

Contract Completeness of Company Bylaws and Entrepreneurial Success

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Abstract

Does reducing the cost for entrepreneurs to write more complete contracts with their financiers enhance entrepreneurial success? To shed light on this question, this paper exploits a 2008 French reform that made it less costly for new firms to choose a legal form allowing more complete financial contracts in the company bylaws. Using comprehensive tax-filing data from 2004 to 2015, we find a marked increase in the adoption of that legal form among new firms, leading to higher growth in capital, labor, and revenues in the first three years after creation. The effects are more pronounced for firms with high marginal returns to capital, suggesting that capital misallocation decreases. Our findings highlight the significant role of legal and financial structures in entrepreneurial success, which has policy implications for promoting entrepreneurship.

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1 Introduction

Financial contract theory is rooted in the notion that contracts between entrepreneurs and investors affect firm value and that the ability to optimize financial contracting at the startup stage enhances firm performance. But to reap the full benefits of optimal contracting, entrepreneurs need to be able to write complex contracts with their financiers, including for instance state contingent allocation of cash flow rights and control rights. While evidence exists that sophisticated firms and financiers write complex contracts that resemble contracts predicted by theory in the special case of venture capital financing (Kaplan and Strömberg, 2003), little is known about the causal effects that such contracts have on firm performance.

In addition, it is well possible that such levels of complexity is not useful for the vast majority of (less sophisticated) entrepreneurs. Complex contracts may even be detrimental if they allow sophisticated investors to take advantage of entrepreneurs' behavioral biases (Landier and Thesmar, 2009).¹ In this case, regulations that restrict the contract space or provide boilerplate contracts can be beneficial.

This paper aims at answering the following questions: Does the ability to optimize financial contracting between entrepreneurs and investors at the startup stage matter for firm performance? If so, through which channel: Does it improve access to external financing? Does it lead to higher investment in capital and labor? Does it matter at the entry stage or for post-entry growth?

Studying the causal impact of the ability to optimize financial contracting on entrepreneurial success is challenging because the choice of writing complex contracts is endogenous. The ideal experiment would compare the performance of a set of firms that can optimize financial contracting to the performance of another set of firms that cannot optimize financial contracting.

In this paper, we study a reform in France that comes close to this ideal experiment. The reform reduced the cost of opting for a corporate legal form that allows startups to write more complete contracts in their company bylaws, while holding fixed the cost of another corporate legal form that restricts company bylaws to a boilerplate contract. This clean policy, combined with rich administrative panel data from tax-filings that provide detailed balance sheets and income statements for the universe of French corporations, allows us to study how lowering the cost of writing complex bylaws affects entrepreneurial success.

1. There are evidence that entrepreneurs display stronger behavioral biases like overconfidence and optimism, e.g., Puri and Robinson (2007), Hvide and Panos (2014), and Astebro, Herz, Nanda, and Weber (2014) and Kerr, Pekkala, and Xu (2018) for surveys.

Company bylaws determine the contractual arrangements between the shareholders of the firm (founders and external investors) regarding capital (e.g., which type of shares are issued, the requirements to sell existing shares and to issue new shares) and governance (e.g., composition and role of boards, voting rights). In France, startups can opt for one of two corporate legal forms that differ in terms of the contractual provisions that can be included in the bylaws of the firm. With the *restricted bylaws* legal form, the bylaws cannot deviate much from a boilerplate contract, which restricts the contracting space both in terms of the possible allocation of cash flow rights and control rights (e.g., no deviation from one-share-one-vote is possible) and in terms of the governance structure (e.g., there is no board). By contrast, with the *flexible bylaws* legal form, there are no restrictions on the financial contracts and governance rules that firms can use.² While until 2008 the flexible bylaws legal form was subject to extra regulatory requirements relative to restricted bylaws, the reform removed the extra requirements and aligned the regulations for both legal forms.

The reform led to a substantial take-up of the flexible bylaws legal form by new firms. The share of new firms that choose the flexible bylaws legal form increased from below 5% prior to the reform to 20% four years after the reform. Importantly, the flexible legal form does not provide other benefits than the ability to tailor write more complete contracts in the bylaws. Therefore, the large take-up of the reform implies that firms were constrained by the higher costs of incorporating with flexible bylaws before the reform and that the ability to write more complete contract is valuable for a sizeable fraction of firms.

The reform disproportionately affected certain industries. In addition, the take-up of the reform was concentrated on new firms, which decide which legal form to adopt when incorporating. By contrast, incumbent firms, which had already made an incorporation choice, were mostly unaffected. The asymmetry between the large take-up by newly created firms and the lack of response by incumbent firms allows us to estimate the effect of the reform by comparing cohorts of new firms to incumbent firms, in industries more exposed to the reform relative to less exposed industries. Therefore, while our identification strategy uses variation across industries, it allows us to control for unobserved industry-specific shocks that might be correlated with the reform, by comparing new and incumbent firms *within* industries.³

2. The restricted bylaws legal form is called *SARL* and the flexible bylaws one is *SAS*. Other legal forms exist but are either for sole proprietorships (*EI*) and for very large firms (*SA*). We exclude these other legal forms from the analysis.

3. In addition to increasing the adoption of flexible bylaws, the reform may also affect entrepreneurial success through another channel. If the regulatory requirements lifted by the reform affect firm outcomes, their removal would affect firm outcomes holding fixed the choice of legal form. In Section 4.1, we show that we can control for this effect and isolate the impact of flexible bylaws adoption by controlling for the pre-reform share of flexible bylaws

Our first set of results on the real effects of the reform show that three years after creation, new firms more exposed to the reform are more likely to be in the top quartile, quintile and decile of the sample distribution in terms of investment in capital and hiring of labor, while the effect is muted in the lower part of the distribution. This non-linear effect is consistent with the notion that flexible bylaws are adopted by entrepreneurs whose growth ambition is in the right tail of the distribution and that allowing entrepreneurs to write more complete financial arrangements with their financiers allows them to raise more equity. The positive effect on hiring is consistent with the notion that labor is not a variable cost but needs to be financed.⁴ The positive effect of the reform on capital and labor input also translate into higher revenues. Firms in more exposed industries exhibit higher equity growth, suggesting that they finance the accumulation of labor and capital by raising equity.

In line with our identifying assumption that our measure of exposure to the reform does not pick up industry-specific shocks, we find that in contrast to newly created firms, incumbent firms in more exposed industries do not outperform peers in terms of capital, labor, sales, or equity. We also provide graphical evidence that new firms and incumbent firms are on a similar trend in the years prior to the reform, and only start to diverge after the reform.

In our second set of results, we decompose the performance of new firms into size at entry versus faster capital accumulation and revenue growth post entry. We find that the reform has a positive impact on both margins. New firms more exposed to the reform starts large, and also to grow faster once they have entered, even after controlling for the difference in initial size at entry produced by the reform.

In our third set of results, we study if more flexible financial contracts not only increase entrepreneurial success, but can also be a source of lower misallocation of capital. To do so, we compute firms' marginal return to capital at entry, measured as the marginal revenue product of capital (MRPK) following the literature on misallocation (e.g., Hsieh and Klenow, 2009; Bau and Matray, 2023), and we study how the reform affects post-entry capital accumulation conditional on firms' marginal return to capital. We find that the reform increases the sensitivity of capital accumulation to the marginal productivity of capital, that is, new firms with high MRPK accumulate more capital post entry than new firms with low MRPK. This implies that capital misallocation is

firms interacted with time fixed effects.

4. E.g., among many others: Hombert and Matray, 2017; Bai, Carvalho, and Phillips, 2018; Caggese, Cunat, and Metzger, 2019; Benmelech, Frydman, and Papanikolaou, 2019; Fonseca and Doornik, 2022; Baghai, Silva, Thell, and Vig, 2021.

reduced.

Related literature. Our paper is related to the literature on the real effects of firms’ legal form. A strand of this literature focuses on the tax implications of different legal forms.⁵ Another strand studies the implications of the choice between incorporating and sole proprietorships.⁶ Other papers focus on specific provisions associated with the firm legal choice, in particular the effect of limited liability.⁷ We contribute to this literature on two dimensions. First, we focus on a different source of difference between legal forms, namely the possibility to write complex financial contracts with financiers. Second, in terms of identification, while most existing papers relies on ex-ante variation in the choice of legal form across firms, we use a policy-induced variation in the adoption of a specific legal form upon firm creation, to track its effects on entrepreneurial success in the medium run.

We also contribute to the literature showing that entry regulation inhibits entrepreneurship, and that policies that simplify procedures and cut the red tape required to create a business are effective at promoting entrepreneurship.⁸ One frequent component of such policies is the creation of boilerplate bylaws. For instance, in their study of the 2005 reform in Portugal, Branstetter, Lima, Taylor, and Venâncio (2014) document that “In order to increase the efficiency of the process, the [Office of Public Services and Reform] developed standardised pre-approved articles of association” (p.813).

Our contribution to this literature is to show that facilitating startups in accessing complex legal forms allowing them to write complex bylaws enhances the chances of entrepreneurial success, in particular at the top of the distribution. Therefore, we highlight the benefits of reducing the costs of setting up complex firms whereas the earlier literature emphasized the benefits of reducing the costs of setting up simple firms. Another implication of our work is that policies aiming at introducing standardized bylaws and contracts should not inadvertently restrict the contract space by suppressing the option for firms to write complex bylaws.

Finally, because the reform we study reduces the cost of financial contracting, our paper contributes to the broader literature on the effect of financing frictions on entrepreneurship and en-

5. This literature has mostly focused on the difference between C-corp and S-corp in the US (Goolsbee, 2004; Elschner, 2013; Yagan, 2015; Chen, Qi, and Schlagenhauf, 2018; Giroud and Rauh, 2018; Tazhitdinova, 2020; Bilicka and Raei, 2023; Allen, Allen, Raghavan, and Solomon, 2023) although recent papers have studied other countries such as Greece (Bilicka, Guceri, and Koumanakos, 2022) and France (Matray, 2023).

6. E.g., Levine and Rubinstein (2016), Astebro and Tag, 2017, and Bellon, Cookson, Gilje, and Heimer (2021).

7. E.g., Akey and Appel (2021).

8. E.g., Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2002), Branstetter, Lima, Taylor, and Venâncio (2014), Amici, Giacomelli, Manaresi, and Tonello (2016), Gregg (2020), and Guzman (2020).

trepreneurial success.⁹

2 Company Bylaws and the 2008 Reform

2.1 Flexible Bylaws versus Restricted Bylaws

The most common legal forms for French small and medium-sized companies are SARL, which stands for *Société à Responsabilité Limitée*, i.e., Limited Liability Company; and SAS, which stands for *Société par Actions Simplifiée*, i.e., Simplified Joint-Stock Company. The qualifier “Simplified” in the SAS name is to be understood relative to the more complex *Société Anonyme*, i.e., Joint-Stock Company, which is the mandatory legal form for publicly listed companies and which we will exclude from our analysis.

The main difference between SARL and SAS is the set of features that can be written in the bylaws of the firm. SARL restricts the set of contractual features and governance rules that can be written in the bylaws. By contrast, SAS imposes almost no restrictions. In the rest of the paper, we refer to SARL as the *restricted bylaws* legal form and to SAS as the *flexible bylaws* legal form. The bylaws determine the contractual arrangements between the shareholders of the company in two main areas: (a) capital—which type of shares can be issued, the conditions under which shares can be traded, rules to issue new shares, etc.; and (b) governance—creation of boards and rules governing their functioning, voting rights, and majority required for different decisions, etc. While firms with the flexible bylaws legal form (hereafter, *flexible bylaws firms*) can freely tailor these provisions to their needs, firms with the restricted bylaws legal form (hereafter, *restricted bylaws firms*) face a limited choice set.

To show that the possibility to tailor the bylaws is actually used by flexible bylaws firms, we download the bylaws of a random sample of 7,118 firms started between 2004 and 2007 from the business registry open data.¹⁰ We detect the presence of contractual provisions in the bylaws based on the presence of certain sequences of words in the bylaws, complemented by extensive manual validation of the classification in this small sample of startups. We focus on the provisions that are mentioned as particularly important in specialized websites and in our private discussions with law practitioners.

9. E.g., Kerr and Nanda (2010), Adelino, Schoar, and Severino (2015), Hombert, Schoar, Sraer, and Thesmar (2020), Adelino, Ma, and Robinson (2017), Kerr, Kerr, and Nanda (2022), and Jensen, Leth-Petersen, and Nanda (2022).

10. We extract the data using the API of the open data public initiative INPI at <https://www.inpi.fr/en/>.

1. *Multiple share classes.* Restricted bylaws firms can only issue one class of shares, ruling out differential cash flow rights and liquidation rights among shareholders and deviations from one-share-one-vote. By contrast, flexible bylaws firms are unrestricted in their ability to issue multiple classes of shares, such as shares with preferred dividends, multiple voting rights, convertibility options, etc. In our random sample of bylaws, we find that 9.6% of flexible bylaws firms started between 2004 and 2007 use the possibility to issue multiple classes of shares.
2. *Share sale restrictions.* In restricted bylaws firms, when a shareholder wants to sell some or all of their shares, the sale must be approved by a majority of shareholders. The required majority can be tailored to some extent and written in the bylaws, but it must be no less than a two-thirds majority, and it cannot be unanimity. By contrast, in flexible bylaws firms, the shareholders decide and write in the bylaws whether the sale of shares is free or subject to shareholder approval. In the latter case, the required approval can be flexibly tailored. For instance, it can be different for different shareholders. In our random sample of bylaws, 5.1% of flexible companies startups opt to have freely tradable shares. A common provision that restricts share sales is an inalienability period during which entrepreneurs or outside investors are not allowed to sell their shares. The inalienability period can be up to 10 years long. In our random sample of bylaws, 51% of flexible bylaws startups have an inalienability provision for the sale of shares with an average length of 3.8 years.
3. *Governance.* The governance structure of restricted bylaws firms is limited to two governance bodies: one or multiple managing directors, and the general assembly of shareholders. No boards of directors or other committees can be created as part of the governance structure. By contrast, the governance of flexible bylaws firms is unrestricted, potentially including one or multiple boards whose role and composition can be freely specified in the bylaws. The governance structure of flexible bylaws firms may also include executive directors, whose roles and powers are specified in the bylaws. The only requirement is that the firm must have a CEO.

Except for the possibility to tailor the bylaws and the changes described in the next section, both types of firms face the same set of regulations and taxes during the sample period. In particular, both legal forms grant limited liability to all shareholders, and neither imposes a minimum number of shareholders. Both types of firms pay an entity-level corporate tax similar to U.S. C-corps, and

face the same tax schedule during the sample period.¹¹

2.2 The Reform

The Economy Modernization Act was promulgated on August 5th, 2008, by the French Parliament, one year after right-wing candidate Nicolas Sarkozy was elected President and his party won a majority in Parliament. The stated goal of the reform was to “stimulate growth and employment by removing the structural and regulatory barriers hindering the French economy.” The reform included several provisions that reduced the cost for entrepreneurs of opting for the flexible bylaws legal form.¹²

The main change introduced by the reform was the removal of the requirement for flexible bylaws firms to have at least 37,000 euro of share capital. Share capital at the creation of a company is equal to the total amount of equity injected by shareholders.¹³ Therefore, until 2008, entrepreneurs opting for flexible bylaws had to come up with at least 37,000 euros of inside plus outside equity. The reform removed this requirement. This change aligns requirements for flexible bylaws firms with those for restricted bylaws firms, which do not have a minimum equity requirement during the sample period. Evidence that the equity requirement for flexible bylaws firms was binding is that four years after the reform, 90% of new flexible bylaws firms are started with less than 37,000 euros of equity.

The reform introduced two other changes that also reduced the cost of the flexible bylaws legal form. Before the reform, all flexible bylaws firms were required to hire external auditors. After the reform, the requirement only applies to firms above a certain size threshold.¹⁴ This change aligns the regulation of flexible bylaws firms with that of restricted bylaws firms, which are subject to the same size threshold for the requirement to hire external auditors. The reform also harmonizes the tax rate on the proceeds from the sales of shares for both types of firms, which was higher for flexible bylaws firms than for restricted bylaws firms before the reform.¹⁵

11. There is a French equivalent of U.S. S-corps, but unlike in the U.S., this legal form is restrictive and mostly limited to unincorporated sole proprietorships, which we do not consider in this paper.

12. The new Act introduced other changes unrelated to flexible bylaws companies: it lifted restrictions on the opening of supermarkets; it imposed limits on payment delays; it simplified the procedure against unfair contract terms; it created the auto-entrepreneur legal form that allows employees to engage in side self-employed activities.

13. Over the life of the company, share capital can be increased by new equity injection or reduced by share buybacks. Retained earnings can also be used to increase share capital. When retained earnings are negative and go below a negative threshold, negative earnings must be debited from share capital.

14. The requirement to hire external auditors applies if at least two out of the following three conditions are met: sales above two million euro; total asset above one million euro; more than 20 employees.

15. The tax rate increased from 1.1% to 3% for flexible bylaws companies while it decreased from 5% to 3% for restricted bylaws companies.

2.3 Take-up of Flexible Bylaws

By reducing the costs of choosing the flexible bylaws legal form, the reform potentially increases entrepreneurs' propensity to adopt that legal form, enabling them to use more complex financial contracts and governance rules. In principle, the reform may affect firms' choice of legal form at any point in their life cycle: at entry, when entrepreneurs select a legal form upon incorporation, but also post entry, as firms are allowed to change their legal form. In practice, however, firms rarely change legal form after entry.

Figure 1 shows the fraction of flexible bylaws firms among newly incorporated firms, and the fraction of flexible bylaws firms among incumbent firms, defined as firms aged between 5 and 10 (see Section 3.2 for the description of the sample). While less than 5% of new firms adopt flexible bylaws prior to the 2008 reform, this fraction increases sharply after the reform and nearly reaches 20% in 2012.

Why not all new firms opt for flexible bylaws if the contract space of restricted bylaws is a subset of the contract space of flexible bylaws? A natural explanation is that writing a contract in a larger contract space is more costly. This may involve cognitive and legal costs. More importantly, a larger and more complex contract space opens the door to mistakes and exploitation of unsophisticated entrepreneurs by more sophisticated co-founders or investors. The boilerplate contract of restricted bylaws protects entrepreneurs against this risk. Private conversation with lawyers and specialized websites emphasizing the simplicity of restricted bylaws are consistent with this interpretation.¹⁶

By contrast, the fraction of incumbents with flexible bylaws exhibits a much more stable pattern. The lack of response by incumbent firms is consistent with previous results. For instance, Gordon and MacKie-Mason (1994) and Giroud and Rauh (2018) find little shifting of legal form between C-corps and S-corps in the U.S. in response to differential tax rates, and Matray (2023) find a similar lack of response between SAS and SARL in France following a large increase in the dividend tax rate of SARL firms.

The large response on the extensive margin coupled with limited response on the intensive margin can be explained by the presence of costs of switching legal form. These costs include administrative costs (convening a general assembly to approve the change in legal form and filing the new bylaws with the firm register), monetary costs (hiring a lawyer to write the new bylaws), and potential legal

16. See for instance <https://www.captaincontrat.com/creer-son-entreprise/choisir-son-statut/sarl-ou-sas-comment-choisir-sa-societe>: “if you don't always want to grow your business, the legal constraints of the [restricted bylaws legal form] can be a source of comfort, and its operation is simpler, if not more tailor-made.”

risks.¹⁷ Organizational inertia might be an additional reason why firms rarely change legal form. Irrespective of the reason, the stickiness of the legal form implies that incumbent firms' choice of legal form is unaffected by the reform, which we also confirm in a difference-in-differences framework in Section 4.3.

Our empirical strategy builds on the large response of new firms to the reform combined with the muted response by incumbents. We use the reform as a shock to new firms' propensity to adopt flexible bylaws, while incumbents constitute a useful control group because they are not affected by the reform.

3 Data

3.1 Data Sources

Business registry. We retrieve information about business creation from the business registry. It contains the list of new businesses with the date of creation, legal form, and industry at creation.

Tax files. We retrieve firm accounting information from tax files (data set FICUS-FARE) for the years 2004 to 2012. The data contain income statement and balance sheet information collected by the Treasury for the universe of firms in the French economy. This information is used to determine tax liabilities and is audited by the tax authority with significant penalties applied in the case of misreporting, which guarantees the high quality and accuracy of the data.

The tax files use the industry classification NAF, which corresponds one-for-one to the European NACE classification. We use version rev.2, 2008 of NAF because it is time-invariant over our sample period 2004–2012.¹⁸

CEO demographics and wage. We retrieve information on CEOs from two matched employer-employee data sets assembled by the Statistical Office based on mandatory employer reports of employees' gross earnings to Social Security. The first data set (DADS) covers all workers subject to contributions to the Social Security "general regime", which includes all employees receiving a wage but excludes owner-managers of SARL firms who are subject to the Social Security "independent

17. The monetary cost can represent around 10% to 25% of the firm net income in our sample of firms. The legal risk arises from the fact that the law prohibits a change in legal form that is only motivated by the intention to escape or reduce social security contributions (Article L243-7-2 of Social Security Code).

18. NAF rev.2, 2008 was introduced in 2008, and the Statistical Office backfilled the industry classification for the years back to 2003.

regime”. The second data set (ACOSS) covers owner-managers of SARL firms.

We first match the firms in the business registry and in the tax files with ACOSS files, since, by definition, one observation in ACOSS corresponds to a CEO. For firms we cannot match with ACOSS, we identify the CEO in DADS using the occupation code corresponding to the CEO (one-digit occupation code equal to 2). If a firm neither appears in ACOSS nor reports employees with CEO occupation in DADS, we identify the CEO as the firm’s employee with the highest wage in DADS. This procedure allows us to identify one CEO per firm-year observation. We retrieve the wage, age, and sex of the CEO.¹⁹

3.2 Sample

We restrict the sample to firms with restricted bylaws legal form (SARL) and flexible bylaws legal form (SAS), and exclude sole proprietorships (*Entreprises Individuelles*), partnerships (*Société en Nom Collectif*), joint-stock companies (*Société Anonyme*), and legal forms used for noncommercial activities. We exclude sole proprietorships and joint-stock companies because the variation used by our identification strategy comes from entrepreneurs who are nudged into the flexible bylaws legal form by the reform, and the legal form these entrepreneurs would have chosen absent the reform is likely to be restricted bylaws. Indeed, legal forms can be ranked in terms of increasing complexity and setup costs as follows: sole proprietorship, restricted bylaws, flexible bylaws, joint-stock company. Therefore, by reducing the cost of flexible bylaws, the reform should affect the margin between restricted bylaws and flexible bylaws but not the other margins.

We construct a sample of new firms and a sample of incumbent firms. The sample of new firms is composed of firms that started between 2004 and 2012. The first observation in the tax files for a new firm may be in the year of incorporation or the year after, depending on the end date of the firm’s first fiscal year. For consistency, we always use the information in the year following the year of incorporation to define firm outcomes at entry, even if the firm has reported information for the year of incorporation.

We remove from the sample new firms that report information only in the year of incorporation and then exit the tax files. We exclude firms started with 50 employees or more (0.2% of new firms) because these firms may be subsidiaries of large corporations and may thus behave like large, mature firms rather than like new firms.

19. ACOSS files are available starting from 2006 while our sample period starts in 2004. For the firms that have a match in ACOSS, we backfill the information from 2006 to 2004 and 2005.

For each year from 2004 to 2012, the sample of incumbent firms is composed of firms aged between five and ten in that year.²⁰ We impose a minimum incumbent age of five to ensure that the incumbent sample does not contain firms started after the reform (the reform is in 2008, and the last year of the sample is 2012). We impose a maximum incumbent age of ten to increase the comparability between new firms and incumbent firms.

For both samples, we exclude, agriculture and extraction (NAF code 1-999), financial firms (NAF code 6400–6999), headquarter companies (NAF code 7010–7019), utilities (NAF code 3500–3999), and activities related to social services and public administration (NAF code 8200-9999), as well as firms that report missing or negative total assets. We detail the construction of the different variables in Appendix A.

4 Empirical Design

4.1 Econometric Framework

Our objective is to estimate the effect of adopting flexible bylaws relative to restrictive bylaws on firms’ outcomes. By increasing firms’ propensity to adopt flexible bylaws, the reform provides a natural experiment to estimate this effect.

We outline a simple econometric framework to determine the conditions under which the reform provides a valid natural experiment. We assume that the reform’s removal of requirements for flexible bylaws companies reduces the cost of adopting flexible status, which will increase flexible status adoption. The removal of requirements may also have a direct effect on firms’ outcomes, conditional on adopting flexible bylaws. This latter effect is a confounding effect that we need to control for.

Specifically, suppose that firm i ’s potential outcome if it adopts restricted bylaws is

$$Y_i^R = u_i. \tag{1}$$

The potential outcome if the firm adopts flexible bylaws and flexible bylaws companies are subject

20. We use the date of creation as recorded in the Business registry. The date of creation corresponds to the date of registration in the *Registre du Commerce et des Sociétés* (“Trade and Companies Register”). For 0.4% of firms, we observe tax files before the date of creation, in which case we take the minimum year for which we observe tax files as date of creation. To avoid left censoring, we put that date of creation as missing if the minimum year corresponds to the first sample year.

to the same requirements as restricted bylaws companies (i.e., after the reform) is

$$Y_i^{F,\emptyset} = u_i + v_i. \quad (2)$$

Equations (1) and (2) allow firms to be heterogeneous regarding the outcome unconditional on the legal form (u_i) and regarding the effect of adopting flexible bylaws (v_i). It is natural to think of u_i and v_i as having a positive correlation because more productive firms may need more complex bylaws to allow them to raise financing and grow faster, but we make no assumption on the covariance between u_i and v_i . Our objective is to estimate the impact of flexible bylaws on firm outcome, i.e., the (average) value of v_i .

The potential outcome if the firm adopts flexible bylaws and flexible bylaws companies are subject to the additional requirements (i.e., before the reform) is

$$Y_i^{F,req} = u_i + v_i + \lambda. \quad (3)$$

λ represents the effect of imposing additional requirements for flexible bylaws companies, conditional on adopting flexible bylaws. λ may be positive or negative.

Suppose the entrepreneur incurs a cost $\gamma > 0$ to adopt flexible bylaws when flexible bylaws companies are not subject to the additional requirements. γ includes cognitive and legal costs of writing bylaws in a larger contract space than restricted bylaws. When flexible bylaws companies are subject to additional requirements, the cost of adopting flexible bylaws is $\gamma + \delta$. The additional cost $\delta > 0$ includes the cost of the minimum startup equity requirement, which arises if issuing outside equity is costly or constrained because of moral hazard (Jensen and Meckling, 1976) or adverse selection (Myers and Majluf, 1984). The equity requirement can also be costly if it forces entrepreneurs to inject more inside equity and thus bear more risk than is optimal.

Each firm adopts flexible bylaws if the benefits of flexible bylaws are greater than the costs. We denote by F_i^\emptyset and F_i^{req} the dummy variables equal to one if firm i adopts flexible bylaws when flexible bylaws companies are not subject to the additional requirements and when they are, respectively. Firms' optimal choice of legal form implies:

$$F_i^\emptyset = 1\{v_i > \gamma\}, \quad (4)$$

$$F_i^{req} = 1\{v_i + \lambda > \gamma + \delta\}. \quad (5)$$

Given the evidence that the removal of the additional requirements for flexible bylaws led to a large increase in adoption of flexible bylaws, we assume that $\delta > \lambda$.

Firm i 's outcome for given requirements for flexible bylaws companies $Z \in \{\emptyset, req\}$ is

$$Y_i^Z = (1 - F_i^Z)Y_i^R + F_i^Z Y_i^{F,Z}. \quad (6)$$

Taking expectation over (u_i, v_i) , the population average outcome for given requirements Z is

$$E[Y_i^Z] = E[Y_i^R] + E[F_i^Z(Y_i^{F,Z} - Y_i^R)]. \quad (7)$$

Therefore, following standard manipulations, the impact of removing requirements on the average outcome is

$$\begin{aligned} E[Y_i^{\emptyset}] - E[Y_i^{req}] &= E[F_i^{\emptyset} - F_i^{req}] \times E[Y_i^{F,\emptyset} - Y_i^R | F_i^{\emptyset} - F_i^{req} = 1] \\ &\quad + E[F_i^{req}] \times E[Y_i^{F,\emptyset} - Y_i^{F,req} | F_i^{req} = 1]. \end{aligned} \quad (8)$$

Equation (8) shows that the reform has two effects on the average outcome. The term on the first line is the impact of the increase in flexible bylaws adoption. It is equal to the product of the increase in flexible bylaws adoption ($E[F_i^{\emptyset} - F_i^{req}]$) and the effect of flexible bylaws on compliers, i.e., on firms that adopt flexible bylaws if requirements are removed but not if requirements are in place ($E[Y_i^{F,\emptyset} - Y_i^R | F_i^{\emptyset} - F_i^{req} = 1]$). The term on the second line is the impact of removing requirements conditional on adopting flexible bylaws. It is equal to the product of the share of flexible bylaws companies when requirements are in place ($E[F_i^{req}]$) and the effect on removing requirements for flexible bylaws on firms that adopt flexible bylaws when requirements are in place ($E[Y_i^{F,\emptyset} - Y_i^{F,req} | F_i^{req} = 1]$).

Suppose the distribution of potential firm outcomes (u_i, v_i) differs across industries, indexed by j . For instance, suppose that in each industry j , the benefits of flexible bylaws $v_i = Y_i^{F,\emptyset} - Y_i^R$ follows a Pareto distribution with scale parameter $\mu_j \in (0, \gamma)$ and shape parameter $\alpha > 1$. Therefore, the increase in flexible bylaws adoption induced by the reform, $Exposure_j \equiv E[F_i^{\emptyset} - F_i^{req}]$, varies across industries.²¹ Intuitively, the share of flexible bylaws companies increases more in industries where many firms benefit from flexible bylaws just enough to be nudged into flexible bylaws when

21. The change in the share of flexible bylaws companies is equal to $E[F_i^{\emptyset} - F_i^{req}] = P[\gamma < v_i < \gamma + \delta - \lambda] = \mu_j^\alpha (\gamma^{-\alpha} - (\gamma + \delta - \lambda)^{-\alpha})$, which is increasing in μ_j .

requirements are removed but not when requirements are in place. Therefore, exposure to the reform varies across industries.

While the average benefits of flexible bylaws, $E[v_i]$, varies across industries, the average benefits *for compliers*, $LATE \equiv E[v_i | F_i^\emptyset - F_i^{req} = 1]$, does not. The reason is that while the mass of compliers vary across industries, compliers in all industries lie in the same range of value of v_i .²² Similarly, flexible bylaws adoption before the reform, $FlexiblePreShare_j \equiv E[F_i^{req}]$, varies across industries, but the effect of removing requirements for flexible bylaws companies conditional on adopting flexible bylaws, $DIRECT \equiv E[Y_i^{F,\emptyset} - Y_i^{F,req} | F_i^{req} = 1]$, does not. Therefore, equation (8) can be rewritten

$$E[Y_i^\emptyset] - E[Y_i^{req}] = Exposure_j LATE + FlexiblePreShare_j DIRECT. \quad (9)$$

Equation (9) implies that we can estimate the effect of flexible bylaws (LATE) by regressing the change in firm outcome on $Exposure_j$ and controlling for $FlexiblePreShare_j$, where $Exposure_j$ is the change in the share of flexible bylaws among new firms in the post-reform period relative to the pre-reform period, and $FlexiblePreShare_j$ is the share of flexible bylaws new firms in the pre-reform period.

4.2 Specification

We estimate

$$Y_{i,j,t \rightarrow t+h} = \beta Exposure_j \times Post_t + \delta_{j2,t} + \alpha_j + \mathbf{X}_{i,t} + \epsilon_{i,j,t} \quad (10)$$

where the LHS variable is an outcome variable measured in year $t+h$ for firm i started in year t in five-digit industry j . As explained in Section 4.1, $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws among new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. In some specifications, we replace $Post_t$ with a collection of year dummies to estimate dynamic effects. α_j denotes industry fixed effects and absorbs time-invariant differences across industries.

An identification challenge when using variations across industries is that industries differ along multiple dimensions. We limit this issue by systematically including $\delta_{j2,t}$, which is a vector of two-digit industry \times year fixed effects. This collection of fixed effects ensures that the coefficient of interest

22. The expected benefits of flexible bylaws for compliers is equal to $E[v_i | \gamma < v_i < \gamma + \delta - \lambda] = \frac{\alpha}{\alpha-1} \gamma (\gamma + \delta - \lambda) ((\gamma + \delta - \lambda)^{\alpha-1} - \gamma^{\alpha-1}) / ((\gamma + \delta - \lambda)^\alpha - \gamma^\alpha)$, which does not depend on μ_j .

β is estimated by using only variation *within* two-digit industries.²³ This implies, for instance, that within two-digit industry Chemical Industries (two-digit code 20), we compare five-digit industries Industrial Gases (five-digit code 20.11.1) with Paints and Varnishes Based on Polymers (five-digit code 20.30.1), but we do not compare these five-digit industries with those in different two-digit industries such as Manufacture of Basic Pharmaceutical Products and Pharmaceutical Preparations (two-digit code 21).

$\mathbf{X}_{i,t}$ is a vector of additional firm-level controls measured at time t , which systematically includes $FlexiblePreShare_j \times Post_t$, where as explained in Section 4.1 $FlexiblePreShare_j$ is the share of flexible bylaws new firms in the pre-reform period. In some specifications, we also include county \times year fixed effects, and multiple CEO characteristics interacted with year fixed effects to account for potential changes in the pool of entrepreneurs. The CEO characteristics include demographic information (sex interacted with age quintiles) as well as quintiles of earnings in the first year as CEO. The inclusion of CEO earnings allows us to control for CEO productivity, although this control might be subject to a bad control problem if CEO earnings are endogenous to firm performance. For this reason, we only include the specification with CEO earnings as a control as an additional robustness test but not in our baseline specification. We cluster standard errors at the level of the shock, that is, by industry.

Identifying assumptions. The identification assumption required to move from (9) to (10) is the usual parallel trend assumption that industry-specific shocks to outcomes in the post-reform period are not correlated with $Exposure_j$. This assumption would be violated if industries more exposed to the reform also experience demand shocks (e.g., changes in export patterns) or supply shocks (e.g., changes in credit supply) around the years of the reform that correlate with the exposure to the reform. For instance, this could happen if the growth in flexible bylaws adoption reflects not only the exposure to the reform but also industry growth opportunities.

We use two complementary strategies to validate that the parallel trend assumption holds. First, we show that firms created in treated industries and in control industries evolve in a parallel fashion along multiple outcomes (capital, labor, revenues) during the years leading up to the reform. This allows us to check that industries more exposed to the reform do not systematically covary more with the business cycle, but does not rule out that industry-specific shocks happen exactly right after the reform is adopted.

23. There are 544 distinct five-digit industries and 51 distinct two-digit industries.

To deal with this more challenging threat to identification, we exploit the fact that while new firms respond strongly to the reform by adopting flexible bylaws when they incorporate, incumbent firms do not. This feature of the empirical setting allows us to use incumbents that belong to the same industry as the newly created firms as an additional control group. If industry-specific shocks affect new firms and incumbent firms similarly, the following triple-difference regression estimated on the stacked sample of new firms and incumbent firms differences out time-varying unobserved industry-specific shocks:

$$Y_{i,j,t \rightarrow t+h} = \beta \text{Exposure}_j \times \text{Post}_t \times \text{New}_i + \gamma_{j,t} + \delta_{j_2,t} \times \text{New}_i + \alpha_j \times \text{New}_i + \mathbf{X}_{i,t} + \epsilon_{i,t} \quad (11)$$

where New_i is a dummy equal to one if firm i is a firm started in year t . For incumbents, the LHS variable is an outcome measured in changes from year t to year $t + h$, which is the analog of an outcome in year $t + h$ for a firm started in year t . Because we now have variation *within* five-digit industry-year (between new and incumbent firms), we can include five-digit industry \times year fixed effects $\gamma_{j,t}$, which absorb *all* unobserved, time-varying shocks at the industry level.²⁴

4.3 Flexible Bylaws Adoption

We start by showing that while our main proxy of exposure to the reform predicts an increase in new firms' adoption of the flexible bylaws legal form, it does not affect incumbent firms' choice of legal form. To do so, we estimate equation (10) separately for new firms and for incumbents, where the dependent variable is a dummy equal to one if the firm chooses the flexible bylaws legal form. In Figure 2, we replace the post dummy with year dummies to estimate the event study relative to 2007, the year prior to the reform. There is no pre-trend for new firms or for incumbents. The effect of the reform on flexible bylaws adoption by new firms starts building up immediately after the reform in a way that closely mirrors the aggregate take-up in Figure 1. By contrast, incumbent firms do not react to the reform.

The increase in flexible bylaws adoption by new firms is mechanical since exposure is constructed as the change in the share of flexible bylaws firms among new firms from before the reform to after the reform. What is *not* mechanical is the absence of pre-trend, the timing of the effect post-reform, and the lack of effect on incumbents. These three patterns are consistent with a causal effect of the reform on the choice of legal form at incorporation. A concern could be that the surge in flexible

²⁴ $\text{Exposure}_j \times \text{Post}_t$ is not included because it is absorbed by $\gamma_{j,t}$, and $\text{Post}_t \times \text{New}_i$ is not included because it is absorbed by $\delta_{j_2,t} \times \text{New}_i$.

bylaws adoption is explained by reduced access to credit in the wake of the 2008-09 financial crisis, inducing new firms to rely more on external financing for which flexible bylaws are better suited. However, the joint dynamics of flexible bylaws adoption and credit growth do not fit with this explanation: in 2010, aggregate credit grows robustly as the effects of the 2008-9 subside while the take-up in flexible bylaws keeps on increasing sharply.²⁵

Table 1 reports the results using the post dummy and including additional controls. Entrepreneurs who start their business in more exposed industries are more likely to choose the flexible bylaws legal form after the reform (column 1). The effect is quantitatively unchanged when we control for geographical shocks (column 2) and for potential changes in the composition of entrepreneurs (column 3).

By contrast, entrepreneurs who had already registered their company prior to the reform are not more likely to change the legal form after the reform (columns 4 to 6). The fact that the exposure to the reform does not affect the legal form of incumbent firms validates the use of incumbent firms in our triple-difference setting to filter out industry-level shocks that might correlate with the effect of the reform. Indeed, under the assumption that such industry-specific shocks affect new firms and incumbents in the same way, the within-industry difference between new firms and incumbents identifies the effect of flexible bylaws, net of industry shocks.

4.4 Firm Creation

The reform may affect the average outcomes of new firms because flexible bylaws adoption increases (as shown in the previous section), and flexible bylaws affect outcomes. This is the mechanism we want to identify. The reform may also affect average outcomes of new firms if it increases the number of entrepreneurs who start a firm, and, as a result, modifies the composition of entrepreneurs who start up. This later channel is a priori unlikely because restricted bylaws firms are always simpler and cheaper to set up than flexible bylaws firms, both before and after the reform. Therefore, by reducing the cost of starting up with flexible bylaws but keeping the cost of starting up with restricted bylaws unchanged, the reform should affect the intensive margin, i.e., the choice between restricted bylaws and flexible bylaws conditional on starting up, but not the extensive margin, i.e., it should not affect the decision to create a firm.

Nonetheless, we test whether the number of new firms changes after the reform. We estimate

25. Source: Banque de France website <https://webstat.banque-france.fr/en/#/node/5384959> series “New loans to domestic NFC up to EUR 1 million, monthly flows cumulated over one year” grows by 12.2% from December 2009 to December 2010.

equation (10) where the dependent variable is the log number of new firms at the industry-year level.²⁶ Since the regression is at the industry level, OLS estimates will attribute an equal weight to each industry, while firm-level regressions attribute more importance to more populated industries. To check that our results are not driven by the choice of weighting, we report the industry-level regression of firm creations using two different weighting scheme: unweighted (OLS), which overweights small industries relative to the firm-level regressions; and weighted by the number of new firms (WLS), which weights industries similarly to the firm-level regressions.

Table 2 reports the results. With both weighing schemes, the reform has no effect on the number of firm starts. Composition effects should therefore be muted, allowing us to interpret the effect of the reform on average firm performance as the effect of allowing entrepreneurs and shareholders to write more complex contracts.

5 Impact of Flexible Bylaws on Entrepreneurial Success

We now study the impact of the reform on entrepreneurial success. To measure the success of new firms, we create dummy variables that equal one if a firm started in year t is above a certain percentile of the distribution for different outcomes (capital, labor, revenues) in year $t + h$. We use five different threshold percentiles to define the success dummy: top half, top tercile, top quartile, top quintile, and top decile. We define percentiles every year to control for aggregate trends in the distribution of firm outcomes. If the firm has exited by year $t + h$, we set the outcome variable to zero. Using a dummy variable to measure success has several advantages. First, it mitigates the role of outliers in a simple and transparent way. Second, it accommodates outcomes equal to zero when firms exit, without creating the large variation around zero arising with log transformations that can produce biased estimates (Cohn, Liu, and Wardlaw, 2022). Third, we can vary the percentile used to define success to study non-parametrically which range of outcomes is affected by the reform.

For incumbent firms, we define firm performance as the growth rate of the same outcome variables from year t to year $t + h$. Growth from t to $t + h$ for an incumbent is a natural equivalent to the level in $t + h$ for a firm started in t , because the level in year $t - 1$ for the new firm is zero by definition. We define success for incumbent firms in the same manner as for new firms by constructing dummies for growth in the top percentile of the distribution. Our main specification

26. We use the inverse hyperbolic sine transformation of the log function defined as $\log(X + (X^2 + 1)^{1/2})$ to handle the few cells where we have zeros. Except for low values of X , the inverse hyperbolic sine is approximately equal to $\log(X) + \log(2)$, and so it can be interpreted as a standard logarithmic dependent variable.

focuses on success at a $h = 3$ year horizon. In Section 5.4, we study the dynamics within the first three years. In Appendix B.1, we show that the results are robust to extending the horizon to four and five years.

In all regressions with firm outcomes as the dependent variable, we normalize $Exposure_j$ by the effect of the reform on the probability of new firms to adopt flexible bylaws, i.e., by the coefficient on $Exposure_j$ in the regression with the same set of controls and with flexible bylaws adoption for new firms as the dependent variable (Table 1, columns 1 to 3 depending on the controls included). Therefore, the coefficient on normalized $Exposure_j$ in the regressions for firm performance can be interpreted as the effect of adopting flexible bylaws on firm performance.

5.1 Capital

By expanding the set of contracts that entrepreneurs and investors can write, flexible bylaws may affect entrepreneurs' ability to finance capital expenditures. We first focus on firm success measured using the dummy equal to one if the firm is in the top quintile of total capital (or growth of total capital for incumbents), which includes property, plants, equipment, and intangible capital. We estimate equation (10) separately for new firms and for incumbents. We replace the post dummy with year dummies to estimate the event study relative to 2007, the year prior to the reform.

In panel (a) of Figure 3, we find that the reform is followed by a sharp increase in the share of new firms that reach the top quintile of capital accumulation in exposed industries. By contrast, there is no effect on incumbent firms. Since the reform triggers an increase in flexible bylaws adoption by new firms but not by incumbents (see Section 4.3), the pattern is consistent with a causal impact of flexible bylaws on firm capital accumulation. In panel (b) of Figure 3, we estimate the triple-difference equation (11), which differences out the effect on incumbents from the effect on new firms. This allows us to control for all the industry-specific shocks that might correlate with exposure to the reform. Consistent with the lack of effect on incumbents in panel (a), the effect on new firms is barely affected and remains significant.

Table 3 reports the results using other threshold percentiles of firm capital to define firm success, and uses the post dummy instead of the year dummies. Each panel corresponds to a different threshold percentile. For instance, the top quintile dummy corresponds to capital above 90 thousand euro three years after creation. Column 1 shows the results of the difference-in-differences equation (10) for new firms. We find that the reform increases success at the top of the distribution. The probability that a new firm reaches the top quintile of capital accumulation increases by

17 percentage points. Since exposure to the reform is normalized by the regression coefficient of the first stage, the estimate can be interpreted as the local average treatment effect of adopting the flexible bylaws legal form. To further gauge the magnitude of the effect, we can compare it to the unconditional probability of reaching the top quintile of the capital distribution for flexible bylaws startups in the pre-reform period, which is equal to 48%. Therefore, the probability of reaching the top quintile increases by $0.17/0.48 = 36\%$.

The effect is muted at lower levels of success. The effect on the probability of reaching above-median capital accumulation is not significant (panel (a) column 3). The lack of effect at lower levels of success is consistent with the notion that flexible bylaws are adopted by firms whose growth ambition is in the right tail of the distribution and that aim at accumulating more capital. Even after the reform, the share of startups that choose flexible bylaws does not exceed 20% (Figure 1), consistent with the well-documented fact that most startups have low growth ambition (Hurst and Pugsley, 2015). Therefore, it is not surprising that the reform has no effect on the probability of reaching above-median capital accumulation.

As discussed in Section 4.1, a standard identification threat is that the exposure to the reform is correlated with industry-specific shocks to entrepreneurial success. Since incumbent firms do not adjust their legal form in response to the reform (Section 4.3), we can run a placebo test using incumbent firms. Consistent with our identifying assumption, we find that in contrast to newly created firms, incumbent firms in industries more exposed to the reform are on the same growth trajectory as incumbent firms in less exposed industries (column 2). This implies that we can interpret the coefficient in column 1 as the causal effect of entrepreneurs' higher propensity to adopt flexible bylaws on firm growth.

In column 3, we stack both samples and estimate the triple-difference regression (11) interacting all the right-hand side variables, including controls and fixed effects, with the dummy *New firm* equal to one for new firms. This specification allows us to test the statistical significance of the difference between columns 1 and 2. In column 4, we include industry \times year fixed effects to directly control for time-varying unobserved shocks across industries. In this case, the coefficient on *Exposure* \times *Post* is no longer identified. In both specifications, the difference between new firms and incumbent firms is statistically significant at all levels of success from top tercile to top decile.

We have already shown that the reform did not change the number of startups (see Section 4.4), so the reform is unlikely to change the composition of entrepreneurs, unless it leads to higher entry by some entrepreneur types and lower entry by other entrepreneur types, although there is no obvious

reason why the reform would reduce entry for any sub-population of entrepreneurs. Nonetheless, in column 5, we include a battery of fixed effects to account for possible changes in the composition of entrepreneurs: commuting zone fixed effects, CEO demographics (sex-by-age quintiles), and quintile of CEO wage as a proxy for the entrepreneur quality, all of them interacted with year fixed effects. These fixed effects hold fixed the composition of entrepreneurs along space and CEO characteristics dimensions. We find that the magnitude of the effect is not affected by these controls, consistent with the absence of composition effect.

Why do firms accumulate more capital when they adopt flexible bylaws? One possibility is that having the possibility to write complex financial contracts makes it easier to raise equity financing, for instance by making it possible to allocate state-contingent cash flow or control rights to shareholders (Kaplan and Strömberg, 2003). We test that hypothesis by studying whether firms created in sectors more exposed to the reform are more likely to be in the first quintile of the equity distribution three years after creation.²⁷

Column 1 of Appendix [Table B.3](#) shows that the probability of raising equity financing increases by 22 percentage points. Since flexible bylaws startups had a 82.4% probability of reaching the top quintile of equity before the reform, the estimate indicates that the probability of reaching the top quintile of equity increases by 26%. In contrast, we don't find any effect for incumbent firms (column 2). Columns 3 to 5 show that the difference between new and incumbent firms is highly significant and cannot be explained by industry-level shocks or a change in the composition of CEOs. Overall, our results support the idea that reducing the cost of writing complex financial contracts makes it easier for entrepreneurs to raise equity financing.

5.2 Labor

We next study the impact of the reform on labor as better access to financing can allow entrepreneurs to hire more labor. Indeed, although labor is often modeled as a fully adjustable variable input across periods, in reality, labor is likely to have a fixed-cost component due to wage rigidity and hiring/firing costs. As a result, when there is a mismatch in the timing of labor costs and cash flow,

27. One limitation of tax files is that we only observe equity stocks, not flows. Therefore, we detect equity issuances by looking for instances in which variation in equity stocks can only be explained by the presence of an equity issuance. Specifically, a firm is said to issue equity in a given year if the variation in share capital plus the variation in other equity components (e.g., provisions) plus dividends exceeds the lagged net income. As for the other firm variables, equity is in levels for new firms, and in growth rates for incumbents. Specifically, equity is measured as the equity at time $t + 1$ plus total equity issuances at time $t + 2$ and $t + 3$ for new firms, and as total equity issuances between time t and $t + 3$ divided by the average of equity at time t and $t + 3$ for incumbent firms.

financial constraints may affect firm labor demand.²⁸ This mechanism may be particularly relevant for new firms, which are to lack internal cash flow and therefore to rely on external funding. In addition, improved access to equity financing might be particularly useful to hire labor given that labor is intangible and, therefore, cannot be easily financed with debt.²⁹

We measure labor input using the total wage bill in order to capture both the quantity and the quality of labor hired. As in the previous section, we measure success as the probability of reaching a given threshold percentile of the distribution of labor (or labor growth for incumbents) at a three-year horizon.

Panel (a) of [Figure 4](#) shows the result of the difference-in-difference regression for reaching the top quintile of labor accumulation, estimated separately for new firms and for incumbents. There is a sharp increase in the share of top performers among new firms in more exposed industries relative to less exposed industries. By contrast, there is no effect on incumbent firms. Panel (b) shows the triple-difference regression that stacks new firms and incumbents and differences out the effect on incumbents from the effect on new firms. Consistent with the effect not being driven by omitted variables correlated with exposure to the reform, the effect on new firms is quantitatively unchanged when we difference out the effect on incumbents.

[Table 4](#) reports the results using other threshold percentiles to define success and including additional fixed effects. The top quintile dummy corresponds to the total wage bill above 126 thousand euro three years after creation. We find that the reform allows newly created firms to hire more labor (columns 1). For example, the probability of reaching the top quintile of the wage bill distribution increases by 14 percentage points. Given that the unconditional probability of reaching the top quintile for flexible bylaws startups in the pre-reform period is 62%, this represents a 23% increase. Consistent with our measure of exposure being orthogonal to other industry shocks, we find no effect on incumbent firms (columns 2). The triple-difference regressions show that the difference between new firms and incumbents is statistically significant (columns 3), even when we include industry-by-year fixed effects to control for unobserved industry shocks (columns 4) and when we control for the composition of entrepreneurs by including entrepreneur characteristics (column 5).

28. See, e.g., Jermann and Quadrini (2012) and Schoefer (2021) for theoretical analyses of labor demand with financing frictions. For empirical evidence, see among many others Peek and Rosengren (2000); Hombert and Matray (2017); Bai, Carvalho, and Phillips (2018); Berton, Mocetti, Presbitero, and Richiardi (2018); Caggese, Cunat, and Metzger (2019); Benmelech, Frydman, and Papanikolaou (2019); Fonseca and Doornik (2022); Baghai, Silva, Thell, and Vig (2021); Benmelech, Bergman, and Seru (2021); Fonseca and Matray (2022).

29. For a discussion of how intangible capital can heighten financing frictions either because of low collateralization or because it exposes investors in firms relying on intangible assets to unique risk factors, see among others Eisfeldt and Papanikolaou (2013); Crouzet and Eberly (2019); Sun and Xiaolan (2019); Falato, Kadyrzhanova, Sim, and Steri (2022), Eisfeldt, Kim, and Papanikolaou (2022); and Crouzet, Eberly, Eisfeldt, and Papanikolaou (2022).

Moreover, similarly to the result on capital, we find that the effect is concentrated on top performers and is muted at lower levels of success, consistent with the fact that flexible bylaws adoption is only relevant in the top part of the distribution of firms' growth potential. Overall, the pattern is similar to that of the effect on capital. Enabling entrepreneurs to write more sophisticated contracts with their investors allows them not only to acquire more capital but also to hire more labor, consistent with the notion that labor needs to be financed.

5.3 Revenues

Figure 5 shows the results for the impact on firm revenues, and displays similar results than the ones for capital and labor. Table 5 reports the results using other threshold percentiles to define success. The top quintile dummy corresponds to revenues above 439 thousand euros three years after creation. Again, we find a strong and significant positive effect from top tercile to top decile, which is robust to differencing out the effect on incumbent firms and to the inclusion of additional fixed effects. Therefore, a higher accumulation of capital and labor for firms more exposed to the reform translates into higher revenues.

5.4 Size at Entry versus Post-Entry Growth

In the previous sections, we have shown that capital, labor, and revenues increase at a three-year horizon for new firms that are more likely to adopt flexible bylaws thanks to the reform. We now explore the firm dynamics to determine if higher inputs and output after three years ($t + 3$) is explained by higher inputs and output at entry, higher post-entry growth, or both. We define a dummy equal to one if the firm is the top quintile of the distribution of the outcome variable at the end of the first year after entry ($t + 1$) for new firms and as the growth rate from year t to year $t + 1$ for incumbents firms. We define another dummy equal to one if the firm is in the top quintile of the distribution of the growth of the outcome variable from year $t + 1$ to year $t + 3$, both for new firms and for incumbent firms. If the firm has exited by year $t + 3$, we assign a value of zero to the level in $t + 3$ such that the growth rate is always defined.

Panel (a) of Table 6 reports the results for size at entry. For all outcomes, we find that the reform allows new firms to enter with more capital (column 1) and more labor (column 2), which translates into higher revenues (column 5). The effect is entirely explained by the reform and not by other industry-level shocks as differencing out incumbents in the triple-difference specification yields similar point estimates (columns 2, 4, and 6).

Panel (b) shows the results for post-entry growth. In these regressions, we control for deciles of initial size (deciles of capital, deciles of labor, and deciles of revenues) interacted with year fixed effects, because we have shown that new firms in treated industries enter with a larger size and size at entry predicts post-entry growth. We find that the reform allows new firms to grow after entry, although the effect is not statistically significant for labor. Therefore, easing the possibility for entrepreneurs to write more complex contracts with their investors allows them to start larger, but also to grow faster after they have entered.

6 Allocative Efficiency

Starting a new business involves a substantial amount of experimentation and uncertainty in the likelihood of success (Kerr, Nanda, and Rhodes-Kropf, 2014, Kisseleva, Mjøs, and Robinson, 2023). One channel through which more flexible contracting with investors can promote growth is because it allows capital to be reallocated faster toward the most efficient firms. The unconditional effects estimated in the previous section are silent on this mechanism.

To test if capital is reallocated towards more productive startups, we analyze the sensitivity of firm growth to firm productivity. We sort firms based on their productivity at the end of the first year, measured as the marginal revenue product of capital (MRPK). Under the assumption that the production function is Cobb-Douglas, we have $MRPK = \partial Y / \partial K = \alpha Y / K$, where α is the factor share of capital. Therefore, the revenue-to-capital ratio Y / K is a consistent measure of MRPK within a group of firms that have the same factor share α , and can be used to rank firms by productivity of capital within such groups. Following the literature on misallocation, we define groups with homogeneous factor shares to be industries (Hsieh and Klenow, 2009, Bau and Matray, 2023). We construct a high MRPK dummy equal to one if the firm is above the median of MRPK within its five-digit industry. Because the distribution of MRPK and potential mismeasurement of inputs are different between new firms and incumbents, we compute the median MRPK separately for new firms and for incumbents.

We estimate how firm growth from $t + 1$ to $t + 3$ depends on productivity at $t + 1$. To do so, we start from the regression equation for post-entry growth estimated in panel (b) of [Table 6](#), and we interact all the right-hand side variables with the high MRPK dummy. Since the identification is now across MRPK within industry-year, we include industry \times year fixed effects.

[Table 7](#) reports the results. We find that the effect of the reform on capital growth is significantly

stronger for new firms with higher productivity of capital (column 1). The difference between the effect on above-median MRPK firms and below-median MRPK firms is similar to the unconditional effect (Table 6, panel (b), columns 1). This implies that the effect is approximately three times as large on productive firms relative to less productive firms (i.e., the effect is 1.5x the unconditional effect on productive firms and 0.5x the unconditional effect on less productive firms). Therefore, facilitating contracting between entrepreneurs and outside investors improves the allocation of capital across new firms, as capital is reallocated towards new firms with a higher return on capital. By contrast, the effect on incumbent firms is again insignificant (column 2), consistent with our identifying assumption.

In column 3, we run a similar analysis for the growth in labor. In this case, we sort firms based on the marginal revenue product of labor (MRPL). Again, under the assumption of a Cobb-Douglas production function, MRPL is proportional to the revenue-to-wage bill ratio Y/L , and under the additional assumption that factor shares are constant within industry, the revenue-to-wage bill ratio provides a consistent within-industry ranking of MRPL. Recall that we find that, while the reform has an effect on the amount of labor input that new firms can hire at entry, it does not have a significant effect on post-entry labor growth (Table 6 panel (b) columns 3). When we interact with MRPL, we still do not find that more productive new firms exposed to the reform experience faster labor growth. Taking stock, the reform has a beneficial effect on both capital and labor at entry, and on post-entry growth of capital but not labor. A potential explanation is that the return to labor is very high at entry for financially constrained startups, i.e., they can operate with little capital but not without workers. Therefore, new firms use their first resources to hire labor. Then, as firms obtain more resources and funding over time, they increase capital expenditures.

7 Conclusion

In this paper, we provide preliminary evidence on the role of bylaws in the growth of new firms. We start by showing that firms opting for a legal form that allows them to tailor their bylaws use sophisticated contracting devices, such as multiple share classes, that are usually associated with VC-backed firms. To identify the effect of entrepreneurs' ability to write complex contracts with investors, we study a reform that lowers the costs of tailoring company bylaws. We find that the reform has a large positive effect on entrepreneurial success. Entrepreneurs more exposed to the reform start with more capital, hire more labor, and produce more. Post-entry growth is also

enhanced. In addition, post-entry capital accumulation becomes more sensitive to firm productivity, implying that capital allocative efficiency improves.

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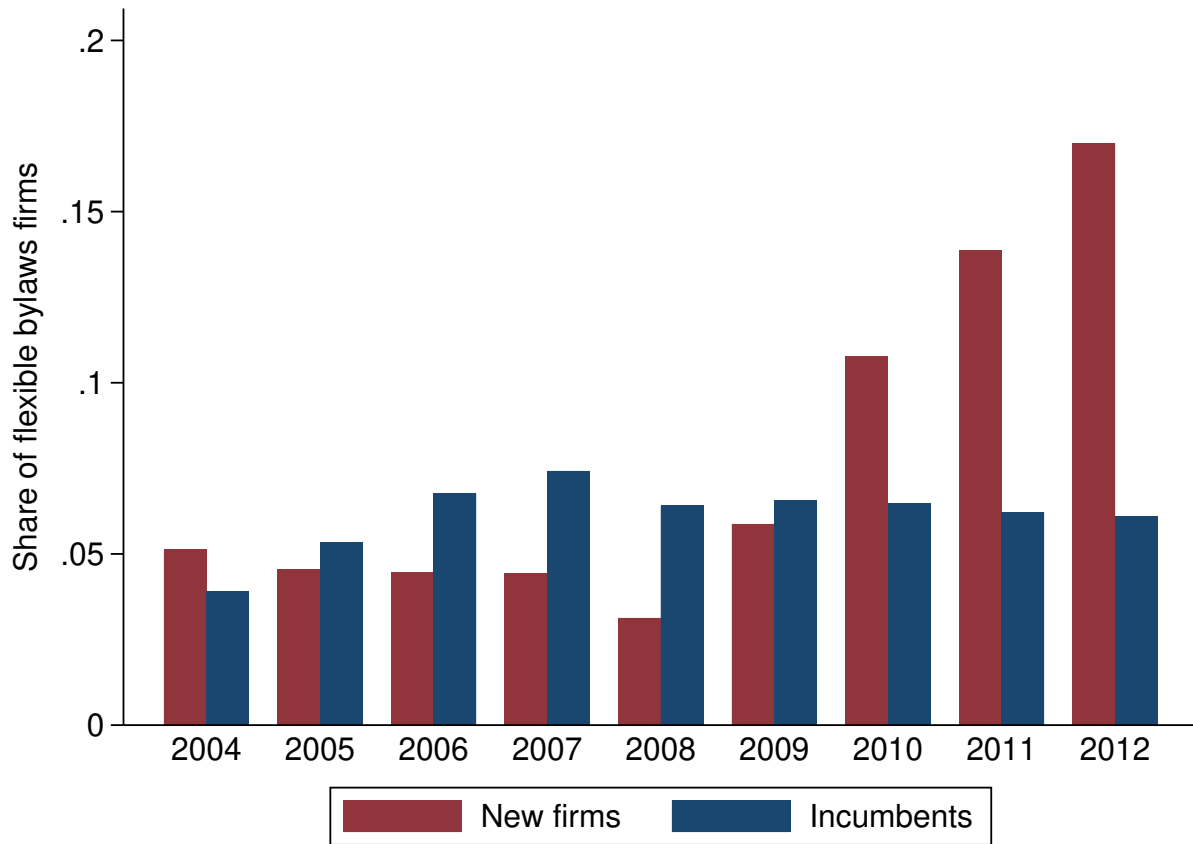
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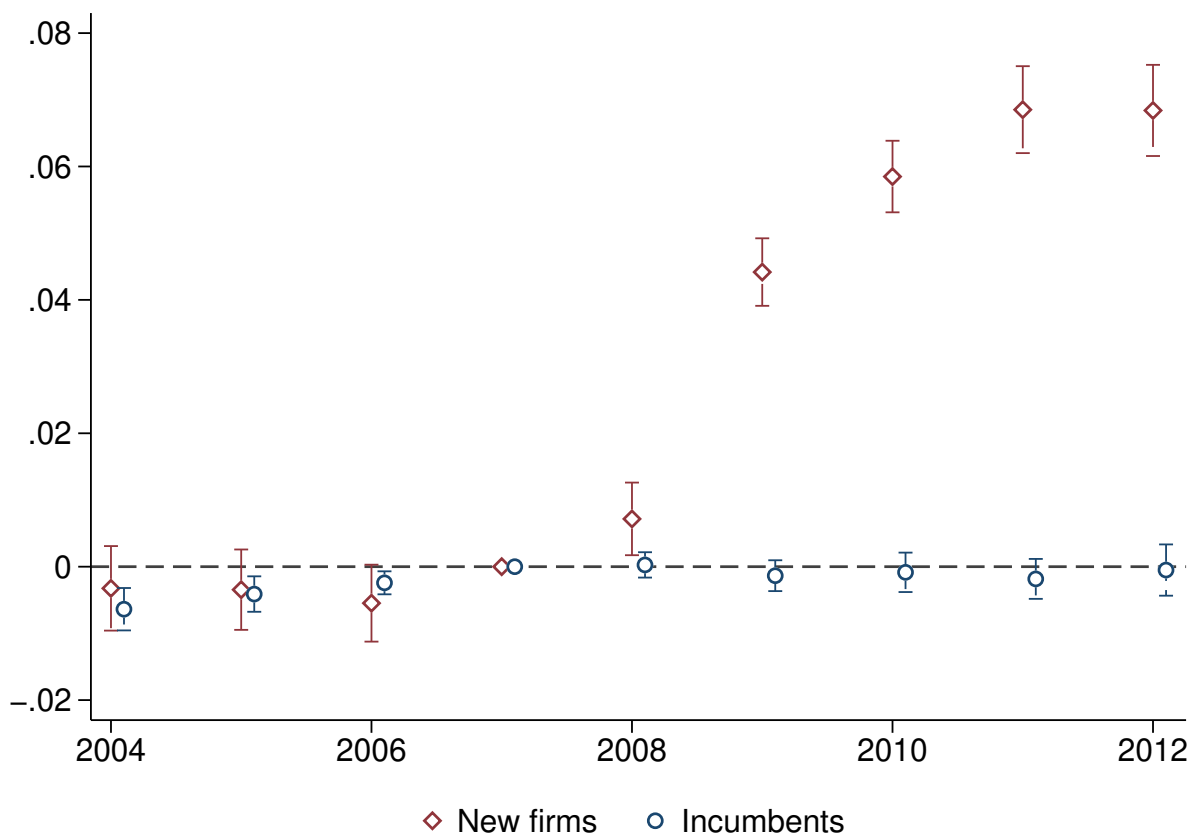
Figure 1: Flexible Bylaws Adoption



The figure shows the evolution of flexible bylaws firms among new and incumbent firms over the 2004-2012 period. Incumbent firms are defined as firms aged between 5 and 10 years.

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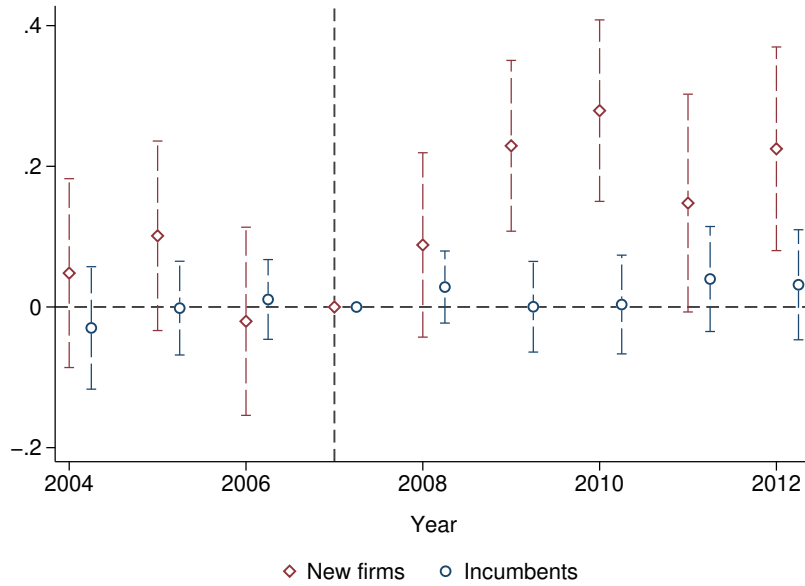
Figure 2: Effect of the Reform on Flexible Bylaws Adoption



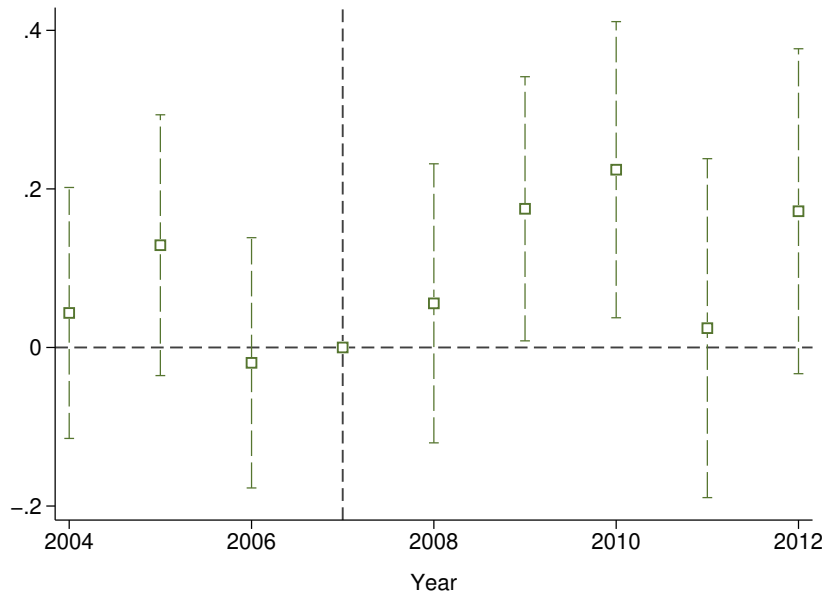
The graph presents the OLS estimates of the β_t coefficients in equation 10, where *Post* is replaced by a collection of year dummies. 2007 is taken as baseline. The dependent variable is a dummy equal to one if the firm chooses the flexible bylaws legal form, and to zero if it chooses the restricted bylaws form. The red diamonds give the point estimates for new firms, the blue circles the estimates for incumbent firms. The bars give the 95% confidence intervals. Standard errors are clustered at the five-digit industry level.

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Figure 3: Effect of the Reform on Capital



(a) New firms vs. incumbents

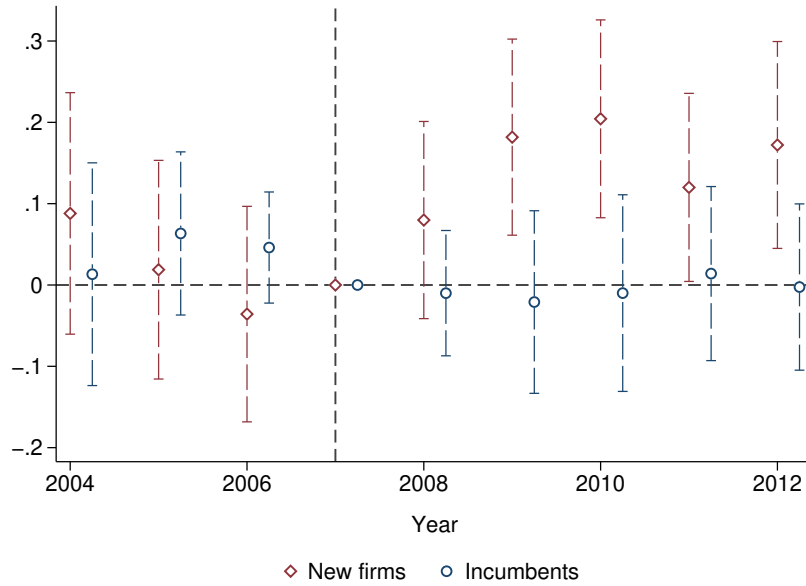


(b) Triple difference

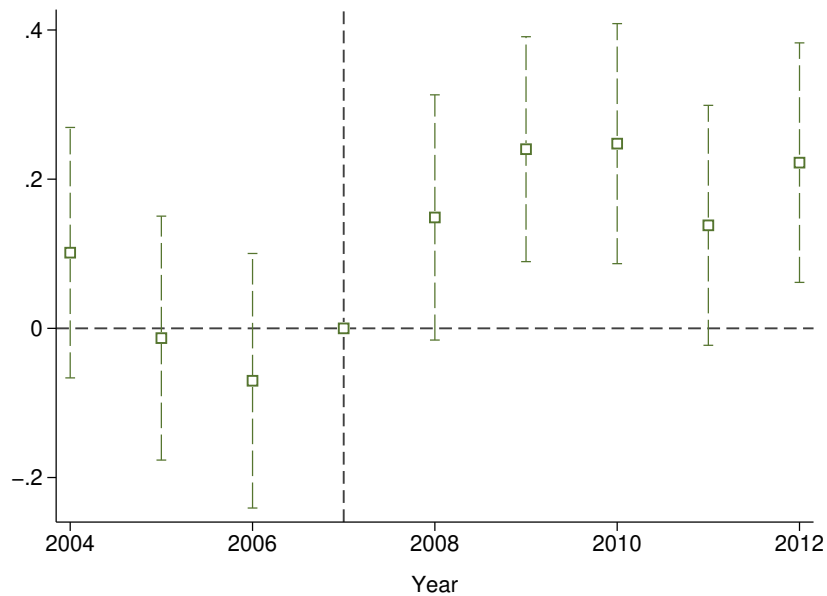
Panels (a) and (b) present the OLS estimates of the β_t coefficients in equation 10 and equation 11, respectively. *Post* is replaced by a collection of year dummies, and 2007 is taken as baseline. The dependent variable is a dummy equal to one if the firm is in the top quintile of the distribution of capital at time $t + 3$ (capital is in levels for new firms, in growth rate between time t and time $t + 3$ for incumbent firms). We measure capital by the firm's fixed assets. The red diamonds give the point estimates for new firms, the blue circles the estimates for incumbent firms, and the green squares give the difference in sensitivity between new and incumbent firms (triple interaction term). The bars give the 95% confidence intervals. Standard errors are clustered at the five-digit industry level.

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Figure 4: Effect of the Reform on Labor



(a) New firms vs. incumbents

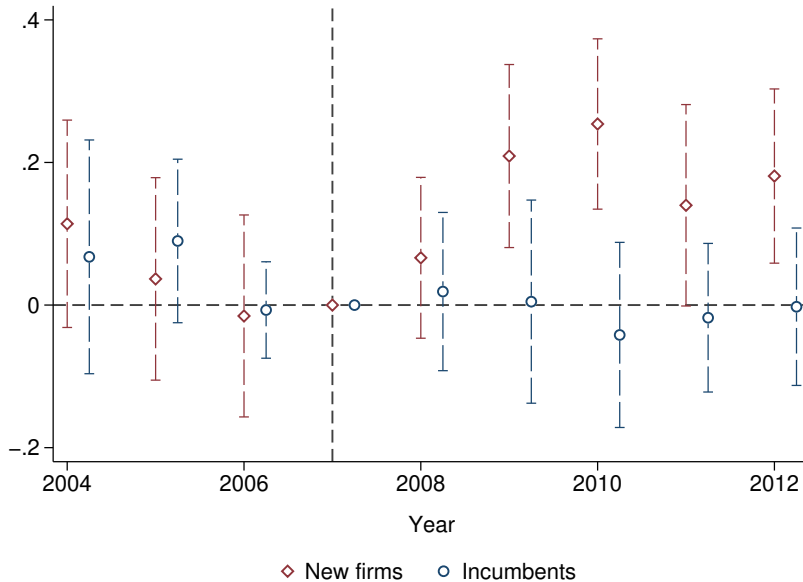


(b) Triple difference

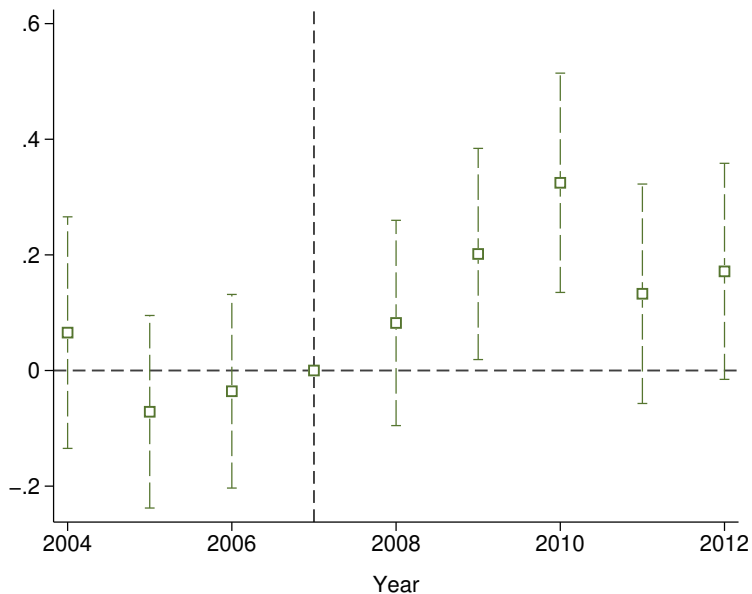
Panels (a) and (b) present the OLS estimates of the β_t coefficients in equation 10 and equation 11, respectively. *Post* is replaced by a collection of year dummies, and 2007 is taken as baseline. The dependent variable is a dummy equal to one if the firm is in the top quintile of the distribution of labor at time $t + 3$ (labor is in levels for new firms, in growth rate between time t and time $t + 3$ for incumbent firms). We measure labor by the firm's total wage bill. The red diamonds give the point estimates for new firms, the blue circles give the estimates for incumbent firms, and the green squares give the difference in sensitivity between new and incumbent firms (triple interaction term). The bars give the 95% confidence intervals. Standard errors are clustered at the five-digit industry level.

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Figure 5: Effect of the Reform on Revenues



(a) New firms vs. incumbents



(b) Triple difference

Panels (a) and (b) present the OLS estimates of the β_t coefficients in equation 10 and equation 11, respectively. *Post* is replaced by a collection of year dummies, and 2007 is taken as baseline. The dependent variable is a dummy equal to one if the firm is in the top quintile of the distribution of revenues at time $t + 3$ (revenues are in levels for new firms, in growth rate between time t and time $t + 3$ for incumbent firms). We measure revenues by the firm's total sales. The red diamonds give the point estimates for new firms, the blue circles the estimates for incumbent firms, and the green squares give the difference in sensitivity between new and incumbent firms (triple interaction term). The bars give the 95% confidence intervals. Standard errors are clustered at the five-digit industry level.

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Table 1: Effect of the Reform on Flexible Bylaws Adoption

<i>Sample:</i>	=1 if Flexible Bylaws					
	New firms			Incumbents		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure×Post	0.063*** (0.00059)	0.062*** (0.0010)	0.060*** (0.0012)	0.0012 (0.0011)	0.0013 (0.0011)	0.00037 (0.0013)
<i>Fixed Effects</i>						
Industry	✓	✓	✓	✓	✓	✓
Industry (2-digit)×Year	✓	✓	✓	✓	✓	✓
County×Year	—	✓	✓	—	✓	✓
CEO demographics×Year	—	—	✓	—	—	✓
CEO wage×Year	—	—	✓	—	—	✓
<i>Controls</i>						
Pre-reform flexible share×Year	✓	✓	✓	✓	✓	✓
Observations	720,859	720,859	688,846	1,614,775	1,614,775	1,432,548

The table presents the OLS estimates of equation 10 for new firms (columns 1 to 3) and incumbent firms (columns 4 to 6). The dependent variable is a dummy equal to one if the firm chooses the flexible bylaws legal form, and to zero if it chooses the restricted bylaws form. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. All specifications include 5-digit industry fixed effects and two-digit industry-by-year fixed effects. The regressions also control for the pre-reform share of firms with flexible bylaws interacted with year fixed effects. Columns 2 and 4 include county-by-year fixed effects. Columns 3 and 6 include age-by-sex-by-year and earnings-by-year fixed effects. Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Table 2: Effect on Firm Creations

	Log(# firms)	
	E-W (1)	# firms (2)
<i>Weights:</i>		
Exposure×Post	-0.0047 (0.026)	0.010 (0.078)
<i>Fixed Effects</i>		
Industry	✓	✓
Industry (2-digit)×Year	✓	✓
<i>Controls</i>		
Pre-reform flexible share×year	✓	✓
Observations	4,635	4,366

The table presents the estimates of equation 10. The dependent variable is the log number of new firms at the five-digit industry-year level. The dependent variable is the inverse hyperbolic sine transformation of the log function defined as $(\log(X + (X^2 + 1)^{1/2}))$ of the number of new firms created. In column 1, the regressions are estimated via OLS. In column 2, the regressions are estimated via WLS, where the weights are equal to the number of new firms in the industry-year so as to weigh industries similarly to firm-level regressions. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. All specifications include 5-digit industry fixed effects and two-digit industry-by-year fixed effects. The regressions also control for the pre-reform share of firms with flexible bylaws interacted with year fixed effects. Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Table 3: Effect of the Reform on Capital

<i>Sample:</i>	New firms	Incumbents	All		
	(1)	(2)	(3)	(4)	(5)
Panel A: Pr[K_{t,t+3} > p50]					
Exposure×Post	0.12* (0.062)	0.059 (0.041)	0.059 (0.041)		
Exposure×Post×New firm			0.061 (0.079)	0.050 (0.079)	0.048 (0.075)
Panel B: Pr[K_{t,t+3} > p66]					
Exposure×Post	0.20*** (0.061)	0.0094 (0.036)	0.0094 (0.036)		
Exposure×Post×New firm			0.19** (0.075)	0.17** (0.079)	0.16** (0.076)
Panel C: Pr[K_{t,t+3} > p75]					
Exposure×Post	0.20*** (0.058)	0.027 (0.032)	0.027 (0.032)		
Exposure×Post×New firm			0.17** (0.070)	0.16** (0.075)	0.18** (0.072)
Panel D: Pr[K_{t,t+3} > p80]					
Exposure×Post	0.17*** (0.050)	0.044 (0.029)	0.044 (0.029)		
Exposure×Post×New firm			0.13** (0.059)	0.12* (0.062)	0.14** (0.059)
Panel E: Pr[K_{t,t+3} > p90]					
Exposure×Post	0.18*** (0.038)	0.013 (0.020)	0.013 (0.020)		
Exposure×Post×New firm			0.17*** (0.042)	0.15*** (0.045)	0.18*** (0.043)
<i>Fixed Effects</i>					
<i>- Interacted with New in columns 3-5</i>					
Industry	✓	✓	✓	✓	✓
Industry (2-digit)×Year	✓	✓	✓	✓	✓
County×Year	—	—	—	—	✓
CEO demographics×Year	—	—	—	—	✓
CEO wage×Year	—	—	—	—	✓
<i>- Not interacted with New</i>					
Industry×Year	—	—	—	✓	✓
<i>Controls (interacted with New in columns 3-5)</i>					
Pre-reform flexible share×Year	✓	✓	✓	✓	✓
Observations	688,846	1,432,548	2,121,394	2,121,394	2,121,394

The table presents the OLS estimates of equation 10 for new firms (column 1) and incumbent firms (column 2), and of equation 11 for new and incumbent firms (columns 3 to 5). The dependent variable is a dummy equal to one if the firm is above a certain percentile of the distribution of capital at time $t + 3$ (capital is in levels for new firms, in growth rate between time t and time $t + 3$ for incumbent firms). We measure capital by the firm's fixed assets. Panel A to E present the results of the estimations using P50, P66, P75, P80, and P90 as percentile in the definition of the left-hand-side variable. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. $New firm_f$ is a dummy equal to one if firm i is a new firm, and to zero if it is incumbent firm. All specifications include 5-digit industry fixed effects and two-digit industry-by-year fixed effects. The regressions also control for the pre-reform share of firms with flexible bylaws interacted with year fixed effects. Column 4 includes five-digit industry-by-year fixed effects. Column 5 includes county-by-year, age-by-sex-by-year, and earnings-by-year fixed effects. All the fixed effects except five-digit industry-by-year fixed effects are estimated separately for new and incumbent firms in columns 3 to 5. Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Table 4: Effect of the Reform on Labor

<i>Sample:</i>	New firms	Incumbents	All		
	(1)	(2)	(3)	(4)	(5)
Panel A: Pr[L_{<i>i,t+3</i>} > p50]					
Exposure×Post	0.056 (0.048)	0.048 (0.048)	0.048 (0.048)		
Exposure×Post×New firm			0.0083 (0.055)	0.052 (0.057)	0.045 (0.058)
Panel B: Pr[L_{<i>i,t+3</i>} > p66]					
Exposure×Post	0.15*** (0.054)	0.017 (0.047)	0.017 (0.047)		
Exposure×Post×New firm			0.13** (0.053)	0.17*** (0.057)	0.21*** (0.053)
Panel C: Pr[L_{<i>i,t+3</i>} > p75]					
Exposure×Post	0.15*** (0.046)	-0.0040 (0.042)	-0.0040 (0.042)		
Exposure×Post×New firm			0.16*** (0.048)	0.20*** (0.054)	0.25*** (0.051)
Panel D: Pr[L_{<i>i,t+3</i>} > p80]					
Exposure×Post	0.14*** (0.042)	-0.0018 (0.037)	-0.0018 (0.037)		
Exposure×Post×New firm			0.14*** (0.047)	0.18*** (0.052)	0.24*** (0.045)
Panel E: Pr[L_{<i>i,t+3</i>} > p90]					
Exposure×Post	0.11*** (0.039)	-0.027 (0.022)	-0.027 (0.022)		
Exposure×Post×New firm			0.14*** (0.045)	0.16*** (0.049)	0.21*** (0.040)
<i>Fixed Effects</i>					
<i>- Interacted with New in columns 3-5</i>					
Industry	✓	✓	✓	✓	✓
Industry (2-digit)×Year	✓	✓	✓	✓	✓
County×Year	—	—	—	—	✓
CEO demographics×Year	—	—	—	—	✓
CEO wage×Year	—	—	—	—	✓
<i>- Not interacted with New</i>					
Industry×Year	—	—	—	✓	✓
<i>Controls (interacted with New in columns 3-5)</i>					
Pre-reform flexible share×Year	✓	✓	✓	✓	✓
Observations	688,846	1,432,548	2,121,394	2,121,394	2,121,394

The table presents the OLS estimates of equation 10 for new firms (column 1) and incumbent firms (column 2), and of equation 11 for new and incumbent firms (columns 3 to 5). The dependent variable is a dummy equal to one if the firm is above a certain percentile of the distribution of labor at time $t + 3$ (labor is in levels for new firms, in growth rate between time t and time $t + 3$ for incumbent firms). We measure labor by the firm's total wage bill. Panel A to E present the results of the estimations using P50, P66, P75, P80, and P90 as percentile in the definition of the left-hand-side variable. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. $New\ firm_f$ is a dummy equal to one if firm i is a new firm, and to zero if it is incumbent firm. All specifications include 5-digit industry fixed effects and two-digit industry-by-year fixed effects. The regressions also control for the pre-reform share of firms with flexible bylaws interacted with year fixed effects. Column 4 includes five-digit industry-by-year fixed effects. Column 5 includes county-by-year, age-by-sex-by-year, and earnings-by-year fixed effects. All the fixed effects except five-digit industry-by-year fixed effects are estimated separately for new and incumbent firms in columns 3 to 5. Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Table 5: Effect of the Reform on Revenues

<i>Sample:</i>	New firms	Incumbents	All		
	(1)	(2)	(3)	(4)	(5)
Panel A: Pr[Y_{i,t+3} > p50]					
Exposure×Post	0.0074 (0.055)	-0.018 (0.048)	-0.018 (0.048)		
Exposure×Post×New firm			0.026 (0.066)	0.058 (0.069)	0.020 (0.071)
Panel B: Pr[Y_{i,t+3} > p66]					
Exposure×Post	0.13** (0.055)	0.0015 (0.050)	0.0015 (0.050)		
Exposure×Post×New firm			0.13* (0.071)	0.15** (0.074)	0.16** (0.072)
Panel C: Pr[Y_{i,t+3} > p75]					
Exposure×Post	0.17*** (0.050)	0.0039 (0.043)	0.0039 (0.043)		
Exposure×Post×New firm			0.16*** (0.059)	0.19*** (0.059)	0.23*** (0.056)
Panel D: Pr[Y_{i,t+3} > p80]					
Exposure×Post	0.15*** (0.046)	-0.024 (0.036)	-0.024 (0.036)		
Exposure×Post×New firm			0.17*** (0.055)	0.21*** (0.055)	0.25*** (0.050)
Panel E: Pr[Y_{i,t+3} > p90]					
Exposure×Post	0.086** (0.036)	-0.017 (0.022)	-0.017 (0.022)		
Exposure×Post×New firm			0.10** (0.041)	0.12*** (0.042)	0.16*** (0.038)
<i>Fixed Effects</i>					
<i>- Interacted with New in columns 3 to 5</i>					
Industry×New	✓	✓	✓	✓	✓
Industry (2-digit)×New×Year	✓	✓	✓	✓	✓
County×New×Year	—	—	—	—	✓
CEO demographics×New×Year	—	—	—	—	✓
CEO wage×New×Year	—	—	—	—	✓
<i>- Not interacted with New</i>					
Industry×Year	—	—	—	✓	✓
<i>Controls</i>					
Pre-reform flexible share×New×Year	✓	✓	✓	✓	✓
Observations	688,846	1,432,548	2,121,394	2,121,394	2,121,394

The table presents the OLS estimates of equation 10 for new firms (column 1) and incumbent firms (column 2), and of equation 11 for new and incumbent firms (columns 3 to 5). The dependent variable is a dummy equal to one if the firm is above a certain percentile of the distribution of revenues at time $t + 3$ (revenues are in levels for new firms, in growth rate between time t and time $t + 3$ for incumbent firms). We measure revenues by the firm's total sales. Panel A to E present the results of the estimations using P50, P66, P75, P80, and P90 as percentile in the definition of the left-hand-side variable. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. $New\ firm_f$ is a dummy equal to one if firm i is a new firm, and to zero if it is incumbent firm. All specifications include 5-digit industry fixed effects and two-digit industry-by-year fixed effects. The regressions also control for the pre-reform share of firms with flexible bylaws interacted with year fixed effects. Column 4 includes five-digit industry-by-year fixed effects. Column 5 includes county-by-year, age-by-sex-by-year, and earnings-by-year fixed effects. All the fixed effects except five-digit industry-by-year fixed effects are estimated separately for new and incumbent firms in columns 3 to 5. Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Table 6: Size at Entry versus Post-Entry Growth

Panel A: Size at entry.						
Dependent variables:	Pr[$K_{t+1} > p80$]		Pr[$L_{t+1} > p80$]		Pr[$Y_{t+1} > p80$]	
Sample:	New firms	All	New firms	All	New firms	All
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure×Post	0.14*** (0.046)		0.24*** (0.049)		0.11** (0.047)	
Exposure×Post×New firm		0.19*** (0.053)		0.21*** (0.058)		0.15*** (0.048)
<i>Fixed Effects</i>						
- Interacted with New in columns 2, 4, and 6						
Industry	✓	✓	✓	✓	✓	✓
Industry (2-digit)×Year	✓	✓	✓	✓	✓	✓
- Not interacted with New						
Industry×Year	—	✓	—	✓	—	✓
<i>Controls (interacted with New in columns 2, 4, and 6)</i>						
Pre-reform flexible share×Year	✓	✓	✓	✓	✓	✓
Observations	688,846	2,121,394	688,846	2,121,394	688,846	2,121,394
Panel B: Post-entry growth.						
Dependent variables:	Pr[$\Delta K_{t+1 \rightarrow t+3} > p80$]		Pr[$\Delta L_{t+1 \rightarrow t+3} > p80$]		Pr[$\Delta Y_{t+1 \rightarrow t+3} > p80$]	
Sample:	New firms	All	New firms	All	New firms	All
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure×Post	0.10** (0.044)		0.26*** (0.042)		0.040 (0.043)	
Exposure×Post×New firm		0.14*** (0.056)		0.18*** (0.044)		0.071 (0.057)
<i>Fixed Effects</i>						
- Interacted with New in columns 2, 4, and 6						
Industry	✓	✓	✓	✓	✓	✓
Industry (2-digit)×Year	✓	✓	✓	✓	✓	✓
Revenues deciles×Year	✓	✓	✓	✓	✓	✓
Capital deciles×Year	✓	✓	✓	✓	✓	✓
Labor deciles×Year	✓	✓	✓	✓	✓	✓
- Not interacted with New						
Industry×Year	—	✓	—	✓	—	✓
<i>Controls (interacted with New in columns 2, 4, and 6)</i>						
Pre-reform flexible share×Year	✓	✓	✓	✓	✓	✓
Observations	688,846	2,121,394	688,846	2,121,394	688,846	2,121,391

The table presents the OLS estimates of equation 10 for new firms (odd columns) and of equation 11 for new and incumbent firms (even columns).

Panel A: The dependent variable is a dummy equal to one if the firm is in the top quintile of the distribution of the variable of interest at time $t + 1$. The variable of interest is in levels for new firms, in growth rate between time t and time $t + 1$ for incumbent firms.

Panel B: The dependent variable is a dummy equal to one if the firm is in the top quintile of the distribution of the variable of interest's growth rate. Growth is measured between time $t + 1$ and time $t + 3$. All specifications include performance decile-by-year fixed effects, where performance is measured at $t + 1$ by revenues, capital, and labor (in levels for new firms, in growth rates between t and $t + 1$ for incumbents).

All: The variable of interest is capital in columns 1-2, labor in columns 3-4, and revenues in columns 5-6. We measure capital by the firm's fixed assets, labor by the firm's total wage bill, and revenues by the firm's sales. P80 is measured separately for new and incumbent firms. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. $New\ firm_f$ is a dummy equal to one if firm i is a new firm, and to zero if it is an incumbent firm. All specifications include 5-digit industry fixed effects and two-digit industry-by-year fixed effects. The regressions also systematically control for the pre-reform share of firms with flexible bylaws interacted with year fixed effects. Even columns include five-digit industry-by-year fixed effects. All the fixed effects except five-digit industry-by-year fixed effects are estimated separately for new and incumbent firms. Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Table 7: Allocative Efficiency

<i>Sample:</i>	New firms	Incumbents	New firms	Incumbents
	(1)	(2)	(3)	(4)
	<u>$\Pr[\Delta K_{t+1 \rightarrow t+3} > \text{p80}]$</u>		<u>$\Pr[\Delta L_{t+1 \rightarrow t+3} > \text{p80}]$</u>	
	(1)	(2)	(3)	(4)
Exposure \times High MRP \times Post	0.29*** (0.062)	0.050 (0.034)	0.027 (0.051)	0.047 (0.031)
<i>Fixed Effects</i>				
Industry \times High MRP	✓	✓	✓	✓
Industry \times Year	✓	✓	✓	✓
High MRP \times Year	✓	✓	✓	✓
Revenues deciles \times Year	✓	✓	✓	✓
Capital deciles \times Year	✓	✓	✓	✓
Labor deciles \times Year	✓	✓	✓	✓
Observations	586,798	1,251,460	605,775	1,218,691

The table presents the OLS estimates of equation 10 for new firms (odd columns) and incumbent firms (even columns). The dependent variable is a dummy equal to one if the firm is in the top quintile of the distribution of the variable of interest's growth rate. Growth is measured between time $t + 1$ and time $t + 3$. The variable of interest is capital in columns 1-2 and labor in columns 3-4. We measure capital by the firm's fixed assets and labor by the firm's total wage bill. P80 is measured separately for new and incumbent firms. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. $New\ firm_j$ is a dummy equal to one if firm i is a new firm, and to zero if it as incumbent firm. MRP (marginal revenue product) corresponds to the marginal revenue product of capital (MRPK) in columns 1-2 and to the marginal revenue product of labor in columns 3-4 (MRPL). MRPK (MPRL) is measured by the ratio of value added to capital (value added to labor) taken at $t + 1$. $High\ MRP_i$ is a dummy equal to one if firm i is above the median of MRP in its industry (the median is computed separately for new and incumbent firms). All specifications include 5-digit industry-by-High MRP, High MRP-by-year, 5-digit industry-by-year fixed effects, and performance decile-by-year fixed effects, where performance is measured at $t + 1$ by revenues, capital, and labor (in levels for new firms, in growth rates between t and $t + 1$ for incumbents). Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Appendix

A Construction of Variables

Capital	Total fixed assets: variable <i>immotot</i> from the tax files.
CEO wage	Gross revenues: variable <i>sbrut</i> from the DADS dataset, <i>revbrut</i> from the ACOSS dataset.
CEO age	Age: variable <i>age</i> from the DADS dataset, <i>age</i> from the ACOSS dataset.
CEO gender	Age: variable <i>sexe</i> from the DADS dataset, <i>sx</i> from the ACOSS dataset.
County	Département: variable <i>depcom</i> from the Business registry.
Equity	Share capital: variable <i>capisoc</i> from the tax files. A firm is said to issue equity if the variation in share capital ($D.capisoc$) plus the variation in other equity components ($D.autcapi$) plus dividends (<i>div</i>) exceeds the lagged net income ($L.r310$).
Firm age	Current year minus year of creation. Year of creation: variable <i>datcrea</i> from the Business Registry.
Labor	Gross total wage bill: variable $saltra_i + charsoc$ from the tax files.
Legal form	2-digit variable $cj = 54$ for SARL; $= 57$ for SAS from the tax files.
MRPK	Marginal revenue of capital: variable vaj /variable <i>immotot</i> from the tax files.
MRPL	Marginal revenue of labor: variable vaj /variable $saltra_i + charsoc$ from the tax files.
Revenues	Total sales: variable <i>ca</i> from the tax files.

B Alternative Specifications

B.1 Alternative Time Horizons

Table B.1: Effect of the Reform on Capital: Four-Year Horizon

<i>Sample:</i>	New firms	Incumbents	All		
	(1)	(2)	(3)	(4)	(5)
Panel A: Pr[K_{i,t+4} > p50]					
Exposure×Post	0.097* (0.055)	0.059 (0.041)	0.059 (0.041)		
Exposure×Post×New firm			0.038 (0.073)	0.037 (0.077)	-0.024 (0.074)
Panel B: Pr[K_{i,t+4} > p66]					
Exposure×Post	0.15*** (0.056)	-0.0031 (0.040)	-0.0031 (0.040)		
Exposure×Post×New firm			0.15** (0.075)	0.16* (0.084)	0.12 (0.078)
Panel C: Pr[K_{i,t+4} > p75]					
Exposure×Post	0.18*** (0.053)	0.040 (0.033)	0.040 (0.033)		
Exposure×Post×New firm			0.14** (0.067)	0.14* (0.074)	0.14** (0.070)
Panel D: Pr[K_{i,t+4} > p80]					
Exposure×Post	0.19*** (0.048)	0.057** (0.028)	0.057** (0.028)		
Exposure×Post×New firm			0.13** (0.057)	0.13** (0.062)	0.14** (0.058)
Panel E: Pr[K_{i,t+4} > p90]					
Exposure×Post	0.18*** (0.037)	0.023 (0.023)	0.023 (0.023)		
Exposure×Post×New firm			0.15*** (0.042)	0.15*** (0.046)	0.17*** (0.044)
<i>Fixed Effects</i>					
<i>- Interacted with New in columns 3-5</i>					
Industry	✓	✓	✓	✓	✓
Industry (2-digit)×Year	✓	✓	✓	✓	✓
County×Year	—	—	—	—	✓
CEO demographics×Year	—	—	—	—	✓
CEO wage×Year	—	—	—	—	✓
<i>- Not interacted with New</i>					
Industry×Year	—	—	—	✓	✓
<i>Controls (interacted with New in columns 3-5)</i>					
Pre-reform flexible share×Year	✓	✓	✓	✓	✓
Observations	688,846	1,432,548	2,121,394	2,121,394	2,121,394

The table presents the OLS estimates of equation 10 for new firms (column 1) and incumbent firms (column 2), and of equation 11 for new and incumbent firms (columns 3 to 5). The dependent variable is a dummy equal to one if the firm is above a certain percentile of the distribution of capital at time $t + 4$ (capital is in levels for new firms, in growth rate between time t and time $t + 4$ for incumbent firms). We measure capital by the firm's fixed assets. Panel A to E present the results of the estimations using P50, P66, P75, P80, and P90 as percentile in the definition of the left-hand-side variable. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. $New\ firm_f$ is a dummy equal to one if firm i is a new firm, and to zero if it is incumbent firm. All specifications include 5-digit industry fixed effects and two-digit industry-by-year fixed effects. The regressions also control for the pre-reform share of firms with flexible bylaws interacted with year fixed effects. Column 4 includes five-digit industry-by-year fixed effects. Column 5 includes county-by-year, age-by-sex-by-year, and earnings-by-year fixed effects. All the fixed effects except five-digit industry-by-year fixed effects are estimated separately for new and incumbent firms in columns 3 to 5. Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Table B.2: Effect of the Reform on Capital: Five-Year Horizon

<i>Sample:</i>	New firms	Incumbents	All		
	(1)	(2)	(3)	(4)	(5)
Panel A: Pr[K_{i,t+5} > p50]					
Exposure×Post	0.33*** (0.10)	0.032 (0.038)	0.032 (0.038)		
Exposure×Post×New firm			0.30*** (0.11)	0.29** (0.11)	0.10 (0.077)
Panel B: Pr[K_{i,t+5} > p66]					
Exposure×Post	0.12** (0.052)	-0.028 (0.042)	-0.028 (0.042)		
Exposure×Post×New firm			0.15** (0.071)	0.18** (0.082)	0.10 (0.073)
Panel C: Pr[K_{i,t+5} > p75]					
Exposure×Post	0.15*** (0.051)	0.032 (0.033)	0.032 (0.033)		
Exposure×Post×New firm			0.12* (0.062)	0.13* (0.069)	0.10 (0.064)
Panel D: Pr[K_{i,t+5} > p80]					
Exposure×Post	0.18*** (0.046)	0.061** (0.029)	0.061** (0.029)		
Exposure×Post×New firm			0.12** (0.055)	0.13** (0.060)	0.11** (0.056)
Panel E: Pr[K_{i,t+5} > p90]					
Exposure×Post	0.16*** (0.038)	0.034 (0.022)	0.034 (0.022)		
Exposure×Post×New firm			0.13*** (0.042)	0.13*** (0.047)	0.15*** (0.045)
<i>Fixed Effects</i>					
<i>- Interacted with New in columns 3-5</i>					
Industry	✓	✓	✓	✓	✓
Industry (2-digit)×Year	✓	✓	✓	✓	✓
County×Year	—	—	—	—	✓
CEO demographics×Year	—	—	—	—	✓
CEO wage×Year	—	—	—	—	✓
<i>- Not interacted with New</i>					
Industry×Year	—	—	—	✓	✓
<i>Controls (interacted with New in columns 3-5)</i>					
Pre-reform flexible share×Year	✓	✓	✓	✓	✓
Observations	688,846	1,432,548	2,121,394	2,121,394	2,121,394

The table presents the OLS estimates of equation 10 for new firms (column 1) and incumbent firms (column 2), and of equation 11 for new and incumbent firms (columns 3 to 5). The dependent variable is a dummy equal to one if the firm is above a certain percentile of the distribution of capital at time $t + 5$ (capital is in levels for new firms, in growth rate between time t and time $t + 5$ for incumbent firms). We measure capital by the firm's fixed assets. Panel A to E present the results of the estimations using P50, P66, P75, P80, and P90 as percentile in the definition of the left-hand-side variable. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. $New\ firm_f$ is a dummy equal to one if firm i is a new firm, and to zero if it is an incumbent firm. All specifications include 5-digit industry fixed effects and two-digit industry-by-year fixed effects. The regressions also control for the pre-reform share of firms with flexible bylaws interacted with year fixed effects. Column 4 includes five-digit industry-by-year fixed effects. Column 5 includes county-by-year, age-by-sex-by-year, and earnings-by-year fixed effects. All the fixed effects except five-digit industry-by-year fixed effects are estimated separately for new and incumbent firms in columns 3 to 5. Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Table B.3: Effect of the Reform on Equity

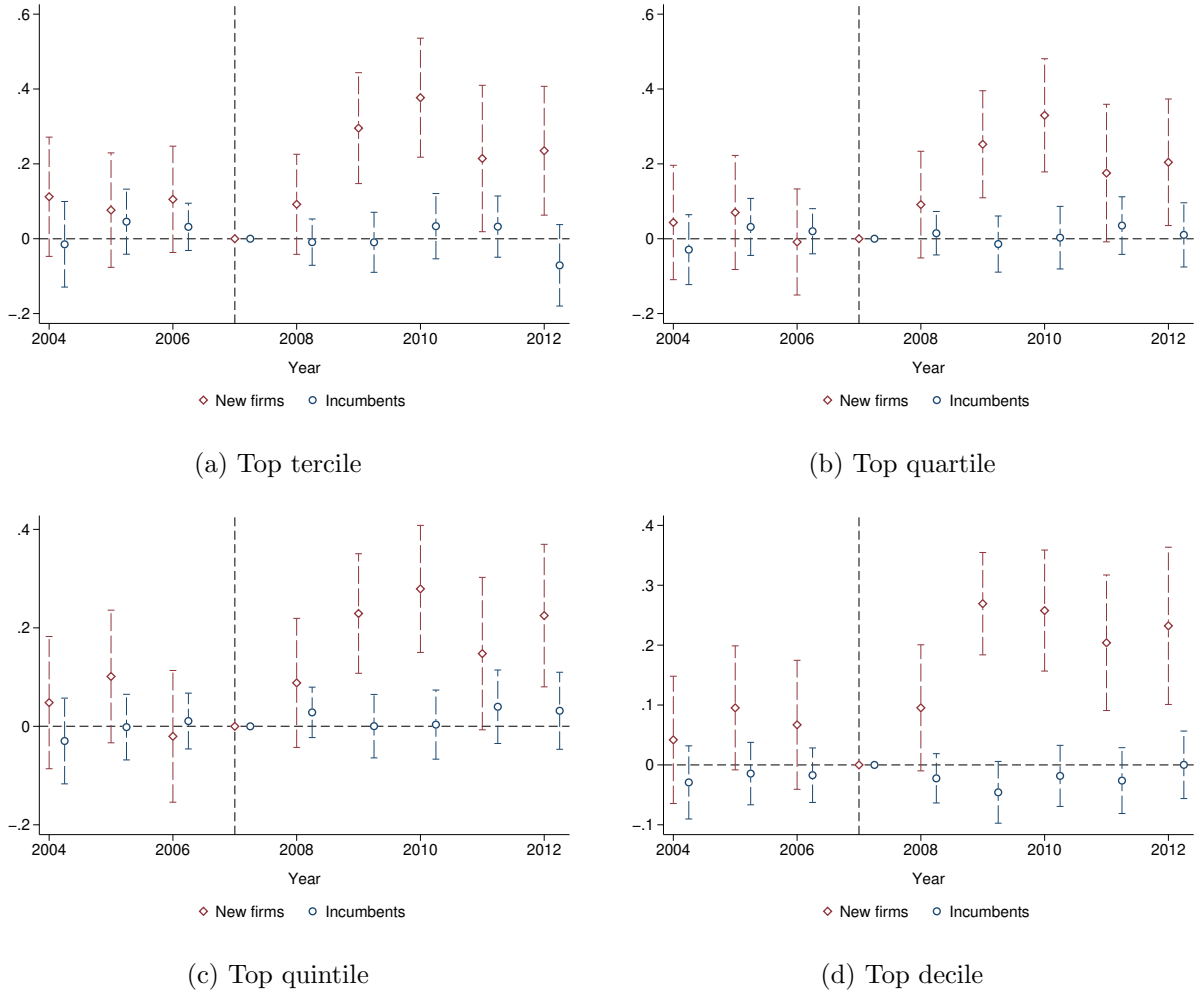
<i>Sample:</i>	New firms	Incumbents	All		
	(1)	(2)	(3)	(4)	(5)
	Panel D: $\Pr[E_{i,t+3} > p80]$				
	(1)	(2)	(3)	(4)	(5)
Exposure×Post	0.22*** (0.082)	-0.038 (0.032)	-0.038 (0.032)		
Exposure×Post×New firm			0.26*** (0.081)	0.32*** (0.066)	0.39*** (0.060)
<i>Fixed Effects</i>					
- <i>Interacted with New in columns 3-5</i>					
Industry	✓	✓	✓	✓	✓
Industry (2-digit)×Year	✓	✓	✓	✓	✓
County×Year	—	—	—	—	✓
CEO demographics×Year	—	—	—	—	✓
CEO wage×Year	—	—	—	—	✓
- <i>Not interacted with New</i>					
Industry×Year	—	—	—	✓	✓
<i>Controls (interacted with New in columns 3-5)</i>					
Pre-reform flexible share×Year	✓	✓	✓	✓	✓
Observations	496,956	995,746	1,492,702	1,492,702	1,492,702

The table presents the OLS estimates of equation 10 for new firms (column 1) and incumbent firms (column 2), and of equation 11 for new and incumbent firms (columns 3 to 5). The dependent variable is a dummy equal to one if the firm is in the top quintile of the distribution of equity at time $t + 3$. Equity is in levels for new firms and measured as the equity at time $t + 1$ plus total equity issuances at time $t + 2$ and $t + 3$. Equity is in growth rates for incumbent firms and measured as total equity issuances between time t and $t + 3$ divided by the average of equity at time t and $t + 3$ (Haltiwanger growth rate). A firm is said to issue equity in a given year if the variation in share capital plus the variation in other equity components (e.g., provisions) plus dividends exceeds the lagged net income. $Exposure_j$ is defined as the industry-level change in the share of flexible bylaws new firms in the post-reform period (2009–2012) relative to the pre-reform period (2004–2007). $Post_t$ is a dummy equal to one from 2009 onwards. $New\ firm_f$ is a dummy equal to one if firm i is a new firm, and to zero if it is an incumbent firm. All specifications include 5-digit industry fixed effects and two-digit industry-by-year fixed effects. The regressions also control for the pre-reform share of firms with flexible bylaws interacted with year fixed effects. Column 4 includes five-digit industry-by-year fixed effects. Column 5 includes county-by-year, age-by-sex-by-year, and earnings-by-year fixed effects. All the fixed effects except five-digit industry-by-year fixed effects are estimated separately for new and incumbent firms in columns 3 to 5. Standard errors are clustered at the five-digit industry level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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B.2 Alternative Cutoffs

Figure B.1: Alternative Cutoffs



The figures present the OLS estimates of the β_t coefficients in equation 10 and equation 11, respectively. *Post* is replaced by a collection of year dummies, and 2007 is taken as baseline. The dependent variable is a dummy equal to one if the firm is in the top part of the distribution of capital at time $t + 3$ (capital is in levels for new firms, in growth rate between time t and time $t + 3$ for incumbent firms). We measure capital by the firm's fixed assets. Panels (a) to (d) use the top tercile, quartile, quintile, and decile as alternative cutoffs, respectively. The red diamonds give the point estimates for new firms, the blue circles the estimates for incumbent firms, and the green squares give the difference in sensitivity between new and incumbent firms (triple interaction term). The bars give the 95% confidence intervals. Standard errors are clustered at the five-digit industry level.

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