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THE VALUE OF WORDS:
EVIDENCE FROM NON-FINANCIAL DISCLOSURE REGULATION

by Antonio Accetturo*, Audinga Baltrunaite†, Gianmarco Cariola*,
Annalisa Frigo* and Marco Gallo*

Abstract

We examine the effects of less stringent non-financial disclosure regulation on operating costs and access to external financing for micro firms in Italy. Since 2016, firms below certain size thresholds have been exempt from filing reports with qualitative information that complements standard balance sheet items. Compliance rates were higher among older and more productive firms, in line with strategic considerations that play a role in influencing policy uptake. However, the benefits of simplified reporting appear limited: using a regression discontinuity design that exploits the multidimensional size cut-offs determining eligibility, we find no evidence of cost savings. Instead, we document a negative impact on ownership transfers and access to credit markets due to increased opacity, suggesting that reduced information disclosure to stakeholders may hinder business dynamism.

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1 Introduction[1]

Reducing information asymmetries plays a crucial role in corporate activity by alleviating the adverse selection problem between firms and their investors (e.g., Leuz and Verrecchia 2000), or by leveling the playing field among them and lowering the cost of capital (Lambert et al., 2007). Moreover, greater information disclosure may enhance market efficiency by boosting liquidity, to the benefit of corporations (Goldstein and Yang, 2017), or by increasing competition, to the benefit of consumers (Board, 2009). Last but not least, disclosure can facilitate the monitoring of managers by shareholders, regulators, or corporate outsiders, thereby improving managerial decision-making and corporate outcomes (e.g., Bushman and Smith 2001; Lambert et al. 2007).

Financial statements are typically regarded as a source of hard information, containing quantifiable and verifiable data such as revenues, expenses, assets, liabilities, and cash flows (Berger and Udell, 2002). Their standardized nature facilitates comparability and auditability, making them especially valuable for investors and financial institutions that rely on objective criteria to assess firm value and creditworthiness (Lambert et al., 2007).

Corporate accounts may also include non-financial information such as management commentary, assumptions behind accounting estimates, segment reporting, and forward-looking disclosure. Although more intense disclosure may increase transparency, efficiency, and accountability, various trade-offs affect its optimal amount (Goldstein and Yang, 2017; Christensen et al., 2021). The first-order disclosure costs stem directly from the administrative burden of preparing, certifying, and disseminating reports containing qualitative information. In addition, firms may incur indirect costs because multiple audiences (e.g., suppliers, competitors) may exploit disclosed information for their own benefit (Feltham and Xie, 1992; Berger and Hann, 2007; Berger et al., 2024), because disclosure may crowd out other forms of information production (Goldstein and Yang, 2019), or because managers may demand higher remuneration to compensate for increased monitoring by external stakeholders (Hermalin and Weisbach, 2012).

The aim of this paper is to analyze the role of qualitative information in balance sheets on firm performance. We analyze the impact of a legislative decree that - starting in 2016 -lowered non-financial reporting requirements for micro firms in Italy (D. Lgs. 139 of 2015

that transposed into the national legislation the 2013 Accounting Directive, 2013/34/EU). Specifically, companies below the relevant size thresholds became eligible to file a new form of the balance sheet (the "micro-firm balance sheet", MFBS) that did not require the preparation of textual documents (so called *Note integrative*) that in Italy complement the standard balance sheet items. *Note integrative* include relevant qualitative information, such as investment description, cost breakdown, or general assessment on firm performance. The reform aimed to reduce the administrative burden for very small and simple businesses that may lack the scale and expertise to bear the cost of producing the more complex required reports.

Since eligibility for the MFBS was based on discontinuous size cut-offs, we exploit this institutional feature through a Regression Discontinuity (RD) design in which the treatment - the absence of mandated textual documents - is instrumented by eligibility to file the MFBS. This policy variation lends itself well to estimating causal parameters of interest. First, it allows us to recover a counterfactual outcome for treated firms by comparing them to otherwise similar firms just above the threshold, under relatively mild continuity assumptions. Second, the size cut-offs that determine eligibility do not coincide with other institutional changes. Third, eligibility is computed using pre-determined balance sheet indicators, leaving virtually no scope for manipulation of the forcing variables. We argue that this quasi-experimental setting provides a clean design for attributing causal interpretations to our estimates.

We analyze the impact of the lower non-financial disclosure requirements on firms' material costs - reflecting compliance costs - and on their ability to attract external financing - reflecting the value of transparency. Our RD estimates reveal no evidence of cost savings, but indicate that access to external financing declines. In particular, we find a short-run negative effect of MFBS adoption on ownership changes, and a medium-run impact on credit access. The effects on bank credit are concentrated on the extensive margin: affected firms are less likely to borrow from a bank. The magnitude of the effect relevant: after three years, a standard deviation increase in the probability to adopt MFBS determines a reduction of almost one-third of a standard deviation of the likelihood to borrow from a bank; the effect is entirely driven by firms that did not borrow from any bank. For firms with existing bank debt (more specifically, borrowers with an outstanding exposure with a single intermediary over €30,000, in the following 'banking relationship'), the elimination of textual documents does not determine neither the exit from the credit market nor a reduction of the amount of credit received. We provide a range of robustness checks and placebo tests to validate these findings.

We next seek to shed light on the mechanisms behind this empirical puzzle - namely, why

companies choose to adopt the MFBS despite experiencing no cost savings and facing reduced access to bank credit. To this end, we conduct a complier characterization analysis (Angrist and Pischke, 2009). Compliance rates are higher among more productive and older firms, suggesting that firms adopting the MFBS feel sufficiently established to prioritize cost-saving strategies over transparency. Compliance is also more frequent in sectors that are less reliant on external finance for investment purposes (using a measure of external finance dependence as in Rajan and Zingales, 1998), where the opportunity cost of providing more complete information is presumably higher. We also find that MFBS adoption is more common in provinces where small banks - less reliant on hard information for lending decisions - are more prevalent.

Our findings suggest that MFBS was adopted by firms not anticipating to be severely affected by the negative consequences of lower non-financial disclosure. Yet, the evidence of reduced access to external finance reveals that the removal of *Note integrative* may limit the financial resources available for firm growth and, in turn, constrain the economic potential of the broader economy.

The economic literature on the role of non-financial disclosure in mitigating information frictions and producing material effects on firm performance remains limited but is expanding. Similar to the effects of enhanced financial reporting, Dhaliwal et al. (2011) find a positive association between voluntary non-financial disclosure and the cost of equity capital; Ilhan et al. (2023) document a positive link between voluntary environmental disclosure and institutional ownership; and Gibbons (2024), using a difference-in-differences approach, show that mandated environmental and social disclosures are associated with more innovation, long-term investment, and higher equity capital. We contribute to this literature in three ways. First, existing studies mostly focus on large, publicly owned companies. Yet, the benefits and costs of disclosure may be especially salient for small enterprises. On one hand, their lack of scale makes them more opaque and thus more susceptible to the costs of information asymmetries. On the other, the relative burden of collecting, analyzing, and presenting qualitative information is higher for smaller firms. Therefore, examining the role of non-financial reporting regulation in small firms can offer valuable policy insights. We provide novel evidence based on a sample of private Italian micro-firms and highlight the potential costs associated with the absence of disclosure. Second, most existing studies are correlational or rely on strong assumptions for causal identification. Our study provides some of the first causally identified insights into the role of disclosure in shaping firm behavior, both in terms of internal operations and external financing. Third, given that hard and qualitative information likely serve different functions, our findings show that complementing standard

financial accounts with textual context adds tangible value, particularly in securing external financing.

No other papers, to the best of our knowledge, have previously exploited the 2013 Accounting Directive to analyze the removal of the obligation to redact textual documents in EU countries. Fornasari et al. (2025) indirectly analyze the impact of another provision of the 2013 Accounting Directive by studying the effects of the adoption of the simplified balance sheets – instead of the ordinary ones – for larger firms, with results that are broadly consistent with ours.[2]

The paper is organized as follows. Section 2 describes the institutional setting and the data. Section 3 outlines the identification strategy. Section 4 presents the results, along with robustness and heterogeneity analyses. Section 5 concludes and discusses policy implications.

2   Institutional setting and data

The empirical strategy of the paper leverages a legislative decree, D.Lgs. 139 of 2015, that introduced a new form of the balance sheet that removed the obligation to redact textual documents called *Note integrative*. According to Italian law, all companies must present these non-financial statements that provide a detailed description on some crucial firm management choices, such as types of investments or cost items. *Note integrative* must also report critical choices in the redaction of financial statements, such as amortization rates or whether the firm received public subsidies, illustrating how this qualitative information is complementary to the standard hard information contained in the balance sheet.

Very small firms falling below the relevant size thresholds became eligible to this new form of the balance sheet that was called a *micro-firm balance sheet* (MFBS). In particular, the eligibility was based on meeting at least two of the three size cut-offs, based on the information filed with firms' financial statements in the previous two consecutive financial years: i) the number of employees not exceeding 5; ii) gross sales not exceeding 350,000 €; iii) assets not exceeding 175,000 €. Italian thresholds were set at half than those mandated by the 2013 EU Accounting Directive (assets: €350,000; sales: €700,000; employees: 10) to take into account the fragmentation of the Italian productive system (Accetturo et al., 2025).

The reform applied starting from the 2016 financial year, and, hence, the eligibility requirements were based on 2014-15 balance sheet data. Table 2.1 summarizes the different reporting

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[2]More specifically, Fornasari et al. (2025) analyze the effects of the adoption of the *Abbreviato* balance sheets with respect to *Ordinario*, see table 2.1 for details.

regimes of corporate financial accounts based on the size-based eligibility cut-offs.

Table 2.1: Balance sheet regulation

Items	Micro (MFBS)	Abbreviato	Ordinario
Items	Income statement* Balance sheet*	Income statement* Balance sheet* Note integrative*	Income statement Balance sheet Note integrative Financial statement Management report
Eligibility cut-offs (at least 2 out of 3 conditions)	Employees ≤ 5 Sales ≤ 350k€ Assets ≤ 175k€	5< Employees ≤ 50 350k€< Sales ≤ 8800k€ 175k€< Assets ≤ 4400k€	Employees > 50 Sales > 8800k€ Assets > 4400k€

Notes: * indicates that the document is of a simplified format. Firms always have the possibility to opt-in to a more complex reporting regime.

We link information from CERVED group on financial accounts of the universe of Italian limited liability companies, including the full set of balance sheet indicators and mandatory textual reports, with the information from the Chambers of Commerce (*Infocamere*) on firm ownership structure. We then complement this dataset with data on firms' labor force from the National Institute for Social Welfare (*Istituto Nazionale Previdenza Sociale*, INPS). Finally, we merge proprietary data on firm credit histories covering all individual borrowers with an outstanding exposure with a single intermediary over €30,000 from the Italian Central Credit Registry (*Centrale dei Rischi*) managed by the Bank of Italy.

We focus on standalone limited liability companies in private non-financial sector over the period 2013-2019 that file a balance sheet, have non-negative value added and have no non-performing loans. Since our identification strategy compares treated and control units around micro-firm thresholds, we exclude from the sample all observations that in a given year exceed at least one of the simplified (*abbreviato*) balance-sheet thresholds (assets: €4,400k; sales: €8,800k; employees: 50).

Table 2.2 compares companies in our sample based on the micro firm status defined in 2014-15 in terms of two of the thresholds described in Table 2.1. Micro firms are somewhat younger and more often located in the central or southern regions of Italy. In terms of sectoral composition, the incidence of the constructions sector is larger among micro companies, at the expense, at large, of manufacturing.

Table 2.2: Descriptive statistics: micro vs. non-micro firms

	(1) Non-micro		(2) Micro		(3) Difference	
	mean	sd	mean	sd	b	t
Firm age	15.55	10.90	14.98	10.78	0.56***	(10.43)
Location						
Center	0.22	0.42	0.25	0.44	-0.03***	(-13.74)
South and Islands	0.25	0.43	0.32	0.47	-0.07***	(-32.80)
North-East	0.22	0.42	0.19	0.39	0.04***	(18.65)
North-West	0.30	0.46	0.24	0.43	0.07***	(29.80)
Sector						
Construction (F)	0.14	0.35	0.19	0.39	-0.05***	(-24.55)
Manufacturing (C)	0.16	0.37	0.12	0.32	0.05***	(27.80)
Services (E, G-S)	0.67	0.47	0.65	0.48	0.02***	(8.52)
Other activities (A, B, D)	0.02	0.15	0.04	0.21	-0.02***	(-24.82)
Observations	86740		75136		161876	

Notes: Firms defined *micro* along all three dimensions are not included; Firms that exceed at least one of the EU-mandated thresholds (assets: €350 000; sales: €700 000; employees: 10) are also dropped. Significance: ***=.01, **=.05, *=.1. Errors are robust.

3 Identification strategy

The empirical strategy leverages the fact that, conditional on meeting one size requirement, falling just below or just above the second size requirement generates a quasi-random variation in the eligibility assignment. To isolate firms that take part in this "natural experiment" we proceed as follows. To start with, we exclude companies that meet all the three size requirements or none of them, as passing or not passing any one of the size cut-offs does not affect the eligibility: they would qualify by the other two requirements or would not qualify by only one. We then condition on one of the requirements being fulfilled and on the second one not being fulfilled and we exploit the third condition as the source of exogenous variation in the eligibility to filing the MFBS.

Table 3.1 illustrates this conditioning scheme by listing the three mutually nonexclusive "experiments" and their sample sizes. For example, corresponding to the first experiment (rows 1 and 2), among firms that are sufficiently small to meet the eligibility criteria in terms of assets but are larger than the required threshold in terms of employment, there are 2,210 firms that exceed the eligibility threshold in terms of sales and 5,913 firms that meet this criterion. Similarly, among firms that are too large to meet the eligibility criteria in terms of assets but are smaller than the required threshold in terms of employment, there are 78,963 firms that exceed the eligibility threshold in terms of sales and 63,859 firms that meet this criterion. The analogous reading applies to the other two experiments.

In principle, this design would allow for six separate discontinuity regressions, based - for each of the three forcing variables - on the mutually alternative verification of the other two conditions. However, sample sizes for each regression are a major constraint, as highlighted in Table 3.1. **Sales is the forcing variable with the highest density of observations around the relative threshold, accounting for over 90% of the 161,876 firms in our sample. More importantly, the number of effective observations used in the local polynomial regression—those selected within the optimal bandwidth—drops sharply for the other two variables: for both assets and employment, the estimation sample is less than half the size of the one based on sales, significantly reducing statistical power. In addition, the asset-based regressions exhibit signs of pre-trends, as both the intensive margin of credit and the change in shareholder composition display statistically significant coefficients even before the reform. Employment-based regressions are further limited by the absence of a first-stage discontinuity in the probability of MFBS adoption at the relevant threshold, likely due to noisy measurement of non-permanent workers.[3]**

For these reasons, we focus on sales as the forcing variable throughout the analysis and, to further increase sample size, we pool the two regressions in which sales is used as the running variable.

Table 3.1: Sample size

Observations:			Non Eligible	Eligible	*of which:* Treated
	Conditioning vars:		**Forcing var:**		
			Sales: ✗	Sales: ✓	
150,945	Assets: ✓	Empl.: ✗	2,210	5,913	1,617
	Assets: ✗	Empl.: ✓	78,963	63,859	23,245
			Empl.: ✗	Empl.: ✓	
77,046	Assets: ✓	Sales: ✗	2,210	4,692	1,115
	Assets: ✗	Sales: ✓	6,285	63,859	23,245
			Assets: ✗	Assets: ✓	
95,853	Empl.: ✓	Sales: ✗	78,963	4,692	1,115
	Empl.: ✗	Sales: ✓	6,285	5,913	1,617

Notes: Firms defined micro along all three dimensions or in any of the three dimensions are not included. Firms that exceed at least one of the eligibility conditions for filing *Abbreviato* balance-sheet are also dropped. ✓ (✗) indicates that the MFBS eligibility condition based on the specific variable is (not) satisfied: the firm falls bellow (above) the threshold established by the regulation.

Figure 3.1 illustrates our empirical design graphically with an example of treated and control units.

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[3]Graphical evidence from the first stage regressions using employment and assets as forcing variables is presented in figure A.1 in the Appendix.

Figure 3.1: RDD with multiple cut-offs
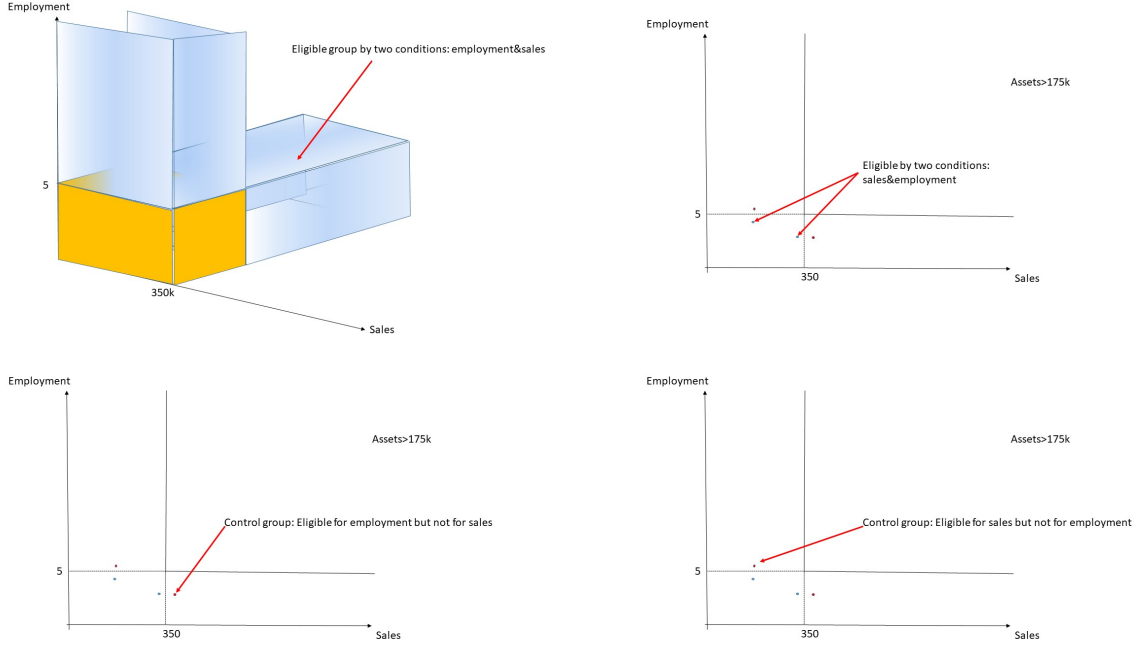
Note: The figure illustrates our identification strategy by the example in which conditional on having exceeded the assets cut-off, the other two dimensions discontinuisly affect the eligibility to file the MFBS.

In our RD Design, the treatment - i.e. filing the MFBS - is instrumented with eligibility. We present two sets of evidence. First, we estimate an Intention To Treat (ITT) effects in a reduced form model in which the outcome variables of interest are regressed on the eligibility to file the MFBS:

$$Y_i = \alpha + \beta Eligible_i + f(Sales_i) + \epsilon_i \tag{1}$$

where Y_i is the outcome of interest for firm i; $Sales_i$ is a measure of sales of firm i, that is calculated as the maximum between balance sheets 2014 and 2015 figures, to take into account the fact that quantitative rules for eligibility must be respected for two consecutive years; $Eligible_i$ is an indicator for firms with $Sales_i$ below the €350k cut-off. Next, to assess the effect of the treatment of interest, we also estimate a fuzzy RD model:

$$Y_i = \theta + \rho MFBS_i + f(Sales_i) + \varepsilon_i \tag{2}$$

$$MFBS_i = \alpha + \beta Eligible_i + f(Sales_i) + \epsilon_i \tag{3}$$

where $MFBS_i$ is an indicator for firms filing a MFBS and the rest of the variables are the same as in equation (1). Treatment variable $MFBS_i$ is measured in 2016, while the eligibility conditions must be verified both in 2014 and 2015.

Both for the estimation of equations (1) and (2), the estimation sample is restricted to the observations within the mean squared error (MSE) optimal bandwidth calculated for the outcome variable in 2016; regressions use the triangular kernel and the first order polynomial for the functional form of the local polynomial $f(Sales_i)$.

The causal interpretation of our findings relies on a number of identification assumptions. First, the standard assumptions for valid inference in the continuity framework (Hahn et al., 2001) require:

- RD1: there is no manipulation of the forcing variable;

- RD2: potential outcomes are continuous.

There are a number of reasons why it is plausible that RD1 is respected. First, the reform is based on cutoffs that do not coincide with any other institutional changes, such as the definition of small or medium enterprises, eligibility to subsidies, etc. Second, eligibility in 2016 is determined based on balance sheet data from 2014 and 2015, with threshold values set in those years, before the treatment period. This makes ex-post manipulation of the forcing variables highly implausible. However, since the reform was under discussion in 2014 and 2015, the possibility of anticipation effects cannot be completely ruled out; to account for this, we test for the continuity of the density function of the forcing variable in Figure 3.2 and find no evidence of manipulation, as we cannot reject the null hypothesis of continuity.

The assumption RD2 cannot be tested directed, as counterfactual outcomes are not observed. It can be indirectly tested by looking at the continuity of the observed outcomes before the treatment as shown in Section 4.

The fuzzy Regression Discontinuity Design also bears on the instrumental variables' identifying assumptions:

- IV1 First-stage: the instrument affects the treatment variable in a substantial manner;

- IV2 Exclusion restriction: the instrument only affects the outcomes through the treatment variable;

- IV3 Monotonicity: the effect of the instrument is either weakly positive or weakly negative for all sub-populations in the sample.

IV1 assumption of the first stage can be directly tested in the data. In fact, in our RD setting it

Figure 3.2: Manipulation of the forcing variable
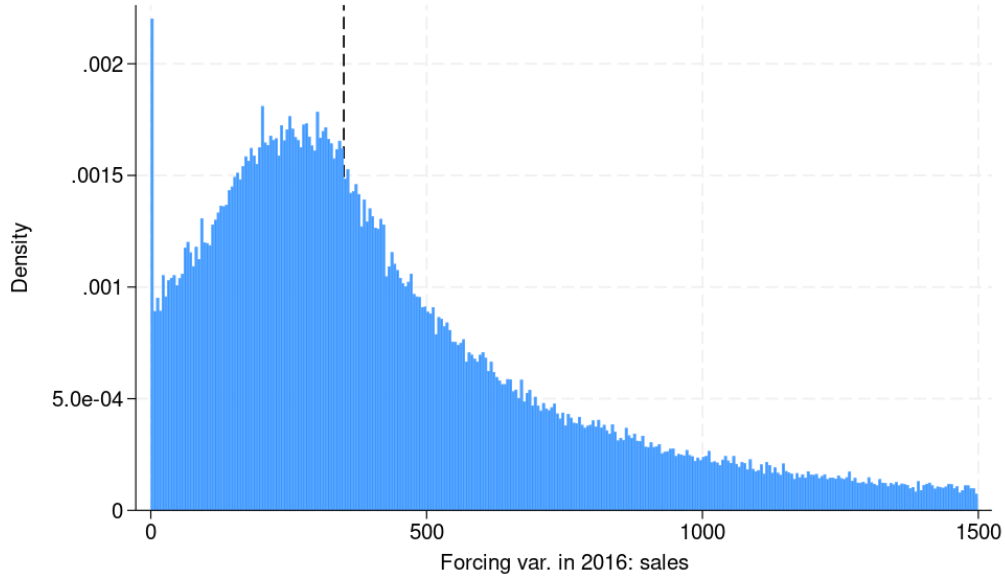
Note: The figure shows the density function of the forcing variable sales for MFBS eligibility in 2016 defined as the maximum value of firm's sales in 2014 and 2015, in €thousands. The null hypothesis of no manipulation cannot be rejected (*p*-value 0.39)

consists of verifying the presence of a discontinuous jump in the treatment variable $MFBS_i$ at the cut-off of the forcing variable $Sales_i$. Figure 3.3 clearly shows that while the probability of filing a MFBS decreases as the firm size increases, it falls discontinuously at the threshold of €350k. In Section 4 we estimate that magnitude of the jump and its statistical significance.

IV2 exclusion restriction requires that the reform does not affect the outcomes of interest through other channels, but the MFBS. In our setting, it is satisfied by design, as no other policies use the same thresholds.

Regarding IV3, as Figure 3.3 illustrates, there is no perfect compliance with the law. We observe, in particular, a non-negligible share of treated firms in the non-eligible group; this could be due to the fact that – due to the novelty of the law – there could have been material mistakes by the firms in understanding whether they were eligible for the MFBS. For the same reasons, the checks performed by the Chambers of Commerce were probably less rigorous. As a result, our setting is characterized by a two-sided non-compliance and the estimation of meaningful causal effects must bear the monotonicity assumption that cannot be tested in the data.

Both ITT and fuzzy RD regressions include controls for sector fixed effects and a dummy for the type of conditioning variable the observation belongs to (i.e. asset or employment).

Figure 3.3: First stage: probability of filing a MFBS
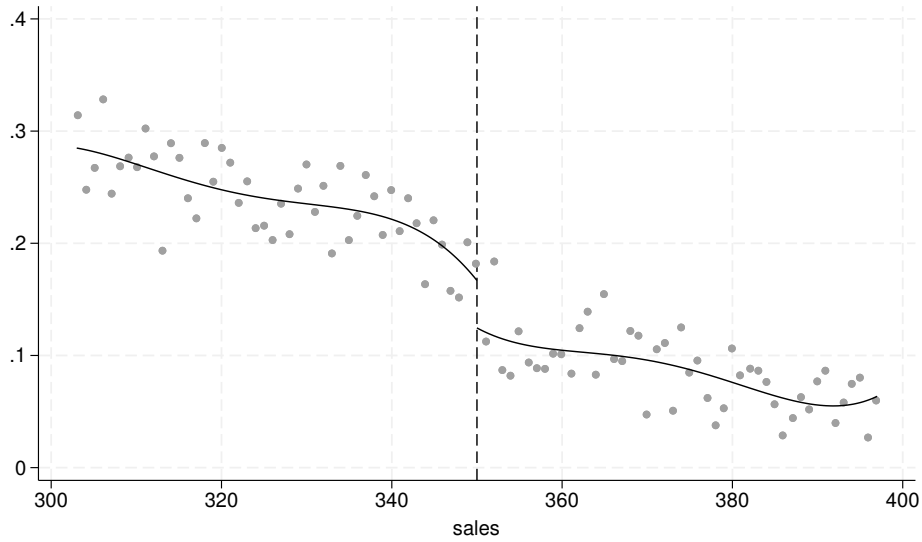
Note: The figure plots the binned averages of the indicator for firms filing a micro firm balance sheet (MFBS) against the forcing variable of sales defined as the maximum value of firm's sales in 2014 and 2015 at the most numerous eligibility threshold.

4 Results

4.a Baseline results

The simplification of non-financial reporting requirements potentially affects a number of outcomes. The first order expected effects relate to costs, as interpreting corporate balance sheet variables, providing additional information and redacting *note integrative* requires time and effort or, as is likely the case of the very small firms affected by the reform, deployment of resources external to the firm (e.g., external consultancy, extra time for company's accountant, etc). To study this channel, we look at the company's cost incidence over sales and its cost composition, distinguishing between cost of services and cost of labor and test whether the reform actually attenuates company's expenditures. However, the redaction of *note integrative* increases the information available on corporate accounts of the company and the company itself. Therefore, we may expect it to be related to company's relationships with its stakeholders. In this paper, we focus on companies' access to external financing, either through equity (proxied by ownership transfers among shareholders) or through bank debt, both on the extensive and on the intensive margins.

We start by studying the ITT effect by estimating sharp RD regressions in which we estimate the MFBS eligibility effects on various outcome variables. Table 4.1 shows the sharp RD

estimates on the following dependent variables: total costs over sales (Panel A); indicator for companies experiencing an ownership transfer with respect to the pre-treatment year (2015) (Panel B); indicator for companies having at least one bank relationship (Panel C); for firms with at least one banking relationship, the number of banks a given company relies on for its debt financing (Panel D); the amount of bank loans over its assets, excluding zeros (Panel E). Columns 1 to 4 (columns 5 to 8) use a linear (quadratic) local polynomial to approximate $F(Sales_i)$ in equation 1. Columns 1 and 5 illustrate the contemporaneous effect (in 2016), while columns 2 and 6, 3 and 7, and 4 and 8 show the lagged effects in 2017, 2018 and 2019, respectively. The different specifications serve the purpose to illustrate the sensitivity of our estimates to alternative choices for key parameters in the RD setting.

Table 4.1, Panel A, shows that there are no effects of MFBS on firm's costs: the coefficients are very small and not statistically different from zero both using linear and quadratic polynomial in sales. This result is confirmed in Table A.1 in the Appendix, which replicates the analysis separately for the cost of labor and the cost of services.

When we look at access to external finance, we document several negative effects. The estimates in Panel B indicate that the eligibility for the adoption of MFBS has negative effects on ownership transfers. The coefficient is statistically significant by 2 percentage points in 2017 and 2018, while it is not significant in 2019. This result is consistent with the idea that MFBS slows down, but not eliminate, the micro-firm acquisition process; *Note Integrative* are therefore able to reduce, at least in the short run, the presence of information frictions in the equity market.

Using as a dependent variable the probability of having at least one loan with an Italian bank, the results reveal a sizable and statistically significant effect starting from 2018, which amounts to roughly 2.5-3 percentage points higher probability of having secured a loan for firms above the relevant eligibility threshold. This finding is consistent with the idea that banks use information in corporate non-financial reporting to determine firm's creditworthiness: in other words, lower firm's transparency results in more difficult access to credit.

However, this effect has no bite if the firm already has a reputation in the credit market. Panel D shows that the effect on the number of banking relationships (when the firm has at least one relationship) is statistically insignificant, indicating that eligibility for MFBS does not alter the size of the pool of banks from which a firm can borrow. Similarly, Panel E suggests that the amount of credit granted (for firms with strictly positive banking loans) remains

unaffected, as the estimated coefficients are close to zero and statistically insignificant.[4]

Table 4.1: ITT: the eligibility effect on firm outcomes

Specification	Linear				Quadratic			
Outcome measured in:	2016	2017	2018	2019	2016	2017	2018	2019
Panel A: Total costs over sales								
Coefficient	-0.0014	-0.0040	0.0003	-0.0051	-0.0009	-0.0025	0.0006	-0.0041
	(0.0045)	(0.0048)	(0.0049)	(0.0051)	(0.0059)	(0.0063)	(0.0065)	(0.0067)
Obs. elig	20465	18690	17632	16662	25384	23183	21835	20607
Obs. not elig	16258	15093	14301	13528	19329	17945	16998	16108
Panel B: Indicator change in shareholders								
Coefficient	-0.0038	-0.0209***	-0.0172**	-0.0088	-0.0056	-0.0248***	-0.0211**	-0.0113
	(0.0051)	(0.0070)	(0.0083)	(0.0090)	(0.0061)	(0.0084)	(0.0099)	(0.0107)
Obs. elig	25253	23042	21761	20487	38413	34863	32697	30646
Obs. no elig	19481	18076	17159	16288	27117	25139	23881	22744
Panel C: Indicator banking linkage								
Coefficient	-0.0112	-0.0155	-0.0265**	-0.0311**	-0.0144	-0.0172	-0.0283**	-0.0329**
	(0.0113)	(0.0118)	(0.0121)	(0.0125)	(0.0116)	(0.0122)	(0.0125)	(0.0129)
Obs. elig	18710	17104	16145	15255	39467	35855	33593	31507
Obs. not elig	15124	14050	13310	12571	27210	25219	23904	22732
Panel D: Number of banks								
Coefficient	-0.0191	-0.0093	-0.0055	-0.0062	-0.0178	-0.0102	-0.0103	-0.0056
	(0.0252)	(0.0269)	(0.0278)	(0.0304)	(0.0267)	(0.0285)	(0.0295)	(0.0322)
Obs. elig	12347	11308	10662	9949	23340	21189	19754	18245
Obs. not elig	10610	9876	9447	8959	18708	17462	16677	15926
Panel E: Bank loans over assets								
Coefficient	0.0044	0.0044	0.0034	-0.0080	0.0047	0.0043	0.0030	-0.0097
	(0.0080)	(0.0083)	(0.0085)	(0.0084)	(0.0095)	(0.0098)	(0.0101)	(0.0100)
Obs. elig	14650	13411	12619	11806	22107	20147	18763	17378
Obs. not elig	12365	11531	11066	10541	18065	16873	16156	15432

Notes: The table shows the conventional RD estimates of the Intention-To-Treat effect of passing the relevant threshold in the forcing variable from the left to the right. The forcing variable is defined as the maximum value of firm's sales in 2014 and 2015 and the most numerous eligibility threshold is used. Regressions control for sector fixed effects and a dummy variable for the type of conditioning variable (i.e. assets or employment). Robust standard errors in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Graphical evidence corroborates our results. In the interest of brevity, we do not report RD graphs for all variables of interest but focus on those that appear to be affected by eligibility. More specifically, we study the effects on different time horizons of MFBS eligibility on the probability of ownership transfers with respect to 2015 and having access to bank credit, as in Panels B and C of Table 4.1. The RD plots in Figure 4.1 and 4.2 confirm regression results. The eligibility effects are visible in 2017 and 2018 for the change in ownership, and in 2018 and 2019 for the probability of having at least a banking relationship. This pattern

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[4]We do not analyze the effect on the cost of credit because this information is available only for a subset of microfirms in *Centrale dei rischi*.
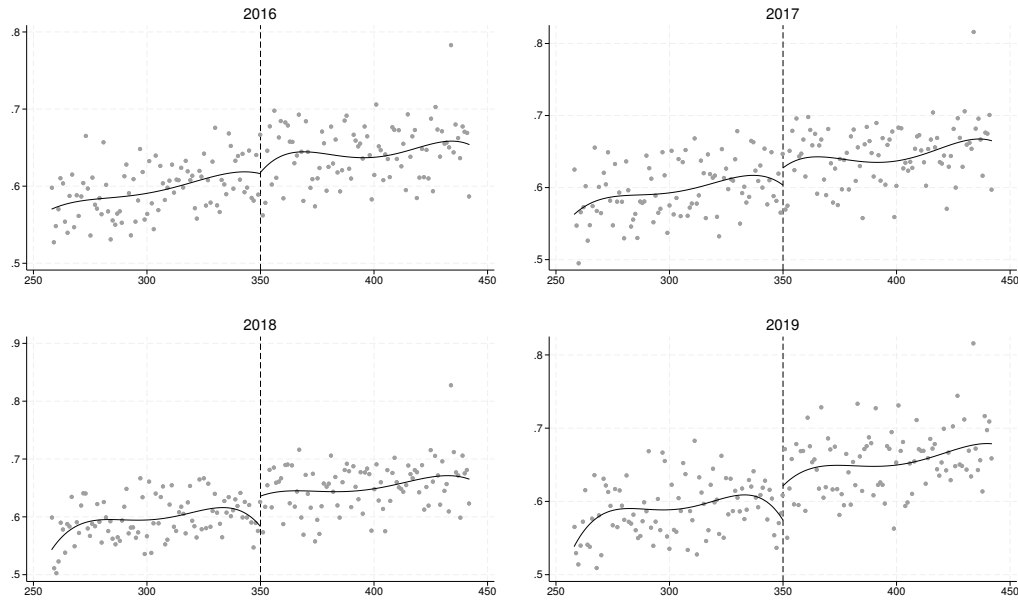
Figure 4.1: Ownership changes with respect to 2015
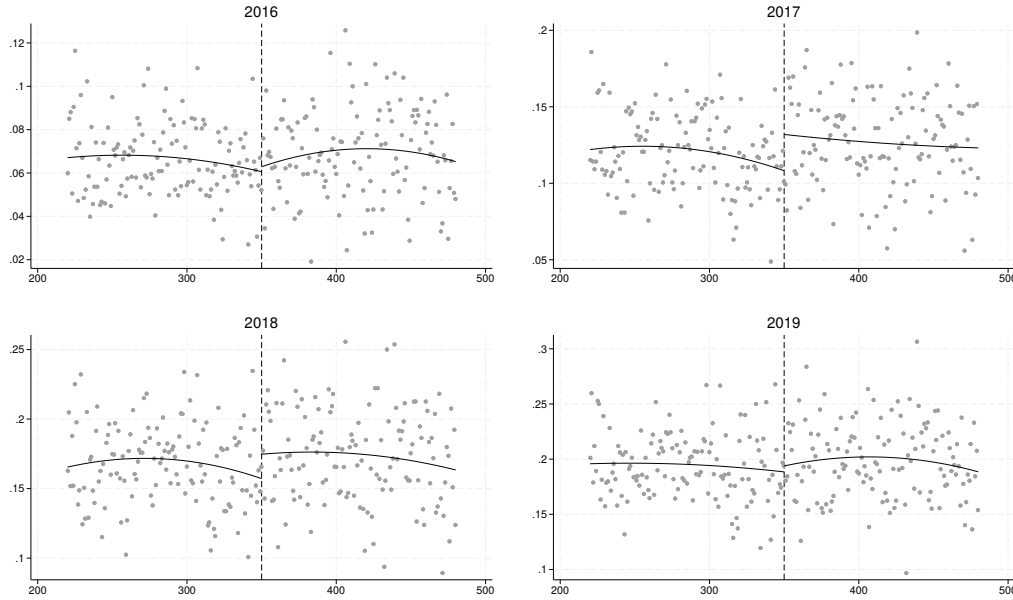
Note: The figure plots the binned averages of the indicator for firms changing their ownership structure with respect to 2015 against the forcing variable of sales defined as the maximum value of firm's sales in 2014 and 2015 at the most numerous eligibility threshold. The eligibility is defined in 2016 and different panels show the contemporaneous and lagged effects of the treatment in 2016 (upper left), in 2017 (upper right), in 2018 (bottom left) and in 2019 (bottom right).

was not present in the years prior to treatment, as shown in Figures 4.3 and 4.4, the evidence that supports the identifying assumption RD2 that requires that the potential outcomes are continuous.

We then present fuzzy RD regressions in which MFBS is instrumented by the eligibility in Table 4.2. The bottom panel reports the first stage coefficient and shows that the change in eligibility status determines an increase in the probability of being treated by roughly seven percentage points on the threshold; the coefficient is statistically significant but relatively low in magnitude due to the presence of a two-sided non-compliance.

In line with the reduced form evidence in Table 4.1, we confirm the results on the change in shareholders (Panel B) and the extensive margin of credit (Panel C). The economic impact of MFBS is sizable. As for the equity market, a standard deviation increase in the probability to be treated in the neighborhood of the cutoff (0.39) reduces the probability to observe a change in shareholders composition by 10 percentage points (-0.27*0.38) in 2018, which corresponds to slightly more than a quarter of the standard deviation of the outcome variable in that year. The effect is quantitatively larger for access to the credit market. A standard deviation increase in the probability to be treated reduces the likelihood to have a banking linkage

Figure 4.2: Extensive margin: probability to have at least one bank relationship

Note: The figure plots the binned averages of the indicator for firms having at least one banking relationship against the forcing variable of sales defined as the maximum value of firm's sales in 2014 and 2015 at the most numerous eligibility threshold. The eligibility is defined in 2016 and different panels show the contemporaneous and lagged effects of the treatment in 2016 (upper left), in 2017 (upper right), in 2018 (bottom left) and in 2019 (bottom right).

Figure 4.3: Falsification test: Ownership change
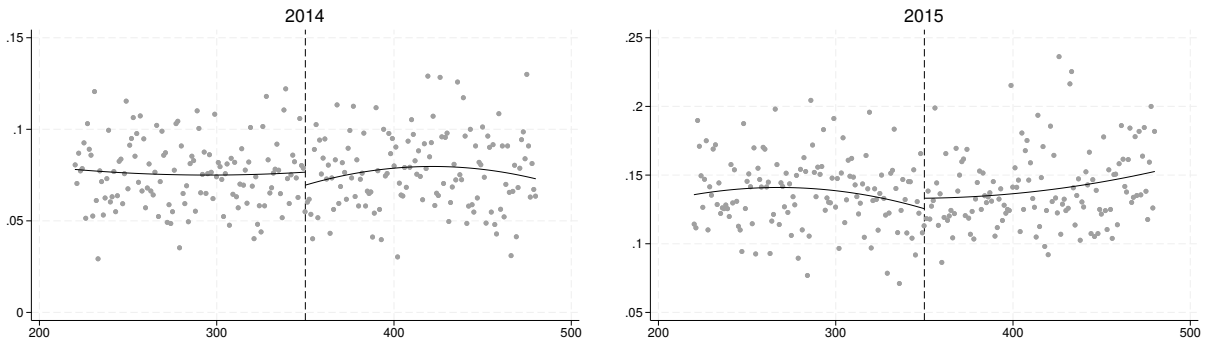
Note: The figure plots the binned averages of the indicator for firms that changed their ownership structure with respect to the previous year against the baseline forcing variable of sales (defined as the maximum value of firm's sales in 2014 and 2015). The eligibility is defined in 2016 and different panels show the effect of the treatment on past outcomes in 2014 (left) and in 2015 (right).

Figure 4.4: Falsification test: probability to have at least one bank relationship
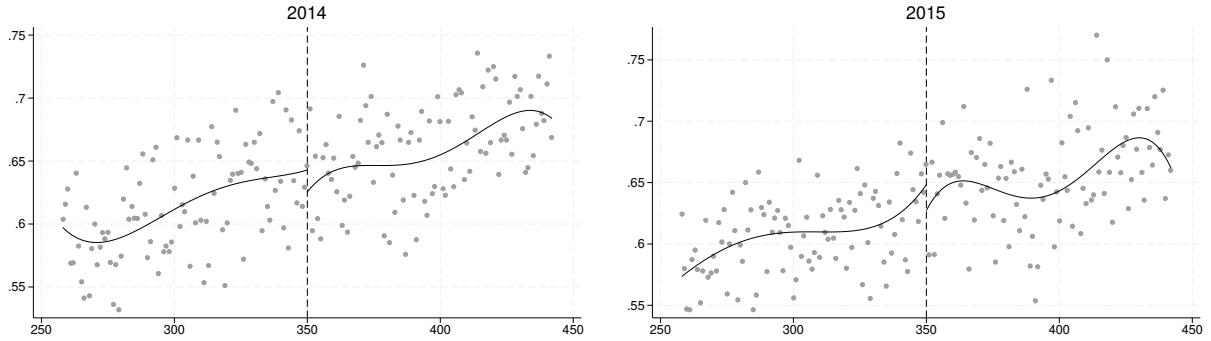

Note: The figure plots the binned averages of the indicator for firms having at least one banking relationship against the baseline forcing variable of sales (defined as the maximum value of firm's sales in 2014 and 2015). The eligibility is defined in 2016 and different panels show the effect of the treatment on past outcomes in 2014 (left) and in 2015 (right).

by 17 percentage points in 2019 (-0.45*0.38), which corresponds to one-third of a standard deviation of the outcome variable in the same year.

Focusing on the result of the extensive margin of the credit market, that is larger and more persistent, we explore the role of firms' entry or exit. The adoption of MFBS can both hamper the entry of firms that did not have a credit relationship before treatment and determine the termination of existing banking relationships, due to increased perceived risks by the credit institution. Table 4.3 shows that the estimated average effect is driven by the reduced entry. For the firms with no banking relations before 2016, a standard deviation increase in the probability to be treated near the threshold (0.40) determines a reduction in the probability to establish a banking relation in 2019 by 26 percentage points (-0.64*0.40), that corresponds to more the three-quarters of a standard deviation of the outcome variable near the cutoff. The impact is therefore large but imprecisely estimated due to reduced sample size. The coefficient for exit is instead very close to zero, confirming the evidence of Panels D and E of Table 4.1 that firms with a reputation in the credit market were basically unaffected by the adoption of MFBS.

4.b   Validation and robustness

We test whether our results are dependent on a specific set of control variables. Table 4.4 shows the results of two alternative fuzzy RDD estimations: in the first panel, for each outcome, we include only the dummy for the type of conditioning variable (i.e. assets or employment), without controlling for sector fixed effects. In the second set of estimates, control for both the dummy for the type of conditioning variable and firm-location fixed

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Table 4.2: Fuzzy RD: the MFBS effect on firm outcomes

Specification	Linear				Quadratic			
Outcome measured in:	2016	2017	2018	2019	2016	2017	2018	2019
Panel A: Total costs over sales								
Coefficient	-0.0139	-0.0468	0.0119	-0.0625	-0.0186	-0.0555	-0.0005	-0.0732
	(0.0632)	(0.0690)	(0.0704)	(0.0697)	(0.0939)	(0.1007)	(0.1076)	(0.1023)
Obs. elig	16289	14877	14055	13290	23172	21147	19955	18847
Obs. not elig	13470	12507	11850	11162	17948	16675	15783	14947
Panel B: Indicator change in shareholders								
Coefficient	-0.0896	-0.3167***	-0.2700**	-0.1293	-0.0879	-0.3694**	-0.3238*	-0.1568
	(0.0807)	(0.1169)	(0.1341)	(0.1376)	(0.0978)	(0.1473)	(0.1683)	(0.1705)
Obs. elig	15784	14405	13650	12896	29340	26718	25186	23678
Obs. no elig	13138	12200	11594	10937	21952	20350	19311	18382
Panel C: Indicator banking linkage								
Coefficient	-0.1688	-0.2883	-0.4482**	-0.4536**	-0.2048	-0.2919	-0.4439**	-0.4826**
	(0.1805)	(0.1921)	(0.2028)	(0.1983)	(0.1727)	(0.1928)	(0.1981)	(0.1979)
Obs. elig	13063	11902	11238	10667	33956	30905	29076	27298
Obs. not elig	11066	10264	9737	9159	24208	22427	21249	20200
Panel D: Number of banks								
Coefficient	-0.2006	-0.1033	-0.0620	-0.0661	-0.2312	-0.1558	-0.1438	-0.0934
	(0.2668)	(0.3079)	(0.3015)	(0.3183)	(0.3184)	(0.3714)	(0.3559)	(0.3760)
Obs. elig	12347	11306	10658	9941	22505	20453	19084	17630
Obs. not elig	10610	9875	9444	8957	18086	16889	16111	15388
Panel E: Bank loans over assets								
Coefficient	0.1038	0.1031	0.1023	-0.0507	0.0602	0.0495	0.0347	-0.1148
	(0.1103)	(0.1195)	(0.1227)	(0.1118)	(0.1121)	(0.1245)	(0.1223)	(0.1169)
Obs. elig	9468	8674	8192	7685	21316	19439	18142	16806
Obs. not elig	8401	7802	7516	7106	17447	16304	15590	14889
First stage								
β	0.0707***	0.0724***	0.0695***	0.0744***	0.0685***	0.0668***	0.0659***	0.0704***
	(0.0114)	(0.0118)	(0.0121)	(0.0123)	(0.0106)	(0.0110)	(0.0113)	(0.0115)

Notes: The table shows the conventional RD estimates of local average treatment effect of filing a MFBS where the treatment is instrumented by passing the relevant threshold in the forcing variable from the left to the right. The forcing variable is defined as the maximum value of firm's sales in 2014 and 2015 and the most numerous eligibility threshold is used. Regressions control for sector fixed effects and a dummy variable for the type of conditioning variable (i.e. assets or employment). Robust standard errors in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

effects (dummy for four Italian macro-regions). Regression results confirm the baseline estimates, even in magnitude.

We next make sure that the discontinuities exploited in our empirical framework are not capturing the impact of some other unobserved variable that is spuriously correlated with firm size instead of the actual effect of the reform. In fact, there may be other relevant size thresholds – not related to disclosure regulation – that result in similar effects. To this end, we consider alternative placebo cut-offs based on the definition of micro companies

Table 4.3: Entry and exit in the credit market

Outcome measured in:	2016	2017	2018	2019
Panel A: Entry (no banking relations before 2016)				
Coefficient	-0.1355	-0.4039	-0.5725*	-0.6387*
	(0.2127)	(0.2664)	(0.3155)	(0.3588)
Obs. elig	4418	3991	3726	3505
Obs. not elig	3380	3118	2936	2707
Panel B: Exit (At least one banking relation before 2016)				
Coefficient	0.1655	0.2451	-0.0298	0.0173
	(0.1491)	(0.1712)	(0.1760)	(0.1674)
Obs. elig	7892	7175	6779	6434
Obs. no elig	7113	6577	6231	5898

Notes: The table shows the conventional RD estimates of local average treatment effect of filing a MFBS where the treatment is instrumented by passing the relevant threshold in the forcing variable from the left to the right. Linear polynomial specification. The forcing variable is defined as the maximum value of firm's sales in 2014 and 2015 and the most numerous eligibility threshold is used. Regressions control for sector fixed effects and a dummy variable for the type of conditioning variable (i.e. assets or employment). Robust standard errors in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

originally provided in the 2013 Accounting directive of the European Union (2013/34/EU) and re-run our baseline estimation.[5] Given that the thresholds suggested in the EU Directive may still reflect salient differences in firm characteristics and performances, and even though we are not aware of any other policy that is based on the same definition of micro firms, we set as placebo values the corresponding default thresholds for micro undertakings outlined in the EU regulation: turnover and total assets should be below €700k million and €350k million, respectively; the average number of employees during the financial year should not exceed 10.[6] Note that, by construction, the estimation sample is not equivalent to the one in the baseline analysis, even if we maintain the sampling criterion of excluding observations exceeding at least one of the simplified (*abbreviato*) balance-sheet thresholds (assets: €4,400k; sales: €8,800k; employees: 50). Table 4.5 reports the results of the ITT analysis using the placebo eligibility thresholds. As expected, RD estimates of firm size exceeding the EU thresholds portray the lack of an association with the savings in total costs (Panel A), composition of shareholders (Panel B) and both the intensive and extensive margin of relationships with banks (Panel C and D). Panel E captures a statistically significant

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[5]When transposing the Directive, Member States had the option to increase or decrease any of the thresholds for small undertakings (up to a maximum). The Italian legislator re-scaled the indicated size thresholds to align them with the national market structure, characterized by smaller firms.

[6]As mentioned in Section 2, the micro undertaking must be within any two of the three size thresholds for two successive accounting periods; the thresholds for the firm categories are periodically adjusted to inflation, with the thresholds mandated in 2013 lasting in force until 2023.

Table 4.4: The effect on firm outcomes with alternative sets of covariates

Specification	Linear				Quadratic			
Outcome measured in:	2016	2017	2018	2019	2016	2017	2018	2019
Panel A1: Total costs over sales (no controls)								
Coefficient	-0.0337	-0.0596	-0.0296	-0.0828	-0.0408	-0.0650	-0.0331	-0.0803
	(0.0778)	(0.0835)	(0.0860)	(0.0840)	(0.0997)	(0.1059)	(0.1128)	(0.1071)
Panel A2: Total costs over sales (controlling for location fixed effects)								
Coefficient	-0.0248	-0.0523	-0.0077	-0.0642	-0.0380	-0.0631	-0.0296	-0.0774
	(0.0667)	(0.0724)	(0.0738)	(0.0728)	(0.1012)	(0.1072)	(0.1146)	(0.1080)
Panel B1: Indicator change in shareholders (no controls)								
Coefficient	-0.0821	-0.3148***	-0.2591*	-0.1255	-0.0740	-0.3586**	-0.2938*	-0.1416
	(0.0804)	(0.1171)	(0.1348)	(0.1385)	(0.0937)	(0.1417)	(0.1602)	(0.1639)
Panel B2: Indicator change in shareholders (controlling for location fixed effects)								
Coefficient	-0.0794	-0.3143***	-0.2666**	-0.1340	-0.0671	-0.3540***	-0.2901*	-0.1408
	(0.0771)	(0.1127)	(0.1294)	(0.1325)	(0.0904)	(0.1365)	(0.1534)	(0.1566)
Panel C1: Indicator banking linkage (no controls)								
Coefficient	-0.1852	-0.3068	-0.4674**	-0.4829**	-0.2255	-0.3127	-0.4624**	-0.5063**
	(0.1831)	(0.1949)	(0.2062)	(0.2027)	(0.1774)	(0.1974)	(0.2027)	(0.2028)
Panel C2: Indicator banking linkage (controlling for location fixed effects)								
Coefficient	-0.1907	-0.2998	-0.4668**	-0.4754**	-0.2388	-0.3118	-0.4707**	-0.5014**
	(0.1779)	(0.1896)	(0.2008)	(0.1959)	(0.1770)	(0.1957)	(0.2019)	(0.2000)
Panel D1: Number of banks (no controls)								
Coefficient	-0.2629	-0.1916	-0.0959	-0.0444	-0.2508	-0.2094	-0.1701	-0.0599
	(0.2991)	(0.3417)	(0.3387)	(0.3495)	(0.3366)	(0.3915)	(0.3794)	(0.3977)
Panel D2: Number of banks (controlling for location fixed effects)								
Coefficient	-0.2622	-0.1977	-0.0935	-0.0434	-0.2451	-0.2087	-0.1649	-0.0574
	(0.2927)	(0.3371)	(0.3321)	(0.3461)	(0.3283)	(0.3830)	(0.3689)	(0.3897)
Panel E1: Bank loans over assets (no controls)								
Coefficient	0.1112	0.1017	0.1241	-0.0299	0.0730	0.0419	0.0454	-0.1030
	(0.1202)	(0.1279)	(0.1342)	(0.1206)	(0.1203)	(0.1327)	(0.1314)	(0.1244)
Panel E2: Bank loans over assets (controlling for location fixed effects)								
Coefficient	0.1031	0.0838	0.1045	-0.0528	0.0663	0.0380	0.0393	-0.1104
	(0.1159)	(0.1251)	(0.1298)	(0.1177)	(0.1157)	(0.1280)	(0.1264)	(0.1205)
First stage (no controls)								
β	0.0693***	0.0716***	0.0684***	0.0728***	0.0668***	0.0659***	0.0648***	0.0692***
	(0.0116)	(0.0120)	(0.0124)	(0.0125)	(0.0108)	(0.0112)	(0.0115)	(0.0117)
First stage (controlling for location fixed effects)								
β	0.0682***	0.0709***	0.0673***	0.0722***	0.0654***	0.0648***	0.0635***	0.0684***
	(0.0116)	(0.0120)	(0.0124)	(0.0126)	(0.0108)	(0.0111)	(0.0115)	(0.0117)

Notes: The table shows the conventional RD estimates of local average treatment effect of filing a MFBS where the treatment is instrumented by passing the relevant threshold in the forcing variable from the left to the right. The forcing variable is defined as the maximum value of firm's sales in 2014 and 2015. In Panel A1, B1, C1, D1, and E1, we only control for the dummy variable for the type of conditioning variable (i.e. assets or employment). Regressions in Panel A2, B2, C2, D2, and E2 control for both the dummy variable for the type of conditioning variable (i.e. assets or employment) and firm-location fixed effects. The mean of the outcome variable for the reference non-eligible group, as well as the number of observations for the eligible and non-eligible estimation samples, are omitted from the table for readability and are available upon request. Robust standard errors in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

increase in the amount of loans for firms above the threshold in 2017 and 2018.

Table 4.5: The effect on firm outcomes with placebo eligibility thresholds

Specification	Linear				Quadratic			
Outcome measured in:	2016	2017	2018	2019	2016	2017	2018	2019
Panel A: Total costs over sales								
Coefficient	-0.0024	-0.0049	-0.0028	-0.0091**	-0.0025	-0.0041	-0.0017	-0.0081
	(0.0040)	(0.0041)	(0.0042)	(0.0045)	(0.0049)	(0.0051)	(0.0051)	(0.0055)
Obs. elig	18915	17549	16743	16130	28379	26255	24945	23987
Obs. no elig	15243	14231	13588	13069	20796	19415	18558	17848
Panel B: Indicator change in shareholders								
Coefficient	-0.0021	0.0027	-0.0036	0.0024	-0.0023	0.0037	-0.0032	0.0012
	(0.0054)	(0.0073)	(0.0086)	(0.0091)	(0.0065)	(0.0087)	(0.0102)	(0.0109)
Obs. elig	24496	22668	21577	20828	38241	35361	33503	32042
Obs. no elig	18582	17369	16628	16031	25865	24181	23150	22311
Panel C: Indicator banking linkage								
Coefficient	0.0090	-0.0032	-0.0007	0.0028	0.0099	-0.0019	0.0007	0.0043
	(0.0097)	(0.0101)	(0.0103)	(0.0105)	(0.0104)	(0.0108)	(0.0109)	(0.0112)
Obs. elig	18705	17348	16544	15957	37474	34635	32730	31348
Obs. no elig	15069	14084	13446	12944	25353	23668	22648	21810
Panel D: Number of banks								
Coefficient	0.0098	0.0067	0.0121	0.0415	0.0022	0.0002	0.0117	0.0401
	(0.0298)	(0.0309)	(0.0318)	(0.0341)	(0.0311)	(0.0322)	(0.0331)	(0.0356)
Obs. elig	16729	15572	14853	14255	33577	30922	29153	27666
Obs. no elig	13656	12769	12297	11795	23629	22210	21386	20512
Panel E: Bank loans over assets								
Coefficient	0.0052	0.0138*	0.0184**	0.0068	0.0071	0.0158*	0.0222**	0.0073
	(0.0075)	(0.0076)	(0.0075)	(0.0076)	(0.0089)	(0.0091)	(0.0089)	(0.0090)
Obs. elig	17692	16432	15659	15001	26957	24992	23664	22571
Obs. no elig	14256	13368	12862	12336	19918	18694	18034	17289

Notes: The table shows the conventional RD estimates of the Intention-To-Treat effect of passing the placebo threshold of 700 thousand €in the forcing variable from the left to the right. The forcing variable is defined as the maximum value of firm's sales in 2014 and 2015. Regressions control for sector fixed effects and a dummy variable for the type of conditioning variable (i.e. assets or employment below the respective placebo threshold). Robust standard errors in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

As an additional validation exercise, we test whether the introduction of the MFBS has any effect on firms' likelihood of exiting the market in the years following implementation, which might induce sample selection. Specifically, we estimate the impact of MFBS adoption on a binary outcome equal to one if the firm exits in 2017, 2018, and 2019. The results, reported in the Appendix Table A.2, do not show a statistically significant effect across specifications, suggesting that the adoption of MFBS did not alter the survival dynamics of the firms in the short to medium term.

Additional robustness based on the choice of the bandwidth and the kernel used confirm our results and are available upon request.

4.c   Characterization of the compliers

The observation that some firms adopt the MFBS despite the lack of cost savings and an increased likelihood of exclusion from equity and credit markets raises the need to study the characteristics of these compliers and the motivations behind their decisions. In order to characterize the firms that adopt the MFBS, we assess the heterogeneity in compliance behavior by interacting – in the first stage of fuzzy RD estimation (see equation (2)) – the eligibility rule with various observable characteristics. This approach is similar to the one outlined by Angrist and Pischke (2009), which is instead based on sample splits.[7]

Table 4.6 reports the results.[8] Compliance rates are higher among firms characterized by older shareholders, longer persistence in the market, higher labor productivity, and more concentrated shareholder structure.[9] These results show that firms choose to withhold information when they have a more established reputation and when transparency needs toward other shareholders are less pressing. We do not find, instead, heterogeneous behavior in terms of markup,[10]; yet, this result should be interpreted with caution because estimates on sectoral markups are available for manufacturing firms only.

The market conditions for access to external finance also play a role in determining compliance. The interaction between the eligibility status and the external finance dependence index at the 2-digit ATECO level is negative and highly significant, thus indicating that the adoption of MFBS is more frequent in industries reliant on internal resources for the investment activities.[11]

Also the structure of local credit markets plays a role. Small banks in Italy are characterized by a more intensive use of soft information for lending decisions; we expect that in areas in which small banks are predominant firms are less compelled to provide detailed information in their balance sheets. To investigate this channel, we use confidential information on the market share of each bank at province level.[12] More specifically, we construct a dummy variable equal to one if the share in the lending market by small banks exceeds 45 per cent,

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[7]This type of heterogeneity analysis in a RD framework was first introduced by Becker et al. (2013).

[8]Appendix table A.3 provides the balancing properties for all variables analyzed in this section.

[9]As a proxy for labor productivity, we use value added per employee based on balance-sheet and INPS data. Information on the shareholders' age and the number of shareholders is taken from the Infocamere dataset. Age is weighted according to individual shares; see Baltrunaite et al. (2024) for details.

[10]We use as a proxy for market concentration the revenue-weighted average markup *à la* DeLoecker et al. (2020), as computed by Ciapanna et al. (2024) for the Italian economy, aggregated at the 2-digit level of the ATECO classification.

[11]External finance dependence is constructed following Rajan and Zingales (1998) as the percentage difference between investments and cash flow on Cerved data.

[12]Provinces in Italy correspond to the NUTS3 level of the European classification. The classification of banks according to their size reflects the total intermediated funds in 2015 (year before treatment).

that corresponds to the top tercile of the distribution. Results show that the compliance was larger in areas where local banks are prevalent; this confirms the fact that the use of MFBS was more intense in local credit markets dominated by financial institutions that rely less on hard information.

Finally, we do not find heterogeneous compliance rates according to the firm's liquidity, defined as the ratio of current assets to current liabilities.

Table 4.6: Characterization of the compliers

Characteristics:		Eligibility dummy	Interaction term
1.a)	Markup (manuf. only, 2 digit NACE, Ciapanna et al., 2024)	0.1024 (0.1989)	-0.0252 (0.1846)
1.b)	Average age of shareholders	0.0036 (0.0318)	0.0013** (0.0006)
1.c)	Age of the firm	0.0517*** (0.0142)	0.0013** (0.0006)
1.d)	Number of shareholders	0.1144** (0.0193)	-0.0186** (0.0075)
1.e)	log VA per employee	0.0017 (0.0380)	0.0193** (0.0095)
2.a)	External finance dependence (2 digit NACE, Rajan and Zingales, 1998)	0.0558*** (0.0123)	-0.0372*** (0.0114)
2.b)	Liquidity index	0.0702*** (0.0115)	-0.0000 (0.0000)
2.c)	Prevalence of local banks	0.0633*** (0.0120)	0.0224* (0.0132)

Note: The characterization of the compliers is based on the interaction between the eligibility dummy and the variable of interest of the first-stage regressions. All regressions control for sector fixed effects and a dummy for the conditioning variable (assets or employment). Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5 Concluding remarks

We study the effects of lower non-financial disclosure regulation on the trade-off between internal costs and access to external financing for a large sample of small limited liability companies in Italy. Our identification strategy exploits an institutional change that exempted firms below multiple size cut-offs from filing reports that complement standard balance sheet items with qualitative information and are visible to companies' stakeholders. We leverage

this feature for a Regression Discontinuity Design that allows to uncover causal estimates of lower reporting requirements on firm outcomes. The evidence reveals no tangible effects on firms' operating costs. Interestingly, we find that withholding non-financial information reduces the firm's capacity to access to bank credit, in line with the idea that disclosure may reduce information asymmetries, especially for new entrants in the credit market. We also find a temporary impact on change in shareholders, a result that hints at the fact that non-financial disclosure documents can accelerate the entry in the equity market.

Our findings show novel evidence that disclosure matters also outside of the realm of large publicly listed companies. In fact, we establish that disclosing qualitative information helps firms to secure external financing, relaxing their credit constraints and potentially boosting their growth prospects. Contrary to the disclosure critiques of being too costly, these simple reports do not appear to impose substantial costs on micro-firms. All in all, our findings may inform policy-making by illustrating potential benefits of regulation or by indirectly showing that non-financial reporting goes beyond the "cheap talk" and carries value in economic exchanges.

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Appendix A Additional outcomes

Figure A.1: First stage: probability of filing a MFBS, employment & assets forcing variables
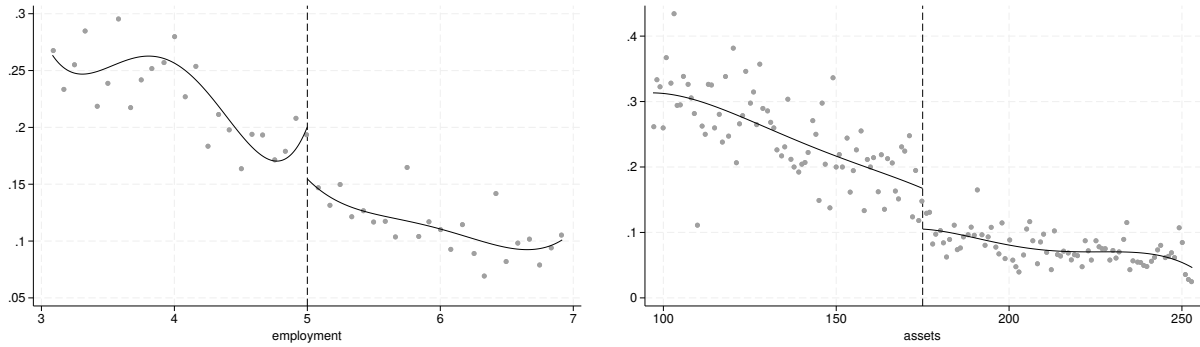
Note: The figure plots the binned averages of the indicator for firms filing a micro firm balance sheet (MFBS) against the forcing variables employment & assets, respectively defined as the maximum value of firm's employees & assets in 2014 and 2015.

Table A.1: The MFBS effect on firm outcomes: cost of labor and services

Specification	Linear				Quadratic			
Outcome measured in:	2016	2017	2018	2019	2016	2017	2018	2019
Panel A: Cost of labour (ITT)								
Coefficient	0.0191	0.0022	-0.0102	0.0131	0.0045	-0.0011	-0.0151	-0.0134
Std. Error	(0.0281)	(0.0045)	(0.0115)	(0.0125)	(0.0301)	(0.0073)	(0.0169)	(0.0121)
Obs. elig	36015	32725	30745	28869	27417	25009	23561	22206
Obs. not elig	25405	23552	22330	21226	20598	19115	18098	17169
Panel B: Cost of services (ITT)								
Coefficient	0.0636	-0.0039	0.0131	-0.0154	0.0754	-0.0097	0.0114	-0.0224*
Std. Error	(0.0478)	(0.0085)	(0.0231)	(0.0104)	(0.0495)	(0.0131)	(0.0251)	(0.0118)
Obs. elig	20465	18690	17632	16662	23850	21788	20551	19422
Obs. not elig	16258	15093	14301	13528	18359	17055	16142	15296
Panel C: Cost of labour (fuzzy RD)								
Coefficient	0.0857	-0.0017	-0.2091	-0.1987	0.0566	-0.0156	-0.2280	-0.1973
Std. Error	(0.3742)	(0.0922)	(0.2161)	(0.1632)	(0.4342)	(0.1092)	(0.2563)	(0.1767)
Obs. elig	13874	12649	11945	11323	27202	24813	23381	22034
Obs. not elig	11694	10861	10300	9685	20472	19002	17990	17064
Panel D: Cost of services (fuzzy RD)								
Coefficient	0.8514	-0.0867	0.1572	-0.2125	1.1440	-0.1374	0.1904	-0.3123*
Std. Error	(0.6144)	(0.1242)	(0.2907)	(0.1296)	(0.7696)	(0.1972)	(0.3932)	(0.1777)
Obs. elig	20465	18690	17632	16662	23850	21788	20551	19422
Obs. not elig	16258	15093	14301	13528	18359	17055	16142	15296
First stage								
β	0.0707***	0.0724***	0.0695***	0.0744***	0.0685***	0.0668***	0.0659***	0.0704***
	(0.0114)	(0.0118)	(0.0121)	(0.0123)	(0.0106)	(0.0110)	(0.0113)	(0.0115)

Notes: The table reports Regression Discontinuity estimates of the effect of filing a MFBS on two outcome variables from balance-sheet data, namely cost of labor and cost of services. For each outcome, we show results from both a sharp RD specification, where eligibility to the simplified regime is used as the treatment, and a fuzzy RD specification, where eligibility is used as an instrument for actual take-up. The forcing variable is defined as the maximum value of firm's sales in 2014 and 2015, and the most populous eligibility threshold is used. All regressions control for sector fixed effects and a dummy for the conditioning variable (assets or employment). Robust standard errors in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Table A.2: The MFBS effect on firm outcomes: probability of exit

Specification	Linear			Quadratic		
Outcome measured in:	2017	2018	2019	2017	2018	2019
Panel A: Probability of exit (ITT)						
Coefficient	0.0023	0.0054	-0.0084	0.0016	0.0076	-0.0073
Std. Error	(0.0070)	(0.0079)	(0.0089)	(0.0081)	(0.0092)	(0.0103)
Obs. elig	22551	23455	22995	37260	38163	37699
Obs. no elig	17563	18128	17847	26014	26490	26244
Panel B: Probability of exit (fuzzy RD)						
Coefficient	0.0225	0.0500	-0.0858	-0.0041	0.0390	-0.1301
Std. Error	(0.0814)	(0.1468)	(0.1623)	(0.1187)	(0.1576)	(0.1813)
Obs. elig	21231	12210	12427	33227	27058	25906
Obs. no elig	16742	10394	10564	23852	20384	19634
First stage						
β	0.0706***	0.0706***	0.0706***	0.0684***	0.0684***	0.0684***
	(0.0114)	(0.0114)	(0.0114)	(0.0106)	(0.0106)	(0.0106)

Notes: The table reports Regression Discontinuity estimates of the effect of filing a MFBS on the probability of firm exit. The dependent variable is a dummy equal to one if the firm exited in year t. We show results from both a sharp RD specification, where eligibility to the simplified regime is used as the treatment, and a fuzzy RD specification, where eligibility is used as an instrument for actual take-up. The forcing variable is defined as the maximum value of firm's sales in 2014 and 2015, and the most populous eligibility threshold is used. All regressions control for sector fixed effects and a dummy for the conditioning variable (assets or employment). Robust standard errors in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Table A.3: Balancing properties

Characteristics:		Eligibility dummy
1.a)	Markup	-0.0000
	(manuf. only, 2 digit NACE, Ciapanna et al., 2024)	(0.0000)
1.b)	Age of shareholders	0.2626
		(0.3563)
1.c)	Age of the firm	0.0248
		(0.3322)
1.d)	Number of shareholders	0.0393
		(0.0358)
1.e)	log VA per employee	-0.0340
		(0.0228)
2.a)	External finance dependence	-0.0000
	(2 digit NACE, Rajan and Zingales, 1998)	(0.0000)
2.b)	Liquidity index	131.8687
		(104.8582)
2.c)	Prevalence of local banks	0.0037
		(0.0152)

Notes: The table reports the balancing properties of the eligibility status on the threshold. All regressions control for sector fixed effects and a dummy for the conditioning variable (assets or employment). Robust standard errors in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.