estimates above yields an elasticity of GDP per capita with respect to legal ambiguity induced by poor drafting slightly above 8 percent in absolute value: $\mathbf{H} = -0.0816$.[32]

Panel (b) of Figure 9 documents a quality gap of roughly 60 percent between the funda-

[31] Gratton et al. (2021) attribute at least part of this deterioration in drafting clarity to the increased political instability that has plagued Italy since the fall of the Berlin Wall in 1989-90. Consistent with this interpretation, the increased reliance on emergency decree legislation accounts for around 50 percent of the overall decline in the drafting quality of Italian laws over time.

[32] This estimate is obtained by substituting the following values into (11). From (2) and Table 3, we obtain $\frac{du}{da} = \bar{\lambda} = 0.012$. Given (4), the estimates in Tables 5 and 9 imply:

$$\frac{d\ln b}{du} \times a = 1.047; \quad \frac{d\ln y_0}{du} \times a = -0.979; \quad \frac{d\ln e}{du} \times a = 1.022; \quad \frac{dg}{du} \times a = -0.272$$

Combining these values with the value of $\frac{du}{da} = 0.012$ finally yields:

$$\mathbf{H} = \left(1.047 - 0.979 - \frac{0.07}{0.05} \times 1.022 - \frac{1}{0.05} \times 0.272\right) \times 0.012 = -0.0816.$$

Table 9: Economic costs of ambiguous laws

	Business creation rate, $\ln b$ (1)	Business exit rate, $\ln e$ (2)	Start-ups size in 1st year, $\ln y_0$ (3)
$\Delta\lambda\times$ Post	1.047**	1.022*	-0.979
	(0.519)	(0.613)	(0.656)
N. observations	77,213	75,928	52,165
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes

Notes: All variables are scaled by their corresponding time-series average within each municipality. The dependent variable in column (1) is business creation, $\ln b_{\ell t}$—the number of new firms; in column (2), it is the business exit rate, $\ln e_{\ell t}$—the fraction of firms that exit during the year; and in column (3), the initial average size of start-ups, $\ln y_{0\ell t}$—the production value in the first year after the entry year. The specification always includes municipality and year fixed effects. Robust standard errors are reported in round brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

mental principles of the Italian Constitution and today's newly issued Italian laws. This implies that if all current Italian laws were drafted with the same clarity as the Constitution's fundamental principles, GDP per capita would be 4.9 percent higher—an annual gain exceeding 100 billion euros in 2024 prices. Combining the estimate for **H** with the approximately 40 percentage point decline in drafting clarity over the past 20 years (see Panel (b) of Figure 9) yields an estimated 3.26 percent reduction in annual GDP per capita. This implies that roughly two-thirds of the total gap relative to the Constitution's benchmark has emerged over the past two decades.

7 Conclusions

We measured the clarity of the corpus of Italian laws and showed that poorly drafted laws introduce statutory ambiguity. Drafting deficiencies are widespread across Italian laws, independent of the intrinsic complexity of their subject matter, and increase the likelihood that lower courts and the Italian Supreme Court of Cassation issue conflicting rulings on the same case. Since this effect varies systematically across courts, we exploited an exogenous change in the assignment of firms to courts to identify the causal effect of legal uncertainty stemming from poor law drafting on economic outcomes. We found that, other factors being constant, an increase in legal uncertainty lowers firm growth and discourages investment. The results are robust to several concerns, including potential confounding from other court-related changes, alternative definitions of the control group and measures of law drafting clarity, as well as the possibility that changes in legal uncertainty are non-

random due to mean reversion. The economic costs of poor law drafting appear substantial. We estimate that Italian GDP would be nearly 5 percent higher if all laws were drafted to the standard of the top quartile of drafting quality among Italian laws, a realistic policy target that coincides with the clarity of the Italian Constitution.

In light of the substantial economic costs of poorly drafted laws, improving legislative clarity is a highly cost-effective intervention. This is now more feasible than ever thanks to recent advances in artificial intelligence and large language models that can support legislators in drafting new laws and simplifying existing ones. A key question remains: why do politicians, when left to their own devices, so often draft laws poorly? Gratton et al. (2021) take an initial step toward answering this question. They suggest that poor drafting may stem from political instability and the ensuing perverse incentives faced by politicians, who prioritize signaling activism over addressing substantive problems. Yet, because legislation involves many actors, including bureaucrats and lobbyists, alternative explanations are possible. Future research should examine the root causes of poor drafting more closely to determine how legislators can be more effectively held accountable to ensure that laws are accessible, intelligible, clear, and predictable.

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Online Appendix

Appendix A shows that different lower courts cite laws with the same average level of legal ambiguity a, an assumption we rely on to interpret the β-coefficients in Equation (OE). Appendix B provides a microfoundation for how greater lower-court misalignment (higher λ) discourages firm investment and other business activities. Appendix C further describes the data. Appendix D provides the relevant excerpts from legal manuals that motivate the baseline measure of drafting clarity discussed in Section 3. Appendix E explains the construction of the principal component of the baseline measure of ambiguity a. Appendix F provides details on the methodology used to construct the ChatGPT-based measure of drafting clarity. Appendix G examines whether judges from courts with lower λ_j are more likely to get promoted to the Court of Cassation. Appendix H shows that the results are invariant when the uncertainty shocks $\Delta\lambda$ are constructed using only pre-2012 Court of Cassation judgments. Appendix I discusses the bootstrapping algorithm. Appendix J reports additional figures and tables.

A Variation in legal ambiguity across lower courts

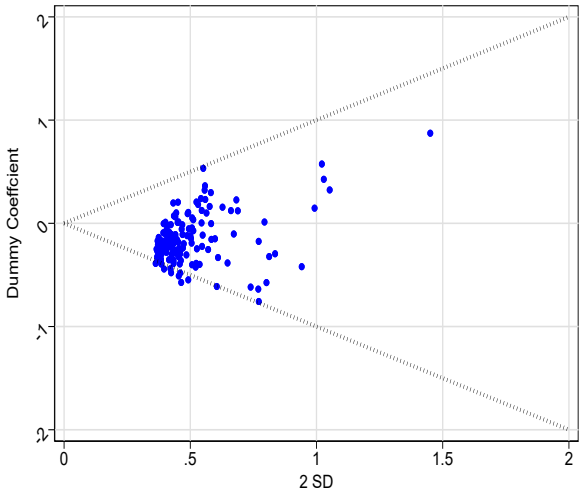
The interpretation of the β-coefficients in the OE-equation relies on the assumption that the ambiguity of laws a does not vary systematically across courts (after adding the full set of controls X_{ijt}). To test this identification assumption, we run the following regression on the sample of Court of Cassation judgments:

$$a_{ijt} = d_j + \gamma' X_{ijt} + \xi_{ijt} \tag{15}$$

where a_{ijt} denotes the average ambiguity due to poor drafting of the laws cited in judgment i at time t, originating from court j. The vector X_{ijt} includes the set of controls described in the legend of Table 3. The term d_j represents a fixed effect for the lower courts from which the appeal to the Court of Cassation originated. We estimate 159 court fixed effects d_j, expressed relative to the omitted reference court (Acqui Terme). We test the null hypothesis that the dispersion of the d_j coefficients is small and statistically insignificant. A lack of significant variation would suggest that legal ambiguity a does not differ systematically across lower courts in Italy. Figure A1 displays the full set of estimated d_j coefficients on the y-axis and two times their corresponding standard deviation on the x-axis. The black dotted

lines correspond to the 45-degree lines with positive and negative slope. Coefficients are not statistically different from zero if they fall within these two lines. There are only four district courts out of 159 that are marginally outside the two 45-degree lines. Overall, this evidence indicates that differences in legal ambiguity across courts are relatively unimportant.

Figure A1: Variation in legal ambiguity across lower courts



Notes: Results from estimating the court fixed effects coefficients d_j in equation (15). The omitted district court is Acqui Terme. The coefficients d_j are plotted on the y-axis, and two times their corresponding standard errors are on the x-axis. The black dotted lines represent the $\pm 45°$ lines.

B A micro-foundation for why greater lower-court misalignment, λ, raises legal uncertainty

We provide a simple microfoundation for why greater lower-court misalignment (higher λ) raises legal uncertainty and can thereby discourage firm investment and other business activities. There are two firms: one located in legal district A and the other in district M. The court in A is perfectly aligned with the Court of Cassation while the court in M is aligned only with probability γ. We refer to $\lambda = 1 - \gamma$ as to the greater *misalignment* of lower court M relative to lower court A. Both firms can undertake an investment κ that requires interpreting the law—say, launching a new product whose compliance with safety or consumer-protection standards is unclear, or adopting a potentially abusive pricing strategy, or using a patented technology when the scope of the patent claims is ambiguous. The net profit from undertaking investment κ is certain and equal to π in both districts, A and M. The legal risk of undertaking investment κ consists of two potential costs. The

first, χ_F, is the cost of being found guilty at the conclusion of the process; the second, χ_G, is the reputational or judicial cost incurred from being declared guilty at least once during the process.

The law is ambiguous. Conditional on undertaking investment κ, there are two possible legal interpretations, L or U, equally likely ex ante, each with probability 1/2. Under interpretation L, investment κ is lawful; under interpretation U, it is unlawful, and the firm is found guilty. This ambiguity is identical for both firms: the law is equally unclear in district A and M. This means that, ex ante, the probability of being found guilty conditional on undertaking the investment κ is the same in A and M. Indeed, if the firm in A makes the investment κ, it is found guilty with probability 1/2:

$$\Pr(\text{Guilty}|\,\text{Location A}) = \frac{1}{2},$$

which equals the probability that the investment κ is declared unlawful by lower court A, whose decision is never reversed by the Court of Cassation. If the firm in M undertakes the investment κ, it is again eventually found guilty with probability 1/2:

$$\Pr(\text{Guilty}|\,\text{Location M}) = \frac{1}{2}\gamma + \frac{\lambda}{2} = \frac{1}{2}.$$

Therefore, the ex ante distribution of final legal outcomes is the same in districts A and M.

We now calculate the firm probability of *being declared guilty at least once* in district A and M. In A the probability is 1/2,

$$\Pr(\text{Guilty at least once} \mid \text{Location A}) = \frac{1}{2},$$

In M it is equal to

$$\Pr(\text{Guilty at least once} \mid \text{Location M}) = \frac{1}{2} + \frac{\lambda}{2} > \frac{1}{2}.$$

This corresponds to the probability of being found guilty in the first instance, equal to $\frac{1}{2}$, plus the probability of being acquitted in the first instance, equal to $\frac{1}{2}$, multiplied by the probability that this decision is overturned by the Court of Cassation, equal to $\lambda = 1 - \gamma$.

Thus the expected firm profits in A are equal to

$$\Pi_A = \pi - \frac{1}{2}\chi_F - \frac{1}{2}\chi_G \tag{16}$$

while expected firm profits in M are equal to

$$\Pi_M = \pi - \frac{1}{2}\chi_F - \frac{1}{2}(1+\lambda)\chi_G \tag{17}$$

If there is a reputational cost of being found guilty at least once, $\chi_G > 0$, the firm's expected return from undertaking the investment κ in district M is lower than in district A, $\Pi_M < \Pi_A$. This difference is larger the greater the misalignment of lower court M with the Court of Cassation, as measured by λ. In this way, greater λ reduces the return to investment and discourages firms from undertaking risky business activities.

C Data description

We use four main data sets: one for Court of Cassation judgments, one for the text of laws, one for firms, and one for the characteristics of lower courts.

Court of Cassation judgments Since 2004, all judgments by the Court of Cassation have been publicly available in PDF format from Wolters Kluwer (2019). We consider the period 2004-2017 and include all judgments originating from locations with a district court that cite at least one law (around 93% of all Court of Cassation judgments during the period). We exclude judgments on appeals originating from one of the 260 locations that host only a Justice of the Peace (5 percent of all observations). Justices of the Peace are temporary honorary magistrates who have jurisdiction only over minor matters. Our final sample includes 485,271 judgments: 193,895 for civil cases and 291,376 for criminal cases. The judgments report the Court of Cassation's decision (whether it upholds the decision of the lower court or reverses it); the location of the lower court whose decision was appealed; all legal norms cited by either the lower court or the Court of Cassation; the date of the decision; the nature of the case (civil or criminal); the section of the Court of Cassation that handled the case (see Table A1); and the name of the president of the section of the Court at the time of the decision. A legal norm may refer to a formal law issued by Parliament, one of the legal codes (*Codice Civile*, *Codice Penale*, *Codice di Procedura Civile*, and *Codice di Procedura Penale*), or a government decree. We refer to all these norms simply as *laws*. Table A1 shows the share of cases handled by the 13 specialized sections of the Court of Cassation and the corresponding reversal rates over the period 2004-2017.

Panel (a) of Figure A2 presents the time profile of the quarterly average number of citations to laws per judgment, which appears to have increased over time. The average reversal

Table A1: Sections of the Italian Court of Cassation, 2004-2017

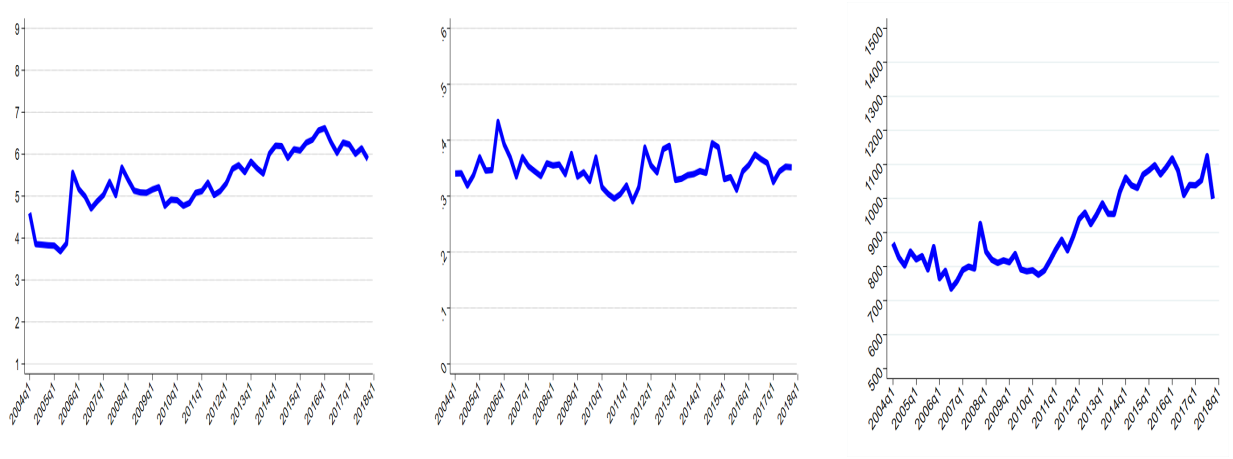
	Judgments, %	Reversals, %
A) Civil		
Section 1	6.7%	44.0%
Section 2	5.6%	33.6%
Section 3	5.9%	24.6%
Section 4	11.8%	33.8%
Section 5	5.8%	48.8%
Section 6	2.6%	60.0%
Section united	1.4%	55.4%
B) Criminal		
Section 1	10.5%	34.2%
Section 2	10.6%	22.2%
Section 3	10.4%	36.9%
Section 4	9.1%	37.6%
Section 5	9.7%	33.7%
Section 6	9.0%	30.9%
Section *feriale*	0.4%	20.6%
Section united	.07%	49.4%

Notes: The table shows the fraction of judgments and the corresponding reversal probability for each section of the Italian Court of Cassation. For a detailed description of the competences of each section of the Italian Court of Cassation, see `https://www.cortedicassazione.it/resources/cms/documents/Tabella_di_organizzazione_1.pdf`. The sample period is 2004-2017 .

probability is 35%, slightly higher for civil than for criminal cases (see Table 1). Reversal probabilities are similar across the criminal sections of the Court but vary substantially across civil sections (see Table A1). Reversal probabilities are notably higher in 5, which specializes in tax issues. Panel (b) of Figure A2 plots the average reversal probability, which remains stable over the period. To proxy for case complexity, we use the number of characters in the judgment. Panel (c) of Figure A2 shows its quarterly average, which rises slightly in the second half of the sample period.

Laws We draw laws from Normattiva (2016). For each law, we compute ten measures to assess both the quality of its writing and the extent to which it is self-contained or dependent on references to multiple other laws—which may conflict with one another—a feature we refer to as dependency ambiguity. We build on established style manuals (Cassese, 1993; Butt and Castle, 2006; Thornton, 1979) to assess the quality of legal writing through eight indicators: number of gerunds per words (multiplied by 1,000); average word length;

Figure A2: Citations, reversal probability and judgments length, 2004-2017



(a) N. Laws per judgment **(b) Reversal probability** **(c) Judgment length**

Notes: Panel (a) plots the average quarterly number of laws cited in a Court of Cassation judgment; Panel (b), the reversal probability; and Panel (c), the average quarterly length of judgments, measured by the number of characters (excluding stop words). The sample includes all Court of Cassation judgments from 2004 to 2017 that cite at least one law and originate from a location with a district court.

average root length; number of modal verbs per word; number of demonstrative adjectives per word; number of embedding and contingency clauses per word; number of pronouns per word; and average phrase length (in characters). We measure dependency ambiguity using two indicators: the presence of a preamble, which often contains numerous legal references presented in an unstructured and opaque manner, and the number of references to other laws in the main body of the text, scaled by total word count. We also leverage the capabilities of LLMs to construct the measure of drafting clarity discussed in Appendix F

We discount the 10 indicators of law drafting clarity using the estimated δ and summarize the discounted indicators with their first principal component. We use the number of gerunds per 1,000 words as the unit of measurement, normalizing its factor loading to one. The first principal component accounts for approximately three-fourths of the total variance in the 10 discounted indicators of drafting clarity. The loadings are all positive and similar in magnitude, implying that each drafting clarity indicator contributes roughly equally (see the Appendix for further details). Panel A of Table 1 reports summary statistics for the resulting measure of legal ambiguity a. As shown, there is substantial variability in the ambiguity of laws cited across different judgments, with its mean roughly equal to its standard deviation.

To better characterize the cases handled by the Court of Cassation, we also classify laws by

topic. For this purpose, we leverage a unique feature of the Italian legislative process: Articles 87 and 90 of the Italian Constitution establish that laws are enacted by the President of the Republic, but the presidential writs must be countersigned by the Prime Minister as well as by all Ministers whose offices are relevant to the subject matter of the law. This feature was introduced by the 1848 Statuto Albertino to protect the king: since "the king can do no wrong," the countersignature by the king's ministers served to preserve the sacred and infallible character of the monarch by making the ministers liable for any hypothetical crime associated with the introduction of a law. The same principle is preserved in the Italian Constitution, where it serves to protect the President of the Republic.

Accordingly, we assign each law to the area of competence of the minister who countersigned the presidential writ, and group the laws into the following topic categories: (i) Agriculture, (ii) Cultural heritage, (iii) Communication, (iv) Defence, (v) Economics, (vi) Foreign Policy, (vii) Law, (viii) Internal affairs, (ix) Public administration, (x) Education, (xi) Labour, (xii) Health, (xiii) Leisure, (xiv) Transport and (xv) a residual category labeled Prime Minister. "Agriculture" corresponds to the all laws signed by the minister of agriculture, fisheries and animal Resources; "Cultural heritage" corresponds to those signed by the minister of cultural heritage or environment; "Communication" to those signed by the minister of transportation or communication; "Defense" to those signed by the minister of defense; "Economics" to those signed by the minister of economy or finance; "Foreign Policy" to those signed by the minister of foreign policy; "Law" to those signed by the minister of justice; "Internal affairs" to those signed by the minister of interior; "Public administration" to those signed by the minister of public administration; "Education" to those signed by the minister of education; "Labour" to those signed by the ministers of labor; "Health" to those signed by the minister of health; "Leisure" to those signed by the minister of sport or tourism; "Transport" to those signed by the minister of infrastructures and transport; and "Prime Minister" corresponds to those signed by the president of the Council of Ministries.

Firms We have data on the universe of limited liability companies from the Company Accounts Data Service (CADS); CADS is a proprietary database owned by CERVED, a private data provider. CADS reports the balance sheet and the income statement of all Italian limited liability companies. It also provides relevant firm demographics, including the year of foundation, the ATECO industry classification, the legal form of the company, and the municipality of the firm's headquarter which we use to assign to each firm and year the measure of legal uncertainty λ of the court that rules in the municipality. CADS is an

unbalanced panel with information on more than 700,000 firms beginning in 1995 and up to 2019. Given the focus on the 2012 reform, we restrict the sample to the years 2008-2017, covering five years before and five years after the reform.

From the CADS data we calculate the growth rate of the value of firm production and the investment rate: the ratio between firm gross investment in either physical or intangible capital and the previous year book value of the capital stock—the sum of all tangible and intangible assets. We also compute a measure of future opportunities equal to the ratio of one-year ahead sales scaled by the capital stock. As a measure of precautionary provisions we take the ratio of provisions for risks and charges over total assets. In some specifications, to account for aggregate cyclical and financial conditions, we also include regional GDP growth rates (at the NUTS-2 level across 20 regions) interacted with ninety-five sector dummies from CADS, as well as the firm's estimated probability of being credit rationed, which we interpret as a proxy for credit supply conditions at the time. We use the Bank of Italy Survey of Industrial and Service Firms (INVIND) to impute, for each firm, the likelihood of being credit rationed. The dependent variable is a dummy equal to one if the firm applied for credit and was denied, or did not apply because it expected to be turned down. Then, using the INVIND dataset, we estimate a probit model that includes a full set of sector and time dummies, regional GDP growth, and an interaction between time dummies and the firm's log sales. The predicted values from this model are then used to impute the probability of credit rationing for each firm in the CADS dataset. In Section 5, we use these variables to estimate the effects of legal uncertainty on firm growth, investment and precautionary provisions.

For each municipality ℓ and year t, we also use CADS to measure: (i) business creation, $b_{\ell t}$, defined as the number of new firms in the municipality; (ii) the business exit rate, $e_{\ell t}$, defined as the fraction of firms present in the municipality at the beginning of the year that exit during the year; and (iii) the initial average size of start-ups, $y_{0\ell t}$, measured as the average value of production in the first year after the entry year. To express percentage changes and handle zeros, the variables $\ln b_{\ell t}$, $\ln e_{\ell t}$, and $\ln y_{0\ell t}$ are constructed by scaling the level variables by their corresponding time-series averages within each municipality. In Section 6 we use i-iii to evaluate Italy's loss of per-capita GDP due to ambiguous laws.

Courts We obtained data on characteristics of the district courts and Court of Cassation judges for the period 2010–2015 through formal requests to the High Council of the Judiciary (*Consiglio Superiore della Magistratura*)—the Italian institution responsi-

ble for the administration of the judiciary—and to the National Association of Magistrates (*Associazione Nazionale Magistrati*)—the organization representing the interests of magistrates in Italy (see ANMA, 2023). For each district court j, we know the number of Court of Cassation magistrates originating from that court over 2010–2015. We also have information on: (i) planned and actual numbers of magistrates and administrative staff; (ii) the average age of magistrates; (iii) their average years of experience at the court and in the judiciary; and (iv) the share of pending cases unresolved for over three years, which we use as a proxy for average case duration. Information on the creation of specialized business courts is obtained from the Camera dei deputati report available at `https://leg16.camera.it/561?appro=598` and from a report by the Associazione Nazionale Magistrati (ANM) available at `https://www.associazionemagistrati.it/print/1631/il-tribunale-delle-imprese.htm`. The 2012 judicial reform introduced nine new business courts; an additional business court was established in 2014. Other existing business courts were operating since 2003. We construct ten dummies for the creation of a business court during our sample period: A dummy equal to 1 from 2012 onward for the nine business courts in Ancona, Cagliari, Campobasso, Catanzaro, L'Aquila, Perugia, Potenza, Trento, Brescia; another dummy equal to 1 from 2014 onward for Bolzano. To measure firm-court distance, we use information on the latitude and longitude of Italian municipalities downloaded from `https://zenodo.org/records/3824502`. We assign each firm and each court to the municipality where they are located. We then compute the geodesic distance between each firm and its court of competence in each year using the STATA command *geodist*.

The mapping of court jurisdiction over municipalities before and after the judicial reform is obtained through an official data request to the Italian Ministry of Justice (2022). We use this table as the official classification of the court consolidation reform that generated the uncertainty shock faced by firms during the period 2008-2017 considered in our analysis. The Italian Ministry of Justice (2022) treats the following 30 district courts as closed: Acqui Terme; Alba; Ariano Irpino; Avezzano; Bassano del Grappa; Camerino; Casale Monferrato; Chiavari; Crema; Lanciano; Lucera; Melfi; Mistretta; Modica; Mondovì; Montepulciano; Nicosia; Orvieto; Pinerolo; Rossano; Sala Consilina; Saluzzo; Sanremo; Sant'Angelo dei Lombardi; Sulmona; Tolmezzo; Tortona; Vasto; Vigevano; Voghera. The law implementing the judicial reform (Law No. 155 of 7 September 2012) initially provided for the closure of the court of Urbino. However, the Italian Constitutional Court (Judgment No. 237, 3-24 July 2013) immediately declared the suppression of the Court of Urbino unconstitutional.

Accordingly, following the classification of the Italian Ministry of Justice (2022), we treat Urbino as unaffected by the reform. Four courts—Avezzano, Lanciano, Sulmona, and Vasto—are classified by the Ministry as closed and absorbed by the courts of L'Aquila and Chieti, in accordance with Article 11(3) of Law No. 155 of 7 September 2012, which originally stipulated that their closure would take effect three years after the decree's entry into force. Only at the end of our sample period, through Law No. 45 of 7 April 2017, was this provision amended, and the closure of these four courts was further postponed as a consequence of the 2016 L'Aquila earthquake. We follow the classification adopted by the Ministry, since the uncertainty shock $\Delta\lambda$ affecting firms stems from the expected consequences of the reform announcement rather than from the implementation of the reform ex post. In this sense, $\Delta\lambda$ can be interpreted as an intention-to-treat shock: after the 2012 reform announcement, firms expected a change in the relevant judicial jurisdiction, and no firm at the time could plausibly have anticipated the 2016 earthquake, or that, even conditional on it, the earthquake could have delayed the planned court closures.

D Baseline measures of legal drafting quality: references to text manuals

Table A2 provides a detailed reference to the relevant legal drafting manuals that underpin the eight indicators for measuring the drafting quality of laws.

Table A2: Relevant excerpts from legal drafting manuals

Measure of draft quality	Details	Reference
Number of gerunds per 1,000 words	"Prefer explicit clauses over implicit ones, especially those using the **gerund**"	Alfieri et al. (2011), pag. 20
Average word/root/sentence lenght	"The readability of a text depends mainly on the structure of the text, on its sentences, and on the **length of the sentences and the words** that make them up [...]" "Many syllables in a word or many words in a sentence are likely to make a document more difficult for the brain to process it."	Vena (2022), page 2; Tonelli et al. (2012), page 474
Number of modal verbs per word	"Avoid using **modal verbs (must, can, want)**. In particular, given the imperative value of the present indicative, do not use the verb "must" to make the obligatoriness of a rule explicit." "It is possible to use the verb **can/may** to indicate an ability or possibility, but not an obligation in the presence of certain circumstances. Example: *The office issues the certificate if the requirements established by law are met...* and not *The office may issue the certificate if the requirements established by law are met.*"	Alfieri et al. (2011), pag. 23-24
Number of demonstrative adjective and personal pronoums per word	"Articles and paragraphs are autonomous units of the text and may be subject to subsequent amendments. Therefore, repeating terms from one paragraph to another or from one article to another is necessary to facilitate understanding of the text and ensure its compatibility with future amendments, and it prevents misunderstandings and ambiguities. Consequently, avoid using **personal pronouns and and demonstrative adjectives** to refer to terms or subjects previously used in the text." "It is preferable to repeat elements that have already been introduced, or that will be introduced again later, rather than using **personal pronouns** and **demonstrative pronouns and adjectives**, which can make the understanding of the text ambiguous."	Osservatorio Legislativo Interregionale (2007), pag. 28; Alfieri et al. (2011), pag. 25
Number of embedding and contingency clauses per word	Gibson (1998) shows that from a psycholinguistic perspective, **contingency clauses** can be analyzed as a form of **syntactic embedding**. Gibson (1998) shows that "increasing the number of nestings soon makes sentence structures unprocessable" and that sentences containing multiply embedded clauses may result in "a structure that is so complex that it is unprocessable for most people".	Gibson (1998), pag. 3

Notes: The table provides excerpts from legal drafting manuals that underpin the eight indicators of drafting quality discussed in Section 3.

E Construction of principal component

We discount the 10 indicators of law drafting clarity using $\delta = 0.09$ and summarize the discounted indicators with their first principal component. We set the number of gerunds per 1,000 words as a unit of measure, normalizing to one its factor loading. Formally we proceed as follows. Let p_s denote the indicator $s = 1, ...10$ for law drafting clarity, with $s = 1$ referring to the number of gerunds per 1,000 words. Let τ denote the age in months of the law mentioned by the Court of Cassation in its judgment at t. For each law cited in a judgment and separately for $s = 1, 2, ...10$, we calculate the discounted measure $(1-\delta)^{\tau/12} \times p_s$. Then, $\forall s$ we take the average of the discounted indicator s across all laws cited in the judgment by the Court and collect the resulting averages in the vector $\mathbf{m}$, whose entry in row s is denoted by m_s. To aggregate the 10 entries of $\mathbf{m}$ and obtain our measure of a we proceed in two steps. First, we calculate the main principal component of the standardized vector $\mathbf{z}$ whose entry in row s is equal to

$$z_s = \frac{m_s - \mu_s}{\sigma_s}$$

where $\mu_s \equiv E(m_s)$ and $\sigma_s = SD(m_s)$ denote the mean and the standard deviation of m_s across all judgments by the Court of Cassation over the period. The loading of the main principal component on the standardized variable z_s is denoted by w_s. Remember that the 10 principal components of $\mathbf{z}$ are equal to $W'\mathbf{z}$ where W is the matrix whose columns are the (orthonormal) eigen vectors of the variance covariance matrix of $\mathbf{z}$ equal to $E(\mathbf{z}\mathbf{z}') = \Sigma_z$, which satisfies $\Sigma_z = W\Lambda W'$ with $WW' = I$ and Λ equal to the diagonal matrix containing the eigen values of Σ_z (all positively valued), ordered in decreasing order. The main principal component of $\mathbf{z}$ reported by STATA is equal to

$$\pi^S = \sum_{s=1}^{10} w_s \cdot z_s = \sum_{s=1}^{10} w_s \cdot \frac{m_s - \mu_s}{\sigma_s}$$

where w_s denotes the entry in row s of the first column of W. Then, we measure the legal ambiguity of the laws cited in the judgment by the Court of Cassation as follows:

$$a(\mathbf{m}) = \frac{\sigma_1}{w_1} \cdot \sum_{s=1}^{10} \frac{w_s}{\sigma_s} \cdot m_s, \tag{18}$$

$a(\mathbf{m})$ is our measure of ambiguity of the law due to drafting clarity in the case heard by the Court of Cassation. It is calculated by normalizing to one the loading on the number

of gerunds per 1,000 words.

Column 1 of Table A3 shows the factor loadings of the first principal component for the standardized vector $\mathbf{z}$. Column 2 shows the loadings for our measure of legal ambiguity expressed in terms of number of gerunds per word $a(\mathbf{m})$—i.e. the loadings corresponding to (18). The loadings on the standardized variables w_s are all positive and are similar in magnitude. This implies that all indicators of drafting clarity $\mathbf{m}$ account roughly equally for the overall variance of $\mathbf{m}$.

Table A3: Factor loadings of main principal component

	Loadings on $\mathbf{z}$ (w_s)	Loadings on π $\left(\frac{\sigma_1}{w_1} \cdot \frac{w_s}{\sigma_s}\right)$
N. gerunds	.315	1
N. contingency clauses	.316	12.85
Word length	.361	0.378
Root length	.361	0.483
Phrase length	.339	0.009
N. modal verbs	.311	236.22
N. adjectives	.321	603.09
N. pronouns	.307	931.51
Fraction with preambles	.252	3.01
N. citations to other laws	.254	232.15

Notes: Column 1 reports the factor loadings on the standardized indicators for law drafting clarity, $\mathbf{z}$. Column 2 reports the loadings on $a(\mathbf{m})$ in (18), setting the number of gerunds per 1,000 words of a new law as the unit of measure.

The ratio between the main eigen value (corresponding to the main principal component) of the variance covariance matrix Σ_z of the standardized variables $\mathbf{z}$ and 10 (the dimension of the vector $\mathbf{z}$) measures the fraction of variance explained by the first principal component. Table A4 shows that this ratio is equal to 74 percent.

Table A4: Eigenvalues of main principal component

	Eigenvalue
Comp1	7.39
Comp2	.965
Comp3	.517
Comp4	.314
Comp5	.247
Comp6	.228
Comp7	.151
Comp8	.139
Comp9	.041
Comp10	.00009
Sum	10

Notes: The table shows the eigenvalues associated with the 10 different components of the standardized (discounted) indicators for quality of law drafting, **z**.

F Measuring law clarity using ChatGPT

Let $\{L_i\}_{i=1}^N$ denote all Italian laws obtained from Normattiva (2016). To construct a law-level index of (lack of) clarity, we apply a large language model (LLM) to each law L_i, excluding a small number of laws whose text length exceeds the context window supported by `gpt-4.1-mini` (hereafter GPT). We use `gpt-4.1-mini` because, relative to other state-of-the-art models, it accommodates longer texts at lower computational cost. In practice we drop the four codes—*Codice Civile*, *Codice Penale*, *Codice di Procedura Civile*, and *Codice di Procedura Penale*—as well as three very long laws, each exceeding five million characters in total, corresponding to: Decreto-Legislativo 13 December 2010, n. 212; Decreto del Presidente della Repubblica 13 December 2010, n. 248; and Decreto-Legge 22 December 2008, n. 200. These three long laws repealed many earlier provisions, often very old, and included a large number of annexes.

We do not ask GPT to report a scalar clarity score $\gamma_i \in \mathbb{R}$ for each law L_i, because this would be unattractive for at least three reasons. First, LLMs exhibit documented weaknesses in numerical reasoning (Levy, 2024). Second, the latent scale underlying γ_i would be opaque, since the units of measurement and the benchmark used to evaluate the laws would not be well defined. Third, the model would need to internalize and consistently apply a detailed, multi-dimensional scoring system in a single numeric response, with no guarantee of stability across prompts or over time. For these reasons, we, instead, treat GPT as a text classifier that reports for each law L_i a full *set* of qualitative features that impede clarity. Formally, let $\mathcal{T}$ be the set of possible laws L_i, and let $\mathcal{S}$ be a countable dictionary of features (strings) that prevent clarity. We define

$$\mathrm{Pre} : \mathcal{T} \to \mathcal{T}, \qquad \ell : \mathcal{T} \to \mathbb{N}, \qquad G : \mathcal{T} \times \mathcal{P} \to \mathcal{Y},$$

where Pre is a deterministic pre-processing operator on texts, ℓ returns token length, G is the GPT mapping with system prompt P in the second argument, $\mathcal{P}$ is the set of admissible prompts, and $\mathcal{Y}$ is the space of GPT outputs. We further define a parser

$$\Phi : \mathcal{Y} \to \mathcal{P}(\mathcal{S}), \qquad \Phi(y) = \text{set of feature strings extracted from } y,$$

and use $C_i := \big|\Phi\big(G(\mathrm{Pre}(L_i), P)\big)\big|$ as our clarity index.

In practice, $\mathrm{Pre}(\cdot)$ applies a sequence of deterministic text transformations to L_i, including removal of repeated special characters, normalization of whitespace, collapse of repeated dots, and trimming. The function $\ell(\cdot)$ counts tokens in the cleaned text, $G(\cdot, P)$ calls

GPT with temperature fixed at zero, and $\Phi(\cdot)$ is a robust JSON parser that extracts the array stored under the key `features_impeding_clarity` and returns its elements as a finite subset $S_i \subset \mathcal{S}$. Finally, the index C_i is the number of distinct qualitative clarity problems that GPT identifies in law i. The procedure can be summarized by the following pseudo-code:

Algorithm 1: Construction of GPT based clarity lists and cardinalities

Input: Corpus of laws $\{L_i\}_{i=1}^{N} \subset \mathcal{T}$;
system prompt P (Prompt F);
preprocessing operator $\mathrm{Pre} : \mathcal{T} \to \mathcal{T}$;
GPT mapping $G : \mathcal{T} \times \mathcal{P} \to \mathcal{Y}$ with temperature fixed at zero;
parser $\Phi : \mathcal{Y} \to \mathcal{P}(\mathcal{S})$;
token length functional $\ell : \mathcal{T} \to \mathbb{N}$;
context window $T_{\max} \in \mathbb{N}$; maximum retry count $K \in \mathbb{N}$.
Output: For each analyzable law i, a feature set $S_i \subset \mathcal{S}$ and its cardinality $C_i := |S_i|$.

for $i \leftarrow 1$ **to** N **do**
  `// Step 1: Deterministic preprocessing of the law text`
  $\widetilde{L}_i \leftarrow \mathrm{Pre}(L_i)$
  `// Step 2: Enforce the GPT context window constraint`
  if $\ell(\widetilde{L}_i) > T_{\max}$ **then**
    mark law i as *dropped due to length*
    continue
  success $\leftarrow 0$
  $k \leftarrow 0$
  `// Step 3: Query GPT up to` K `times with temperature zero`
  while *success* $= 0$ ***and*** $k < K$ **do**
    $k \leftarrow k + 1$
    $Y_{i,k} \leftarrow G\big(\widetilde{L}_i, P\big)$
    `// Step 4: Parse JSON output into a feature set`
    $S_{i,k} \leftarrow \Phi(Y_{i,k})$
    if $S_{i,k}$ *is a well defined finite subset of* $\mathcal{S}$ **then**
      success $\leftarrow 1$
      $S_i \leftarrow S_{i,k}$
  `// Step 5: Handle persistent failures`
  if *success* $= 0$ **then**
    mark law i as *dropped due to parsing or API failure*
    continue
  `// Step 6: Compute cardinality of distinct features`
  $C_i \leftarrow |S_i|$
return $\big\{(S_i, C_i) : i \in \{1, \ldots, N\}$ and S_i is defined$\big\}$

The prompt translated into English is reported in the main text. Here below the original

Prompt in Italian:

> Sei un GPT incaricato di valutare la chiarezza del testo delle leggi italiane, identificando tutte le caratteristiche che compromettono la chiarezza. Per ‘‘chiarezza’’ si intende la trasparenza linguistica e sintattica del testo, il grado di comprensibilitÃ del contenuto e l’assenza di ambiguitÃ interpretativa a livello lessicale, indipendentemente dalla materia trattata.
>
> Fornisci esclusivamente un elenco completo e dettagliato di tali caratteristiche in formato JSON. Non includere spiegazioni o testo aggiuntivo.
>
> Example of JSON output:

```
{"features_impeding_clarity": ["Caratteristica 1", "Caratteristica 2", "Caratteristica 3", ...]}
```

Prompt 1: Expected JSON output format, Italian

Figure A3: Word cloud of features that undermine the clarity of laws, in Italian



Notes: The plot presents a word cloud in Italian of the key features undermining the clarity of Italian laws, as identified by the GPT analysis.

G Court misalignment and Court of Cassation promotion

At the time of appointment to the Court of Cassation, a judge may be serving at any court. However, in Italy, only judges who have served for at least seven years at one of the 29

Courts of Appeal are eligible to apply. Applications are reviewed by the Superior Council of the Judiciary, which evaluates candidates based on their technical and professional skills, diligence, and service records. We examine whether judges from courts with lower λ_j are more likely to be promoted to the Court of Cassation. Such a pattern would suggest that the two judicial bodies share relevant legal traits. For each Court of Cassation judge in 2022, we know their previous lower court location before appointment. With this information, we run the following regression:

$$n_j = \psi\lambda_j + \gamma' Z_j + \epsilon_j \tag{19}$$

where n_j is a dummy variable equal to one if at least one judge from lower court j is sitting at the Court of Cassation. The coefficient ψ measures the effect of the intellectual alignment of court j on the probability of having a judge at the Court of Cassation. The set of controls Z_j includes the size of the lower court, the average tenure of judges in the court, whether the lower court is an Appeal Court, whether the lower court was absorbed by the 2012 reform, and (in a robustness checks) the lower-court j average reversal rate, as measured by the s_j dummies from regression (7).

We test whether the coefficient ψ in (19) is negative, which would indicate that the probability of observing a judge from court j sitting at the Court of Cassation is lower if court j has a greater λ_j coefficient. The first column of Table A5 below shows the baseline results. The second column adds, as a control, the lower-court average reversal rate, as captured by the court-j fixed effect s_j. The effect of the court-j average reversal rate s_j is not statistically significant, while the coefficient ψ is negative, statistically significant, and substantively important: reducing λ_j by one standard deviation (equal to 0.0199) results in an increase in the probability that the Court of Cassation includes a judge from the lower court by approximately 4.5 percentage points, equivalent to a 17 percent increase relative to the mean of the dependent variable.

Table A5: Judges at the Court of Cassation and their lower court of origin

	Dep. var.: Dummy for judge at the Supreme Court	
	(1)	(2)
Coefficient on legal ambiguity, λ_j	-2.209**	-2.414**
	(0.982)	(0.967)
Fixed effect in levels, s_j		-0.272
		(0.225)
N. observations	160	160
Adjusted R^2	0.284	0.282
Lower court controls	Yes	Yes

Notes: Results from estimating the regression in (19). The sample includes all judges sitting at the Court of Cassation in 2022. The dependent variable is a dummy equal to one if at least one judge from lower court j is sitting at the Court of Cassation. The regression in column 2 also includes the court-j average reversal rate, as captured by the fixed effect s_j from regression (7). Robust standard errors are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

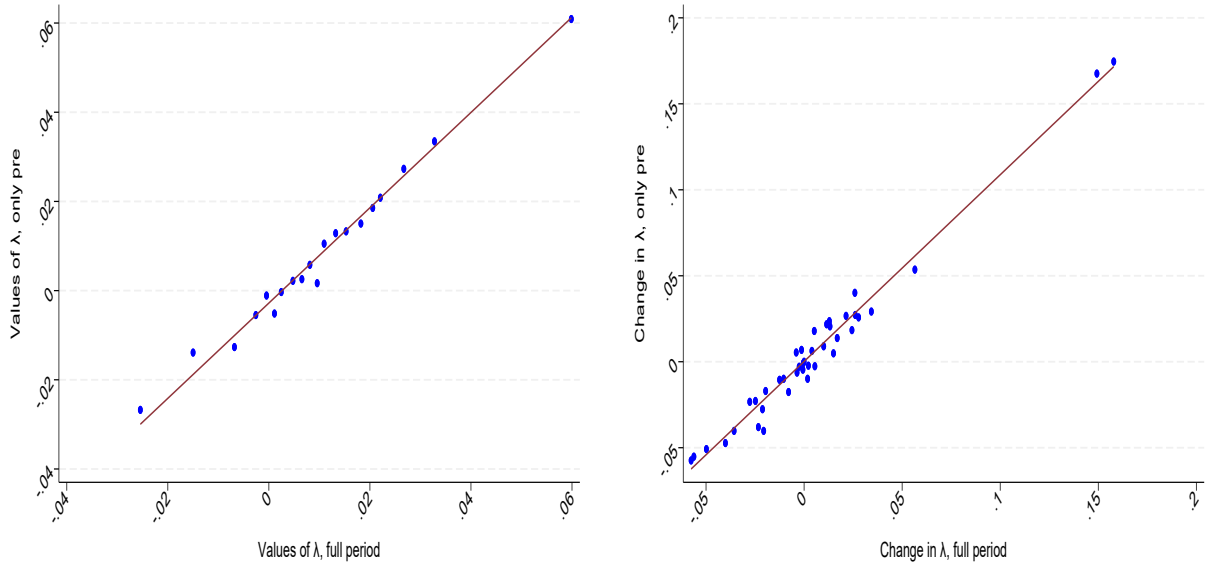
H Uncertainty shocks using only pre-2012 cases

Given the substantial lag between initial lower-court decisions and subsequent rulings by the Court of Cassation, the baseline reform-induced shocks to legal uncertainty in the paper, $\Delta\lambda$, are constructed using cases that originated almost exclusively prior to the 2012 reform. In the main paper, we use the full sample to maximize estimation efficiency. In this appendix, we show that our results remain unchanged when the legal uncertainty shocks $\Delta\lambda$ are constructed using only pre-2012 cases. In particular: (i) the estimated λ coefficients in levels are not significantly different between the pre-2012 (2004-2012) and the full period (2004-2017) periods; and (ii) the firm-level effects estimated using uncertainty shocks $\Delta\lambda$ based solely on pre-2012 cases are virtually unchanged.

We estimate the λ coefficients using only the pre-2012 sample of legal cases and compare these estimates with those obtained from the full sample period, as in the main text. To construct the pre-2012 sample, we use the ruling date of the decision by absorbing or unaffected lower courts as the relevant timing variable. Figure A4 compares the properties of the λ coefficients estimated from the full sample and the pre-2012 sample. The x-axis reports the full-sample estimates used in the main paper; the y-axis, the estimates from the pre-2012 sample. Panel (a) shows a scatter plot (with regression line) of the 160 λ

coefficients in levels. The coefficients are statistically similar between the full and the pre-2012 periods, with a correlation of 0.967. Panel (b) compares the uncertainty shocks $\Delta\lambda$ for treated municipalities across the two samples; the correlation is again very high, at 0.990.

Figure A4: Comparison of λ estimates: full sample vs. pre-2012 period



(a) Levels of the λ coefficients **(b) Uncertainty shocks, $\Delta\lambda$**

Notes: Panel (a) shows the relationship between the λ coefficients in levels estimated from the full sample (x-axis) and the pre-2012 period (correlation = 0.967). Panel (b) shows the relationship between the uncertainty shock $\Delta\lambda$ for treated municipalities, estimated from the full sample (x-axis) and the pre-2012 period (correlation = 0.990).

Table A6 reports estimates of the firm-level effects of the legal uncertainty shock, $\Delta\lambda$, constructed using only pre-2012 cases. These estimates are obtained from equation (9) including the baseline controls. The control group consists of firms located in municipalities unaffected by the court consolidation. Column 1 report results for firm growth, column 2 for investment and column 3 for precautionary provisions. Consistent with the baseline results, we find a negative and statistically significant coefficient of legal uncertainty, λ, on firm growth. Using only pre-2012 cases, the effects on firm growth are quantitatively similar to those reported in the main paper: the point estimate is 18 percentage points smaller, while the standard deviation of the uncertainty shock, $\Delta\lambda$, is about 10 percentage points larger (0.047 versus 0.043 under the baseline measure). Similar patterns emerge for investment, where point estimates are 15 percentage points smaller, and for precautionary provisions, where point estimates are 12 percentage points smaller, again with a standard

deviation of the uncertainty shock larger by 10 percentage points.

Table A6: Firm outcome effects using only pre-2012 cases period

Dep. var.:	Growth rate	Investments rate	Precautionary savings
	(1)	(2)	(3)
$\Delta\lambda$×Post	-0.219***	-0.258***	0.0106**
	(0.076)	(0.099)	(0.004)
N. observations	3,833,047	2,888,559	3,639,150
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Other FE	Yes	Yes	Yes

Notes: Results from estimating (9) using uncertainty shocks $\Delta\lambda$ constructed solely from the pre-2012 sample period. The standard deviation of $\Delta\lambda$ across treated municipalities is 0.047. The dependent variable is the firm growth rate in column 1, the investment rate in column 2, and precautionary savings in column 3. The control group includes all firms in municipalities unaffected by the court consolidation. The controls are the same as in panel B of Table 5. Robust standard errors clustered at the firm level are reported in round brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

I Bootstrapping algorithm

We conduct bootstrapping for equation (OE) on firm-level outcomes in four steps: (i) we stratify the draws at the court district level, and for each draw, we estimate equation (UE) on Court of Cassation judgments; (ii) we repeat this procedure 1,000 times and collect the distribution of estimates of λ_j and s_j for each lower court j; (iii) given the distribution of λ_j and s_j for each court j, we bootstrap the regression (OE) by randomly selecting, for each court district j, a value of λ_j and s_j from the distribution generated in step (ii) and then draw the sample of firms stratifying the sampling at the court district level and clustering at the firm level; and (iv) we repeat step (iii) 1,000 times and compute the standard deviation of the estimates across all draws. We now discuss the procedure in more detail, starting with the (UE) regression and then turning to the (OE) regression.

I.1 UE regression

The dataset of Court of Cassation rulings is denoted by $\mathcal{D} = \{(r_{ijt}, \pi(m_{ijt}, \delta), X_{ijt}) : i \in \mathcal{I}, j \in \mathcal{J}, t \in \mathcal{T}\}$ where $\mathcal{I}$ is the set of Court of Cassation cases, $\mathcal{J}$ is the set of lower courts, and $\mathcal{T}$ is the set of dates. We start with the EU regression as follows:

Algorithm 2: EU regression

Require: Dataset $\mathcal{D} = \{(r_{ijt}, a_{ijt}, X_{ijt}) : i \in \mathcal{I}, j \in \mathcal{J}, t \in \mathcal{T}\}$

Ensure: We collect regression coefficients $\{\lambda_j^{(b)}\}_{j\in\mathcal{J},b=1}^{1000}$ and $\{s_j^{(b)}\}_{j\in\mathcal{J},b=1}^{1000}$

We initialize the sets for collecting coefficients: $\Lambda = \emptyset$ and $S = \emptyset$ **for** $b = 1$ *to* 1000 **do**

$\mathcal{D}^{(b)} = \emptyset$ (we initialize sample dataset) **for** *each stratum* $j \in \mathcal{J}$ **do**

$\mathcal{D}_j = \{(r_{ijt}, a_{ijt}, X_{ijt}) \in \mathcal{D} : j \text{ is fixed}\}$ (Data for stratum j)

We draw a random sample $\mathcal{S}_j$ from $\mathcal{D}_j$ with replacement

$\mathcal{D}^{(b)} = \mathcal{D}^{(b)} \cup \mathcal{S}_j$ (Add stratum sample to current dataset)

We run UE-regression model: $r_{ijt} = \lambda_j a_{ijt} + s_j + \gamma' X_{ijt} + \xi_{ijt}$ using $\mathcal{D}^{(b)}$ **for** *each stratum* $j \in \mathcal{J}$ **do**

$\lambda_j^{(b)}$ = estimated coefficient of a_{ijt} for stratum j

$s_j^{(b)}$ = estimated fixed effect coefficient for stratum j

$\Lambda = \Lambda \cup \{(j, \lambda_j^{(b)})\}$ (Collect λ_j coefficient for stratum j)

$S = S \cup \{(j, s_j^{(b)})\}$ (Collect s_j coefficient for stratum j)

We obtain empirical distribution $\hat{F}_j$ from $\{(\lambda_j^{(b)}, s_j^{(b)})\}_{b=1}^{1000}$ for each $j \in \mathcal{J}$

return $\{\hat{F}_j\}_{j\in\mathcal{J}}$

I.2 OE regression

The firm level dataset is denoted by $\mathcal{D}_{\text{OE}} = \{(Y_{nlt}, X_{nlt}) : n \in \mathcal{N}, l \in \mathcal{L}, t \in \mathcal{T}\}$ with firm indices $n \in \mathcal{N}$, location indices $l \in \mathcal{L}$, and time indices $t \in \mathcal{T}$. We now proceed to the (OE) regression:

Algorithm 3: OE Regression

Require: Empirical distributions $\{\hat{F}_j\}_{j \in \mathcal{J}}$ from Algorithm 1 and firm level dataset $\mathcal{D}_{\text{OE}}$.
Require: Court jurisdiction mapping $J(n)$ indicating the pre-reform court jurisdiction for each firm $n \in \mathcal{N}$
Ensure: We collect OE regression estimates in $\{\beta^{(k)}\}_{k=1}^{1000}$

We initialize the vector collecting results $\mathcal{B} = \emptyset$ **for** $k = 1$ *to* 1000 **do**

  $\mathcal{D}^{(k)} = \emptyset$ (Initialize sample dataset)

$\Theta^{(k)} = \emptyset$ (Initialize coefficient collection)

  for *each court* $j \in \mathcal{J}$ **do**

    We randomly sample $(\lambda_j^{(k)}, s_j^{(k)})$ from empirical distribution $\hat{F}_j$

$\Theta^{(k)} = \Theta^{(k)} \cup \{(\lambda_j^{(k)}, s_j^{(k)})\}$

We partition firms based on their pre-reform court jurisdiction: $\mathcal{N}_j = \{n \in \mathcal{N} : J(n) = j\}$ for each $j \in \mathcal{J}$

  for *each court jurisdiction* $j \in \mathcal{J}$ **do**

    We draw a random sample $\mathcal{S}_j$ of firms from $\mathcal{N}_j$ with replacement

    for *each sampled firm* $n \in \mathcal{S}_j$ **do**

      $\mathcal{D}_n = \{(Y_{nlt}, X_{nlt}) \in \mathcal{D}_{\text{OE}} : n \text{ is fixed}\}$ (We take observations for firm n for all time periods)

$\mathcal{D}^{(k)} = \mathcal{D}^{(k)} \cup \mathcal{D}_n$ (Add all firm observations to sample)

We run OE regression using $\mathcal{D}^{(k)}$ and coefficients $\Theta^{(k)}$, clustering by firms $n \in \mathcal{N}$

$\beta^{(k)}$ = estimated OE regression coefficients

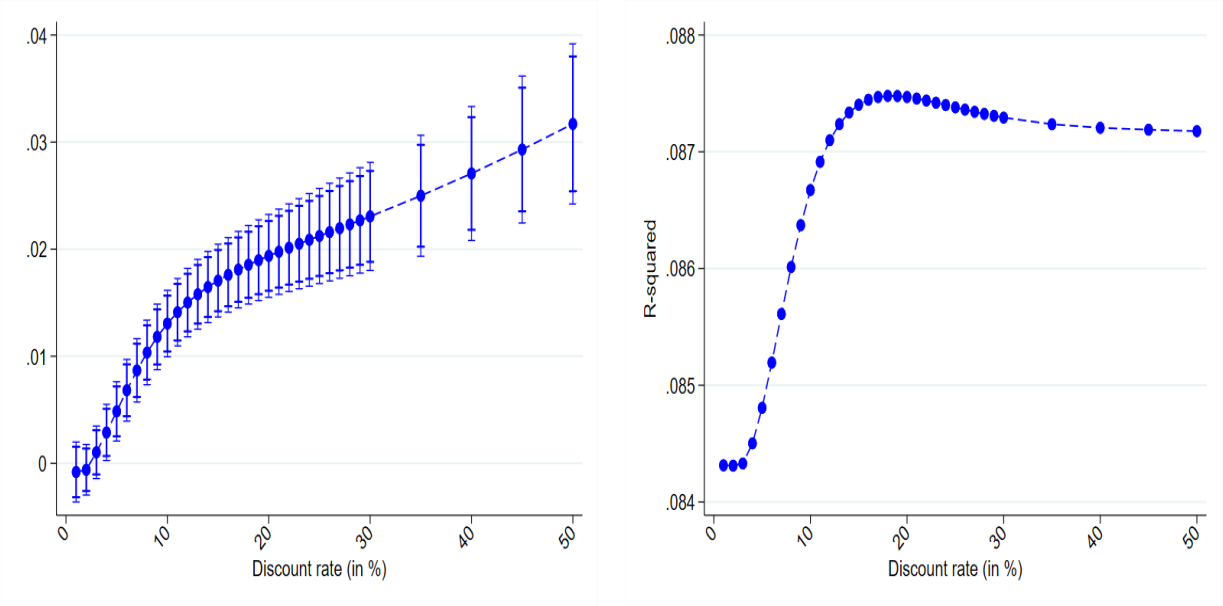
$\mathcal{B} = \mathcal{B} \cup \{\beta^{(k)}\}$ (We collect OE regression coefficients)

return $\mathcal{B}$ and calculate summary statistics from $\{\beta^{(k)}\}_{k=1}^{1000}$

J Additional figures and tables

Alternative estimation of the depreciation rate δ As an alternative method for estimating the rate δ at which legal ambiguity is resolved over time, we estimated δ nonlinearly using the Uncertainty Equation in (6), recognizing that legal ambiguity a_{ijt} is constructed as in Appendix E and is therefore a function of δ. We focused on the baseline drafting clarity measure. Panel (a) of Figure A5 shows how the estimated λ coefficient in (6) varies with δ, while Panel (b) shows how the R^2 of the regression (6) changes with δ. The R^2 is maximized at around 11 percent, close to the baseline estimate used in the main text.

Figure A5: Effects of depreciation rate δ on ambiguity coefficient λ and R^2



(a) Effect on ambiguity coefficient λ **(b) Effect on R^2**

Notes: Panel (a) reports how the ambiguity coefficient λ changes with depreciation rates δ between 0% and 50%. Panel (b) reports the corresponding R^2 values.

Drafting quality of financial and tax laws We consider the sample of all laws cited at least once by the Civil Sections of the Court of Cassation over the period 2004-2017. We construct two dummy variables: one for laws cited only by "Section 3," which specializes in Financial Cases; and one for laws cited only by "Section 5," which specializes in Tax Cases. Laws cited only by Section 3 and Section 5 represent 5.3 and 11.2 percent, respectively, of all laws ever cited by the Court of Cassation. Table A7 reports estimates from a regression using the sample of laws ever cited by the Court of Cassation. The dependent variable is the log of the baseline drafting clarity measure at enactment (without discounting) in column 1 and the GPT measure in column 2. The key regressors are the two indicator variables equal to one if a law was cited only by Section 3 or only by Section 5. All specifications include a full set of year-of-enactment fixed effects to control for the trend in drafting quality documented in Section 6. Using the baseline measure, we find that, relative to the average law cited by the Court of Cassation, laws cited only by Section 3 and Section 5 are better drafted by 1.5% and 2.5%, respectively. The differences are statistically significant but small relative to the standard deviation of the drafting clarity measure (in logs) in the sample, which equals 0.125. The analysis based on the GPT measure confirms that laws specialized in finance and tax matters are not, on average, more poorly drafted according to our drafting quality indicators.

Table A7: Legal ambiguity in financial and fiscal trials

Dep. var.:	Drafting quality (Log)	
	Baseline measure	GPT measure
	(1)	(2)
Section 3, Financial cases	-0.015***	-0.049***
	(0.004)	(0.010)
Section 5, Tax cases	-0.025***	-0.066***
	(0.003)	(0.007)
N. observations	12,612	12,610
Adjusted R^2	0.042	0.135
Year of approval FE	Yes	Yes

Notes: The dependent variable is the log of the baseline drafting clarity measure in column 1 and the GPT measure in column 2. The unit of observation is a law. The sample includes all laws ever cited by the Civil Sections of the Court of Cassation over 2004-2017. "Section 3, Financial cases" and "Section 5, Tax cases" are indicator variables equal to one if the law was cited only by Section 3 or Section 5, respectively. Laws cited only by Section 3 and Section 5 represent 5.3% and 11.2%, respectively, of all laws cited by the Court of Cassation. All specifications include year-of-approval fixed effects. The standard deviation of the baseline drafting clarity measure (in logs) is 0.125. Robust standard errors are reported in square brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

IV estimates of the reversal equation Table A8 reports the IV estimates of the reversal equation corresponding to column 1 or column 5 of Table 3, where we instrument legal ambiguity, a, with an indicator for whether the Court of Cassation judgment cites at least one decree. In columns 1 and 2 legal ambiguity is constructed using the baseline measure of drafting clarity; in columns 3 and 4 using the GPT measure. The regressions include the same set of controls detailed in the legend of Table 3. Columns 1 and 3 report the coefficient on the decree indicator in the first-stage regression. Column 2 and 4 reports the coefficient on legal ambiguity in the second-stage regression, where the dependent variable is the reversal probability.

The IV estimates indicate that legal ambiguity has a positive and statistically significant effect on the reversal probability when instrumented with the decree indicator. The first-stage regression shows that decrees are significantly more poorly drafted than other laws cited by the Court of Cassation. The F-statistic is large, indicating that the decree indicator is a strong instrument. These results suggest that the variation in drafting quality that drives the effect of legal ambiguity on the reversal probability reflects the poor drafting of laws enacted during periods of political emergency or instability, when legislators act quickly under limited monitoring.

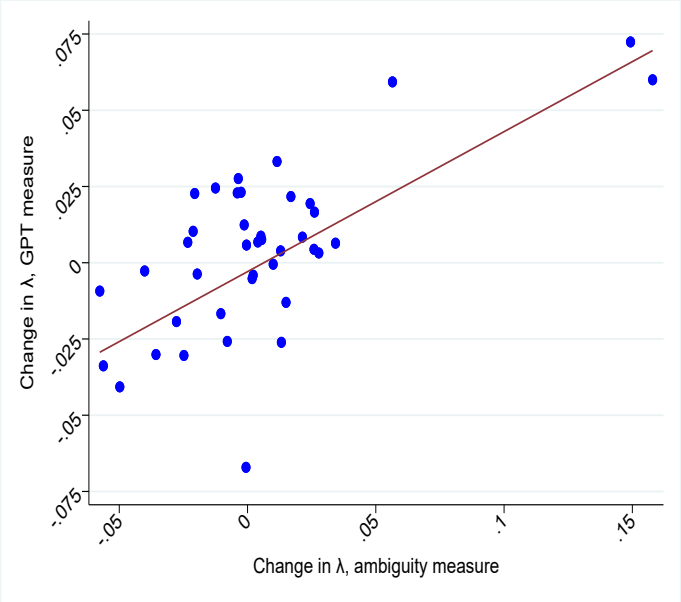
Table A8: Uncertainty equation: IV using decrees

	Baseline ambiguity measure		GPT measure	
	First stage	Second stage	First stage	Second stage
Dep. var.:	Drafting quality	Reversal prob.	Drafting quality	Reversal prob.
	(1)	(2)	(3)	(4)
Decree dummy	1.140***		0.866***	
	(0.137)		(0.210)	
Legal ambiguity		0.047***		
		(0.008)		
Legal ambiguity, GPT measure				0.042***
				(0.013)
F-stat	69.11		17.06	
Adjusted R^2	0.491	0.002	0.480	-0.030
N. observations	456,770	456,770	273,734	273,734

Notes: IV estimates corresponding to columns 1 and 5 of Table 3. Columns 1 and 2 use the baseline measure of drafting quality; columns 3 and 4 use the GPT measure. All regressions include the same set of controls described in the legend of Table 3. Columns 1 and 3 report the coefficient on the decree indicator in the first-stage regression, where the measure of legal ambiguity, a, is regressed on an indicator equal to one if the Court of Cassation judgment cites at least one decree. Columns 2 and 4 report the coefficient on legal ambiguity in the second-stage regression, where the dependent variable is the reversal probability. Robust standard errors clustered at the president judge level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Relation between uncertainty shock $\Delta\lambda$ calculated with baseline and GPT measure Figure A6 shows a bin scatter plot of the changes in λ ($\Delta\lambda$) estimated using the baseline drafting quality measure and the GPT drafting clarity measures in absorbed municipalities. The correlation between the two measures is 67%.

Figure A6: Correlation of $\Delta\lambda$ estimated with baseline and GPT drafting clarity measures in absorbed municipalities



Notes: Correlation: 0.674

Unpacking the court level coefficients Table A9 presents the results obtained when the controls included jointly in columns 1, 5, and 9 of Table 7 are instead introduced separately. In Table A9, the dependent variable is firm growth (columns 1-4), investment (columns 5-8), and precautionary provisions (columns 9-12). Within each set of four columns, the first column estimates equation (9) adding to the baseline controls, used in panel B of Table 5, only district court size, average judicial tenure, staff shortages, and trial length; the second column adds to the baseline controls only the court-j average reversal rate corresponding to the fixed effect s_j from regression (7); the third column adds only a dummy for the presence of a specialized business court in the jurisdiction; and the fourth column adds only the time-varying distance between the firm and the court.

Table A9: Effect of legal uncertainty, separate court controls

Dep. var.:	Growth rate				Investment rate				Precautionary savings			
	Basic court controls (1)	Controlling for Δs coefficient (2)	Business court control (3)	With distance to court (4)	Basic court controls (5)	Controlling for Δs coefficient (6)	Business court control (7)	With distance to court (8)	Basic court controls (9)	Controlling for Δs coefficient (10)	Business court control (11)	With distance to court (12))
$\Delta\lambda\times$ Post	-0.252***	-0.277***	-0.279***	-0.272***	-0.287***	-0.313***	-0.294***	-0.303***	0.011**	0.010**	0.013**	0.012**
	(0.085)	(0.085)	(0.086)	(0.085)	(0.110)	(0.111)	(0.111)	(0.110)	(0.005)	(0.005)	(0.005)	(0.005)
N. obs.	3,833,047	3,833,047	3,833,047	3,737,118	2,888,559	2,888,559	2,888,559	2,814,415	3,639,150	3,639,150	3,639,150	3,547,844
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents results from estimating equation (9). The dependent variable is the firm growth rate in columns 1-4, the investment rate in columns 5-8, and the precautionary savings rate in columns 9-12. The uncertainty shock $\Delta\lambda$ is calculated using the baseline measure of drafting clarity. All specifications include the control variables detailed in Panel B of Table 5, as detailed in the corresponding legend. Columns 1, 5, and 9 add to the baseline controls the following set of time-varying court-level variables: district court size, average judicial tenure, staff shortages, and trial length. Columns 2, 6, and 10 add to the baseline controls the the court-j average reversal rate, as measured by the fixed effect s_j from regression (7). Columns 3, 7, and 11 add to the baseline controls a dummy for the presence of a specialized business court in the year. Columns 4, 8, and 12 add to the baseline controls the time-varying distance between the firm and the court. The control group consists of all firms in municipalities unaffected by the court consolidation. Robust standard errors clustered at the firm level are reported in round brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Court level coefficients in Table 7 Table A10 presents the court level coefficients corresponding to column 1, 5, and 9 of the regression reported in Table 7 of the main text. The dependent variable is the firm growth rate in column 1, the investment rate in column 2, and the precautionary savings rate in column 3. The uncertainty shock $\Delta\lambda$ is calculated using the baseline measure of drafting clarity. All specifications include the baseline control variables used in panel B of Table 5, plus the set of time varying court-level variables whose coefficients are reported in the table: district court size, average judicial tenure, staff shortages, trial length, the court average reversal rate corresponding to the fixed effect s_j from regression (7), a dummy for the presence of a business court in the year, and the firm distance to the court. The control group consists of all firms in municipalities unaffected by the court consolidation.

Table A10: Effect of legal uncertainty shock, court level coefficients

Dep. var.:	Growth rate	Investment rate	Precautionary savings
	(1)	(2)	(3)
Court size $\times 10^{-3}$	-0.05	-0.08*	0.001
	(0.03)	(0.05)	(0.002)
Court average judge tenure $\times 10^{-3}$	0.9***	-0.1	0.02*
	(0.1)	(0.2)	(0.01)
Court staff shortage $\times 10^{-3}$	0.2	0.8	-0.01
	(0.3)	(0.5)	(0.02)
Court trial length $\times 10^{-3}$	0.06***	0.02***	0.0002
	(0.008)	(0.01)	(0.0005)
Court average reversal rate, s_j	-0.030	-0.038	-0.0053***
	(0.025)	(0.031)	(0.0016)
Business court dummy	0.003	-0.006*	-0.00006
	(0.002)	(0.003)	(0.0002)
Distance to reference court $\times 10^{-3}$	-0.4***	-0.1	-0.01***
	(0.08)	(0.1)	(0.005)
N. observations	3,737,118	2,814,415	3,547,844
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Other FE	Yes	Yes	Yes

Notes: The table presents the court level coefficients corresponding to column 1, 5, and 9 of Table 7. The dependent variable is the firm growth rate in column 1, the investment rate in column 2, and the precautionary savings rate in column 3. The uncertainty shock $\Delta\lambda$ is calculated using the baseline measure of drafting clarity. All specifications include the baseline control variables used in panel B of Table 5, plus the set of time varying court-level variables whose coefficients are reported in the table. The control group consists of all firms in municipalities unaffected by the court consolidation. Robust standard errors clustered at the firm level are reported in round brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.