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Abstract

We develop a strategy to measure the economic costs of poorly written laws, a potential threat to the rule of law. Using the full corpus of Italian legislation, we show that legal uncertainty—measured by the probability of disagreement between the Supreme Court of Cassation and lower courts—is higher for cases involving poorly written laws and varies systematically across courts. To identify the economic impacts, we exploit a reform that reassigned firms to courts. We estimate that GDP would be 5 percent higher if laws had been written as clearly as the Constitution, with two-thirds of the loss accruing over the past 20 years.

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1 Introduction

The rule of law is at the foundation of well-functioning institutions (Hayek, 1960). It requires that laws "must be 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., Guiso and Parigi, 1999; Bloom, Bond, and Van Reenen, 2007; 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.

We start by measuring the drafting quality of laws and then assess its impact on legal uncertainty. We apply text analysis to the entire corpus of Italian laws—more than 75,000 statutes comprising roughly 97 million words—from Normattiva (2016). We measure the drafting quality of a law using a set of stylistic indicators drawn from law drafting manuals (Cassese, 1993; Butt and Castle, 2006), such as sentence length and structure, word clarity, and the frequency of references to other laws, which affects whether the law is self-contained. There is evidence that many Italian laws are poorly written and lack clarity. For instance, 85% of sentences in Italian legislation exceed the 25-word maximum that linguists recommend to ensure clarity (see Cortellazzo and Pellegrino (2003), Fioritto (1997), Alfieri et al. (2011) and Cassese (1993)).[3] Because legal ambiguity may dissipate over time, we discount each indicator by the age of the law and aggregate all indicators into a single measure using principal component analysis.[4]

An ambiguous law can lead two judges to rule differently on the same case, even when

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

[4] As discussed in Gratton et al. (2021), drafting quality is largely orthogonal to the substantive content of laws because legislative activity responds to the salient topics of the time, whereas drafting quality reflects the prevailing level of political instability—two largely uncorrelated factors.

presented with identical information. Greater statutory ambiguity increases the likelihood of judicial disagreement and makes the legal consequences of individuals' actions less predictable. In short, it increases *legal uncertainty*. To measure the effect of drafting quality 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, Supreme Court), which serves as the court of last resort. The Supreme Court has a *nomophylactic* mandate, meaning it ensures the uniform application and interpretation of legal norms across the country over time (Art. 65, RD January 30, 1941, n. 12). Appeals to the Supreme Court can be made only on the grounds of an alleged error in legal interpretation, and no additional evidence can be introduced. Only differing interpretations of the law can lead to disagreement between the Supreme Court and a lower court, which makes it close to an ideal experiment where two judges are asked to interpret the same law in the same case based on identical evidence.[5]

We process all written judgments issued by the Supreme Court between 2004 and 2017, totaling approximately 620,000 judgments. We assign the previously constructed drafting quality measure to the laws cited by the Supreme Court. Then, we estimate how the drafting quality of these laws affects the probability that the lower court and the Supreme Court disagree on a case, which we term the *reversal probability*. We control for a comprehensive set of case-specific characteristics, including the identities of the Supreme Court and lower court judges, as well as the case's topic and complexity, which may affect the level of judicial discretion (D'Amato, 1983; Katz, 2010). We find evidence that poor drafting, by creating statutory ambiguity, increases the reversal probability. This effect is more pronounced for newer laws, suggesting that statutory ambiguity decreases over time as the Supreme Court fulfills its nomophylactic mandate.[6]

We extend the reversal probability model by interacting the measure of law-drafting quality with a full set of dummy variables for the lower court whose case is appealed to the Supreme Court. The interaction coefficients reveal systematic differences between lower courts. We take these court-specific coefficients as a measure of the legal uncertainty stemming from statutory ambiguity that firms face under each court's jurisdiction. Differences across lower courts may reflect variation in intellectual alignment with the Supreme Court, as well as

[5]Another important institutional feature brings the design closer to the ideal experiment: Unlike in the US, France, or Germany, appealing to the Supreme Court in Italy is relatively common and inexpensive; approximately 32% of cases decided by a lower court are subsequently reviewed by the Supreme Court.

[6]This evidence aligns with recent experimental findings indicating that the difficulty of understanding legal documents stems from poor writing (Martinez et al., 2022) and that even legal experts prefer documents written in clear, plain language (Martinez et al., 2023).

enduring legal traditions and doctrines that persist despite judge turnover.[7] Consistent with this hypothesis, we find that judges from lower courts who disagree less frequently with the Supreme Court are significantly more likely to be promoted to it, suggesting the presence of shared legal traits between the two judicial bodies.

We study the universe of Italian limited liability companies, assigning to each firm and each period the corresponding measure of legal uncertainty, based on the jurisdiction of its headquarters at the time. To identify causal effects, we exploit a judicial reform that reassigned firms to a different court's jurisdiction. In 2012, as discussed in Pezone (2022), 31 of the 165 district courts were suppressed. Firms in those districts were placed under the authority of other courts, resulting in a change in their legal uncertainty. We exploit this variation to estimate the causal 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 average-firm investment rate (investment-to-capital ratio) and its production growth rate. All else equal, in the five years following the reform, a one standard deviation increase in legal uncertainty reduces the annual firm's growth rate by 1.2 percentage points (26% of the sample median), while the investment rate declines by 1.3 percentage points (equivalent to 12% of the sample median). We also find evidence that firms respond to increased legal uncertainty by accumulating precautionary provisions, consistent with the hypothesis that formal insurance markets either do not cover or fail to fairly price risks arising from ambiguous laws (Dietz and Niehorster, 2019, p. 4).

We perform several robustness checks. To ensure that the legal uncertainty shock is not confounded by other court-related factors discussed in Pezone (2022), we control for changes in key court characteristics, including court size, magistrate experience, trial length, and the average reversal probability of all appeals from the court. We also confirm that our estimates are robust to variations in the control group used for identification, and to restricting the sample to the set of firms which are active throughout the entire period in analysis. Finally, we apply alternative estimators for continuous treatment effects and address concerns that changes in legal uncertainty may not be random, as legal uncertainty may exhibit mean reversion, as noted by de Chaisemartin and Lei (2024).

Long-run steady-state GDP per capita depends on the average firm growth rate as well as on the business creation rate, the firm exit rate, and firm size at entry. We recover the elasticity of GDP per capita with respect to legal drafting quality by estimating its separate

[7] Differences in the legal culture of local courts are also present in the US; see Church (1985) and Ostrom et al. (2007) for a classification of local court culture types.

effects on each of these margins. We use this elasticity to evaluate the economic costs of poor law drafting. We estimate that if all Italian laws were drafted with the same clarity as the fundamental principles of the Italian Constitution, current GDP per capita would be 4.9 percent higher. Around two-thirds of the resulting losses have materialized over the past two decades, due to a marked deterioration in the drafting quality of 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. Improving the clarity of legal texts is arguably a relatively cost-effective way to strengthen the rule of law and, in turn, foster long-run economic growth.

The effects of uncertainty are both theoretically well understood (see Dixit and Pindyck, 1994) and empirically documented. Most of the literature has focused on firm investment responses to economic uncertainty arising from unpredictable fluctuations in demand or 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). To our knowledge, we are the first to provide a measure of legal uncertainty caused by the quality of legal drafting and to study its effects.

The legal uncertainty we identify arises from disagreement and heterogeneity in judges' subjective interpretations of laws, aligning closely with Knights (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 such interpretative ambiguity affects judicial predictability and, consequently, economic performance.

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 quality. 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 institutional environment. Then we discuss identification and the econometric specification.

2.1 Institutional background

The Italian judicial system is composed of district courts, courts of appeal, the Supreme Court of Cassation, and other specialized courts.[8] Before the 2012 reform, there were 165 district courts; this number was reduced to 136 afterward. Each district court has jurisdiction over multiple municipalities, defining its legal district. There are 29 Courts of Appeal, each with jurisdiction over several district courts. This number has remained unchanged during the sample period. Every city that hosts a Court of Appeal also hosts a district court. Rulings from district courts can be appealed directly to the Supreme Court in two cases: 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 district court rules on an appeal from a Justice of the Peace. As a result, appeals to the Supreme Court from cities hosting a

[8] Other courts include Justices of the Peace, which handle minor civil, administrative, and criminal cases; Juvenile Courts, which deal with cases involving individuals under 18; Courts of Freedom, which review precautionary measures; Supervisory Court which oversees the execution of criminal sentences; Review Court which reviews precautionary measures like pre-trial detention and Judges for Preliminary Investigations, who gather evidence to decide if criminal proceedings should begin.

district court can originate from that district court, the Court of Appeal (if present), or other local courts.

The role of the Supreme Court is to ensure consistent interpretation of laws across the country over time—the already mentioned nomophylactic mandate. This means that, in each appeal, the Supreme Court (i) makes its decision based on the same evidence and information considered by the lower court; (ii) reviews only whether the lower court followed proper legal procedures and correctly interpreted the law. The Supreme Court can either uphold or reverse the lower court's decision. If it 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. A reversal typically indicates that the Supreme Court and the lower court interpreted the relevant laws differently in the case at hand.[9]

The Supreme Court is divided into 13 specialized sections: 6 handle civil cases and 7 handle criminal cases. Important or complex cases are heard jointly by all civil or criminal sections—the so-called rulings by the joint sections (*sezioni unite*). Table 1 shows the share of cases each section handled and their respective reversal rates over the period 2004-2017.

2.2 Identification framework

We identify the effects of law drafting quality 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-output equation*, which relates firm growth, investment, and other business decisions to legal uncertainty. Causal identification comes from the 2012 judicial reform, which introduced 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 it differently. As the Supreme Court's preferred interpretation becomes clearer over time,

[9]The mandate of the Supreme Court in Italy is narrower than in the US, reflecting differences between civil law and common law systems. In Italy, any party can appeal to the Supreme Court by claiming that lower courts misinterpreted the law. In the US, where each case sets a binding example for future cases (the so-called *stare decisis* principle), the Supreme Court can choose which cases to hear. Consequently, the Italian Supreme Court handles a much larger caseload and has many more judges. In 2018, the Italian Supreme Court heard more than 35,000 cases—about one-third of all lower court cases—handled by over 100 judges. By contrast, the US Supreme Court heard only 70 cases, handled by just 9 judges.

Table 1: Sections of the Italian Supreme Court, 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 Supreme Court. For a detailed description of the competences of each section of the Italian Supreme Court, see `https://www.cortedicassazione.it/resources/cms/documents/Tabella_di_organizzazione_1.pdf`. The sample period is 2004-2017 .

legal ambiguity may gradually vanish.[10] Accordingly, we assume that the ambiguity of a law with drafting quality p and age τ is:

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

where δ is the rate at which legal ambiguity is dispelled over time, estimated below to be 9 percent per year ($\delta = 0.09$).

Let r_{ijt} be an indicator variable taking value 1 if case i, initially judged by lower court j, is reversed by the Supreme Court 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 Supreme Court might disagree on case i, even if the evidence is the same. First, it

[10]Unlike in common law countries, Supreme Court judgments in Italy are binding only for the specific case at hand. However, the Supreme Court clarifies its preferred interpretation of laws through its written rulings, gradually reducing statutory ambiguity over time.

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, represented by ξ_{ijt}. Then 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 court j's *misalignment* with the Supreme Court: a higher λ_j indicates that lower court j is more likely to interpret laws with ambiguity a_{ij} differently from the Supreme Court. s_j is a court j fixed effect, reflecting the composition of the cases handled by court j as well as other court characteristics. c_i represents proxies for the fundamental complexity of case i, including the case topic, the length of the Supreme Court's judgment, and other fixed effects discussed below. Finally, ξ_{ijt} is a random shock arising because the lower court and the Supreme Court 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

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

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 D. Firms in districts where the local court more frequently misinterprets ambiguous laws face greater legal uncertainty, u_j. A higher u_j means that the legal consequences of firm decisions are more unpredictable and that the court is more likely to misjudge firm behavior, resulting in financial and reputational costs.

As legal uncertainty u_j increases, firms may respond by reducing capital investment, R&D, and any business activity that relies on legal protection, ultimately hindering firm growth, which we take as a summary indicator of the overall effects of legal uncertainty on firm performance. Let $Y_{n\ell t}$ denote a variable of interest for firm n, in location ℓ, at time t. $Y_{n\ell t}$ could refer to firm growth, investment, precautionary savings, etc. 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 (exogenous) changes in the legal uncertainty caused by ambiguous laws $u_{j(\ell,t)}$ as well as to 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 $Y_{n\ell t}$:

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

$\bar{Y}_n$ measures the growth/investment opportunities for firm n. $X_{n\ell t}$ are firm specific controls and a full set of time effects. β_Y measures the effect of the legal uncertainty due to ambiguous laws on firm outcome Y. ϕ_Y measures the effect of other sources of legal uncertainty faced by firm n, as measured by $E_n\left(r_{ij(\ell,t)t}\right) - u_{j(\ell,t)}$. After using (UE), we obtain the following equation for firm outcome Y:

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

where $d_n \equiv \bar{Y}_n + \phi E_n\left(c_i\right)$ is a fixed effect for firm n that controls for firm opportunities $\bar{Y}_n$ as well as for the legal complexity of all cases faced by the firm, as measured by $E_n\left(c_i\right)$. We are interested in estimating the coefficient β_Y, which measures the effect of legal ambiguity due to poor drafting on firm outcome Y.

2.3 Econometric framework

To separately identify the contribution of legal uncertainty due to ambiguous laws, $\beta_Y u_{jt}$ in (OE), and the firm fixed effect d_n, we need some (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; that is, for some ℓ, $j(\ell,t) \neq j(\ell,t')$ for $t < t_0$ and $t' \geq t_0$—where $j(\ell,t)$ identifies the court with jurisdiction over firms in location ℓ at time t.

In practice we proceed in two steps. First, we use (UE) to run the OLS regression

$$r_{ijt} = \hat{\lambda}_j a_{ijt} + \hat{c}_i + \hat{s}_j + \hat{\xi}_{ijt} \tag{3}$$

where $\hat{s}_j$ is a court-j fixed effect and $\hat{c}_i$ are controls for the complexity of case i. By estimating (3) we identify $\hat{\lambda}_j$: the contribution of legal ambiguity to legal uncertainty in the jurisdiction of court j. We start by assuming that, after controlling for a set of time-varying court characteristics, the court fixed effects $\hat{\lambda}_j$ and $\hat{s}_j$ do not change as a result of the reform. An assumption that we test later.

Then, given the estimated $\hat{\lambda}_j$ and $\hat{s}_j$ from regression (3), we estimate (OE). Since both the λ_j's and the dummy coefficients s_j's are generated regressors, we calculate standard errors for equation (OE) by bootstrapping, combining information from both equations (UE) and (OE); see Appendix E for further details.

To better see the identification, consider (OE) in first difference:

$$\Delta Y_{n\ell t} = \hat{\beta}_Y \Delta \hat{\lambda}_{j(\ell,t)} + \hat{\phi}_Y \Delta \hat{s}_{j(\ell,t)} + \Delta \varsigma_{n\ell t}$$

where $\Delta\hat{\lambda}_{j(\ell,t)}$ and $\Delta\hat{s}_{j(\ell,t)}$ are zero except at t_0 for firms in locations assigned to the jurisdiction of a new court. The regression (OE) implies that, under suitable assumptions discussed below, $\hat{\beta}_Y$ is a consistent estimate of $\beta_Y \times \bar{a}$:

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

3 Data

We use four main data sets: one for Supreme Court judgments, one for the text of laws, one for firms, and one for the characteristics of lower courts. Table 2 reports descriptive statistics.

Supreme Court judgments Since 2004, all judgments by the Supreme Court 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 Supreme Court judgments during the period).[11] Our final sample includes 485,271 judgments: 193,895 for civil cases and 291,376 for criminal cases. The judgments report the Supreme Court'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 Supreme Court; the date of the decision; the nature of the case (civil or criminal); the section of the Supreme Court that handled the case (see Table 1); 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. Table 2 shows that, on average, judgments refer to 0.8 laws, 4 code articles, and 0.7 decrees. We refer to all these norms simply as *laws.*

Panel (a) of Figure 1 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 probability is 35%, slightly higher for civil than for criminal cases (see Table 2). Reversal probabilities are similar across the criminal sections of the Court but vary substantially

[11] 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 simple matters.

Table 2: Descriptive statistics

	Average	Median	Standard deviation
A) Judgments by Supreme Court			
All cases			
Reversal probability	.349	0	.476
N. words (no stop words)	915.7	698	945.7
N. laws cited	.779	0	1.274
N. codes cited	3.942	3	4.034
N. decrees cited	.699	0	1.164
Ambiguity of laws, a	2.541	1.898	2.557
N.observations	485,271		
Civil cases			
Reversal probability	.388	0	.487
N. words (no stop words)	1091.6	913	731.02
N. laws cited	1.287	1	1.624
N. codes cited	4.456	3	4.613
N. decrees cited	1.037	1	1.495
Ambiguity of laws, a	2.249	1.319	2.714
N. observations	193,895		
Criminal cases			
Reversal probability	.323	0	.467
N. words (no stop words)	798.6	577	1048.6
N. laws cited	.440	0	.813
N. codes cited	3.599	3	3.556
N. decrees cited	.473	0	.800
Ambiguity of laws, a	2.736	2.130	2.427
N. observations	291,376		
B) Firms			
Investment/Capital	.270	.110	.691
Firm growth rate	.087	.047	.616
Provisions/Assets	.016	0	.047
Future sales/Assets	1.147	.976	1.153
Z score	3.988	4	2.378
Sales (log)	6.632	6.380	2.367
N. observations	5,090,258		
C) Lower courts			
Size (no. judges)	93.3	38.3	113.5
Judges tenure	21.1	21.4	4.2
Staff shortage	8.7	4.3	9.0
N. observations	6,832,128		

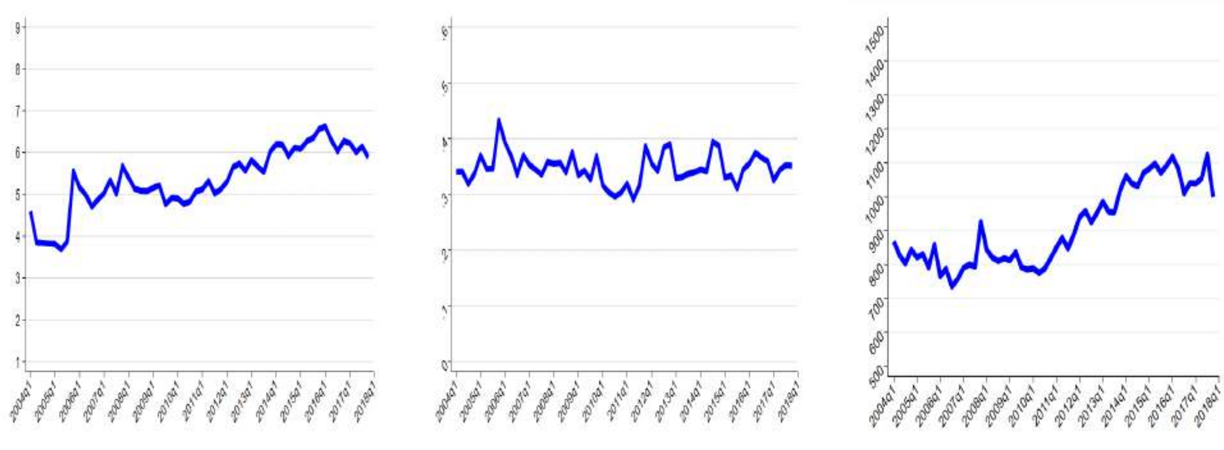
Notes: The table reports descriptive statistics (mean, median, and standard deviation) for the main variables used in the analysis. Panel A focuses on Supreme Court judgments. Reversal probability refers to the likelihood that the Supreme Court reverses a lower court ruling. Ambiguity of laws is the average ambiguity of all laws cited in a judgment, calculated using a 9 percent discount rate. Panel B presents statistics on firms for the 2008-2017 period. Panel C presents statistics on lower courts. Court size and staff shortage are measured in number of judges; judge tenure is measured in years.

across civil sections (see Table 1).[12] Panel (b) of Figure 1 plots the average reversal prob-

[12] Reversal probabilities are notably higher in Section 4, which specializes in labor issues, and in Section 5, which specializes in tax issues. This is consistent with the widely accepted view that tax legislation in Italy—and also in other countries including the US (D'Amato, 1983)—is complex, changing over time, quite unpredictable and ambiguous. The Association of Italian businesses and National Council of Tax Accoun-

ability, which remains stable over the period. To proxy for case complexity, we use the number of characters in the judgment. Panel (c) of Figure 1 shows its quarterly average, which rises slightly in the second half of the sample period.

Figure 1: Citations, reversal probability and judgments length, years 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 Supreme Court 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 Supreme Court judgments from 2004 to 2017 that cite at least one law and originate from a location with a district court.

Laws We draw laws from Normattiva (2016) and process through text analysis all existing laws. 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; average root length; number of modal verbs per word; number of demonstrative adjectives per word; number of 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,

tants have described the Italian fiscal system as characterized by "a frenetic discontinuity and frequent changes, continuous new requirements, special regimes, etc. [...] that, in some instances, compromises certainty, and stability... simplifications are invoked by professionals in the field," see Confindustria and Consiglio Nazionale dei Dottori Commercialisti (2019).

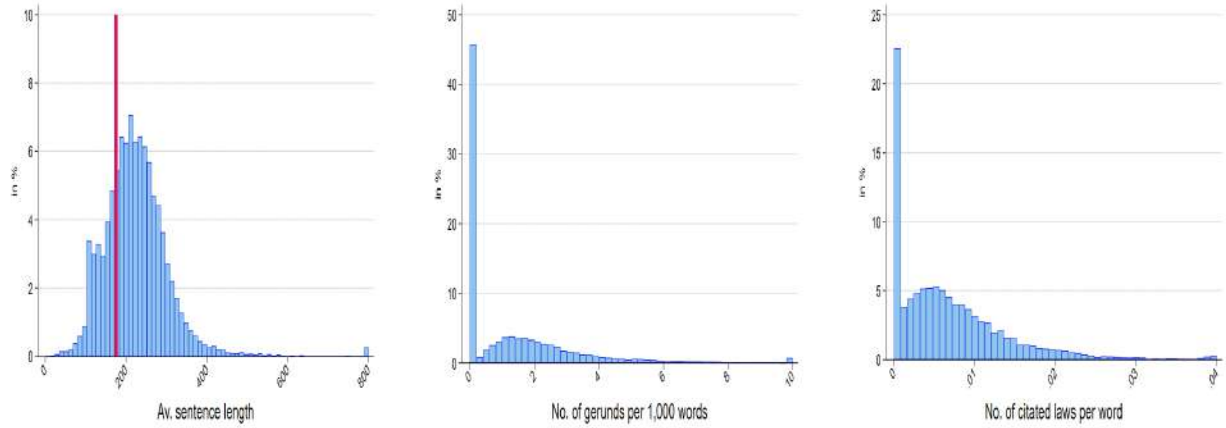
and the number of references to other laws in the main body of the text, scaled by total word count.

These ten indicators are intuitive and serve as proxies for whether the meaning, scope, and interpretation of a law are ambiguous due to poor drafting. 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 (Cortellazzo and Pellegrino, 2003; Fioritto, 1997; Alfieri et al., 2011). 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 2 considers the union of all laws cited in Supreme Court 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 Supreme Court contain excessively long sentences. There is substantial dispersion in both the use of gerunds per word and the number of other laws cited within a law. 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.

To identify the rate δ at which legal ambiguity is dispelled over time in (1), we study how the reversal probability r_{ijt} is affected by the age of the laws cited in Supreme Court judgments. We find that younger laws are more likely to lead to a reversal (see Table A1 in the appendix). Since the reversal probability declines by approximately 9 percent per each year of age of the law, we set $\delta = 0.09$. We discount the 10 indicators of law drafting quality using the estimated δ and summarize the discounted indicators with their

Figure 2: Some drafting quality indicators



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

Notes: The sample is the union of all laws cited in judgments by the Supreme Court over the period 2004-2017. In panel (a) length of sentences is the average number of characters in all sentences in the law and the vertical red line corresponds to 25×7=175 characters which identifies sentences of (roughly) 25 words as the average number of characters per word is 7. In Panel (b) the number of gerunds per words is calculated as the total number of gerunds in a law divided by the total number of words in its main text, multiplied by 1,000. In Panel (c) the number of citations to other laws is the ratio between the total number of laws cited by a law and the total number of words in its main text. The bin 800 in panel (a) includes all laws with average sentence length greater than 800 characters; the bin 0.01 in panel (b) includes all laws with number of gerund per (thousand) word greater or equal than 0.01; the bin 0.04 in panel (c) includes all laws with number of citations per word greater or equal than 0.04.

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 quality. The loadings are all positive and similar in magnitude, implying that each drafting quality indicator contributes roughly equally (see the Appendix for further details). Panel A of Table 2 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 Supreme Court, 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.[13]

[13]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

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.[14]

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 expected 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

introduction of a law. The same principle is preserved in the Italian Constitution, where it serves to protect the President of the Republic.

[14] "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.

rationed, which we interpret as a proxy for credit supply conditions at the time.[15] In Section 5, we use these variables to estimate the effects of legal uncertainty on firm-level 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 Supreme Court judges for the period 2010–2015 through formal requests to the High Council of the Judiciary (*Consiglio Superiore della Magistratura*)—the Italian institution responsible 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 Supreme Court 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.

4 Estimating the legal uncertainty equation

We begin by estimating the uncertainty equation in (UE), initially assuming that ambiguous laws have a uniform effect on legal uncertainty across all courts. We then examine whether this effect varies across courts.

[15] 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.

4.1 Uncertainty equation

We estimate the uncertainty equation in (UE) on our sample of Supreme Court judgments:

$$r_{ijt} = \lambda \times a_{ijt} + s_j + \gamma' X_{ijt} + \xi_{ijt} \tag{5}$$

where i denotes the legal case heard by the Supreme Court, j represents the lower court whose ruling was appealed, and t is the date of the Supreme Court 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 Supreme Court in its judgment, as previously discussed. s_j indicates one of the 160 dummies for the lower court whose decision was appealed to the Supreme Court.[16] X_{ijt} is a vector of controls comprising 29 dummies for whether the appeal was on a decision by one of the 29 Courts of Appeal; a full set of dummies for the section of the Supreme Court examining the case (see Table 1); dummies for the identity of the section president at the time of the decision; time dummies for the year-quarter t of both the Supreme Court and district court decisions; a dummy for whether the lower court decision was by a Justice of the Peace; a dummy for whether it was issued by another special court (Juvenile Court, Court of Freedom, Supervisory Court, Review Court, Judge for Preliminary Investigations); 16 indicators for the topic of the laws relevant to the case, constructed using the ministries that signed them; and two indicators of case complexity: the length of the judgment and the number of laws cited in the judgment. Lastly, ξ_{ijt} is an error term. We cluster standard errors by the president of the Supreme Court section presiding over the appeal, as the president may influence rulings, causing correlated residuals in the regression (5). The key parameter in (5) is λ, which measures how legal ambiguity from drafting quality affects reversal probabilities.

Table 3 shows the estimates of the effect of a_{ijt} on reversal probabilities. Results are shown for all judgments (column 1), civil case judgments only (column 2), and criminal case judgments only (column 3). Legal ambiguity due to poor drafting increases reversal probabilities across all specifications and is statistically significant in both civil and criminal cases, with a stronger effect in civil cases. This suggests that poorly drafted laws have pervasive effects on legal uncertainty, which increases across the two broad segments of the legal system. The estimated effect of legal ambiguity on the reversal probability for all cases is $\lambda = 0.012$. This implies that a one-standard deviation decline in drafting quality raises the reversal probability by 3 percentage points—approximately 10% of the mean reversal

[16] The number of estimated s_j dummies 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 Supreme Court judgments on cases originating from them.

probability.

Table 3: Uncertainty equation: average effects

	Dep. var.: Reversal probability		
	All judgements (1)	Civil only (2)	Criminal only (3)
Legal ambiguity	0.012***	0.016***	0.007***
	(0.002)	(0.003)	(0.001)
N. observations	456,770	188,068	268,686
Adjusted R^2	0.085	0.123	0.070

Notes: Results from estimating regression (5) on Supreme Court judgments citing at least one law from 2004-2017. All regressions include 160 s_i dummies for the location of the lower court whose decision was appealed, and control vector X_{ijt}, which comprises: dummies for the Supreme Court section examining the case (see Table 1); dummies for the identity of the section president at decision time; time dummies for the year-quarter t of both the Supreme Court and district court decisions; length (in characters) of the Supreme Court 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, Review 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Measurement error

A potential concern is that we do not observe the true legal ambiguity due to poor drafting, a_{ijt}, but only a proxy $\tilde{a}_{ijt} = a_{ijt} + \epsilon_{ijt}$, where ϵ_{ijt} denotes measurement error. Consequently, the OLS estimate of λ in (5) may be downward biased. A standard approach to address this bias is to instrument the mismeasured variable with an alternative proxy capturing the same construct (Ashenfelter and Krueger, 1994). To implement this, we use two proxies for legal ambiguity, $\tilde{a}^1_{ijt}$ and $\tilde{a}^2_{ijt}$, and estimate regression (5) instrumenting $\tilde{a}^1_{ijt}$ with $\tilde{a}^2_{ijt}$. The resulting IV estimate is consistent for the true population coefficient λ obtained in the absence of measurement error, and allows us to evaluate the magnitude of the attenuation bias caused by measurement error.

To implement the strategy, we construct $\tilde{a}^1_{ijt}$ using the first seven indicators of writing quality (number of gerunds, number of contingency clauses, word length, root length, phrase length, number of modal verbs, number of demonstrative adjectives, number of pronouns) along with the first indicator of structural ambiguity (fraction with preamble). In contrast, $\tilde{a}^2_{ijt}$ is constructed using the eighth indicator of writing quality (average phrase length) plus the second indicator of structural ambiguity (number of citations to other laws). The

Table 4: Uncertainty equation: some robustness

	Dep. var.: Reversal probability		
	IV analysis	Heterogeneity analysis	
	All judgements	without imprisonment	with imprisonment
	(1)	(2)	(3)
Legal ambiguity	0.022***	0.005***	0.009***
	(0.003)	(0.001)	(0.002)
N. observations	456,770	180,002	88,670
Adjusted R^2	0.0196	0.069	0.087

Notes: The set of controls is as described in the legend of Table 3. Column 1 presents the IV estimate of λ in (5), where the measure of legal ambiguity is $\tilde{a}^1_{ijt}$ instrumented with $\tilde{a}^2_{ijt}$. $\tilde{a}^1_{ijt}$ is constructed using word length, root length, and the number of gerunds, contingency clauses, modal verbs, demonstrative adjectives, pronouns, and the dummy for a presence of a preamble; $\tilde{a}^2_{ijt}$ is constructed using phrase length and the number of citations to other laws. The F-statistic for the instrument is 1,663. Column 2 reports the estimated λ for criminal judgments without imprisonment risk; Column 3 for those with possible imprisonment terms. Robust standard errors clustered at the level of the president judge are shown in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

factor loadings remain unchanged in the aggregation used to obtain $\tilde{a}^1_{ijt}$ and $\tilde{a}^2_{ijt}$. The set of controls X_{ijt} is the same as those described in the legend of Table 3. The F-statistic for the instrument is 1,663. Column 1 of Table 4 reports the results. The estimated IV coefficient increases from 0.012 in Table 3 to 0.022 in Table 4 which we take as prima-facie evidence that measurement error could cause some mild attenuation bias.

4.3 Selection

We consider the year 2010 and compare the distribution of legal ambiguity a for all Italian laws with that of the subset of laws cited by the Supreme Court in the year. Table 5 reports descriptive statistics for the two distributions; see also Figure A1 in the Appendix. In 2010, only around 20% of all Italian laws were cited by the Supreme Court. On average, legal ambiguity a is about three times higher in the set of laws cited by Supreme Court judgments than in the population of all Italian laws. However, the standard deviation is twice as large in the subset of cited laws relative to the overall population, indicating that the laws cited by the Supreme Court exhibit substantial variation in legal ambiguity, which is useful for identification.

A potential concern is that cases appealed to the Supreme Court may not be a random sample of all legal cases in Italy: poorly drafted, ambiguous laws a may encourage the

Table 5: Ambiguity in all laws and Supreme Court-cited laws, year 2010

	Average	Median	Standard deviation	N
Ambiguity of law a: All	1.75	.291	3.88	71,135
Ambiguity of law a: Cited in judgments	5.35	3.14	6.13	13,773

losing party to appeal in the hope of reversal. These endogenous decisions to appeal could create a positive selection bias, potentially causing an upward bias in the estimate of λ in equation (5). Our prior belief is that selection issues are quantitatively minor given the institutional setup. In Italy, appeals to the Supreme Court are based solely on claims that lower courts misinterpreted laws and are relatively inexpensive, accessible, and common—approximately 32% of cases from lower courts reach the Supreme Court.

To assess more formally whether endogenous selection is a major concern, we note that in criminal cases, self-selection is less problematic than in civil cases, as the stakes are higher and authorities are required to prosecute crimes.[17] Self-selection in appeals to the Supreme Court is even less relevant when the defendant's personal freedom is at stake. If the estimate of λ does not significantly decrease in subsamples where self-selection is plausibly weaker, we can reasonably conclude that it is unlikely to significantly bias our results. Column 2 of Table 4 reports the estimate of λ from the sample of criminal judgments without risk of imprisonment; Column 3 reports the estimate for cases involving possible imprisonment terms. We find no evidence that λ is lower in cases involving the risk of imprisonment relative to those without, suggesting that self-selection in the decision to appeal is unlikely to fully drive our estimates.

4.4 Variation across courts

To examine how the effects of ambiguous laws on legal uncertainty vary across lower courts, we estimate the uncertainty equation (5), allowing the coefficient on legal ambiguity, λ, to differ by lower court, resulting in a total of 160 court-specific coefficients λ_j:

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

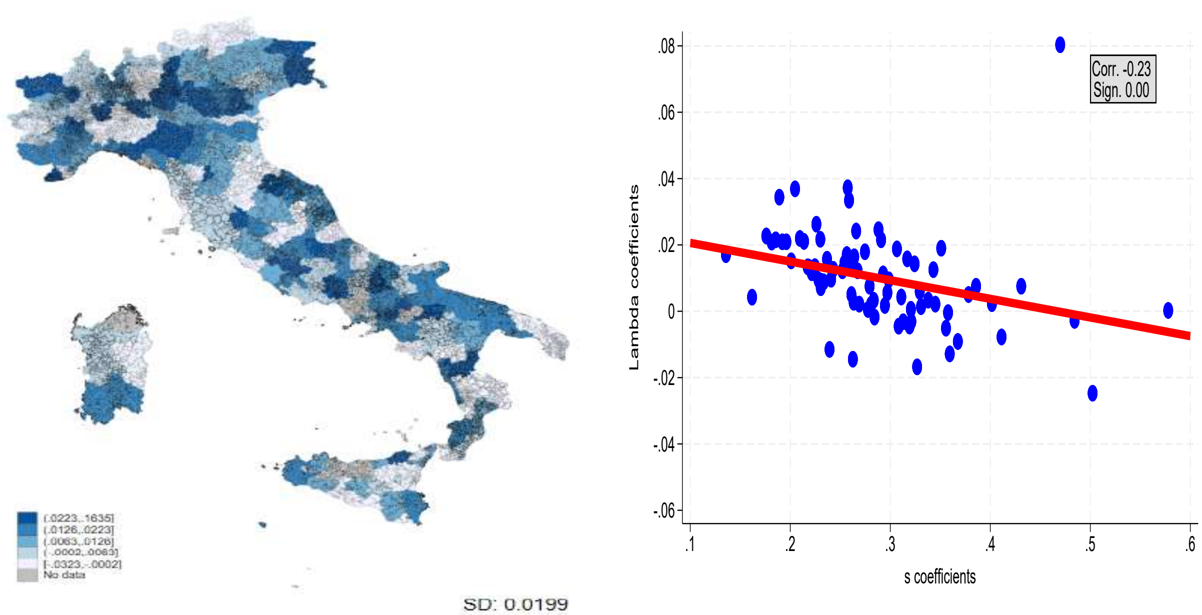
In this specification, s_j represents a comprehensive set of dummy variables for the location of the lower courts. The control variables X_{ijt} are the same as those described in the legend

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

of Table 3. Differences in λ_j measure how legal uncertainty due to ambiguous laws varies across court jurisdictions: for two given lower courts j and j', $\lambda_j > \lambda_{j'}$ means that, on average, court j is more likely than court j' to misinterpret poorly drafted laws, especially when the text is more ambiguous (i.e., for greater values of a_{ijt}). The heterogeneity in λ_j exposes firms under the jurisdiction of different courts to different levels of legal uncertainty arising from ambiguous laws.

Panel (a) of Figure A2 shows the geographical distribution of the coefficients λ_j. Panel (b) plots the relation between the 160 λ_j coefficients and the corresponding s_j coefficients in (22). In panel (a) darker areas correspond to a greater value of λ_j. An F test for the equality of the λ_j coefficients across courts strongly rejects the null hypothesis implying that the legal uncertainty due to ambiguous laws differs systematically across courts. Of the 160 estimated λ_j coefficients, 120 are estimated to be positive and 64 of them are significantly different from zero. The map shows substantial heterogeneity in the estimated values of λ_j across court jurisdictions. Smaller courts tend to have larger estimated λ_j, but the correlation with court size is weak (0.04) and statistically insignificant. Interestingly, there are no clear North-South differences across jurisdictions.

Figure 3: Court-specific coefficients on legal ambiguity, $\boldsymbol{\lambda_j}$



(a) Court-specific λ-coefficients

(b) Correlation between λ_j and s_j

The correlation between λ_j and s_j is low and negative (minus 0.2), consistent with the hypothesis that these coefficients capture distinct characteristics of lower court j. We interpret λ_j as reflecting the intellectual alignment of lower court j with the Supreme Court, while s_j captures the average complexity of cases handled by court j, which influences the likelihood of disagreement between the lower court and the Supreme Court even when laws are optimally drafted ($a_{ijt} = 0$).

A lower court and the Supreme Court are more likely to interpret ambiguous laws similarly when they are more closely aligned intellectually. Differences in intellectual alignment reflect variations in judges' legal traditions and prevailing schools of thought or doctrines. These institutional traits tend to persist over time, independently of the identities of the current judges. To test this interpretation more formally, we examine whether judges from courts with lower λ_j are more likely to be promoted to the Supreme Court. Such a pattern would suggest that the two judicial bodies share relevant legal cultural and intellectual traits.

For each Supreme Court judge in 2022, we know their previous lower court location before appointment.[18] With this information, we run the following regression:

$$n_j = \psi \lambda_j + \gamma' Z_j + \epsilon_j \tag{7}$$

where n_j is a dummy variable equal to one if at least one judge from lower court j is sitting at the Supreme Court. The coefficient ψ measures the effect of the intellectual alignment of court j on the probability of having a judge at the Supreme Court. 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 court-j fixed effect in log level s_j, estimated in the uncertainty equation in (6).

We test whether the coefficient ψ in (7) is negative, which would indicate that the probability of observing a judge from court j sitting at the Supreme Court is lower if court j has a greater λ_j coefficient. The first column of Table 6 below shows the baseline results. The second column adds, as a control, the court-j fixed effect in level s_j. The effect of the court-j fixed effect in level 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 Supreme Court 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.

[18] At the time of appointment to the Supreme Court, 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.

Table 6: Judges at the Supreme Court 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 (7). The sample includes all judges sitting at the Supreme Court in 2012. The dependent variable is a dummy equal to one if at least one judge from lower court j is sitting at the Supreme Court. The regression in column 2 also includes the court-j fixed effect s_j estimated from the regression in (6). Robust standard errors are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5 Firm responses to legal uncertainty

As discussed in Section 2, to test the effects of legal uncertainty on firm outcomes, we exploit the 2012 judicial reform, which induced time variation in the level of legal uncertainty faced by firms in a subset of municipalities ℓ. We first describe the reform, and then estimate its effect on firm growth using the specification in (OE). We subsequently extend the analysis to firm investment and precautionary provisions, and later conduct several robustness exercises. In Section 6, we further broaden the scope to examine effects on business creation, firm exit, and the average size of firms at entry.

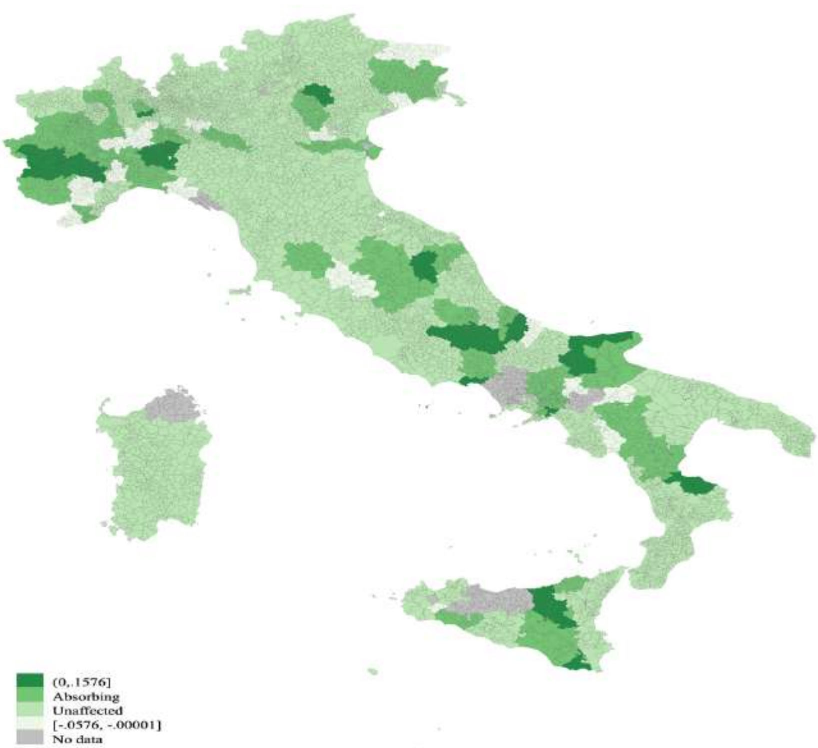
5.1 The judicial reform

In 2012, which we conventionally identify as $t = 0$, 31 district courts were closed.[19] Firms previously under the jurisdiction of a closed court were automatically reassigned to a nearby

[19]The closed district courts were: Acqui Terme; Alba; Ariano Irpino; Avezzano; Bassano del Grappa; Camerino; Casale Monferrato; Chiavari; Crema; Lanciano; Lucera; Melfi; Mistretta; Modica; Mondov'i; Montepulciano; Nicosia; Orvieto; Pinerolo; Rossano; Sala Consilina; Saluzzo; Sanremo; Sant'Angelo dei Lombardi; Sulmona; Tolmezzo; Tortona; Urbino; Vasto; Vigevano; Voghera, see Italian Ministry of Justice (2021).

district court, and court personnel were transferred accordingly.[20] Only chief judges, deputy judges, and other senior officials from the closed courts had the option to relocate to an equivalent position in another court; see Pezone (2022) for further discussion of the reform. As a result, the reform generated three types of municipalities: *treated* municipalities, where firms experienced a change in legal uncertainty of size $\Delta\lambda$ because they were transferred to the jurisdiction of a different court, i.e., $j(\ell,-1) \neq j(\ell,0)$; *absorbing* municipalities which were under the jurisdiction of a district court that, after the 2012 reform, absorbed one or more courts that were closed; and the remaining *unaffected* municipalities. Firm located in absorbing or unaffected municipalities before the reform stayed under the same court's jurisdiction , so $j(\ell,-1) = j(\ell,0)$. Figure 4 shows the geographic distribution of the three types of municipalities across Italy. Unaffected municipalities are shown in light green; absorbing municipalities in dark green; treated municipalities with an increase in legal uncertainty ($\Delta\lambda > 0$) in very dark green; and those with a decrease in legal uncertainty ($\Delta\lambda < 0$) in very light green. The reform affected 1,235 out of 8,093 municipalities spread across northern, central, and southern Italy, with a slightly higher concentration in the northwest.

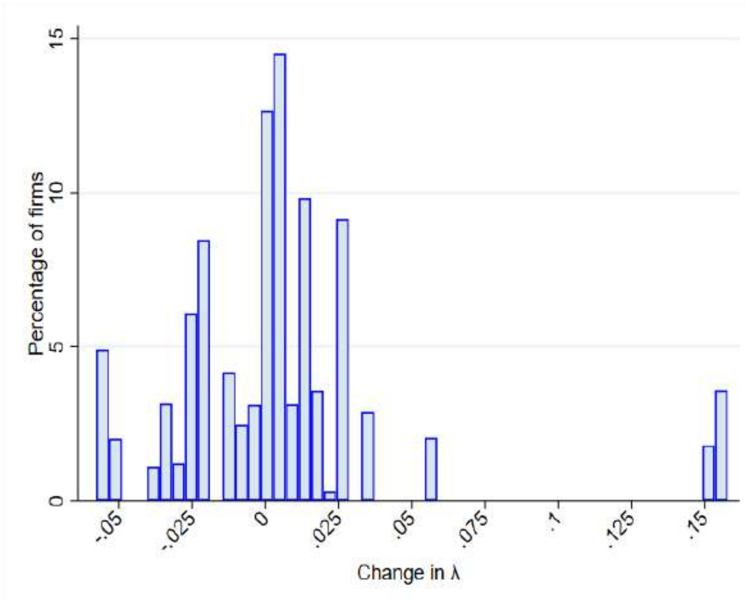
Figure 4: Changes in court-specific coefficients on legal ambiguity, $\Delta\lambda$



[20]Note that firms have no discretion over which court has jurisdiction over them, as the reform assigns it based on the location of the firmâ€™s registered office.

As many as 115,609 firms (6.1 percent of the total sample) were located in treated municipalities. Figure 5 shows the distribution of the change in legal uncertainty, $\Delta\lambda_j$, experienced by treated firms: 51.6 percent of them faced an increase in legal uncertainty due to ambiguous laws, while the remainder experienced a decrease or no change. Overall, the distribution of $\Delta\lambda_j$ is centered around around zero and exhibits substantial variation.

Figure 5: Firm-level distribution of changes in legal ambiguity coefficients, $\boldsymbol{\Delta\lambda_j}$



Notes: Histogram of firm-level changes in legal uncertainty, $\Delta\lambda_j$, for the 115,609 affected firms in the 1,235 treated municipalities. The standard deviation of the distribution of $\Delta\lambda_j$ is 0.0425.

Table 7 presents descriptive statistics for the year prior to the reform (2011), separately for treated, absorbing, and unaffected districts. Panel A reports firm-level statistics, while Panel B reports court-level statistics. Firm characteristics were broadly similar between treated and untreated municipalities (either absorbing or unaffected) targeting . Since the goal of the reform was to close smaller courts to achieve economies of scale and reduce costs, closed courts had a median of 6.7 judges, compared to 12.7 in absorbing courts and 20.7 in unaffected courts.

We implicitly assume that the court-specific coefficients on legal ambiguity for absorbing courts, λ_j, are unaffected by the reform. To test this hypothesis, we augment equation (6) with the triple interaction term $a_{ijt} \times \text{ABSO}_j \times \text{POST}_t$ to capture the differential impact of the reform on legal ambiguity in municipalities served by courts that absorbed others. The variables ABSO_j and POST_t are dummy indicators for absorbing courts and the post-reform period, respectively. To control for the underlying main effects, we also include all

Table 7: 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.

the corresponding double interaction terms. Accordingly, we estimate:

$$r_{ijt} = \lambda_j \, a_{ijt} + \lambda^A \, a_{ijt} \times \text{ABSO}_j \times \text{POST}_t + \lambda^P a_{ijt} \times \text{POST}_t + d^A \text{ABSO}_j \times \text{POST}_t + \gamma' X_{ijt} + \xi_{ijt} \tag{8}$$

The set of controls X_{ijt} includes those detailed in the legend of Table 3, as well as the court fixed-effect dummies s_j. The coefficient λ^A is the parameter of interest, as it captures the differential effect of the reform on the impact of legal ambiguity for absorbing municipalities. Column 1 of Table 8 presents the estimation results. There is no evidence that the reform differentially affected the impact of legal ambiguity in absorbing courts. In theory, the effect of the reform on absorbing courts may vary depending on whether the absorbed court has a higher or lower λ_j. To test this possibility, we estimate an extended version of equation (8) that includes an additional interaction term between the pre-reform difference in λ_j between absorbing and absorbed courts and the post-reform indicator, i.e. the term $a_{ijt} \times (\lambda_{\text{ABSO}_j} - \lambda_{\text{TREAT}_j}) \times \text{POST}_t \times \text{ABSO}_j$. We also include the full set of corresponding interaction terms to account for the associated main effects. The results are reported in Column 2 of Table 8. Again, the interaction terms are not statistically significant, further supporting the hypothesis that the court-specific effects of legal ambiguity for absorbing courts have remained stable after the reform.

Table 8: **Post-reform stability of $\boldsymbol{\lambda_j}$ coefficients**

	Dep. var.: Reversal probability	
	(1)	(2)
Ambiguity×Post×Absorbing	0.001	0.001
	(0.004)	(0.004)
Ambiguity×λ-Difference×Post×Absorbing		0.442
		(0.306)
N. observations	454,969	454,969
Adjusted R^2	0.086	0.086

Notes: Results from estimating regression (8). The sample includes all judgments by the Supreme Court that cite at least one law over the period 2004-2017. The regressions include the same controls as those detailed in the legend of Table 3 plus the interaction terms Absorbing×Post and Ambiguity×Post and in column 2, also include the triple interaction terms Absorbing×λ-Difference×Post and Ambiguity×λ-Difference×Post. Robust standard errors clustered at the president judge level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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:

$$Y_{n\ell t} = \beta_Y \lambda_{n\ell t} + f_n + f_t + \gamma' X_{n\ell t} + u_{n\ell t} \tag{9}$$

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. In the baseline specification, $X_{n\ell t}$ includes regional GDP growth interacted separately with sector dummies 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. 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. This is in line with standard accelerator models of investment (Harrod, 1936) and serves as a proxy for a firm's capacity to absorb adverse shocks by drawing on future cash flows. In some specifications, we also include the court-specific fixed effects in levels, s_j, estimated from equation (UE), along with several court-level controls. To assess robustness, we also present results using a simple two-way fixed effects specification that includes only firm and time dummies. The effect of legal uncertainty due to ambiguous laws on variable Y is measured by β_Y.

To clarify the econometric methodology, let T_n be an indicator for whether firm n is treated by the reform, and let $POST_t$ be a dummy for 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 (equal to zero for untreated municipalities). Then, we can write:

$$\lambda_{n\ell t} = \lambda^-_{n\ell} + \Delta\lambda_{n\ell} \times T_n \times POST_t \tag{10}$$

For untreated firms, $T_n = 0$, legal uncertainty remains constant over time at $\lambda^-_{n\ell}$. For treated firms, $T_n = 1$, $\lambda_{n\ell t}$ changes after the reform, increasing or decreasing depending on the sign of $\Delta\lambda_{n\ell}$. Using (10) to replace $\lambda_{n\ell t}$ into (9) yields

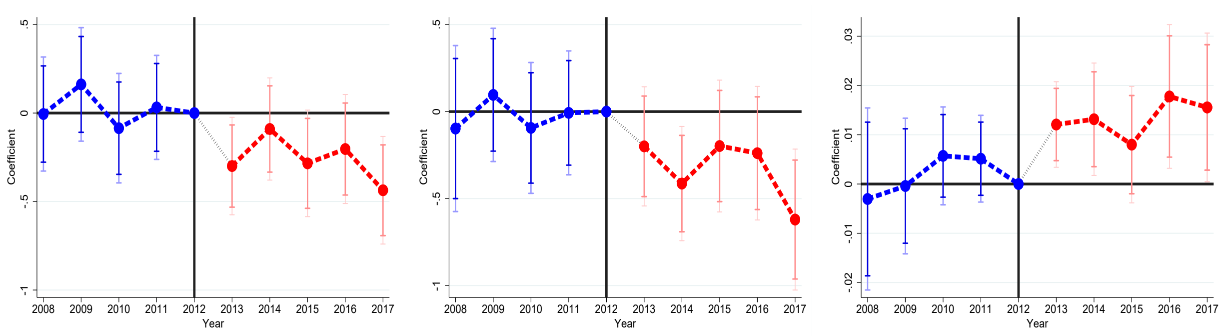
$$Y_{n\ell t} = \beta_Y \Delta\lambda_{n\ell} \times T_n \times POST_t + \tilde{f}_n + f_t + \gamma' X_{n\ell t} + v_{n\ell t} \tag{11}$$

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 (11) 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 time variation in legal uncertainty 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 (11) 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, as there is prima facie evidence suggesting that variation in $\Delta\lambda_{n\ell}$ is plausibly random and unlikely to be driven by mean reversion. Table 8 shows that the level of legal uncertainty in absorbing districts remained unchanged following the reform. The distribution of changes in legal uncertainty, $\Delta\lambda_{n\ell}$, shown in Figure 5, is approximately symmetric around zero and exhibits substantial variation. Finally, Table 7 indicates that variation in $\Delta\lambda_{n\ell}$ appears uncorrelated with firm characteristics. To address residual concerns about potential nonrandom changes in $\Delta\lambda_{n\ell}$, we then implement 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 on its pre-treatment level $\lambda^-_{n\ell}$;

second, the main equation in 11 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 Panel (a) of Figure 6 plots year-by-year estimates of the response of treated firms' growth rates to changes in $\lambda_{n\ell}$ relative to control firms over a 10-year window (5 years before and 5 years after the reform), with the 2012 coefficient normalized to zero. These estimates are obtained by estimating equation (11), 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 set of controls are as in the baseline specification. The control group consists of all firms located in districts unaffected by the reform. Panels (b) and (c) are analogous but focus on the investment rate and precautionary provisions, respectively. As the figure shows, there is no evidence of pre-trends: in all years before the reform, the effect of legal uncertainty on growth is estimated precisely at zero: the F-test of the hypothesis that all pre-reform effects are zero yields a p-value of 0.60.[21] 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 6: Parallel trends



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

Notes: Year-by-year estimates of treated firms' response to $\lambda_{n\ell}$ relative to control firms over a 10-year window, with the 2012 coefficient normalized to zero. These estimates are obtained by estimating equation (11) where the coefficient on $\Delta\lambda_{n\ell} \times T_n$ is interacted with a full set of pre- and post-reform time dummies. The F test of the hypothesis that all effects are zero prior to the reform yields a p-value of 0.60, 0.86 and 0.48 for growth rate, investment rate and precautionary savings, respectively. Controls include 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 (measured as the pre-reform sales percentile) interacted with the firm's Z-credit score. The control group consists in the set of firms in unaffected districts.

[21] 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 9 reports the average post-reform effects estimated from equation (11), with bootstrapped standard errors shown in square brackets. Columns 1-3 present estimates of the impact on firm growth, varying the composition of the control group. Column 1 reports the baseline specification, using firms in unaffected municipalities as the control group–consistent with Figure 6. 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. Despite differences in sample composition, the results are stable and indicate that legal uncertainty exerts a negative and precisely estimated effect on firm growth across all three specifications. Economically, a one standard deviation increase in legal uncertainty ($\Delta\lambda = 0.0425$) reduces firm growth by 1.2 percentage points, equivalent to 14% of the sample mean and 26% of the median firm growth rate.

Investment and precautionary provisions To better characterize how legal uncertainty hinders firm growth, we examine its effects on capital accumulation and precautionary provisions. Because investment is often irreversible, uncertainty tends to have a strong negative impact on firms' capital accumulation decisions—a finding well established in the literature, see among others, Abel and Eberly (1994, 1996); Guiso and Parigi (1999); Bloom (2009); Bloom, Bond, and Van Reenen (2007). In addition, firms typically respond to greater risk by increasing precautionary provisions to buffer potential future costs.

Panels (b) and (c) of Figure 6 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). The 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 before the reform. The divergence emerges only afterward, consistent with treated firms responding to heightened legal uncertainty by reducing investment and increasing precautionary provisions, both of which contribute to the observed slowdown in firm growth.

The last six columns of Table 9 report the average post reform effects of the uncertainty shock $\Delta\lambda$ on firms' investment rates (Columns 4-6) and on precautionary provisions (Columns 7-9). In response to increased legal uncertainty, the rate of investment drops, with an estimated coefficient that is similar across control groups. One standard deviation increase in $\Delta\lambda_{n\ell}$ (equal to 0.0425) lowers the investment rate by 1.3 percentage points, which represents 12 percent of the investment rate of the median firm. When considering precautionary provisions we find that a one standard deviation increase in the legal uncertainty shock raises firm precautionary savings by 5.6 percent of the sample mean.

Table 9: Effect of legal uncertainty on firms growth, investment and precautionary savings

Dep. var.:	Growth rate			Investments rate			Precautionary savings		
Control group:	Unaffected (1)	Absorbing (2)	All districts (3)	Unaffected (4)	Absorbing (5)	All districts (6)	Unaffected (7)	Absorbing (8)	All districts (9)
$\Delta\lambda\times$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
Year$\times$Regional GDP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector$\times$Regional GDP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit score$\times$Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year$\times$Credit rationing FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results from estimating (11). 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 is the effect of the change in legal uncertainty in the post-reform period for firms in treated municipalities. Controls include 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 (measured as the pre-reform sales percentile) interacted with the firm's Z-credit score. In columns 4-9, the 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Robustness

We now present a set of robustness checks on the effects of changes in legal uncertainty, $\Delta\lambda_{n\ell}$, on firm growth, investment, and precautionary provisions. All specifications are variations of equation (11), using firms in unaffected municipalities as the control group.

Table 10 presents robustness checks for the estimated effects on firm growth. In column 1, we augment the baseline specification with a set of (time varying) district-level controls, measured before and after the reform: the average number of judges, the average age of judges, the number of unfilled staff positions, and the average trial length. These time-varying controls help ensure that the estimated effects of legal uncertainty are not confounded by other concurrent changes in court characteristics that could affect district court efficiency and, consequently, firm behavior. The estimated effect of the legal ambiguity shock on firm growth remains largely unchanged.

Table 10: Effect of legal uncertainty on firm growth, additional analysis

Dep. var.:	Firm growth rate			
	With court controls (1)	No controls for aggregate shocks (2)	Only firms in the entire period (3)	Controlling for changes in s coefficients (4)
$\Delta\lambda\times$ Post	-0.274***	-0.292***	-0.280***	-0.276***
	(0.085)	(0.082)	(0.084)	(0.0855)
N. observations	3,833,047	3,994,686	3,437,521	3,833,047
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year×Regional GDP FE	Yes	No	Yes	Yes
Sector×Regional GDP FE	Yes	No	Yes	Yes
Credit score×Size FE	Yes	No	Yes	Yes
Year×Credit rationing FE	Yes	No	Yes	Yes

Notes: The table presents results from estimating (11). The dependent variable is firm growth rate. The variable $\Delta\lambda\times$Post is the effect of the reform-induced change in legal uncertainty. As a baseline, all specifications include the control variables detailed in the legend of Table 9, except in column 2, which includes only firm and year fixed effects. Column 1 adds to the baseline controls a set of time varying court-level variables: district court size, average judicial tenure, staff shortages and trial length. Column 3 restricts the analysis to the subsample of firms continuously present throughout the entire sample period. Column 4 augments the baseline specification with the first difference of the court fixed effect in levels, s_j, as estimated in equation (UE). The control group consists in the set of firms in unaffected districts. Robust standard errors clustered at the firm level are reported in round brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In column 2, we estimate a simple two-way fixed effects model, including only time and firm fixed effects as controls. The estimated coefficient is slightly larger and remains highly statistically significant. In column 3, we estimate equation (11) on the balanced panel of firms continuously present throughout all ten years of the sample period. The estimated coefficient is of the same magnitude and statistical significance as in the baseline specification, suggesting that our results are not driven by sample attrition or business turnover. Finally, in column 4, we extend the baseline specification by including changes in the court fixed effects, s_j, as an additional control. These fixed effects capture heterogeneity across courts in the complexity of cases they handle. The estimated effect of the legal uncertainty shock remains largely unchanged, consistent with the interpretation that Δs and $\Delta\lambda$ capture largely orthogonal court-level characteristics.

Table 11 presents the same set of robustness checks for firm investment. Adding court-level controls leaves the results virtually unchanged, while estimating a simple two-way fixed effects model reduces the estimated effect somewhat, though it remains economically meaningful and statistically significant.

Table 11: Effects on firm investments rates, additional analysis

Dep. var.:	Investment rate			
	With court controls (1)	No controls for aggregate shocks (2)	Only firms in the entire period (3)	Controlling for changes in s coefficients (4)
$\Delta\lambda \times$Post	-0.298***	-0.215**	-0.312***	-0.314***
	(0.110)	(0.101)	(0.110)	(0.111)
N. observations	2,888,559	3,216,510	2,664,352	2,888,559
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year×Regional GDP FE	Yes	No	Yes	Yes
Sector×Regional GDP FE	Yes	No	Yes	Yes
Credit score×Size FE	Yes	No	Yes	Yes
Year×Credit rationing FE	Yes	No	Yes	Yes

Notes: The table presents results from estimating (11). The dependent variable is the firm investments rate. The remaining details are as described in the legend of Table 10 where the baseline controls also include next year's sales scaled by firm total assets.

Finally, Table 12 extends the robustness checks to precautionary provisions. The results are essentially unchanged, with estimated effects ranging from 0.012 to 0.100, well within the range of those obtained in the main specification.

Table 13 presents the results using the two-step estimator of de Chaisemartin and Lei (2024), which addresses concerns about potential non-random changes in $\Delta\lambda_{n\ell}$, due to the pre-reform level of uncertainty $\lambda^-_{n\ell}$. Panel A reports the results from the first stage, in which the change in legal uncertainty, $\Delta\lambda_{n\ell}$, is regressed on its pre-treatment level, $\lambda^-_{n\ell}$. Panel B presents the estimated coefficient on the legal uncertainty shock from the

Table 12: Effects on firm precautionary savings, additional analysis

Dep. var.:	Precautionary savings			
	With court controls (1)	No controls for aggregate shocks (2)	Only firms in the entire period (3)	Controlling for changes in s coefficients (4)
$\Delta\lambda\times$ Post	0.0112**	0.0116**	0.0115**	0.0103**
	(0.005)	(0.005)	(0.005)	(0.005)
N. observations	3,639,150	4,709,548	3,331,578	3,639,150
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year$\times$Regional GDP FE	Yes	No	Yes	Yes
Sector$\times$Regional GDP FE	Yes	No	Yes	Yes
Credit score$\times$Size FE	Yes	No	Yes	Yes
Year$\times$Credit rationing FE	Yes	No	Yes	Yes

Notes: This table presents results from estimating (11). The dependent variable is firms precautionary savings. The remaining details are as described in the legend of Table 11.

second stage (our coefficient of interest), obtained using the residuals from the first-stage regression, $\Delta\lambda_{n\ell} - E\left(\Delta\lambda_{n\ell}\right)$, as the independent variable and controlling for the predicted change $E\left(\Delta\lambda_{n\ell}\right)$.[22] 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 consider the three control groups discussed earlier: unaffected districts, absorbing districts, or both. Bootstrapped standard errors for the second stage regressions are reported 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 9. 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 9. There is some evidence that the estimated effect of legal uncertainty shocks 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.

[22]In the first stage, to fully capture potential non-linearities in the relationship between changes in legal uncertainty and its pre-reform level, we use a quadratic B-spline, selected according to Stataâ€™s default settings. In the second stage, we collapse 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 13: 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}	-0.485***	-0.113***	-0.129***	-0.485***	-0.118***	-0.131***	-0.483***	-0.112***	-0.128***
	(0.004)	(0.004)	(0.001)	(0.005)	(0.005)	(0.001)	(0.004)	(0.004)	(0.001)
Panel B: Second stage									
$\Delta\lambda_{nl} - \mathbb{E}_n[\Delta\lambda_{nl}\|\lambda^{-}_{nl}]$	-0.312**	-0.255**	-0.275**	-0.467***	-0.372**	-0.349**	0.002	0.009*	0.006
	(0.125)	(0.120)	(0.118)	(0.151)	(0.145)	(0.142)	(0.005)	(0.005)	(0.005)
	[0.117]	[0.114]	[0.120]	[0.136]	[0.137]	[0.144]	[0.004]	[0.005]	[0.004]
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean of D.V.	-0.040	-0.041	-0.040	-0.004	-0.003	-0.003	0.000	0.000	0.000
N. observations	90024	356584	424892	74148	286072	341868	86243	340826	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 (11), using as the key independent variable the residuals from the first-stage regression, $\Delta\lambda_{n\ell} - E\left(\Delta\lambda_{n\ell}\right)$, and controlling for the predicted change $E\left(\Delta\lambda_{n\ell}\right)$. 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 9. 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 of ambiguous laws

The previous evidence shows that higher legal uncertainty due to legal ambiguity a reduces firm growth g. However, a can affect aggregate GDP per capita through additional margins. Specifically, legal ambiguity may influence affect aggregate growth through: (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 of start-ups y_0. These four margins jointly determine the long-run level of GDP per capita. We now estimate the effects of a on these four margins and use them to quantify the impact of legal ambiguity on Italian GDP per capita. We begin by developing the theoretical framework and then proceed to quantify the 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.[23] Firms die at Poisson arrival rate e. The number of new businesses per capita is b. In this setting, steady-state aggregate GDP per capita is given by

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

which in logs becomes

$$\ln y = \ln b - \ln (e-g) + \ln y_0. \tag{12}$$

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 (12). Then, a one percent increase in a leads to a long-run percentage change in output per capita equal to:

$$\mathbf{H} \equiv \frac{d \ln y}{d \ln a} = \left[\frac{d \ln b}{du} + \frac{d \ln y_0}{du} - \frac{e}{e-g} \cdot \frac{d \ln e}{du} + \frac{1}{e-g} \cdot \frac{dg}{du} \right] \times a \times \frac{du}{da}. \tag{13}$$

Let a_t denote the level of ambiguity arising from poor drafting in the average Italian law at time t. 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 $\dot{L}_t = lL_t$. New legislation is drafted with an average degree of poor drafting p. Existing ambiguity is

[23]In practice, assume that firm output is given by the Cobb-Douglas production function $\tilde{y} = e^z k^\eta$, where z denotes firm's 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 depreciation rate and i_t is firm investment. Then the growth rate of firm output, g, is given by $g = g_z + \eta g_k$, which is the sum of the growth rate of TFP, g_z, and that of capital, g_k, with the latter weighted by the capital share η.

resolved over time at rate $\delta = 0.09$. Then, the evolution of legal ambiguity a_t is governed by

$$\dot{a}_t = pl - (\delta + l)a_t, \tag{14}$$

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

$$\ln a = \ln\left(\frac{l}{l+\delta}\right) + \ln p. \tag{15}$$

Figure 7, panel (a) plots the time series of the total number of new words of legislation enacted each year in Italy since the mid-sixties, in logs ($\ln L_t$). The solid red line shows the actual series; 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. This, in turn, suggests that the first term in equation (15), $l + \delta$, has remained approximately constant over time. The red solid line in Panel (b) of Figure 7 shows the average degree of poor drafting of all new laws enacted in a given year, p_t, in logs, calculated using our principal component with the same factor loadings reported in Table A2. The horizontal black dotted line represents the degree of poor drafting (in logs) of the fundamental principles of the Italian Constitution. Figure 7 indicates that the drafting quality of Italian laws has declined by approximately 40% since the late 1990s. Relative to the average newly enacted laws in the 2000s, the drafting quality of the Italian Constitution is about 60% higher.

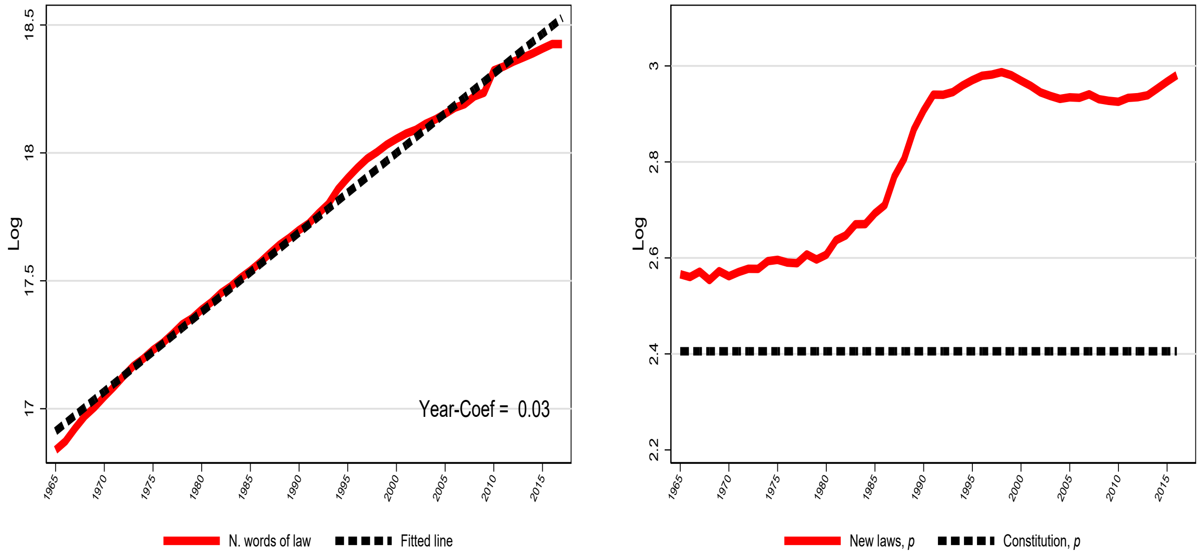
6.2 Quantification of costs

To evaluate $\mathbf{H}$ in (13), we exploit differences across municipalities and over time in firm creation $b_{\ell t}$, firm exit rates, $e_{\ell t}$ and average firm size at entry $y_{0\ell t}$, along with reform-induced changes in legal uncertainty. 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:

$$z_{\ell t} = \beta_z \Delta\lambda_{\ell t} \times POST_t + f_\ell + f_t + u_{n\ell t} \tag{16}$$

Table 14 reports the results from estimating equation (16): column (1) shows results for business creation, $\ln b_{\ell t}$; column (2), for the business exit rate; and column (3), for the initial average size of start-ups, $\ln y_{0\ell t}$. The estimates indicate that an increase in legal uncertainty raises business turnover: firm exit rates increase, which in turn stimulates business creation. Additionally, legal uncertainty reduces firm size at entry, consistent with the empirical evidence on investment discussed in Section 5.

Figure 7: Number of words of new legislation and degree of poor drafting over time



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

Notes: Panel (a) plots the total number of new words of legislation enacted each year in Italy. 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 (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.

Given the definition of legal uncertainty arising from poor drafting, $u = \bar{\lambda}a$, in (UE), we use Table 3 to conclude that

$$\frac{du}{da} = \bar{\lambda} = 0.012. \tag{17}$$

(4) shows that β_z in (16) measures $\frac{dz}{du} \cdot a$. Then, we use the estimates reported in Tables 9 and 14 to conclude that:

$$\frac{d\ln b}{du} \times a = 1.047; \quad \frac{d\ln y_0}{du} \times a = -0.979; \quad \frac{d\ln e}{du} \times a = 1.022; \quad \frac{dg}{du} \times a = -0.272 \tag{18}$$

We assume a steady-state growth rate of GDP per capita of 2 percent, $g = 0.02$, and an average business exit rate of 7 percent, $e = 0.07$, which roughly corresponds to the average over our sample period. Given these values, combining (17) with the estimates in (18) implies that the elasticity of GDP per capita with respect to legal ambiguity caused by

Table 14: **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	77213	75928	52165
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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

poor drafting, **H** in equation (13), is slightly above 8 percent in absolute value:

$$\mathbf{H} \equiv \frac{d\ln y}{d\ln a} = \left(1.047 - 0.979 - \frac{0.07}{0.05} \times 1.022 - \frac{1}{0.05} \times 0.272\right) \times 0.012 = -0.0816 \quad (19)$$

Panel (b) of Figure 14 documents a quality gap of roughly 60 percent between the fundamental 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 quality over the past 20 years (see Panel (b) of Figure 14) yields an estimated 3.26 percent reduction in annual GDP per capita. This implies that about two-thirds of the total gap relative to the constitution's benchmark has materialized over just the past two decades.

7 Conclusions

We used text analysis to measure the drafting quality of the universe of Italian laws and showed that poorly drafted laws introduce statutory ambiguity: they 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 impact of legal uncertainty stemming from poor law drafting. We found that, holding other factors constant, an increase in legal uncertainty lowers firm growth and investment rates. The results appeared to be robust to several concerns, including potential confounding from other court-related changes following the reform, alternative definitions of the control group, and the possibility that changes in legal uncertainty were non-random due to mean reversion. The economic costs of legal uncertainty stemming from poor legislative drafting appear to be substantial. Our estimates indicate that Italian GDP would be almost 5 percent higher if all laws were drafted to the standard of the Italian Constitution, and that the decline in drafting quality observed over the past two decades accounts for roughly two-thirds of this gap.

Given the substantial economic costs associated with poorly drafted laws, improving legislative drafting quality would be a cost-effective intervention with potentially large economic returns. A key question remains: why do politicians, when left to their own devices, so often draft laws poorly? Gratton et al. (2021) take a step toward answering this question, suggesting that poor drafting may stem from political instability and from the perverse incentives faced by politicians—particularly less competent ones—who prioritize signaling activism over addressing substantive problems. Yet several alternative explanations are possible, and future research should delve deeper into the root causes of poor drafting and explore how legislators can be more effectively disciplined to ensure that laws remain accessible, intelligible, clear, and predictable.

While our analysis has shed light on the relationship between drafting quality, statutory ambiguity, legal uncertainty, and firm behavior, other channels are likely at play. Unlike other forms of uncertainty, legal uncertainty arising from ambiguous laws can sometimes be strategically manipulated. As Jeremy Bentham noted in 1808, "the power of the lawyer is in the uncertainty of the law." Statutory ambiguity may thus create incentives to gamble on legal outcomes (Wiener 1950, chap. 7), shaping entrepreneurial behavior in ways that depend on ethical orientation. While legal uncertainty imposes additional risks on those less able or willing to navigate an ambiguous legal system, it may also create strategic advantages for unscrupulous entrepreneurs willing to undertake reckless ventures in the hope of exploiting legal loopholes, which could ultimately undermine confidence in institutions and the proper functioning of markets. Future research should then seek to investigate more closely the specific mechanisms through which some entrepreneurs benefit from ambiguous legal environments while others lose out, as well as the broader implications of these disparities for institutional quality and long-term economic performance.

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Additional material

Section A discusses the estimates of the depreciation rate δ in (1). Section B provides details on the construction of the principal component to calculate a_{ijt} in the uncertainty equation (3). Section C reports some additional figures. Section D 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. Section E discusses the bootstrapping algorithm.

A The discount rate

We estimate the following regression using the sample of Supreme Court judgments that cite at least one law (formal law, article of legal codes, or government decree) over the period 2004-2017:

$$r_{ijt} = \sum_{\tau} \beta^{\tau} \mathit{Citation\_Age}^{\tau}_{ijt} + s_j + \gamma' X_{ijt} + \xi_{ijt} \tag{20}$$

where $\mathit{Citation\_Age}^{\tau}_{ijt}$ is a dummy variable equal to one if the Supreme Court 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 dummies s_i. Since we dropped 5 district courts (La Spezia, Santangelo dei Lombardi, Santa Maria Capua Vetere, Tempio Pausania, Termini Imerese) with no Supreme Court judgments on cases originating from them, there are 160 lower court dummies s_i in total. The regression also includes the controls X_{ijt} which comprise: a full set of dummies for the section of the Supreme Court hearing the case (see Table 1); dummies for the identity of the section president at the time of the decision; time dummies for the year-quarter t of the Supreme Court decision; time dummies for the year-quarter t of the district court decision; a dummy for whether the appeal concerns a decision by a Justice of the Peace; and a dummy for whether the appeal concerns a decision by other courts (Juvenile Court, Court of Freedom, Supervisory Court, Review Court, Judge for Preliminary Investigations). The estimated coefficients β^{τ} are reported in Table A1. There is indication that the reversal probability declines by approximately 9 percent per each year of age of the law. Indeed, combining the estimates in Table A1, and noting that the age brackets are spaced five years apart, we have: $0.0172 \times 2 + 0.0456 = 0.08 \sim 0.91^{15} \cdot 0.0646 + 0.91^{10} \cdot 0.0627 + 0.91^{5} \cdot 0.0627$, which justifies setting $\delta = 0.09$.

Table A1: Computation of the depreciation rate

	Dep. var.: Reversal probability
Citation law aged 0-5 years	0.0646***
	(0.0100)
Citation law aged 6-10 years	0.0627***
	(0.0082)
Citation law aged 11-15 years	0.0456***
	(0.0085)
Citation law aged 16-20 years	0.0172**
	(0.0087)
Citation law with age higher than 20	-0.0035
	(0.0111)
Observations	456,770
Adjusted R^2	0.080

Notes: Results from estimating regression 20. The variables "Citation law aged 0-5/6-10/11-15/16-20/higher than 20 years" are dummy variables indicating whether the judgments cites at least one law in the corresponding age bracket. The sample includes all judgments by the Supreme Court that cite at least one law over the period 2004-2017. All regressions include 160 s_i lower court dummies and the vector of controls X_{ijt} which comprises: a full set of dummies for the section of the Supreme Court which examines the case (see Table 1); 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 Supreme Court 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, Review Court, Judge for Preliminary Investigations). Robust standard errors clustered at the president judge level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Construction of principal component

We discount the 10 indicators of law drafting quality 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 quality, 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 Supreme Court 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 Supreme Court 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 Supreme Court as follows:

$$a(\mathbf{m}) = \frac{\sigma_1}{w_1} \cdot \sum_{s=1}^{10} \frac{w_s}{\sigma_s} \cdot m_s, \tag{21}$$

$a(\mathbf{m})$ is our measure of ambiguity of the law due to drafting quality in the case heard by the Supreme Court. It is calculated by normalizing to one the loading on the number of gerunds per 1,000 words.

Column 1 of Table A2 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 (21). The loadings on the standardized variables w_s are all positive and are similar in magnitude. This implies that all indicators of drafting quality $\mathbf{m}$ account roughly equally for the overall variance of $\mathbf{m}$.

Table A2: 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 quality, $\mathbf{z}$. Column 2 reports the loadings on $a(\mathbf{m})$ in (21), setting the number of gerunds per word 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 A3 shows that this ratio is equal to 74 percent.

Table A3: Eigenvalues of main principal component

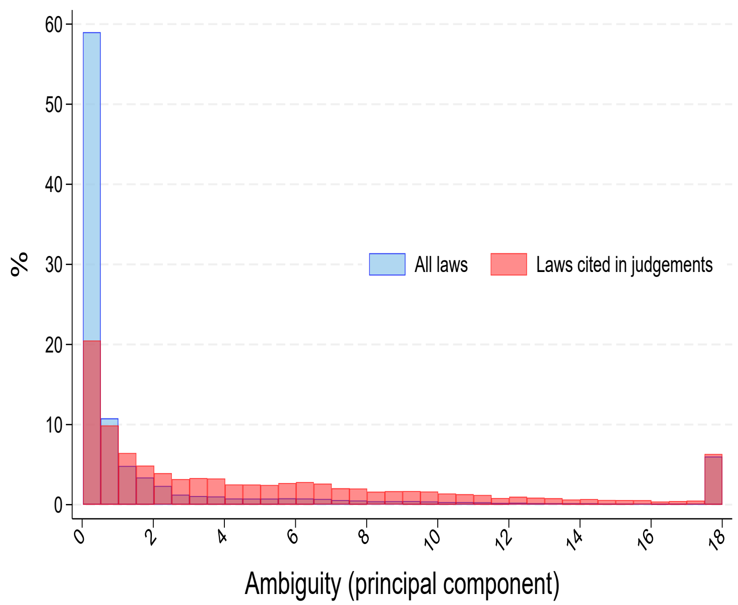
	Eigenvalue
Comp1	7.39
Comp2	.965
Comp3	.517
Comp4	.314
Comp5	.247
Comp6	.228
Comp7	.151
Comp8	.139
Comp9	.041
Comp10	.00009
Sum	10

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

C Additional figures

The red histogram in Figure A1 plots the distribution of legal ambiguity a for the set of all laws cited by the Supreme Court in 2010. The top bin groups all laws with legal ambiguity greater than or equal to 18, which corresponds to the top 5% of the distribution of laws cited in Supreme Court judgments. All other bins have an equal width of 0.5. The lowest bin corresponds to the interval $(0, 0.5]$. The distribution of legal ambiguity a for the entire universe of Italian laws in 2010 corresponds to the blue histogram in Figure A1, which is systematically shifted to the left.

Figure A1: Distribution of Ambiguity in 2010: All Laws versus Supreme Court-Cited Laws



Notes: The red histogram corresponds to the distribution of legal ambiguity a for the set of all laws cited by the Supreme Court in 2010. The blue histogram corresponds to the distribution of a for the entire universe of laws in 2010.

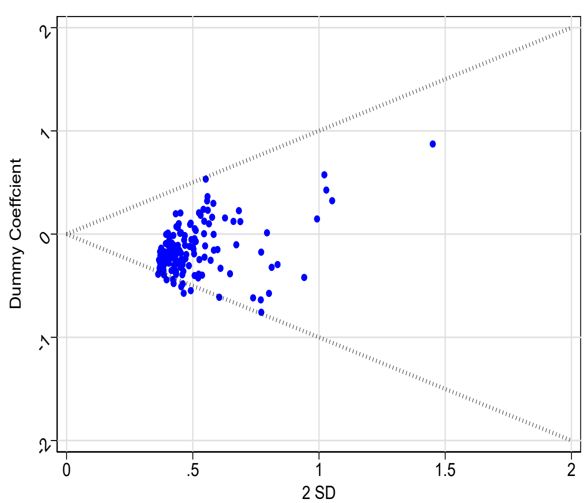
D Variation in legal ambiguity across lower courts

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 controlling for the full set of controls X_{ijt}). To test this identification assumption, we run the following regression on the sample of Supreme Court judgments:

$$a_{ijt} = d_j + \gamma' X_{ijt} + \xi_{ijt} \tag{22}$$

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 full 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 Supreme Court originated. We estimate 159 court fixed effects d_j, expressed relative to the omitted reference court (Acqui Terme). Our interest lies in testing whether the variation of the d_j coefficients is small and statistically insignificant. A lack of significant variation would suggest no strong evidence that legal ambiguity a differs systematically across lower courts in Italy. Figure A2 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 A2: Variation in legal ambiguity across lower courts



Notes: Results from estimating the court fixed effects coefficients d_j in equation (22). 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.

E 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 Supreme Court 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 IE regression.

E.1 UE regression

The dataset of Supreme Court 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 Supreme Court 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 1 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: Λ = ∅ and S = ∅
for b = 1 to 1000 do
    D^(b) = ∅ (we initialize sample dataset)
    for each stratum j ∈ J do
        D_j = {(r_ijt, a_ijt, X_ijt) ∈ D : j is fixed} (Data for stratum j)
        We draw a random sample S_j from D_j with replacement
        D^(b) = D^(b) ∪ S_j (Add stratum sample to current dataset)
    end for
    We run UE-regression model: r_ijt = λ_j a_ijt + s_j + γ'X_ijt + ξ_ijt using D^(b)
    for each stratum j ∈ J do
        λ_j^(b) = estimated coefficient of a_ijt for stratum j
        s_j^(b) = estimated fixed effect coefficient for stratum j
        Λ = Λ ∪ {(j, λ_j^(b))} (Collect λ_j coefficient for stratum j)
        S = S ∪ {(j, s_j^(b))} (Collect s_j coefficient for stratum j)
    end for
end for
We obtain empirical distribution F̂_j from {(λ_j^(b), s_j^(b))}_{b=1}^{1000} for each j ∈ J
return {F̂_j}_{j∈J}
```

E.2 IE 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 IE regression:

Algorithm 2 IE 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 IE regression estimates in $\{\beta^{(k)}\}_{k=1}^{1000}$

```
We initialize the vector collecting results 𝓑 = ∅
for k = 1 to 1000 do
    𝒟^(k) = ∅ (Initialize sample dataset)
    Θ^(k) = ∅ (Initialize coefficient collection)
    for each court j ∈ 𝒥 do
        We randomly sample (λ_j^(k), s_j^(k)) from empirical distribution F̂_j
        Θ^(k) = Θ^(k) ∪ {(λ_j^(k), s_j^(k))}
    end for
    We partition firms based on their pre-reform court jurisdiction: 𝒩_j = {n ∈ 𝒩 : J(n) = j} for each j ∈ 𝒥
    for each court jurisdiction j ∈ 𝒥 do
        We draw a random sample 𝒮_j of firms from 𝒩_j with replacement
        for each sampled firm n ∈ 𝒮_j do
            𝒟_n = {(Y_nlt, X_nlt) ∈ 𝒟_OE : n is fixed} (We take observations for firm n for all time periods)
            𝒟^(k) = 𝒟^(k) ∪ 𝒟_n (Add all firm observations to sample)
        end for
    end for
    We run IE regression using 𝒟^(k) and coefficients Θ^(k), clustering by firms n ∈ 𝒩
    β^(k) = estimated IE regression coefficients
    𝓑 = 𝓑 ∪ {β^(k)} (We collect IE regression coefficients)
end for
return 𝓑 and calculate summary statistics from {β^(k)}_{k=1}^{1000}
```