

Mandatory Disclosure of Investor Complaints and Firm Risk: Evidence from India

AVTORJI: Ni navedeno

POVZETEK: Članek raziskuje, kako se likvidni trgi odzivijo na obvezno izvajanje o podatkih o nasiljanih zastopanih in podatkih o podjetniškem riziku. V času, ko je 1. decembra 2015 v Indiji vnesli pravila o obvezni izvajanju o nasiljanih zastopanih, je izveden raziskovalni projekt, ki je obravnaval 722 indijskih podjetij od finančnega leta 2011 do 2021. Raziskovalci nameravali so oceniti, kako se podjetniško riziko in idiosinkratično riziko spremeni, ko se podjetja zaposlijo v obvezni izvajanju o nasiljanih zastopanih. Natančno so najdeni rezultati, da podjetja z visoko pred-izvajansko nasiljeno zastopanje so zaznamovala večjega podnebnega rizika, kar se je znotraj petletnega obdobja znotraj 4,7% podjetjaščega rizika.

METODA: Raziskovalci so uporabili različico metode razlike v različicah, ki je okupila podatke o 722 indijskih podjetjih od finančnega leta 2011 do 2021. Podatki so bili zbrani, saj so pravila obveznega izvajanja zaposlila vse odbrojene podjetja. Pred-trendni testi in tri pseudo-izdelki so bili neznačilni, kar je podpiralo natančnost rezultatov.

KLJUČNE UGOTOVITVE:

- Podjetja z visoko pred-izvajansko nasiljeno zastopanje so zaznamovala večjega podnebnega rizika.
- Podnebno riziko je povečalo o 2,6% med 25. in 75. percentilom pred-izvajanske nasiljene zastopanje.
- Efekt se ni zaznamal pri implementaciji, ampak je znotraj petletnega obdobja znotraj 4,7% podjetjaščega rizika.

POMEN ZA TRGOVANJE: Raziskovalni prispevek je pomemben za trgovce, ker pokaže, da obvezno izvajanje o nasiljanih zastopanih lahko poveča podnebno riziko podjetij. Trgovci bi morali upoštevati te ugotovitve pri ocenjevanju rizikov in investicij v podjetja, ki so zaposlila v obvezni izvajanju o nasiljanih zastopanih.

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Abstract

We examine how equity markets respond to mandatory disclosure of shareholder complaints. India's Listing Obligations and Disclosure Requirements Regulations (LODR), effective 1 December 2015, require listed firms to disclose quarterly the number of investor complaints received, resolved, and pending. Although the mandate applied to all listed firms, their informational exposure varied with pre-mandate complaint intensity. Firms with higher prior complaint intensity revealed more information about previously private grievances. Using 6,993 firm-year observations for 722 Indian listed firms from financial year 2011 to 2021, we find that a shift from the 25th to the 75th percentile of pre-mandate complaint intensity is associated with a 2.6% increase in idiosyncratic risk. Systematic risk is unaffected, and the total-risk effect persists after conditioning on systematic risk. Notably, the effect is absent in the implementation year but accumulates thereafter, reaching 4.7% by financial year 2021. Pre-trend tests and three pseudo-event placebos are insignificant, and the pattern is unaffected by the onset of the COVID-19 pandemic. Overall, the evidence suggests that the repeated disclosure of realized shareholder grievances is gradually reflected in equity prices. Rather than reducing uncertainty, greater visibility of shareholder grievances increases firm-specific uncertainty, revealing an unintended informational consequence of mandatory disclosure.

Keywords: mandatory disclosure; investor complaints; idiosyncratic risk; LODR; India

JEL classification: G14; G18; G34; K22

1. Introduction

Mandatory disclosure regimes rest on the premise that forcing private information into public view improves the informational efficiency of prices. Less well understood is how markets respond when the mandated information concerns a firm's treatment of its own shareholders. Investor complaints, grievances about share transfers, non-receipt of dividends, unanswered correspondence, and similar failures of shareholder service are a direct measure of a firm's conduct toward its investors or shareholders. They are also, in most jurisdictions, invisible to outsider participants. This paper studies what happens when investors' grievances become visible. In September 2015, the Securities and Exchange Board of India notified the Listing Obligations and Disclosure Requirements (LODR) Regulations, effective 1 December 2015, a date falling within the Indian financial year 2016, which runs from 1 April 2015 to 31 March 2016. Regulation 13 requires every listed entity to file with the stock exchange, within twenty-one days of each quarter-end, a statement of investor complaints that are pending, received, disposed of, and remaining unresolved. The mandate converted complaint data from a private signal, observable only to the firm and the regulator, into a standardized and recurring public disclosure. Crucially, the size of that information shock varied across firms: an entity with a long record of unresolved grievances revealed far more than one with a clean history.

Our setting is closely analogous to the United States risk-factor mandate studied by Campbell et al. (2014), who find that mandated risk disclosures are firm-specific rather than boilerplate and are reflected in systematic risk, idiosyncratic risk, information asymmetry, and firm value. Kravet and Muslu (2013) find that increases in risk-related disclosure are associated with higher return volatility around filing dates. Two features distinguish our setting. First, the disclosed content is not a managerial narrative about prospective risks but a verifiable count of realized investor or

shareholder grievances, which sidesteps the boilerplate concern entirely. Second, the disclosure recurs quarterly rather than annually, allowing us to observe how a repeated mandate is absorbed over time.

We exploit variation in disclosure intensity in a difference-in-differences design covering 722 Indian listed firms over financial years 2011 to 2021 (April 2010 through March 2021). Because the regulation applied to all listed entities simultaneously, there is no untreated control group in the conventional sense, and a binary indicator is uninformative: 705 of 779 firms with usable data, or more than ninety percent, received at least one complaint before the mandate. We therefore adopt a continuous, dose-type design, as formalized by Callaway et al. (2024), measuring treatment by the intensity of a firm's pre-mandate complaint record.

Three findings emerge. First, firms with higher pre-mandate complaint intensity experienced a significant increase in idiosyncratic risk relative to firms with lower intensity. Moving from the 25th to the 75th percentile of pre-mandate complaint intensity, roughly from two to thirty-three complaints per year, raises daily idiosyncratic volatility by 0.073 percentage points, or 2.6 percent of its sample mean. Second, the effect is confined to the idiosyncratic component: systematic risk shows no treatment effect, and the total-risk result survives conditioning on systematic risk. Third, and most distinctively, the effect does not appear at implementation but accumulates over the following five years, rising monotonically to 4.7 percent of the mean by FY2021.

The gradual onset is the paper's central contribution and distinguishes a recurring disclosure requirement from an announcement shock. A single quarterly filing conveys little. A multi-year series reveals whether complaint volumes are persistent or transitory, whether resolution is improving, and how a firm compares with peers now subject to identical reporting requirements.

Investors must accumulate a track record before the signal becomes informative and must develop interpretive conventions for a disclosure with no market precedent.

The identification rests on several tests. Event-study lead coefficients are individually and jointly insignificant. Pseudo-event placebos assigned to financial years 2012, 2013, and 2014 within the pre-mandate window return null results across all outcomes. Estimates are robust to industry-by-year fixed effects, which absorb the effects of demonetization, the goods and services tax rollout, and sector-level pandemic exposure; to alternative clustering; to propensity score matching; and to the exclusion of firms with zero complaint records, which may reflect non-filing. Because the largest coefficients fall in FY2020 and FY2021, we explicitly test whether the pandemic, rather than the mandate, drives the result and find no discontinuity at the pandemic boundary.

The study contributes to three strands of literature. To examine the capital-market consequences of mandated investor-complaint disclosure, the study provides evidence from a disclosure whose content is neither financial nor forward-looking. It shows that such disclosure alters the risk profile of the firms it exposes. The literature on idiosyncratic volatility and firm-specific information offers a setting in which the arrival of new firm-specific information is plausibly exogenous to the firm and precisely dated. Moreover, research on governance regulation in emerging markets, including Black and Khanna (2007) and Dharmapala and Khanna (2013), documents substantial value effects from India's earlier Clause 49 reforms and finds that a low-cost procedural mandate produced measurable capital-market consequences.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 develops hypotheses. Section 4 sets out the data and identification strategy. Section 5

reports results. Section 6 discusses interpretation, Section 7 concludes, and Section 8 sets out limitations.

2. Institutional Background

Before LODR, disclosure obligations for Indian listed companies rested on the Listing Agreement, a contractual arrangement between the exchange and the issuer, of which Clause 49 was the principal governance provision. Enforcement rested on the exchanges rather than directly on the regulator; formats were not uniform, and compliance was uneven. Black and Khanna (2007) document a positive market reaction to the announcement of Clause 49, and Dharmapala and Khanna (2013) show that the value effects of the reform depended materially on the introduction of financial penalties in 2004, evidence that in this jurisdiction, the enforceability of a listing obligation matters as much as its substance.

The LODR Regulations replaced the contractual regime with a statutory one, consolidating disclosure obligations under SEBI's direct enforcement authority. Regulation 13 requires listed companies to maintain a grievance redressal mechanism, register on SEBI's SCORES electronic complaints platform, and file a quarterly statement of investor complaints with the exchange within twenty-one days of quarter-end. The statement is public and uses a standardized format, making complaint records comparable across firms and over time for the first time.

The regulation took effect on 1 December 2015. Indian firms report on an April-to-March financial year, which we label by its year of completion: financial year 2016 runs from 1 April 2015 to 31 March 2016. The effective date, therefore, falls within financial year 2016, which contains four months of compliance, while financial year 2015 (1 April 2014 to 31 March 2015) precedes the regulation entirely. We accordingly designate financial year 2016 as the first treated year and

financial year 2015 as the omitted base period. Financial year 2017 is the first year of complete compliance. This timing has a direct empirical implication. Because only one-third of the financial year 2016 has been treated, the effect operating through disclosure should be attenuated in that year relative to subsequent years, even absent any gradual diffusion. The base period, by contrast, is uncontaminated, so the event-study leads provide a clean test of parallel trends.

3. Theoretical Framework and Hypothesis Development

3.1 Disclosure, firm-specific information, and idiosyncratic risk

The purpose of mandatory disclosure is to reduce information asymmetry between insiders and outside investors (Healy & Palepu, 2001; Verrecchia, 2001). Diamond and Verrecchia (1991) find that greater disclosure can reduce the cost of capital by improving liquidity, while Leuz and Wysocki (2016) survey the broader evidence and suggest that the consequences of disclosure regulation depend heavily on what is disclosed and to whom it matters. A distinct strand asks how the arrival of firm-specific information affects the second moment of returns. Roll (1988) suggests that a large share of return variation is unexplained by market and industry factors, and Morck et al. (2000) demonstrate that higher firm-specific return variation is a marker of more informative pricing. Durnev et al. (2003) find that firms with greater idiosyncratic return variation exhibit stronger associations between current returns and future earnings. Ferreira and Laux (2007) directly show that firms with more open information environments exhibit higher idiosyncratic volatility, which they attribute to greater private information collection and flow.

The most direct evidence comes from settings where disclosure is mandated rather than chosen. Campbell et al. (2014) exploit the SEC's 2005 requirement that firms include a risk factor section in Form 10-K and find that the resulting disclosures are firm-specific rather than boilerplate and

are reflected in both systematic and idiosyncratic risk. Kravet and Muslu (2013) demonstrate that increases in textual risk disclosure are associated with higher return volatility and trading volume around filing dates. Nelson and Pritchard (2016) examine the shift from voluntary to mandatory risk-factor disclosure and document how the mandate altered firms' reporting behavior. Taken together, these strands imply that a disclosure mandate releasing genuinely firm-specific information should raise idiosyncratic volatility. The mechanism does not require that the disclosed information be bad news. What matters is that it is new, firm-specific, and uncertain in its implications. Investor complaints satisfy all three conditions. They measure conduct at the level of the individual firm rather than exposure to common shocks; they were unobservable to outsiders before the mandate; and their implications for future performance are ambiguous, since a complaint record may signal operational strain, weak internal controls, dismissive management, or merely a large retail shareholder base.

This prediction runs counter to a strand of recent work finding that disclosure reduces firm-specific risk. Reber et al. (2022) show that voluntary ESG disclosure at initial public offering lowers both idiosyncratic volatility and downside tail risk, interpreting the effect as firms signaling compliance with societal norms and thereby building reputation capital with investors. Boulton (2024), exploiting the staggered adoption of mandatory ESG disclosure regulations across countries, finds substantially lower IPO underpricing where such mandates exist and reads this as evidence that they reduce information asymmetry. The distinction from our setting lies in what is disclosed rather than in whether disclosure is compelled. ESG disclosure, voluntary or mandatory, concerns a firm's policies, practices, and intentions. Its content is, to a degree, discretionary; it is forward-looking; and it can credibly reassure, which is why Reber et al. (2022) interpret it as a signaling device. Investor complaint statements are a verifiable count of realized failures in shareholder servicing

over which management has no reporting discretion. Reassurance about future conduct resolves uncertainty; disclosure of a previously concealed defect creates it, because outsiders must now form a view about a problem whose severity and persistence they have no basis to assess. We therefore expect the sign of the effect to be opposite to that documented in the ESG literature.

The same feature confers an advantage relative to the risk-factor literature. The principal critique of mandated risk disclosure is that firms respond with boilerplate language that conveys little incremental information, a concern that Campbell et al. (2014) address directly by demonstrating that risk-factor disclosures are, in fact, firm-specific. That defense is unnecessary here: a numerical filing of grievances received and resolved admits no boilerplate, and its informational content does not depend on managerial willingness to be candid. The variation we exploit is in how much was revealed. A firm whose pre-mandate record contained few complaints disclosed little that investors did not already assume. A firm with an extensive record disclosed a great deal. If the disclosure channel operates as theory predicts, the increase in firm-specific uncertainty should scale with the volume of previously private information made public.

H1. Following the LODR disclosure mandate, firms with higher pre-mandate investor complaint intensity experience a larger increase in idiosyncratic risk than firms with lower complaint intensity.

3.2 The idiosyncratic channel

Total return volatility decomposes into a systematic component, reflecting the firm's exposure to priced common factors, and an idiosyncratic component, reflecting firm-specific variation. Campbell et al. (2001) demonstrate that these components move independently over time, and that idiosyncratic volatility can rise while market volatility remains flat. The distinction matters for

interpretation: an increase in total risk driven by systematic risk implies a change in the firm's relationship to the macroeconomy, whereas an increase in idiosyncratic risk implies a change in firm-specific uncertainty.

Campbell et al. (2014) find that mandated risk-factor disclosure is associated with both systematic and idiosyncratic risk, which is unsurprising given that risk factors routinely describe macroeconomic, regulatory, and commodity exposures alongside firm-specific ones. Complaint disclosure has no such dual content. It conveys nothing about the cyclical nature of demand, the operating leverage of the cost structure, or the sensitivity of cash flows to interest rates. It conveys information about how a firm treats its shareholders. If the effect documented in H1 operates through the information channel we propose, it should therefore be absent from systematic risk and should survive conditioning on it. This yields a sharp and falsifiable prediction, and one that differentiates our setting from the prior literature. The total risk effect should be approximately equal in magnitude to the idiosyncratic-risk effect; it should persist when systematic risk is included as a control; and systematic risk itself should show no treatment effect. Failure of any of these would suggest the result reflects some correlated change in firms' economic exposures rather than the release of firm-specific information.

H2. The increase in total risk among complaint-intensive firms operates through the idiosyncratic rather than the systematic component of return volatility.

3.3 Gradual information diffusion

Event studies of disclosure regulation typically look for a discrete response at implementation (Greenstone et al., 2006; Bushee & Leuz, 2005), and event studies of Indian governance reform have followed the same template (Black & Khanna, 2007). That framing suits a one-time

revelation. It suits a recurring quarterly filing considerably less well. Three arguments imply gradual rather than immediate incorporation. First, the informational content of a repeated disclosure accumulates. A single quarter's complaint count is a noisy draw; persistence is only observable across many quarters, and persistence is what distinguishes a structural governance problem from a transient operational disruption. Pastor and Veronesi (2003) formalize the idea that investors learn about firm characteristics gradually from a sequence of noisy signals, and that uncertainty about those characteristics is itself priced. Under this logic, the informativeness of the complaint series grows mechanically with the length of the available record.

Second, a newly mandated disclosure arrives without an interpretive convention. Investors know how to read an earnings surprise; they have no prior for what a given complaint volume implies about future cash flows. Hirshleifer and Teoh (2003) demonstrate that limited attention and processing constraints cause information to be incorporated into prices with a delay, particularly when it requires effort to interpret. You and Zhang (2009) suggest that investors underreact to information in costly-to-process filings. Hong and Stein (1999) model the gradual diffusion of information across investor populations, generating slow price adjustment even in the absence of any friction in the disclosure itself. Third, cross-sectional comparability improves over time. The value of any single firm's complaint statement rises once peers are filing comparable statements and a distribution exists against which to benchmark. This mechanism is specific to standardized mandatory investor complaints disclosure and has no counterpart in a voluntary regime.

Each of these mechanisms predicts the same empirical signature: a treatment effect near zero in the first year of compliance, growing monotonically thereafter. This pattern is distinguishable from an announcement effect, which would produce an immediate jump followed by a flat or decaying path, and from a spurious trend, which would appear as a pre-mandate divergence in the leads.

H3. The risk effect of mandatory complaint disclosure emerges gradually, increasing with time elapsed since the mandate rather than appearing immediately upon compliance.

4. Data and Research Design

4.1 Sample construction

The sample comprises National Stock Exchange (NSE)-listed Indian firms with continuous complaint and financial data over the financial years 2011 to 2021. Investor-complaint data are drawn from the interim quarterly reports of firms in the *Prowess IQ* database, maintained by the Center for Monitoring Indian Economy (CMIE) from June 2010 to December 2015, and from firms' quarterly filings on the NSE website¹ through March 2021. We aggregate the quarterly data to the financial year (April to March) for the analysis. Financial and governance variables, as well as daily stock price and the market index (Nifty 50) data, are obtained from the CMIE Prowess *dx* database. All variables are winsorized at the 1st and 99th percentiles to reduce the influence of outliers.

We impose two restrictions. Firms without pre-2016 complaint data cannot be assigned a treatment intensity and are excluded (60 firm-year observations). Firms observed only before or only after the mandate contribute nothing to a difference-in-differences comparison and are also excluded (192 firm-year observations). The resulting estimation sample contains 6,993 firm-year observations for 722 firms, of which 3,234 fall in the pre-mandate period and 3,759 in the post-mandate period. The panel is unbalanced, but every firm appears on both sides of the event by

¹ <https://www.nseindia.com/companies-listing/corporate-filings-investor-complaints>

construction. For the specifications with industry-by-year fixed effects, we exclude a further 110 singleton observations, leaving 6,883 observations for 712 firms.

4.2 Variable measurement

Following Campbell et al. (2001) and Ferreira and Laux (2007), we decompose return volatility into systematic and idiosyncratic components using the Capital Asset Pricing Model of Sharpe (1964) and Lintner (1965). For each firm-year, we estimate

$$R_{it} - R_{ft} = \alpha_i + \beta_i (R_{mt} - R_{ft}) + \varepsilon_{it}$$

on daily data, where R_{it} is the return on firm i , R_{mt} the return on the market index, and R_{ft} the risk-free rate. Idiosyncratic risk is the standard deviation of the estimated residuals; total risk is the standard deviation of daily excess returns over the financial year; and systematic risk is the estimated slope coefficient. All three are expressed in percentage points. A firm-year is retained only if at least 120 daily return observations are available, ensuring that the regression is estimated on at least approximately half a trading year and that the resulting volatility measures are not unduly influenced by thin trading. Table 1 defines the remaining variables.

Treatment intensity is the natural logarithm of one plus the firm's mean annual number of investor complaints (Com_Int) over the pre-mandate window, financial year 2011 to 2015. The logarithmic transformation accommodates the pronounced right skew in the raw counts, with a sample skewness exceeding 6. Because intensity is fixed at pre-mandate values, it cannot respond to the regulation, and because it is time-invariant, it is absorbed by the firm fixed effects.

4.3 Identification strategy

The listing obligations and disclosure requirements (LODR) are applied to all listed firms simultaneously; the setting does not permit an untreated control group. A binary treatment indicator is uninformative here: 705 of 779 firms received at least one investor complaint before the mandate, leaving 74 controls, and such a specification yields no detectable effect. We therefore estimate

$$Y_{it} = \beta (Intensity_i \times Post_t) + \gamma'X_{it} + \alpha_i + \delta_{jt} + \varepsilon_{it}$$

where Y_{it} is total or idiosyncratic risk, $Post$ equals one from financial year 2016, $\gamma'X_{it}$ is a vector of control variables, α_i are firm fixed effects, and δ_{jt} are industry-by-year fixed effects constructed from two-digit NIC codes interacted with financial year. Standard errors are clustered at the firm level. The fixed effects absorb both $Intensity$ and $Post$ and are not reported; only the interaction is estimated. Industry-by-year fixed effects absorb any shock common to firms within an industry-year, thereby addressing the two contemporary confounds in this window, demonetization in November 2016 and the goods and services tax rollout in July 2017, as well as differential sectoral exposure to the pandemic.

We estimate an event-study specification in which intensity is interacted with indicators for each financial year relative to the mandate, with 2015 omitted as the base period. Lead coefficients jointly test parallel trends. Because our design uses continuous treatment, we note Callaway et al.'s (2024) caution that two-way fixed effects estimates with continuous treatment admit multiple interpretations and can be difficult to interpret as summaries of causal effects. Our coefficient should be understood as a weighted average of dose-specific effects under a parallel-trends assumption. As a partial safeguard, we report a binary specification that compares the extreme quartiles of intensity, which is closer to a conventional average treatment effect in Section 5.6.

4.4 Descriptive statistics

Table 1 defines the variables. Tables 2 to 4 report descriptive statistics for the full sample and separately for the pre- and post-mandate periods, and Table 5 reports pairwise correlations. First, complaint intensity is extremely skewed: the median firm receives roughly 10 complaints per year, while the maximum exceeds 1,500, motivating the logarithmic transformation. Second, and more consequential for identification, mean idiosyncratic risk is essentially identical before and after the mandate, at 2.85 in both periods, and mean total risk moves only from 3.01 to 3.02. There is thus no aggregate increase in volatility over the sample period that the design might spuriously attribute to the mandate; the effects reported below are differential movements between more and less complaint-intensive firms.

The coefficient on the interaction is positive and significant at the 5% level across all six columns. For idiosyncratic risk, the estimate is 0.0288 with firm and year fixed effects, and 0.0316 with industry-by-year fixed effects. For total risk, the corresponding estimates are 0.0275 and 0.0306. That the coefficients rise slightly under the more demanding fixed-effects structure is notable: identification then comes from within-industry-year variation in complaint intensity, so any shock common to an industry in a given year, including demonetization, the GST rollout, and sector-level pandemic exposure, is fully absorbed.

The economic magnitude is modest but not trivial. An interquartile increase in the treatment variable, corresponding to a move from roughly two to thirty-three complaints per year, raises daily idiosyncratic risk by 0.073 percentage points, or 2.6 percent of its sample mean of 2.85 percentage points. The equivalent figure for total risk is 2.3 percent.

Table 1. Variable definitions

Variables		Definitions
1	Com_Int	Treatment intensity is the natural logarithm of one plus the firm's mean annual number of investor complaints over the pre-mandate window, financial year 2011 to 2015.
2	Intensity	Number of investor complaints received by the firms during the financial year.
3	Total_Risk	Total risk is measured as the Standard Deviation of daily excess stock returns (Stock Returns minus risk-free return. ² , daily wise) for each financial year.
4	Idio_Risk	Idiosyncratic risk is measured as the standard deviation of the residuals from a regression of daily excess stock returns on daily excess market returns using the Capital Asset Pricing Model (CAPM) for each financial year.
5	Post2016	Post2016 equals 1 for financial year 2016 and later, and 0 otherwise.
6	Prom_Holdgs	Percentage of Promoters' Ownership.
7	Indd	Percentage of Independent Directors on the board.
8	Fii	Percentage of Foreign Institutional Investors' Ownership.
9	Board Size	Natural logarithm of the number of directors on the board.
10	Size	Natural logarithm of the firm's total assets.
11	Ronw	Return (%) on Net worth or shareholders' fund.
12	Lev	Leverage is measured as the total debt-to-total assets ratio.
13	Syst_Risk	Systematic risk is measured as the beta from a regression of daily excess stock returns on daily excess market returns, using the Capital Asset Pricing Model (CAPM), for each financial year.
14	Sales_Growth	Sales Growth (%) is calculated as the change in sales divided by the previous financial year's sales, expressed as a percentage.
15	Ncfo	Signed Natural logarithm of the Net Cashflow from Operations.

Notes: This table defines the variables used throughout the analysis. Risk measures are computed from daily returns within each financial year using the Capital Asset Pricing Model of Sharpe (1964) and Lintner (1965). Idiosyncratic risk is the standard deviation of residuals from a regression of daily excess stock returns on contemporaneous excess market returns, estimated separately for each firm-year; total risk is the standard deviation of daily excess returns; systematic risk is the estimated slope coefficient or beta. All three are expressed in percentage points, and a firm year is retained only if at least 120 daily return observations are available. Financial years run from 1 April to 31 March and are labeled by year of completion, so Financial Year 2016 spans 1 April 2015 to 31 March 2016.

² Risk Free Return is collected from the <https://faculty.iima.ac.in/iffm/Indian-Fama-French-Momentum/> site.

Table 2. Descriptive statistics: full sample

Variable	N	Mean	S.D.	Min	25th Pctl	Median	75th Pctl	Max
Idio_Risk	6993	2.85	0.98	1.19	2.11	2.78	3.45	6.93
Total_Risk	6993	3.02	0.97	1.27	2.30	2.98	3.61	6.97
Intensity	6993	56.08	182.57	0.00	2.33	9.50	33.40	1527.00
Com_Int	6993	2.46	1.63	0.00	1.20	2.35	3.54	7.33
Post2016	6993	0.54	0.50	0.00	0.00	1.00	1.00	1.00
Prom_Holdgs	6993	55.01	15.98	0.00	45.11	55.96	67.67	99.33
Indd	6993	44.26	9.65	20.00	38.46	44.44	50.00	71.43
Fii	6993	6.34	8.70	0.00	0.02	2.28	9.89	55.10
Boardsize	6993	2.36	0.28	1.61	2.20	2.40	2.56	3.04
Lev	6993	0.17	0.16	0.00	0.05	0.12	0.25	0.75
Size	6993	9.37	1.61	5.46	8.24	9.25	10.46	14.84
Ronw	6993	4.85	23.73	-127.61	0.03	7.20	15.83	54.26
Syst_Risk	6993	0.86	0.45	0.06	0.53	0.81	1.14	2.14
Sales_Growth	6993	7.33	35.24	-80.17	-6.38	5.53	17.75	203.68
Ncfo	6993	4.35	5.18	-9.30	3.88	6.21	7.66	11.11

Notes: The estimation sample comprises 6,993 firm-year observations for 722 Indian listed firms over FY2011 to FY2021, of which 3,234 fall in the pre-LODR period and 3,759 in the post-LODR period. Financial years run from 1 April to 31 March and are labeled by year of completion, so FY2016 spans 1 April 2015 to 31 March 2016. Post2016 equals one from FY2016 onward, the financial year containing the LODR effective date of 1 December 2015. Intensity is the firm's mean annual pre-LODR investor complaints, and Com_Int is its natural logarithm; both are time-invariant. Risk measures are in percentage points. Variable definitions appear in Table 1.

Table 3. Descriptive statistics: pre-LODR period (Financial Year 2011–FY2015)

Variable	N	Mean	S.D.	Min	25th Pctl	Median	75th Pctl	Max
Idio_Risk	3234	2.85	1.02	1.19	2.08	2.73	3.44	6.93
Total_Risk	3234	3.01	1.02	1.27	2.24	2.91	3.61	6.97
Syst_Risk	3234	0.84	0.48	0.06	0.47	0.77	1.15	2.14
Intensity	3234	56.72	181.66	0.00	2.40	9.75	34.40	1527.00
Com_Int	3234	2.48	1.64	0.00	1.22	2.37	3.57	7.33
Prom_Holdgs	3234	55.03	15.97	0.00	44.97	55.55	67.28	99.33
Indd	3234	46.77	10.03	20.00	41.67	45.45	50.00	71.43
Fii	3234	6.31	8.72	0.00	0.02	2.22	9.91	55.10
Boardsize	3234	2.30	0.29	1.61	2.08	2.30	2.48	3.04
Lev	3234	0.18	0.16	0.00	0.05	0.14	0.27	0.75
Size	3234	9.23	1.58	5.46	8.11	9.06	10.31	14.18
Ronw	3234	5.37	24.08	-127.61	0.57	7.60	16.32	54.26
Sales_Growth	3234	12.05	35.32	-80.17	-1.38	9.36	22.40	203.68
Ncfo	3234	3.95	5.28	-9.30	3.48	5.94	7.31	11.11

Notes: Statistics are computed over the 3,234 firm-year observations preceding the LODR mandate. Financial years run from 1 April to 31 March and are labeled by year of completion, so financial year 2016 spans 1 April 2015 to 31 March 2016. FY2015 ends on 31 March 2015 and therefore precedes the effective date of 1 December 2015 in its

entirety, so no observation in this panel is subject to the disclosure requirement. Risk measures are expressed in percentage points.

Table 4. Descriptive statistics: post-LODR period (Financial year 2016–2021)

Variable	N	Mean	S.D.	Min	25th Pctl	Median	75th Pctl	Max
Idio_Risk	3759	2.85	0.94	1.19	2.14	2.82	3.45	6.93
Total_Risk	3759	3.02	0.93	1.27	2.34	3.02	3.61	6.97
Syst_Risk	3759	0.89	0.42	0.06	0.59	0.84	1.13	2.14
Intensity	3759	55.53	183.37	0.00	2.33	9.25	32.00	1527.00
Com_Int	3759	2.45	1.63	0.00	1.20	2.33	3.50	7.33
Prom_Holdgs	3759	54.99	15.99	0.00	45.36	56.33	67.77	93.80
Indd	3759	42.10	8.75	20.00	36.36	41.67	46.67	71.43
Fii	3759	6.37	8.69	0.00	0.02	2.40	9.87	50.15
Boardsize	3759	2.40	0.26	1.61	2.20	2.40	2.56	3.04
Lev	3759	0.16	0.16	0.00	0.04	0.11	0.23	0.75
Size	3759	9.50	1.63	5.46	8.34	9.43	10.58	14.84
Ronw	3759	4.40	23.43	-127.61	0.00	6.74	15.31	54.26
Sales_Growth	3759	3.27	34.67	-80.17	-9.77	2.38	13.50	203.68
Ncfo	3759	4.68	5.07	-9.30	4.13	6.42	7.88	11.11

Notes: Statistics are computed over the 3,759 firm-year observations from FY2016 onward. Financial years run from 1 April to 31 March and are labeled by year of completion, so FY2016 spans 1 April 2015 to 31 March 2016. FY2016 contains four months of compliance, with the mandate taking effect on 1 December 2015; FY2017 is the first financial year of complete compliance. The comparison with Table 3 shows that the mean idiosyncratic and total risk are essentially unchanged between periods, indicating that the treatment effects reported below reflect differential rather than aggregate movements in volatility.

Columns (5) and (6) add systematic risk as a control. The treatment coefficient falls only modestly, from 0.0275 to 0.0246 and from 0.0306 to 0.0263, and remains significant, while systematic risk itself enters with a large and precisely estimated coefficient of approximately 0.57 to 0.60. The persistence of the treatment effect in the presence of a control that absorbs the great majority of the variation in total volatility is the first piece of evidence for H2. These results support H1. Three features stand out. First, all four lead coefficients are individually indistinguishable from zero, with absolute t-statistics below 1.04, and are jointly insignificant for both outcomes ($F = 0.85$, $p = 0.497$ for idiosyncratic risk; $F = 0.84$, $p = 0.498$ for total risk).

Table 5. Pairwise correlation matrix

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Idio_Risk	1.000							
(2) Total_Risk	0.987*** (0.000)	1.000						
(3) Intensity	0.006 (0.597)	0.011 (0.371)	1.000					
(4) Com_Int	-0.021* (0.077)	-0.021* (0.086)	0.609*** (0.000)	1.000				
(5) Post2016	0.004 (0.708)	0.008 (0.506)	-0.003 (0.785)	-0.010 (0.426)	1.000			
(6) Syst_Risk	0.129*** (0.000)	0.274*** (0.000)	0.031** (0.010)	0.003 (0.830)	0.053*** (0.000)	1.000		
(7) Prom_Holdgs	-0.136*** (0.000)	-0.138*** (0.000)	0.032*** (0.008)	0.074*** (0.000)	-0.001 (0.911)	-0.015 (0.216)	1.000	
(8) Indd	0.013 (0.276)	0.011 (0.373)	0.018 (0.125)	0.015 (0.197)	-0.242*** (0.000)	-0.024** (0.044)	-0.116*** (0.000)	1.000
(9) Fii	-0.338*** (0.000)	-0.313*** (0.000)	0.007 (0.539)	0.012 (0.308)	0.003 (0.786)	0.062*** (0.000)	-0.240*** (0.000)	0.073*** (0.000)
(10) Board Size	-0.334*** (0.000)	-0.303*** (0.000)	-0.010 (0.426)	-0.029** (0.014)	0.186*** (0.000)	0.134*** (0.000)	0.038*** (0.001)	-0.087*** (0.000)
(11) Lev	0.060*** (0.000)	0.069*** (0.000)	0.015 (0.207)	0.014 (0.257)	-0.057*** (0.000)	0.052*** (0.000)	-0.057*** (0.000)	0.010 (0.425)
(12) Size	-0.424*** (0.000)	-0.375*** (0.000)	0.038*** (0.001)	0.020* (0.096)	0.084*** (0.000)	0.209*** (0.000)	-0.009 (0.432)	-0.018 (0.142)
(13) Ronw	-0.327*** (0.000)	-0.320*** (0.000)	0.017 (0.155)	0.024** (0.048)	-0.020* (0.087)	-0.024** (0.042)	0.104*** (0.000)	0.006 (0.635)
(14) Sales_Growth	-0.103*** (0.000)	-0.095*** (0.000)	0.010 (0.402)	0.014 (0.226)	-0.124*** (0.000)	0.024** (0.045)	0.062*** (0.000)	0.025** (0.039)
(15) Ncfo	-0.278*** (0.000)	-0.272*** (0.000)	0.036*** (0.003)	-0.005 (0.696)	0.071*** (0.000)	-0.014 (0.256)	0.029** (0.014)	0.013 (0.266)
(9) Fii	1.000							
(10) Board size	0.264*** (0.000)	1.000						
(11) Lev	-0.025** (0.033)	0.082*** (0.000)	1.000					
(12) Size	0.511*** (0.000)	0.498*** (0.000)	0.223*** (0.000)	1.000				
(13) Ronw	0.167*** (0.000)	0.095*** (0.000)	-0.222*** (0.000)	0.061*** (0.000)	1.000			
(14) Sales_Growth	0.038*** (0.002)	-0.014 (0.246)	-0.021* (0.077)	0.021* (0.074)	0.203*** (0.000)	1.000		
(15) Ncfo	0.172*** (0.000)	0.229*** (0.000)	0.027** (0.022)	0.265*** (0.000)	0.229*** (0.000)	0.038*** (0.002)	1.000	

Notes: The table reports Pearson correlation coefficients over the full estimation sample of 6,993 firm-year observations, with p-values in parentheses. Variable definitions appear in Table 1. * p < 0.10, ** p < 0.05, *** p < 0.01.

5. Results

5.1 Baseline difference-in-differences results

Table 6. Complaint disclosure and firm risks: baseline difference-in-differences estimates

	(1) Idio_Risk (Yr FE)	(2) Idio_Risk (IndYr FE)	(3) Total_Risk (Yr FE)	(4) Total_Risk (IndYr FE)	(5) Total_Risk (Yr FE + Beta)	(6) Total_Risk (IndYr FE + Beta)
Com_Int X Post-2016	0.0288** (2.2096)					
Com_Int X Post-2016		0.0316** (2.4112)				
Com_Int X Post-2016			0.0275** (2.1739)			
Com_Int X Post-2016				0.0306** (2.3804)		
Com_Int X Post-2016					0.0246** (1.9940)	
Com_Int X Post-2016						0.0263** (2.1016)
Promo_Hold	-0.0065*** (-3.0823)	-0.0065*** (-3.0388)	-0.0041* (-1.9326)	-0.0044** (-2.0139)	-0.0076*** (-3.7231)	-0.0076*** (-3.7297)
Indd	-0.0016 (-1.2943)	-0.0017 (-1.3312)	-0.0015 (-1.1596)	-0.0016 (-1.2232)	-0.0018 (-1.5127)	-0.0018 (-1.4785)
Fii	-0.0079*** (-2.8404)	-0.0070** (-2.4629)	-0.0050* (-1.7740)	-0.0043 (-1.4662)	-0.0080*** (-3.0079)	-0.0073*** (-2.6686)
Board Size	-0.1089* (-1.7608)	-0.0894 (-1.4781)	-0.1019* (-1.6727)	-0.0864 (-1.4372)	-0.1118* (-1.8599)	-0.0951 (-1.6333)
Leverage	0.1508 (1.1397)	0.1523 (1.0808)	0.1438 (1.0968)	0.1503 (1.0707)	0.1630 (1.2669)	0.1545 (1.1287)
Firm Size	-0.1356*** (-3.5183)	-0.1227*** (-3.2031)	-0.0878** (-2.3346)	-0.0746** (-1.9708)	-0.1703*** (-4.5659)	-0.1619*** (-4.3924)
Ronw	-0.0030*** (-5.5041)	-0.0029*** (-5.0967)	-0.0027*** (-4.8470)	-0.0027*** (-4.6002)	-0.0031*** (-5.8450)	-0.0030*** (-5.4646)
Sales Growth	-0.0004 (-1.6460)	-0.0005* (-1.7432)	-0.0004 (-1.4630)	-0.0005 (-1.5058)	-0.0005* (-1.8388)	-0.0006** (-2.0090)
Ncfo	-0.0016 (-0.8167)	-0.0016 (-0.7858)	-0.0011 (-0.5525)	-0.0009 (-0.4438)	-0.0013 (-0.6959)	-0.0013 (-0.7064)
Syst_Risk					0.5694*** (18.2409)	0.6010*** (18.0298)
Constant	4.8217*** (12.1899)	4.6469*** (11.2955)	4.3608*** (11.2319)	4.2116*** (10.2787)	4.8975*** (12.9033)	4.7474*** (12.1736)
Observations	6,993	6,883	6,993	6,883	6,993	6,883
Adj. R-Squared	0.6571	0.6634	0.6519	0.6562	0.6859	0.6919
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No
Industry X Year FE	No	Yes	No	Yes	No	Yes

Notes: The table reports difference-in-differences estimates of the effect of the LODR investor-complaint disclosure mandate on firm risk. Complaint intensity (Com_Int) is the natural logarithm of one plus the firm's mean annual

investor complaints over the pre-LODR window FY2011–FY2015. It is time-invariant by construction and is therefore absorbed by the firm fixed effects; Post2016 is absorbed by the year or industry-year fixed effects. Only the interaction is estimated. Columns (1), (3), and (5) include firm and year fixed effects; columns (2), (4), and (6) replace year with industry-by-year fixed effects constructed from two-digit NIC codes interacted with financial year, which absorb any shock common to firms within an industry-year and reduce the sample by 110 singleton observations. Columns (5) and (6) additionally control for systematic risk, isolating the idiosyncratic channel. Financial years run from 1 April to 31 March and are labeled by year of completion, so FY2016 spans 1 April 2015 to 31 March 2016. Standard errors are clustered at the firm level; t-statistics appear in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The parallel-trends assumption is therefore supported on a base period that precedes the mandate entirely. Second, the coefficient on the event year itself is 0.0038, with a t-statistic of 0.23, which is statistically and economically zero. This is precisely what the institutional timing predicts: financial year 2016 contains only four of twelve months of compliance, and firms had filed at most one or two quarterly statements by its close. Three features stand out. First, all four lead coefficients are individually indistinguishable from zero, with absolute t-statistics below 1.04, and are jointly insignificant for both outcomes ($F = 0.85$, $p = 0.497$ for idiosyncratic risk; $F = 0.84$, $p = 0.498$ for total risk).

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5.2 Dynamic effects

The pooled estimates conceal a pronounced time profile. Table 7 reports the event-study specification, and Figure 1 plots the coefficients.

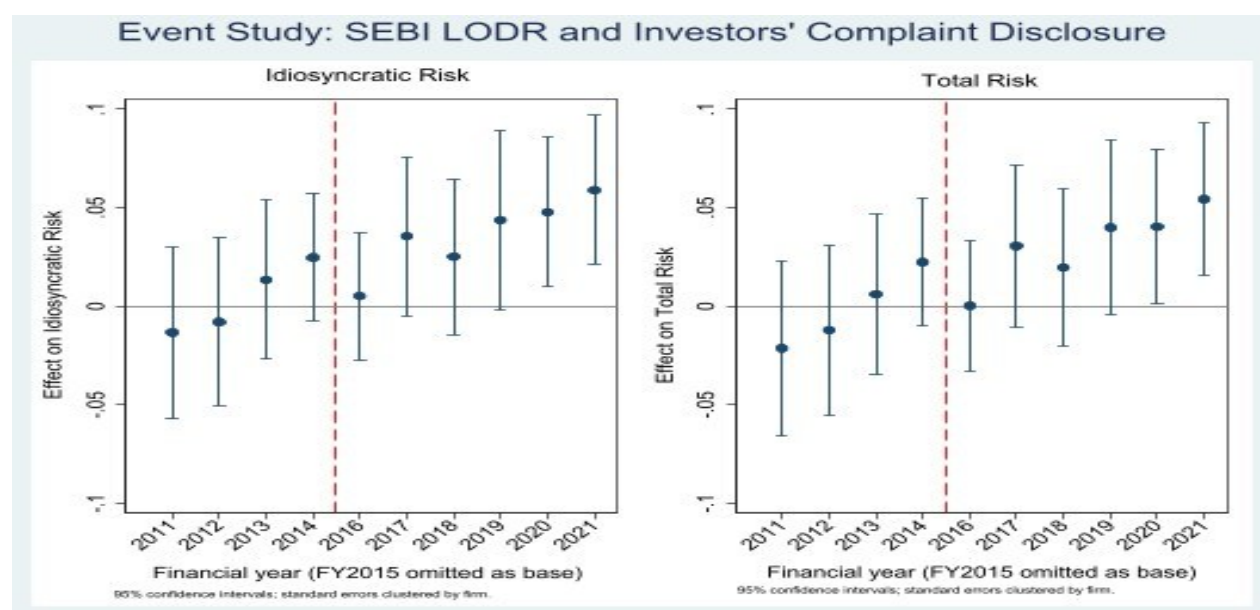
Table 7. Dynamic effects of complaint disclosure: event-study estimates

	(1) Idio_Risk	(2) Total_Risk	(3) Total_Risk (Syst_Risk)
Event_Year _{T-5}	-0.0083 (-0.3747)	-0.0160 (-0.7035)	-0.0045 (-0.2266)
Event_Year _{T-4}	-0.0114 (-0.5240)	-0.0148 (-0.6725)	-0.0067 (-0.3226)
Event_Year _{T-3}	0.0135 (0.6394)	0.0067 (0.3149)	0.0188 (0.9479)
Event_Year _{T-2}	0.0174 (1.0388)	0.0151 (0.8992)	0.0132 (0.8668)
Event_Year	0.0038 (0.2312)	-0.0007 (-0.0436)	0.0045 (0.3050)
Event_Year _{T+1}	0.0340* (1.6663)	0.0293 (1.3927)	0.0340* (1.8062)
Event_Year _{T+2}	0.0236 (1.1729)	0.0184 (0.9079)	0.0236 (1.2652)
Event_Year _{T+3}	0.0424* (1.8393)	0.0390* (1.7308)	0.0313 (1.4419)
Event_Year _{T+4}	0.0461** (2.3907)	0.0389* (1.9522)	0.0429** (2.4553)
Event_Year _{T+5}	0.0575*** (2.9720)	0.0531*** (2.6831)	0.0497*** (2.7929)
Syst_Risk			0.6001*** (17.9717)
Controls	Yes	Yes	Yes
Constant	4.6348*** (11.2489)	4.2081*** (10.2473)	4.7342*** (12.1392)
Observations	6,883	6,883	6,883
Adjusted R-squared	0.6636	0.6563	0.6919
Pre-trend F Stats.	0.85	0.84	0.68
Pre-trend p-value	0.497	0.498	0.605
Firm FE	Yes	Yes	Yes
Industry x Year FE	Yes	Yes	Yes

Notes: The table reports event-study estimates in which complaint intensity is interacted with indicators for each financial year relative to the mandate. Complaint intensity (Com_Int) is the natural logarithm of one plus the firm's mean annual investor complaints over the pre-LODR window FY2011–FY2015. It is time-invariant by construction and is therefore absorbed by the firm fixed effects; Post2016 is absorbed by the year or industry-year fixed effects. Only the interaction is estimated. FY2015, which precedes the effective date entirely, is the omitted base period. Event

year t corresponds to FY2016, which contains four months of compliance; FY2017 ($t+1$) is the first year of complete compliance. All specifications include firm and industry-by-year fixed effects, as well as the full set of controls. The pre-trend F-statistic tests the joint hypothesis that all lead coefficients are equal to zero. Column (3) adds systematic risk as a control. Standard errors are clustered at the firm level; t-statistics appear in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1. Parallel trends and dynamic treatment effects



Notes: The figure plots event-study coefficients and 95 percent confidence intervals from regressions of idiosyncratic volatility (left panel) and total volatility (right panel) on pre-LODR complaint intensity interacted with financial-year indicators. Both specifications include firm and industry-by-year fixed effects, as well as the full set of controls. FY2015 is omitted as the base period, and the dashed line marks its position, separating pre- from post-mandate years. Confidence intervals are constructed from standard errors clustered at the firm level.

The pattern for total risk is materially identical, and column (3) shows it survives conditioning on beta. This is the signature predicted by H3 and is difficult to reconcile with an announcement effect, which would produce an immediate jump followed by decay. The dip in 2018 interrupts an otherwise monotonic sequence. That year corresponds to the goods and services tax rollout of July 2017, a disruption sufficiently broad that even industry-by-year fixed effects may not fully absorb its firm-level heterogeneity. We do not attach interpretive weight to a single year that remains within one standard error of its neighbors.

5.3 The idiosyncratic channel

Hypothesis 2 predicts that the effect should be absent from systematic risk. Table 8 tests this directly by using the capital asset pricing model (CAPM) systematic risk (Beta) as the dependent variable.

Table 8. Systematic risk as the dependent variable

	(1) Syst_Risk (Yr FE)	(2) Syst_Risk (IndYr FE)
Com_int x Post2016	0.0050 (0.6834)	0.0070 (1.0268)
Controls	Yes	Yes
Constant	-0.9426*** (-4.7783)	-0.8915*** (-4.3836)
Observations	6,993	6,883
Adjusted R-squared	0.5130	0.5375
Firm FE	Yes	Yes
Year FE	Yes	No
Industry x Year FE	No	Yes

Notes: The table reports estimates using systematic risk, the firm-year market-model slope coefficient, as the dependent variable. Because complaint disclosure conveys information about a firm's treatment of its shareholders rather than about its exposure to priced common factors, no treatment effect is predicted. A null result, therefore, supports the interpretation that the effects documented in Tables 6 and 7 operate through the idiosyncratic component of volatility. Complaint intensity (Com_Int) is the natural logarithm of one plus the firm's mean annual investor complaints over the pre-LODR window FY2011–FY2015. It is time-invariant by construction and is therefore absorbed by the firm fixed effects; Post2016 is absorbed by the year or industry-year fixed effects. Only the interaction is estimated. Standard errors are clustered at the firm level; t-statistics appear in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The treatment coefficient is 0.0050 with a t-statistic of 0.68 under year fixed effects and 0.0070 with a t-statistic of 1.03 under industry-by-year fixed effects. Neither approach approaches conventional significance. Taking with the near-equality of the idiosyncratic and total-risk coefficients in Table 6 and the persistence of the effect when systematic risk is controlled for, the evidence supports hypothesis 2: the disclosure mandate altered firm-specific uncertainty without changing firms' exposure to priced common factors. This is what one would expect of a disclosure that conveys information about shareholder servicing rather than about economic exposures, and

it distinguishes our setting from the mandated risk-factor disclosures studied by Campbell et al. (2014), which affected both components.

Table 9. Pseudo-event placebo tests within the Pre-LODR window

	(1) Idio_Risk 2012	(2) Idio_Risk 2013	(3) Idio_Risk 2014	(4) Total_Risk 2012	(5) Total_Risk 2013	(6) Total_Risk 2014
Com_Int X Post2012	0.0059 (0.3672)					
Com_Int X Post2013		0.0198 (1.3657)				
Com_Int X Post2014			0.0118 (0.8889)			
Com_Int X Post2012				0.0101 (0.6229)		
Com_Int X Post2013					0.0216 (1.4689)	
Com_Int X Post2014						0.0163 (1.1833)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.2587*** (6.3179)	4.2593*** (6.3752)	4.2528*** (6.3298)	4.1339*** (5.9717)	4.1382*** (6.0214)	4.1279*** (5.9778)
Observations	3,220	3,220	3,220	3,220	3,220	3,220
Adj. R-Squared	0.7097	0.7100	0.7098	0.7057	0.7060	0.7059
Firm Fe	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports placebo estimates in which the mandate is falsely assigned to FY2012, FY2013, and FY2014. The sample is restricted to the pre-LODR window FY2011–FY2015, so no observation is genuinely treated, and the coefficient of interest should be indistinguishable from zero. Complaint intensity (Com_Int) is the natural logarithm of one plus the firm's mean annual investor complaints over the pre-LODR window FY2011–FY2015. It is time-invariant by construction and is therefore absorbed by the firm fixed effects; Post2016 is absorbed by the year or industry-year fixed effects. Only the interaction is estimated. All specifications include firm and year fixed effects, as well as the full set of controls. Standard errors are clustered at the firm level; t-statistics appear in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.4 Placebo tests

Table 9 reports pseudo-event placebo tests in which the mandate is falsely assigned to FY2012, FY2013, and FY2014, with the sample restricted to the pre-mandate window. None of the six coefficients approaches significance. For idiosyncratic risk, the estimates are 0.0059, 0.0198, and 0.0118, with t-statistics of 0.37, 1.37, and 0.89; the corresponding total-risk estimates are 0.0101, 0.0216, and 0.0163. Every placebo coefficient is smaller than the true estimate of 0.0316, and the

pattern shows no tendency for later pseudo-events to produce larger effects. The design does not generate spurious treatment effects when applied to a period in which no treatment occurred.

Table 10. Sensitivity of inference to the clustering structure

	(1) Idio_Risk (Firm)	(2) Idio_Risk (Industry)	(3) Idio_Risk (2-way)	(4) Total_Risk (Firm)	(5) Total_Risk (Industry)	(6) Total_Risk (2-way)
Com_int x Post2016	0.0316** (2.4112)					
Com_int x Post2016		0.0316** (2.2366)				
Com_int x Post2016			0.0316** (2.4080)			
Com_int x Post2016				0.0306** (2.3804)		
Com_int x Post2016					0.0306** (2.1945)	
Com_int x Post2016						0.0306** (2.3735)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.6469*** (11.2955)	4.6469*** (10.3280)	4.6469*** (12.1139)	4.2116*** (10.2787)	4.2116*** (9.6280)	4.2116*** (10.8048)
Observations	6,883	6,883	6,883	6,883	6,883	6,883
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.6634	0.6634	0.6634	0.6562	0.6561	0.6561
Clustering	Firm	Industry	Firm & Year	Firm	Industry	Firm & Year

Notes: The table reports the baseline specification under alternative variance estimators. Coefficients are identical across columns; only inference differs. Firm-level clustering uses 722 clusters, industry clustering uses 44 two-digit NIC industries, and two-way clustering uses firm and financial year. Because the year dimension contains only eleven clusters, the two-way estimates rest on weak asymptotic foundations, and the variance matrix requires a Cameron-Gelbach-Miller correction; they are reported for completeness and should be read with caution. All specifications include firm and industry-by-year fixed effects, which exclude 110 singleton observations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.5 Robustness

Table 10 examines the sensitivity of inference to the clustering structure, and Table 12 reports three further robustness exercises. Coefficients are by construction identical across columns of Table 10; only inference differs. Clustering at the industry level, using 44 two-digit NIC industries, yields t-statistics of 2.24 and 2.19 for idiosyncratic and total risk. Two-way clustering by firm and financial year yields 2.41 and 2.37. All remain significant at the five percent level. Because the

year dimension contains only 11 clusters, the two-way estimates rest on weak asymptotic foundations; we therefore retain firm-level clustering (722 clusters) as the baseline.

Table 11. Robustness: sub-period decomposition, pre-pandemic window, and exclusion of potential non-filers

	(1) Idio_Risk Split	(2) Idio_Risk Pre- COVID	(3) Idio_Risk Excl. non- filers	(4) Total_Risk Split	(5) Total_Risk Pre- COVID	(6) Total_Risk Excl. non- filers
Com_Int x Post (2016-19)	0.0232*			0.0227*		
	(1.7097)			(1.7021)		
Com_Int x Post (2020-21)	0.0488***			0.0468***		
	(3.1543)			(3.0536)		
Com_Int x Post2016		0.0238*	0.0319**		0.0232*	0.0309**
		(1.7265)	(2.4229)		(1.7160)	(2.4006)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.6443***	4.8729***	4.6575***	4.2091***	4.4259***	4.2217***
	(11.2949)	(9.3003)	(11.2814)	(10.2741)	(8.4869)	(10.2681)
Observations	6,883	5,677	6,858	6,883	5,677	6,858
Adj. R-squared	0.6636	0.6708	0.6649	0.6563	0.6661	0.6580
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports three robustness exercises. The split columns decompose the post-mandate period into FY2016–FY2019 and FY2020–FY2021; because the event-study coefficients rise monotonically with time since treatment, the larger late-period estimate reflects cumulative build-up rather than pandemic-specific moderation, a reading supported by the linear extrapolation tests reported in the text. The pre-COVID columns restrict the sample to FY2011–FY2019. The final columns exclude eight firms, comprising 44 firm-year observations, that record zero complaints in every sample year and may therefore represent non-filing rather than genuine non-receipt. Complaint intensity (Com_Int) is the natural logarithm of one plus the firm’s mean annual investor complaints over the pre-LODR window FY2011–FY2015. It is time-invariant by construction and is therefore absorbed by the firm fixed effects; Post2016 is absorbed by the year or industry-year fixed effects. Only the interaction is estimated. All specifications include firm and industry-by-year fixed effects, as well as the full set of controls. Standard errors are clustered at the firm level; t-statistics appear in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Columns (1) and (4) of Table 11 decompose the post-mandate period. The FY2016 to FY2019 coefficient for idiosyncratic risk is 0.0232, and the FY2020 to FY2021 coefficient is 0.0488, roughly twice as large. Because FY2020 and FY2021 coincide with the pandemic, this raises the question of whether the pandemic, rather than the mandate, drives the later estimates. Three pieces

of evidence argue otherwise. First, industry-by-year fixed effects absorb sector-level pandemic exposure. Second, and most directly, the event-study coefficients for FY2020 and FY2021 lie on the trajectory implied by FY2018 and FY2019: tests of linear extrapolation fail to reject in both cases ($F = 0.22$, $p = 0.637$ for FY2020; $F = 0.21$, $p = 0.650$ for FY2021), indicating no discontinuity at the pandemic boundary. Third, the effect is already positive and significant at the ten percent level in FY2019, before the pandemic. The larger late-period coefficients, therefore, reflect the cumulative build-up predicted by H3 rather than a pandemic-specific shock. Consistent with this, restricting the sample to FY2011 through FY2019 in columns (2) and (5) yields coefficients of 0.0238 and 0.0232, approximately three-quarters of the full-sample magnitude and significant at the ten percent level; the attenuation is expected given that the restriction removes both a fifth of the observations and the years in which the accumulated effect is largest. Columns (3) and (6) exclude eight firms, comprising 44 firm-year observations, that record zero complaints in every sample year and may therefore represent non-filing rather than genuine non-receipt. The estimates are 0.0319 and 0.0309, essentially unchanged from the baseline, indicating that the treatment of ambiguous zero records does not drive the results.

Table 12 addresses selection on observables by using kernel propensity score matching between firms in the highest quartile of complaint intensity and those in the lowest quartile, based on pre-period covariate means. Matching reduces the mean standardized bias from 6.4 to 1.2 percent and Rubin's B from 31.2 to 5.3, with all individual covariate differences insignificant after matching. The propensity model is itself jointly insignificant, indicating that pre-period characteristics are weak predictors of complaint intensity and providing evidence in favor of the design independent of the matched estimates.

Table 12. Estimates on the propensity-score matched (PSM) sample

	(1) Idio_Risk (Yr FE)	(2) Idio_Risk (IndYr FE)	(3) Total_Risk (Yr FE)	(4) Total_Risk (IndYr FE)
Com_int x Post2016	0.0291** (2.1300)			
Com_int x Post2016		0.0350** (2.4941)		
Com_int x Post2016			0.0278** (2.1008)	
Com_int x Post2016				0.0352** (2.5607)
Controls	Yes	Yes	Yes	Yes
Constant	4.8513*** (8.4258)	4.3881*** (6.9402)	4.3240*** (7.6268)	3.8830*** (6.1425)
Observations	3,448	3,331	3,448	3,331
Adj. R-squared	0.6428	0.6488	0.6381	0.6428
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Industry x Year FE	No	Yes	No	Yes

Notes: The table reports estimates on a sample constructed by kernel propensity-score matching of firms in the highest quartile of pre-LODR complaint intensity to those in the lowest quartile, using pre-period means of the control variables. Matching reduces the mean standardized bias from 6.4 to 1.2 percent and Rubin's B from 31.2 to 5.3, with all individual covariate differences insignificant after matching. The propensity model is itself jointly insignificant, indicating that pre-period characteristics are weak predictors of complaint intensity. Because matching retains only extreme-quartile firms, the estimand is a high-versus-low contrast rather than the full intensity gradient. Complaint intensity (Com_Int) is the natural logarithm of one plus the firm's mean annual investor complaints over the pre-LODR window FY2011–FY2015. It is time-invariant by construction and is therefore absorbed by the firm fixed effects; Post2016 is absorbed by the year or industry-year fixed effects. Only the interaction is estimated. Standard errors are clustered at the firm level; t-statistics appear in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In the matched sample, the coefficients are 0.0350 for idiosyncratic risk and 0.0352 for total risk under industry-by-year fixed effects, somewhat larger than the full-sample estimates despite roughly half the observations. Because matching retains only extreme-quartile firms, this estimate is a high-versus-low contrast rather than the full intensity gradient and is not directly comparable in magnitude with Table 6.

5.6 Binary treatment specification

As an alternative parameterization and as a partial response to the interpretive concerns raised by Callaway et al. (2024) regarding continuous-treatment two-way fixed-effects estimation, Tables

13 to 15 report a binary design in which firms in the highest quartile of pre-mandate complaint intensity are treated, and those in the lowest quartile serve as controls.

Table 13. Binary difference-in-differences: highest versus lowest quartile of complaint intensity

	(1) Idio_Risk (Yr FE)	(2) Idio_Risk (IndYr FE)	(3) Total_Risk (Yr FE)	(4) Total_Risk (IndYr FE)
High_int (Q4) x Post2016	0.1135** (2.0212)			
High_int (Q4) x Post2016		0.1390** (2.4496)		
High_int (Q4) x Post2016			0.1105** (1.9868)	
High_int (Q4) x Post2016				0.1406** (2.4876)
Controls	Yes	Yes	Yes	Yes
Constant	4.8500*** (8.4004)	4.3942*** (6.9442)	4.3260*** (7.6206)	3.8923*** (6.1592)
Observations	3,493	3,376	3,493	3,376
Adjusted R-squared	0.6456	0.6519	0.6404	0.6455
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Industry x Year FE	No	Yes	No	Yes

Notes: The table reports estimates under a binary treatment definition in which firms in the highest quartile (Q4) of pre-LODR complaint intensity are treated, those in the lowest quartile serve as controls, and firms in the intermediate quartiles are excluded. This specification is reported as an alternative parameterization of the continuous design, as shown in Table 6. Because it compares extremes rather than estimating a per-unit gradient, the coefficients are larger in magnitude and not directly comparable to those in Table 6. Financial years run from 1 April to 31 March and are labeled by year of completion, so FY2016 spans 1 April 2015 to 31 March 2016. Standard errors are clustered at the firm level; t-statistics appear in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The estimates are 0.1135 and 0.1390 for idiosyncratic risk, and 0.1105 and 0.1406 for total risk, all significant at the 5% level. The larger magnitudes reflect a comparison of extremes rather than a per-unit gradient and are not comparable with those in Table 6. The binary event study reproduces every qualitative feature of the continuous specification. Lead coefficients are jointly insignificant, and indeed more sharply so than in the continuous design ($F = 0.40$, $p = 0.812$ for idiosyncratic risk; $F = 0.50$, $p = 0.738$ for total risk). The event-year coefficient is again insignificant, at 0.0586, and coefficients rise thereafter to 0.2652 by FY2021, significant at the one

percent level. Placebo tests under the binary definition are likewise null across all six specifications, with t-statistics ranging from 0.23 to 0.77. The continuous and binary designs, which use different variations and imply different estimands, yield the same conclusions for every hypothesis, reassuring the robustness of the findings to the treatment parameterization.

Table 14. Dynamic effects under binary treatment: event-study estimates

	(1) Idio_Risk	(2) Total_Risk
Event_Year _{T-5}	0.0107 (0.0999)	-0.0106 (-0.0965)
Event_Year _{T-4}	0.0055 (0.0513)	0.0086 (0.0792)
Event_Year _{T-3}	0.0333 (0.3192)	0.0201 (0.1913)
Event_Year _{T-2}	0.0880 (1.0343)	0.0936 (1.1096)
Event_Year	0.0586 (0.7354)	0.0567 (0.7111)
Event_Year _{T+1}	0.1980** (2.0906)	0.1968** (2.0562)
Event_Year _{T+2}	0.1101 (1.2285)	0.1067 (1.1850)
Event_Year _{T+3}	0.1533 (1.4258)	0.1499 (1.4194)
Event_Year _{T+4}	0.2225** (2.3089)	0.2132** (2.1854)
Event_Year _{T+5}	0.2652*** (2.7470)	0.2665*** (2.7432)
Controls	Yes	Yes
Constant	4.3679*** (6.8934)	3.8683*** (6.1132)
Observations	3,376	3,376
Adj. R-Squared	0.6516	0.6453
Pre-Trend F	0.40	0.50
Pre-Trend P-Value	0.812	0.738
Firm FE	Yes	Yes
Industry X Year FE	Yes	Yes

Notes: The table reports event-study estimates under the binary treatment definition of Table 13, with firms in the highest quartile of pre-LODR complaint intensity treated and those in the lowest quartile serving as controls. FY2015, which precedes the effective date entirely, is the omitted base period; event year t corresponds to FY2016, which contains four months of compliance. All specifications include firm and industry-by-year fixed effects, as well as the full set of controls. Standard errors are clustered at the firm level; t-statistics appear in parentheses. * p < 0.10, ** p <

0.05, *** $p < 0.01$.

Table 15. Pseudo-event placebo tests under binary treatment

	(1) Idio_Risk 2012	(2) Idio_Risk 2013	(3) Idio_Risk 2014	(4) Total_Risk 2012	(5) Total_Risk 2013	(6) Total_Risk 2014
Q4 x Post2012	0.0169 (0.2314)					
Q4 x Post2013		0.0452 (0.6979)				
Q4 x Post2014			0.0296 (0.4704)			
Q4 x Post2012				0.0371 (0.4975)		
Q4 x Post2013					0.0508 (0.7740)	
Q4 x Post2014						0.0453 (0.6947)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.5561*** (4.1312)	3.5672*** (4.1689)	3.5536*** (4.1334)	3.4412*** (3.9444)	3.4519*** (3.9748)	3.4364*** (3.9423)
Observations	1,615	1,615	1,615	1,615	1,615	1,615
Adjusted R-squared	0.6990	0.6991	0.6990	0.6928	0.6929	0.6929
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports placebo estimates under the binary treatment definition, with the mandate falsely assigned to FY2012, FY2013, and FY2014. The sample is restricted to the pre-LODR window FY2011–FY2015 and to the highest and lowest quartiles of complaint intensity. All specifications include firm and year fixed effects, as well as the full set of controls. Standard errors are clustered at the firm level; t-statistics appear in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6. Discussion

Our results support all three hypotheses. Firms whose pre-mandate complaint records were larger experienced greater increases in idiosyncratic volatility once those records became public; the effect operated through the idiosyncratic rather than the systematic component of return variation; and it emerged gradually rather than through implementation. Together, the first two findings identify the mechanism. If the mandate had altered firms' economic exposures, through regulatory

burden, compliance costs, or changes in investor composition, we would expect systematic risk to respond. It does not. If, instead, the mandate released firm-specific information that outsiders could not previously observe, we would expect unsystematic, or idiosyncratic, risk to rise while systematic risk remained unchanged. That is what we find. The result is consistent with the interpretation in Morck et al. (2000) and Durnev et al. (2003) that firm-specific return variation or risk reflects the incorporation of firm-specific information into prices.

The direction of the effect merits emphasis because it runs opposite to much recent work on disclosure and idiosyncratic risk, which finds that greater transparency reduces firm-specific volatility. We suggest reconciliation lies in the distinction between reassurance and revelation. Studies of ESG and risk-management disclosure examine what a firm says about its own policies and intentions; such disclosure can credibly resolve uncertainty. Investor complaints are neither police nor intentional, but rather a verifiable record of failures in shareholder servicing. Disclosing them does not resolve uncertainty about the firm; it creates uncertainty that did not previously exist in the public information set, because outsiders must now form a view about a problem whose severity, persistence, and implications for future conduct they have no basis to assess.

The third finding is the most distinctive and carries the clearest implication for how disclosure mandates should be evaluated. The near-zero coefficient in the implementation year, followed by five years of monotonic increase, is inconsistent with the event-study framing that dominates the disclosure regulation literature. An announcement effect would have appeared immediately and decayed. What we observe instead is accumulation. Three mechanisms are consistent with this pattern: the informativeness of a repeated disclosure grows with the length of the series, since persistence can only be assessed over many quarters; a novel disclosure lacks an interpretive convention and is therefore processed with a delay; and cross-sectional comparability improves as

the population of filers stabilizes. Our design cannot separate these three, and we do not attempt to do so.

The practical implication for regulators is that evaluations conducted shortly after implementation may understate the effects of a recurring disclosure requirement. Had we observed only the first two years of the LODR regime, we would have concluded that the mandate had no measurable capital-market consequence. The effect in financial year 2021 is roughly fifteen times the effect of implementation. Finally, the absence of heterogeneity itself is informative. In unreported specifications, we find no evidence that the effect varies with foreign institutional ownership. The complaint–risk relationship is broad-based rather than concentrated among firms with sophisticated investor clienteles, consistent with a diffuse repricing of firm-specific uncertainty.

Splitting the sample by financial versus non-financial service firms (reported in Table 16 of the Appendix) leaves this reading intact. The relationship is established among non-financial firms, which constitute much of the sample, and is neither established nor excluded among financial firms, where the subsample is too small to support inference. If a sectoral difference exists, one plausible mechanism is that financial firms already operate under sector-specific grievance redressal arrangements, such as banking ombudsman schemes, and so derive comparatively little incremental informational benefit from the LODR mandate. We offer this as an interpretation consistent with the point estimates rather than as a finding that the split-sample evidence establishes.

7. Conclusion

This study asks how a firm's risk changes when a regulator mandates disclosure of its shareholders' grievances. Using India's LODR mandate and a panel of 722 firms over eleven

financial years, we find that firms with larger pre-mandate complaint records experienced significant, gradually accumulating increases in idiosyncratic risk following disclosure: an interquartile increase in complaint intensity raises daily idiosyncratic volatility by 2.6 percent of the sample mean initially, rising to 4.7 percent by the end of the sample. The effect is confined to the idiosyncratic component; systematic risk is unaffected, and the result survives conditioning on it. This pattern is consistent with the release of firm-specific information, rather than any change in firms' exposure to common factors, as the operative channel.

The effect is statistically indistinguishable from zero in the implementation year and rises monotonically over the following five years; pre-trends are flat, placebo tests are null, and the trajectory shows no discontinuity at the onset of the pandemic. Two implications follow. For the disclosure literature, recurring mandates appear to be absorbed through accumulation rather than announcement, so evaluations conducted in a narrow window around implementation may understate the effect. For regulators, even a low-cost procedural requirement, such as a quarterly form, can carry measurable capital-market consequences once the disclosed information is comparable and standardized across firms.

8. Limitations and Directions for Future Research

Our design has several limitations. First, because LODR is applied to all firms simultaneously, we identify the effect of variation in disclosure intensity rather than the mandate itself; year fixed effects absorb any common effect, so our estimates reflect the differential effect of greater prior information disclosure. Second, the continuous-treatment design carries interpretive costs, since two-way fixed-effects estimands with continuous treatment are weighted averages of dose-specific effects that need not be economically meaningful (Callaway et al., 2024); our binary specification

mitigates but does not eliminate this concern. Third, we cannot distinguish between the three mechanisms that could generate gradual diffusion, accumulating series length, developing interpretive conventions, and improving cross-sectional comparability, as all three predict the same monotonic path. Fourth, complaint counts are a coarse measure of shareholder-servicing quality that does not distinguish serious grievances from trivial ones. Fifth, the sample's final two years coincide with the COVID-19 pandemic; although we find no discontinuity at the pandemic boundary, differential pandemic exposure among complaint-intensive firms cannot be fully ruled out. Finally, our evidence concerns risk rather than value; we find no effect on Tobin's Q, and the setting is a single emerging market with concentrated promoter ownership. Whether comparable mandates affect value elsewhere, and whether disclosure accelerates complaint resolution, are natural questions for future work.

Appendix

Table 16. Sectoral heterogeneity: split samples

	(1) Idio_Risk Non-financial	(2) Idio_Risk Financial	(3) Total_Risk Non-financial	(4) Total_Risk Financial
Com_int x Post2016	0.0314** (2.3034)			
Com_int x Post2016		0.0477 (1.0127)		
Com_int x Post2016			0.0306** (2.2978)	
Com_int x Post2016				0.0438 (0.9103)
Controls	Yes	Yes	Yes	Yes
Constant	4.7203*** (10.5004)	3.8457*** (3.9639)	4.2663*** (9.5436)	3.6101*** (3.6536)
Observations	6,445	438	6,445	438
Adjusted R-squared	0.6623	0.6593	0.6572	0.6321
Firm FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes
Industry x Year FE	Yes	No	Yes	No

t-statistics in parentheses. Financial-firm columns use year rather than industry-year fixed effects; with 70 financial firms, the two are near-collinear. Split-sample coefficients are not formally comparable across subsamples. Standard errors clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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