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LABOR MARKET REGULATION AND FACTORY ADJUSTMENT IN INDIA

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Labour Market Regulation and Factory Adjustment in India

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ABSTRACT:

This study examines how the 2014 labour-law reform in Rajasthan affected manufacturing factories. The reform raised the employment threshold for Chapter V-B of the Industrial Disputes Act from 100 to 300 workers. Using factory-level panel data from the Annual Survey of Industries for 2010–2020 and a triple-difference design, the study compares factories with 100–299 baseline workers to those with 300 or more workers. The analysis examines capital and plant intensity, machine intensity, labour productivity, management premia, IT asset share, and labour composition. The results show no statistically significant effects on most of the key outcome variables. In contrast, the reform appears to have significantly reduced the share of contract workers and increased the share of regular workers. Overall, the findings suggest that labour composition was the primary margin of factory adjustment, leaving other adjustment margins, like technology (proxied by share of IT assets), capital deepening (measured through plant and capital intensity), and machinery adoption with no conclusive results.

While the employment-level effects of employment protection legislation (EPL) have been heavily scrutinized in labour economic literature, the corresponding adjustments in capital accumulation, technology adoption, and within-plant productivity remain relatively under-explored. As observed in the foundational literature, employment flows and job security regulations are well-studied, but capital deepening and productivity are not (Cingano et al, 2016). This imbalance is particularly evident in developing countries like India, where rigid labour market regulations are frequently blamed for sluggish manufacturing growth. In November 2014, the Indian state of Rajasthan initiated a landmark reform by amending Chapter V-B of the Industrial Disputes Act (IDA) of 1947, raising the threshold of its applicability from 100 to 300 workers and exempting medium-sized factories from seeking prior government approval for retrenchments (Goldar, 2023; Bhattacharjea, 2021). While subsequent empirical studies have documented the immediate employment-level impacts of this reform, there remains room for exploration in analyzing its broader consequences on capital-labour substitution (referred to as capital intensity in this paper), machine intensity (based on electricity consumption), and technology adoption (proxied by share of IT assets, and plant and machinery) at the factory level.

Understanding these adjustments is important because rigid labour regulations can act as an implicit "regulatory tax" on labour, which would encourage firms to adjust their overall production techniques. When a government makes it easier for factories to fire workers, it alters the relative user costs of inputs, which may lead factories to alter their reliance on machinery and technology rather than simply adjusting the headcounts of their workforces (Hasan et al, 2013). However, the existing literature on the 2014 Rajasthan reform has focused mainly on aggregate or firm-level employment and output, leaving the core questions of capital-labour substitution and active machine utilization unanswered (Chaudhary and Sharma, 2022). This study aims to contribute to this strand of literature by analyzing factory-level adjustments in capital (as mentioned above), technology adoption and labour composition in Indian factories following the 2014 pro-employer reforms.

Before we evaluate the empirical evidence from this study, it is necessary to establish the competing theoretical predictions for these adjustments, as the effects of labour market deregulation on physical capital and productivity are highly ambiguous (Cingano et al, 2016). According to Hasan et al (2013), standard competitive models predict that reducing firing costs lowers the effective cost of labour, inducing firms to substitute away from capital toward labour, which decreases capital depth. In this study, *plant intensity* is defined as the ratio of plant and machinery (closing, net of depreciation) and total workers. On the other hand, in models with search frictions and wage bargaining, easing firing restrictions mitigates the "hold-up" problem that discourages physical capital investment, leading to capital deepening (inferred from Cingano et al, 2016). Additionally, if physical capital and firm-specific human capital are complementary, a positive relationship between EPL and capital intensity arises because EPL protects and induces investments in specific skills (Janiak and Wasmer, 2014); and so by easing EPL in this framework would decrease capital depth by reducing worker tenure.

This ambiguity extends to *machine intensity* (defined as electricity consumed per manday in this study) and *management premia* (defined as wages per supervisory worker and wages per regular worker in this study). For machine intensity, if advanced machinery and labour are substitutable, lowering firing costs will lead firms to substitute machine operation with manual labour, decreasing machine intensity. However, if modern technology complements a highly stable workforce, a more flexible firing environment might encourage plants to hire regular workers who utilize advanced, electricity-driven machinery, raising machine intensity. Besley and Burgess (2004), report that workers having too much bargaining power will reduce investment to their own detriment (especially if labour and capital are complements) and there is no clear effect on wages (can't say if they will rise or fall). Bertrand et al, (2025), state that contract workers earn substantially less than regular workers, a shift from contract toward regular employment could increase the regular wage share[1].

Finally, the theoretical predictions for *productivity* (defined in this study as nominal output per worker) are similarly divided. On one hand, relaxing firing laws can raise labour productivity because managers can dismiss unproductive workers to achieve optimal skill matches, and workers may increase their effort due to the heightened threat of dismissal due to lack of formal labour unions (Goswami and Paul, 2021). Sofi (2026) reports that the theoretical link between contract workers and labour productivity is ambiguous, due to the multifaceted nature of the relationship between contract workers and firms. The author states that contract workers and their employers are less likely to invest on their skill development, resulting in poor human capital formation but it is also possible that the employment of contract workers can help companies to fill gaps in the regular labour, to avoid underutilization of resources.

Prior empirical evidence from other countries highlights this non-monotonic relationship. For instance, in the United States (which is a low-EPL environment) the adoption of wrongful-discharge protection induced capital-labour substitution and a decline in TFP (Cingano et al, 2016; Autor, 2007). Conversely, panel studies across highly regulated European countries document a negative relationship between EPL and capital-labour ratios (reported by Cingano et al, 2016; Cingano, 2010). In India, the empirical debate over the 2014 Rajasthan reform remains divided and has focused mainly on employment-level effects (Goldar, 2023; Goldar, 2024). Using state-level synthetic controls and plant-level DiD, Chaudhary and Sharma (2022) find no aggregate manufacturing employment or output effects, but the authors note that firms saw an increase in contract employment and a reduction in permanent employment. On the other hand, Goldar (2023) uses the ASI plant-level data to show that the 2014 reform increased both regular and contract employment.

This empirical impasse leaves several important questions unanswered, as most studies rely on measures of employment and total fixed capital, which do not capture active machine utilization or the internal wage dynamics of the factory. This study contributes to the empirical literature in three key aspects.

Firstly, highly granular outcomes are used here to represent the direct mechanisms of technology adoption: isolating specific plant and machinery assets (plant intensity) and using electricity consumed per manday (machine intensity) as a proxy for machine operations. The study also looks at capital intensity as a whole

[1] Also, Bashir et al. (2025) specifically look at the wage gap between contract and permanent workers.

(which is total fixed assets of the factory per worker) and here the differences in how the reform has impacted plant and machinery as a specific component of the fixed assets[2] are also compared.

Secondly, the management premia among the regular workers of a factory is evaluated to understand how flexibility alters internal wage structures; specifically to see if the reform impacts the internal distribution of compensation across worker categories. It also checks the impact of the reform on IT assets shares and regular and contract labour shares. This would be insightful for understanding if the labour composition has changed due to the reform.

Thirdly, while most existing plant-level designs rely on DiD designs, this study implements a triple-difference (DDD) design on factory-level Annual Survey of Industries (ASI) microdata (2010–2020, excluding 2015 which is the treatment year), using size-based thresholds to isolate the component of the change that is unique to the interaction between Rajasthan, the treated size group, and the post-reform period.

Following the size-contingent design of Cingano et al. (2016) which exploits a threshold-based policy change to identify causal effects, this paper implements a triple-difference (DDD) identification strategy (Chaudhary and Sharma (2022) and Goswami and Paul (2021) also employ this methodology in their research on this strand of literature). The treatment group is defined as factories with baseline employment of 100–299 workers, (who directly experienced the lifting of the government approval requirement under Chapter V-B of the IDA) and is compared to a control group of factories with 300 or more workers who remained bound by the firing restrictions. To eliminate confounding state-level shocks and national size-specific trends, this size-contingent difference is compared to the corresponding difference in control states that did not implement the reform. The baseline models are estimated in an unbalanced panel of factories from the ASI spanning 2010 to 2020 (excluding the reform year 2014-15), controlling for state-year, state-treatment, and year-treatment fixed effects and then adding factory fixed effects.

Under our preferred specification, which includes factory fixed effects, we find no statistically significant post-reform change in capital intensity, plant intensity, machine intensity, management premia, or labour productivity. However, we find a relatively robust and statistically significant shift in labour composition: the share of contract workers fell by approx 13 percentage points, while the share of regular workers rose by approx 7 percentage points, relative to the comparison groups. There is no evidence that the reform changed factories' IT asset share found in this study. These results suggest that the primary adjustment margin activated by the Rajasthan reform was the composition of the workforce rather than capital deepening or technology adoption.

[2] It would be interesting to look at the other individual components of the total fixed assets of a factory, like transportation, land, rents, etc; to understand the impact of the reform on these features. Furthermore, it would provide us an insight as to if individual components of capital were affected by the reform and how it has changed over time after the reform was imposed. It would enable us to see if there is a particular component that drives the relationship between capital outcomes and the impact of the reform. These are mentioned as an extension of this study and have not been included in this paper.

In this study, contract worker share allows us to measure informality in the organized sector, as reported by ASI data. India has over 90% of its entire workforce and 85% of its non-agricultural workforce engaged in the informal sector (informality is defined as those without any social insurance) (Mehrotra, 2019). Mehrotra (2019) also reports that since the Goods and Service Tax (GST) was implemented in 2017, the share of formal workers has slightly risen. Furthermore, the author states that there has been a rising share of regular wage employment in the country (as seen in rising wages in rural and urban areas till 2011-12); these and similar, other such positive factors in India have mitigated the disadvantages of informality in India's workforce. A more detailed analysis regarding the Indian informal sector and its attributes would be possible using the PLFS (Periodic Labour Force Survey, started in 2017).

The PLFS reports a variety of employment related variables and uses the definition of informality provided by the NSSO (National Sample Survey Office - informal sector enterprises comprise of all unincorporated proprietary and partnership enterprises) to categorise work into two categories (one as the principal and the other as subsidiary activities). Although, the PLFS overlooks the hybrid livelihood of India's urban informal economy (Desai, 2025). Also, it is important to note that Djidonou and McGregor (2022) report that informal workers have experienced productivity increases and there has been a decline in the share of informal workers in manufacturing over time in India. A detailed analysis of this is currently out of the scope of this paper but the findings from this study are encouraging and can be explored further in depth using PLFS data. MoSPI has PLFS categorise respondents into: Broad Status Employment which tell us what type of work are people engaged in. Also, contract employment is widely discussed in the Indian literature as a manifestation of the informalisation of the organised sector, as firms substitute contract workers for directly employed workers (Goldar, 2012; Kapoor and Krishnapriya, 2023).

In this paper, we first provide a brief background to understand the reform and what it entails. Then we discuss the data and sample selection, acknowledge the missingness and limitations of the data. This is followed by our proposed models and results.

BACKGROUND[3]

2.1 — The pre-reform regulatory environment

The Industrial Disputes Act, 1947 (IDA) governs industrial disputes involving regular workers, including their hiring, firing and the closure of factories. Within the Act, Chapter V-B imposes additional restrictions on establishments above a specified employment threshold. Chapter V-B originally applied to establishments employing 300 or more workers, but a central-government amendment in 1982 lowered the threshold to 100 workers. After the Rajasthan reform in 2014, the threshold was increased to 300 workers again (bringing it back up to the pre-1982 amendment levels).

[3] This section is supplemented by reports from the Labour Bureau and the Labour Department.

For establishments covered by Chapter V-B, retrenchment of regular workers required prior government permission. The Chapter also required a 90 days' advance notice for closure. These requirements were applied in addition to the provisions of Chapter V-A (which states that establishments with 50 or more regular workers were subject to rules governing lay-offs, retrenchment and closure).

In short, the restrictions under Chapter V-B made employment adjustment more regulated for establishments above the 100-worker threshold. Chaudhary and Sharma (2022) describe the IDA as governing the hiring and firing of regular workers and identify Chapter V-B's requirement of official permission for retrenchment as a particularly important employment-protection provision.

This regulatory framework also distinguishes between regular (permanent) workers and contract workers. Contract workers were not subject to the IDA in the same way as regular workers were, and the use of contract labour had become an important means through which firms could retain employment flexibility.

2.2 — The 2014 Rajasthan reform

In November 2014, Rajasthan amended several industrial labour laws. It is important to note here that Rajasthan was the first state to implement these reforms. This provides us with the earliest and clearest policy change that has been studied in this paper. The most prominent amendment to the IDA raised the employment threshold above which an establishment required government permission for retrenchment under Chapter V-B from 100 to 300 workers. This meant that establishments with employment between 100 and 299 workers were no longer subject to the Chapter V-B requirement for requiring prior government approval for retrenchment, while establishments above the new threshold remained subject to the government requirement (Chaudhary and Sharma, 2022; and Goldar, 2023).

The Rajasthan reform formed part of a wider set of amendments to labour legislation. The state introduced reforms in four Acts: Industrial Disputes Act, Factories Act, Contract Labour Act, Apprentices Act (Goswami and Paul, 2021). The state also increased the minimum union-membership requirement from 15 to 30 percent of the establishment's workforce and introduced a three-year limit for workers wishing to raise an objection concerning their discharge or retrenchment. At the same time, in 2014, the applicability threshold of the Contract Labour (Regulation and Abolition) Act was raised from 20 to 50 contract workers, while the threshold for application of the Factories Act was raised from 10 to 20 workers for establishments using power and from 20 to 40 workers for establishments not using power (as reported by Goswami and Paul, 2021)[4].

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[4] The subsequent adoption of similar reforms by other states occurred in later years (2015 onwards), creating staggered treatment timing rather than the single 2014 treatment that is the focus of this study. Incorporating those states as a part of the study would change the analysis from estimating the effect of the Rajasthan reform to estimating the effects of reforms across multiple states. Furthermore, it is to be noted that this staggered adoption of the reform by other states is a limitation of this study and may lead to attenuation bias.

The Chapter V-B amendment provides a source of size-based variation as its effect was concentrated on establishments in the 100–299 worker range. Goldar (2023) identifies establishments with 100–299 directly employed workers in the year 2013–14 as a size class expected to be affected by the change in the Chapter V-B threshold.

2.3 — Why this reform is useful for identification

The DDD design employed in this study is similar to the one used by Chaudhary and Sharma (2022). The Rajasthan reform provides a useful setting for causal analysis as its principal change to Chapter V-B is explicitly size-dependent. Factories below the new 300-worker threshold experienced a change in the regulatory environment, while establishments above the threshold continued to face the original Chapter V-B requirement. Chaudhary and Sharma (2022) use this feature to compare establishments with 100–300 workers with larger establishments, and by comparing these differences between Rajasthan and other states. Similarly, Goldar (2023) reports that the 100–299 group was expected to be affected by the threshold increase[5].

Goldar (2023) describes Rajasthan as undertaking the reform in 2014 and notes that no other state significantly changed its labour regulations in that year or the preceding few years, describing the episode as a unique quasi-natural experiment. However, the evidence in the papers does not support describing the reform as completely unanticipated. Goswami and Paul (2021) note that Gujarat, Madhya Pradesh, Uttar Pradesh and Haryana had already begun considering greater labour-market flexibility around 2014. From this result, in Goldar's own paper (2023), the author notes that there was a possibility of an anticipation effect, particularly by 2017 or later as other states debated adopting this reform.

Goswami and Paul (2021) find that establishments with more than 100 and fewer than 300 workers in 2014 experienced a significant increase in total employment and contractual workers relative to establishments with more than 300 workers, in Rajasthan relative to Punjab, after the reform. They also find a decline in the share of directly employed workers among these establishments.

Chaudhary and Sharma (2022) similarly find that the amendment reduced the implicit regulatory cost of labour for affected firms and increased their use of contract workers, while finding no discernible effect on total employment or output. Their firm-level estimates therefore provide evidence that the statutory change affected the behaviour of establishments in the treatment size range.

Taken together, all these features make the Rajasthan reform suitable for exploiting the interaction between state, establishment size and time: the reform occurred in Rajasthan in 2014, changed the regulatory treatment

[5] More detail has been provided in section 3.3 and in footnote 10.

of establishments below the new 300-worker threshold, and has been shown in existing firm-level studies to have affected outcomes for establishments in the affected size range[6].

DATA

3.1 — Data source

The Ministry of Statistics and Programme Implementation (MoSPI) collects establishment-level data on India's organized manufacturing sector. Key variables include capital, employment and wages, inputs and outputs, etc. The first ASI study was conducted in 1960, replacing the earlier surveys, namely: Census of Manufacturing Industries (CMI — launched in 1946), and Sample Survey of Manufacturing Industries (SSMI — launched in 1949). These surveys ended in 1958 after which 1959 was taken as the reference year for the ASI released in 1960.

The survey schedule has changed substantially since it was first released in 1960. For the sample used in the study, the ASI data has been organized into several blocks, all of which focus on different details about the unit of observation, which is factory-year. Block A & B give us the identification, owner and factory details, and operational status. Block C & D have details regarding the fixed assets (opening-closing balances, depreciation) and working capital and loans. Block E & F consist of employment details, mandays, wages, labour costs, and operating expenses. Block G, H, I, & J have information regarding input consumption, energy details, and product-wise output. The subsequent blocks K and L (available for certain years in the ASI schedule) have information regarding Information and Communication technology (ICT) usage and Energy Conservation (EC) measures but due to data for these blocks not being available, they have not been considered for the purpose of this study.

The data has been sampled from 2009-10 to 2019-20, excluding the year of the policy implementation, 2014-15[7]. The cross-section data was merged across the years using the factory ID which is the unique identifier for this data. Although these are anonymized and do not reveal the identity of establishments, this identifier allows establishments to be tracked across survey years.

Despite the expansive data which was provided upon request directly from the MoSPI, there are certain limitations. This data excludes the informal sector (under 10 workers). Also, owners of multiple units within the same state/industry can submit a single consolidated return which in turn obscures plant-level granularity. These have also been acknowledged and reported by literature in this field. Furthermore, like any survey data,

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[6] This study uses regular, permanent and direct workers interchangeably. Also, the unit of study is factory-year and this study also uses factory, firm, establishment and plant terminology interchangeably. These two statements are to be noted as they refer to regular workers and factories, respectively.

[7] Throughout this paper, ASI year 2010 refers to the 2009–10 financial year, ASI year 2020 refers to 2019–20 and so on. 2014-15 (referred to as 2015) is the treatment and excluded from the study. Additionally, 2013-14 (referred to as 2014) is the baseline year for the event study.

ASI too is subject to non-sampling errors, non-response bias, and structural shifts in sampling frame definitions over time.

3.2 — Variable construction and definitions

Below are the variables directly constructed using concepts, items, and definitions from directly the ASI Instruction Manual, and supplemented by the ASI Schedule.

Contract Workers - These workers are those who are not employed directly by the factory but are engaged through a third party, such as an agency or contractor.

Contract_Share - This is the ratio between contract workers and total workers

- *Formula*: Contract_share = Contract Workers/Total Workers

Regular Workers - are those whose nature of employment is regular and includes permanent, probationers and temporary workers. These workers are employed directly.

Regular_Share - This is the ratio between regular workers and total workers

- *Formula*: Regular_share = Regular Workers/Total Workers

Supervisory staff - Include all persons holding positions of supervision or management (on regular or contract basis) regardless of classification under the Factories Act, 1948.

Total Workers - are all persons employed directly by the factory, including regular and casual workers[8] (those who are employed on a day to day basis when need arises but without assurance of continuous employment), as well as workers engaged through contractors and paid wages or salaries. This includes workers involved in manufacturing processes and ancillary activities incidental to manufacturing, such as cleaning machinery or factory premises.

Wages - are the gross remuneration paid to workers during the accounting year that can be expressed in monetary terms. This includes basic wages, overtime payments, dearness allowance, compensatory and house-rent allowances, payments for paid leave and holidays, and regular production bonuses. Wages are measured before deductions such as income tax, provident fund contributions, and Employees' State Insurance (ESI) contributions.

Wage_Regular - This variable measures the total gross wages paid to directly employed regular workers.

Wage_Supervisory - This variable measures the total gross salaries paid to supervisory and managerial staff holding positions involving supervision or management.

Wage_sup_pc - is the ratio of supervisory wages and supervisory staff (in per capita terms)

- Formula: Wage_sup_pc = Wage_Supervisory/ Supervisory_staff

[8] Under the IDA, Chapter VA, casual workers are excluded from the prohibition on lay-offs (Section 25M), but they are not excluded from retrenchment protection if they satisfy the continuous-service requirement (Section 25F).

Wage_reg_pc - is the ratio of regular worker wages and regular workers (in per capita terms)

- Formula: Wage_reg_pc = Wage_Regular/ Regular Workers

Management_premia - is the ratio of wages per supervisory worker and wages per regular worker (ratio of per capita terms). It measures the relative pay gap between supervisory staff and regular workers.

- Formula: Management_premia = Wage_sup_pc / Wage_reg_pc

IT Assets - comprise computer hardware and software used in the operation of the business. These include computers, related hardware and peripherals, uninterruptible power supplies (UPS), constant-voltage transformers (CVT), printers, and customised or database software. Computers integrated into operating machinery are excluded and are classified under plant and machinery.

IT_share - measures the share of computer equipment and software in the factory's total fixed assets. It is calculated as the proportion of total fixed assets represented by computer hardware, peripherals, and software assets.

- *Formula*: IT_share = IT Assets / Total Fixed Assets

Plant and Machinery - comprise equipment used in the execution of production and other factory operations. This includes power-generating machinery, metal-working machinery, furnaces, cranes, electrical installations used for operating the plant, moulds, air-conditioning equipment used for cooling machinery, and capital equipment acquired through hire-purchase or constructed for own use. Plant and machinery are measured at their original/book cost net of depreciation.

Plant_machinery_Share - measures the share of plant and machinery in the factory's total fixed assets. It is calculated as the ratio of the book value of plant and machinery to the total depreciated book value of fixed assets owned by the factory. Total fixed assets comprise land, buildings, plant and machinery, transport equipment, computer equipment, pollution-control equipment, and other fixed assets.

- *Formula*: Plant_machinery_Share = Plant and Machinery / Total Depreciated Book Value of Fixed Assets

Plant_Intensity - measures the ratio of the cleaned value of plant and machinery to the average total number of workers directly and indirectly employed in the production process.

- *Formula*: Plant_Intensity = Plant and Machinery / Total Workers

Fixed assets - those assets that have generally normal productive life of more than one year; these are capital items which are repeatedly used in the process of production over the duration of life of the fixed assets. This covers all types of assets, new or used or own constructed, deployed for productions, transportation, living or recreational facilities, hospitals, schools, etc. for factory personnel. It includes land, building, plant and machinery, transport equipment, etc. and excludes intangible assets except computer software.

Capital_Intensity - measures the ratio of the cleaned value of total fixed assets to the average total number of workers directly and indirectly employed in the production process.

- *Formula*: Capital_Intensity = Fixed assets / Total Workers

Electricity Consumed - is to the total quantity of electricity purchased and consumed by the factory during the accounting year for manufacturing operations.

Total Worker Days (Mandays Worked) - measure the number of worker-days recorded during the accounting year.

Machine_Intensity - measures electricity consumption per manday worked and is used as a measure of the intensity of electricity use in production. It measures how production changes electricity use relative to labour.

- *Formula*: Machine_Intensity = Electricity Consumed / Total Worker Days

Total_Output_Combined - measures total ex-factory gross output, including electricity generated and sold by the factory.

Output_per_Worker - is defined as total combined ex-factory gross output generated per worker employed in the factory. This measure is nominal output per worker. ASI data does not provide firm-level deflators but the Wholesale Price Index (WPI)[9] can be used to get real output per worker.

- *Formula*: Output_per_Worker = Total_Output_Combined / Total Workers

3.3 — Sample selection

In this study, a panel for the ten years from 2010 to 2014 and 2016 to 2020 has been created. It has 613,373 factory-year observations spread across 242,466 unique factories. There are 9,048 factories that appear in all ten years of the sample, and 121,255 factories appear in only one year of the sample. The panel is unbalanced and the missing values for the outcome variables have been flagged (created dummy variables to check the percentage missingness).

For our key outcome variables, we have the following missing values which range from 3.3% to 11.3%. The total number of observations in the sample that we consider are 123,888. It is important to note that the estimation sample varies across outcomes because observations with missing values are excluded from the regression.

[9] The WPI is a commodity level indicator, so constructing an industry-level deflator would require mapping WPI commodities to ASI industries. We have not looked at this deflator in this study and it is only mentioned as a possible extension. Since deflating by an industry-year price index (the WPI) is mathematically equivalent to including industry×year FE, this study has included industry×year FE for all output per worker specifications. A further check that has been considered is industry×state×year FE.

Variable	Missing	Non-missing	% Missing
Machine intensity	7,291	116,597	5.89%
Plant intensity	5,445	118,443	4.40%
Capital intensity	4,092	119,796	3.30%
Management Premia	11,195	112,693	9.04%
Labor Productivity	14,053	109,835	11.34%

Table 1: Missingness in the data

The treatment factories are Rajasthan factories with 100–299 workers at the baseline, and the control factories are those with 300 or more workers at the baseline. The comparison group includes factories in both Rajasthan and other states, allowing the DDD design to compare changes across both state and factory-size groups. This choice follows the size-based identification strategy used by Chaudhary and Sharma (2022), who use the size-dependent nature of the Rajasthan reform and similarly define firms with 100–300 workers as the treatment group and firms above this threshold as the control group. This choice avoids relying on establishments below the original 100-worker threshold as the primary counterfactual.

Goldar (2023) notes the presence of a threshold effect around 100 workers and argues that some factories may keep regular employment below 100 by using contract workers, motivating his separate analysis of the broader 50–299 size class. The larger establishments (more than 1,000 workers) constitute only a small fraction of the 300-or-more group. As firms with fewer than 100 workers were never subject to Chapter V-B in the first place, the amendment changed firing cost among firms that had between 100 and 300 workers at the baseline. Using 300 or more workers is an appropriate control group as it continued to be subject to IDA Chapter V-B even after the amendment[10].

[10] A description of the factory sizes have been mentioned in Appendix A. Also, it is important to note that Goldar (2023) reports that the 50–99 group is a potentially useful alternative comparison group, but it is not necessarily an uncontaminated counterfactual because the literature identifies behavioural responses around the 100-worker threshold; i.e., the existence of a threshold effect. It is used as a robustness check for developing this paper further and the results are highlighted in the appendix Section B.

State Groups	Treatment	Obs	Mean Baseline Workers	Median Baseline Workers	Standard Deviation Baseline Workers
Other states	300+ (large)	5365	901	524	1863
Other states	100-299 (small)	9051	171	158	56.3
Rajasthan	300+ (large)	174	822	551	713
Rajasthan	100-299 (small)	284	173	164	52.7

Table 2: details of the treatment and control groups

The sample consists of 14,874 factories that satisfy the baseline employment criteria of at least 100 workers. Out of these, there are 284 factories in Rajasthan that fall within the 100–299 worker treatment, while 174 Rajasthan factories have 300+ workers and therefore serve as the within-Rajasthan comparison group. Doing a regression discontinuity design (RDD) here would have been interesting and could have provided information regarding the reform impact across similar sized factories, but due to sample size restrictions (only 10 and 11 factories which make up the 275-299 and 300-325 worker threshold), it is not possible to do so. The corresponding groups in other states provide the cross-state comparison. The numbers in the baseline employment between the Rajasthan treatment group and the corresponding group in other states is comparable, but it is important to note that the larger number of comparison-state factories reflects the broader pool of eligible establishments.

3.4 — Descriptive statistics and pre-reform visual evidence

The outcome variables show substantial dispersion, which motivates the use of logarithmic transformations for them.

Variable	Mean	SD	P10	Median (P50)	P90	Obs
Total workers[11]	462	1,153	100	237	895	121,157
Log plant intensity	12.30	2.24	9.82	12.60	14.70	118,443
Log capital intensity	13.2	2.05	11	13.4	15.30	119,796
Log machine intensity	3.06	1.77	0.98	3.24	5.00	116,597
Log management premia	1.48	0.8	0.56	1.43	2.50	112,693
Log output per worker	14.80	1.43	13.20	14.90	16.40	109,835
Contract worker share	0.47	0.35	0.00	0.50	0.95	89,415
Regular worker share	0.67	0.35	0.14	0.79	1	116,001

Table 3: Descriptive statistics for the sample population

[11] This refers to factory size. So the average number of workers in a factory for our restricted sample is 462.

The above table displays the descriptive statistics for the key outcome variables and the contract-worker measure. The sample contains substantial variation across factories in total workers employed and the intensity of capital and electricity use (machine use). Also, it is important to note here that while total workers is the sum of regular and contract workers, there are factories that have not reported contract workers or have zero contract workers (missingness in the data) which is why, due the differences in the sample sizes, the sum of the contract and the regular shares does not equal 1. The median factory employs 237 workers, while the mean is considerably higher at 462, reflecting the presence of larger establishments in the sample.

Table 4 below, shows how the main outcomes evolve across treatment and comparison groups before and after the reform, and illustrates why a simple before-and-after comparison cannot fully identify the reform effect.

	Pre-reform period (2010-2014)				**Post-reform period (2016-2020)**			
Variable	**Small - Others**	**Small - Rajasthan**	**Large - Others**	**Large - Rajasthan**	**Small - Others**	**Small - Rajasthan**	**Large - Others**	**Large - Rajasthan**
Total workers	184.81 (124.88)	187.81 (124.19)	795.84 (1429.55)	778.33 (780.90)	207.25 (210.70)	199.30 (176.58)	929.28 (2026.01)	969.46 (1071.19)
Plant intensity	11.94 (2.30)	12.22 (1.77)	12.58 (2.28)	13.00 (1.82)	12.25 (2.13)	12.49 (1.83)	12.78 (2.21)	13.38 (1.58)
Capital intensity	12.76 (2.12)	13.15 (1.49)	13.29 (2.03)	13.70 (1.60)	13.15 (1.95)	13.48 (1.64)	13.53 (2.09)	14.01 (1.51)
Machine intensity	2.85 (1.82)	3.19 (1.74)	3.09 (1.76)	3.57 (1.57)	3.08 (1.75)	3.32 (1.64)	3.25 (1.71)	3.75 (1.42)
Management Premia	1.45 (0.80)	1.46 (0.82)	1.48 (0.80)	1.51 (0.81)	1.50 (0.79)	1.39 (0.78)	1.52 (0.82)	1.48 (0.80)
Labor Productivity	14.51 (1.51)	14.73 (1.41)	14.81 (1.37)	14.99 (1.27)	14.84 (1.43)	15.01 (1.40)	15.04 (1.32)	15.34 (1.19)
Contract share	0.62 (0.29)	0.62 (0.31)	0.56 (0.28)	0.56 (0.33)	0.39 (0.37)	0.40 (0.38)	0.41 (0.35)	0.52 (0.38)

Note: The table reports the means, with standard deviations in parentheses. The sample consists of factories with at least 100 workers at the 2014 baseline. Treatment factories have 100–299 baseline workers, while control factories have 300+ baseline workers.

Table 4: comparison table between pre- and post- reform according to treatment for key outcome variables and contract shares

The above table compares the four groups before and after the reform. The descriptive changes highlight the fact that outcomes evolved over time in all groups, which makes a before-after comparison insufficient.

For instance, plant and capital intensity, and machine intensity increase between the pre- and post-reform periods in both Rajasthan and other states. Nominal labour productivity also increases across groups. Management premia falls for Rajasthan but rises for the 'others' under these specifications. Contract share also decreases here. The purpose of the DDD estimation used in this study is to determine whether the changes observed among Rajasthan's small factories were different from the changes experienced by the relevant comparison groups.

METHODOLOGY

4.1 — The estimand and the identifying assumption

The empirical strategy exploits the 2014 labour-law reforms in Rajasthan as a quasi-natural experiment and examines their effects using factory-level ASI data (Goswami and Paul, 2021). A key change relevant to this study is the amendment to Chapter V-B of the Industrial Disputes Act (IDA), which raised the employment threshold above which firms were required to obtain government approval for retrenchment and closure from 100 to 300 workers. The reform is responsible for changing the regulatory environment faced by medium-sized manufacturing establishments while leaving establishments above the new threshold subject to the pre-existing Chapter V-B requirements (Chaudhary and Sharma, 2022).

In this study, the focus is on factories whose baseline employment is on either side of the reform threshold, small (100-299) and large (300+) workers. Small factories at the baseline are referred to as the treated group, while large factories with 300 or more workers are the control group. Treatment status is assigned using the baseline employment and is fixed over the subsequent sample periods. Similar to Chaudhary and Sharma's design, this study also follows the institutional feature of the reform whereby the regulatory treatment of firms changed according to employment size. This study too, uses the same general source of identifying variation in their firm-level analysis (Rajasthan × Treatment × Post). Chaudhary and Sharma define firms with 100–300 regular workers at baseline as the treatment group and firms with more than 300 regular workers as the control group, as the former experienced a change in firing costs whereas the latter remained subject to Chapter V-B of the IDA. Furthermore, they compare this treatment-control difference between Rajasthan and the other states, which is described as a triple difference.

ASI reports observations by financial year, from April to March. *To clarify: 2015 is April 1st 2014 to March 31st 2015 (as per ASI naming conventions). The reform policy was implemented in November 2014. Therefore, the year 2015 is excluded from our study (all other years follow the same naming convention).* In short, the treatment period starts in 2016.

The model that this paper uses is:

$$Y_{ist} = \beta(Rajasthan_s \times Small_i \times Post_t) + \eta_k + \epsilon_{ist}$$

Where Y_{ist} denotes the outcome for factory i, in state s, and in year t; and here:

- $Small_i = 1$ if baseline employment is 100–299; and $Small_i = 0$ if baseline employment is ≥300;
- $Post_t = 1$ from 2016 onward;
- η_k = Fixed Effects as per different model specification.
- SE clustered at factory level.

And what β is: It captures the differential post-reform change in the outcome among Rajasthan plants in the small worker group relative to (i) larger Rajasthan establishments and (ii) corresponding factories in other states.

Mathematically, the DDD design is:

$$\beta = [\text{Rajasthan DiD}] — [\text{other-state DiD}]$$
$$\textit{Rajasthan DiD} = (Y_{R,\,T,\,Post} — Y_{R,\,T,\,Pre}) — (Y_{R,\,C,\,Post} — Y_{R,\,C,\,Pre})$$
$$\textit{Other-state DiD} = (Y_{O,\,T,\,Post} — Y_{O,\,T,\,Pre}) — (Y_{O,\,C,\,Post} — Y_{O,\,C,\,Pre})$$

The first difference (the time difference: post — pre) measures the change in outcomes over time for each state-by-factory-size group.

The second difference (treatment — control) compares the evolution of treated and comparison-size factories.

The third difference (Rajasthan DiD — other-state DiD) compares this size-specific change in Rajasthan with the corresponding size-specific change in the comparison states. Under the identifying assumption that, in the absence of the policy, the treated-versus-control difference would have evolved similarly in Rajasthan and the comparison states, β identifies the causal effect of the policy.

The Rajasthan DiD identifies the differential change in Rajasthan following the reform, but can also capture other Rajasthan-specific shocks occurring during the same period. Similarly, a comparison of smaller (treated) and larger factories could capture changes affecting firms of different sizes nationally. The DDD combines these comparisons to isolate the component of the change that is specific to the interaction between Rajasthan, the affected size group, and the post-reform period.

The logic of this study is closely related to the firm-level strategy of Chaudhary and Sharma (2022). The DDD equation includes Post × Treatment, Post × Rajasthan and Post × Treatment × Rajasthan, with year-specific dummies and firm fixed effects. The Post × Treatment term controls for contemporaneous nationwide shocks that vary by firm size while the Post × Rajasthan term controls for shocks that vary between Rajasthan and other states but affect the two size groups similarly. The triple interaction captures the residual differential post-reform change for treated-size firms in Rajasthan.

Identification Strategy:
Parallel trends assumption: In the absence of the Rajasthan reform, the difference in outcome trends between small and large factories would have evolved similarly in Rajasthan and the comparison states. This assumption cannot be tested for the post-reform counterfactual because the untreated outcome for treated Rajasthan factories after the reform is not observed. But, its plausibility can be tested using pre-reform data, as was done in the event study. As the treatment status is related to factory size, the groups can differ across different characteristics across employment, production, capital, productivity, etc. The event study done in this paper is used to assess whether the pre-reform data is consistent with our main assumption.

The preliminary estimates are reported for the main outcomes, which include plant intensity, total capital intensity and machine intensity. It also includes output per worker as a productivity outcome and management premia as a labour composition outcome. There are also checks done for adjustment margins of firms which include another labour composition outcome (regular vs contract workers) and digital capital (measured through IT assets).

Event Study:

The figure and table below showcase the event-study estimates relative to the omitted pre-reform reference year. We fix 2014 as our pre-reform, baseline year. The pre-reform coefficients provide a statistical assessment of whether the treatment and comparison groups followed similar trends before the reform, while the post-reform coefficients show how the estimated treatment effect evolves over time.

A standard DiD comparing Rajasthan with other states can capture Rajasthan-specific post-reform shocks. But by using the fact that the reform is applied as per factory size, the DDD can isolate the differential post-reform change among factories directly exposed to the relaxation of factory sizes.

Version 1 (Model 1):

Year	Plant Intensity	Capital Intensity	Machine Intensity	Management Premia	Labour Productivity[12]
2010	−0.1023 (0.2239)	0.0393 (0.1538)	−0.0712 (0.1623)	**-0.2417**** **(0.1197)**	0.1901 (0.1435)
2011	0.1567 (0.1641)	0.1373 (0.1294)	0.0588 (0.1508)	**-0.2222**** **(0.1125)**	**0.2791**** **(0.1324)**
2012	0.0845 (0.1379)	0.1606 (0.1147)	0.0700 (0.1334)	-0.1017 (0.1002)	0.1468 (0.1116)

[12] Labor Productivity includes industry × year fixed effects in the event study

2013	0.1122 (0.0991)	0.0744 (0.0809)	0.0412 (0.1040)	0.0872 (0.0931)	0.0815 (0.0987)
2016	**−0.2502**** **(0.1158)**	-0.0878 (0.0805)	−0.1623 (0.1096)	-0.1298 (0.0957)	-0.0588 (0.1091)
2017	−0.1401 (0.1404)	-0.0498 (0.0983)	−0.0947 (0.1056)	**-0.2070**** **(0.1010)**	-0.0610 (0.1160)
2018	**−0.2209*** **(0.1210)**	-0.0327 (0.0998)	−0.0932 (0.1133)	**-0.1714*** **(0.1013)**	-0.1622 (0.1213)
2019	0.0278 (0.1226)	0.0883 (0.1050)	−0.0824 (0.1214)	-0.0735 (0.1173)	0.1070 (0.1133)
2020	**−0.2210*** **(0.1310)**	-0.1247 (0.1128)	−0.1193 (0.1281)	-0.0920 (0.1178)	0.0041 (0.1320)
Rajasthan × Year FE	Yes	Yes	Yes	Yes	Yes
Rajasthan × Treatment FE	Yes	Yes	Yes	Yes	Yes
Year × Treatment FE	Yes	Yes	Yes	Yes	Yes
Industry x Year FE	-	-	-	-	Yes
Observations	118,443	119,796	116,597	112,693	109,835
R^2	0.0227	0.0212	0.0090	0.0018	0.2096

Note: Standard errors clustered at the factory level are reported in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: event study description for model 1

The Version 1 event-study estimates, relative to the omitted 2014 baseline year, show that there is limited evidence of significant differential trends in most outcomes, with two exceptions. Plant intensity is negative and significant at the 5% level in 2016 (−0.2502), and negative and significant at the 10% level in both 2018 (−0.2209) and 2020 (−0.2210); all other plant-intensity year coefficients, both pre- and post-reform, are statistically insignificant.

Labour productivity has one individually significant pre-reform coefficient, in 2011 (0.2791, significant at 5%). Capital intensity and machine intensity show no statistically significant coefficients in any year here. Management premia has the largest number of significant coefficients of the five outcomes, with the estimates in 2010 (−0.2417), 2011 (−0.2222), and 2017 (−0.2070) significant at 5%, and 2018 (−0.1714) significant at 10%.

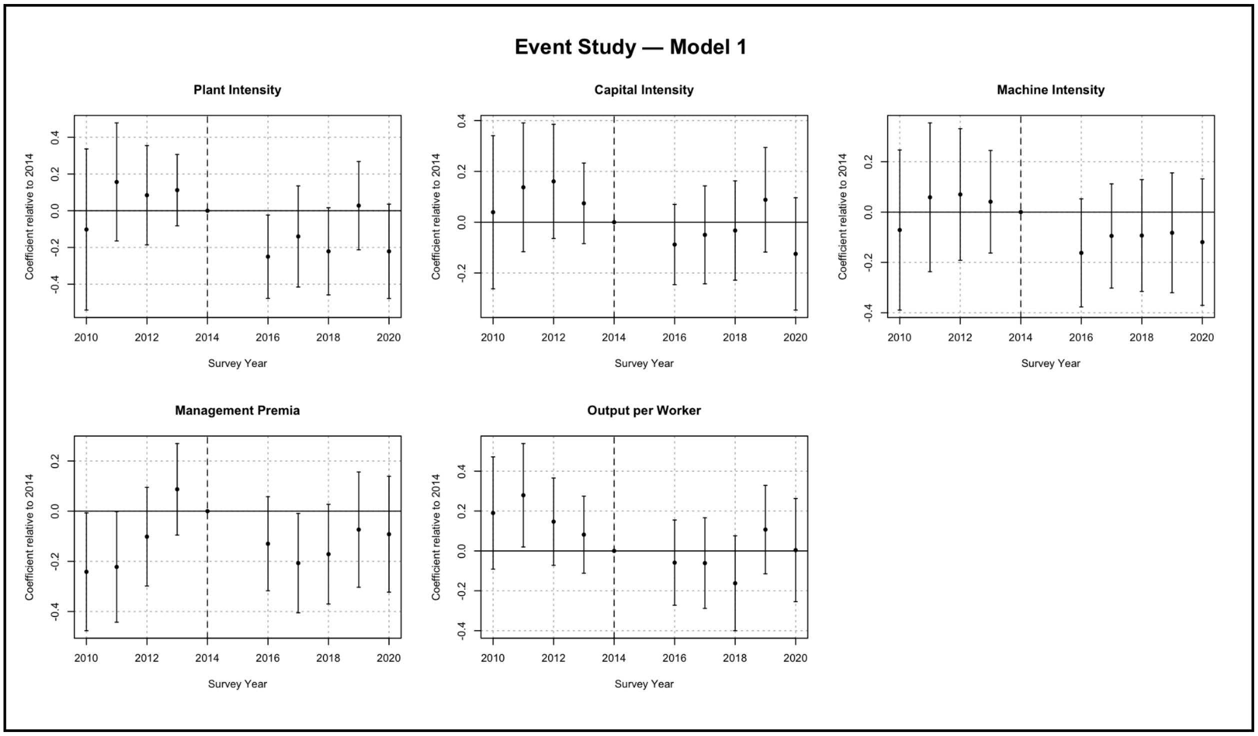

Figure 1: Event study for key outcome variables as per model 1

From figure 1, we can see the distribution across the outcomes (plotted at 95% confidence interval):

- *Plant Intensity:* Here, points hover near zero and cross it through 2013, then dip negative from 2016 onward, with 2016 the clearest case of a bar sitting completely below zero.
- *Capital Intensity:* Here, every bar crosses the zero line in every year, pre- and post-reform, it is the flattest, most centered-on-zero panel of the five outcomes.
- *Machine Intensity:* It has a similar pattern to capital intensity, where all bars cross zero, with a slight negative drift after 2016 that never separates from zero.
- *Management Premia:* This panel clearly lies under zero for most of the timeline, including several pre-reform years (2010, 2011 – here the bar is below zero). It has the clearest visual pre-trend concern of the five outcomes.
- *Output per Worker:* 2011 stands out as the one pre-reform point lying above zero; post-reform points drift back toward and below zero but with wide, zero-crossing bars.

Version 2 (Model 2):

Here we add factory fixed effects to control for unobservable firm characteristics.

Year	Plant Intensity	Capital Intensity	Machine Intensity	Management Premia	Labour Productivity
2010	-0.2213 (0.1651)	-0.0529 (0.1036)	-0.1010 (0.1180)	**-0.1987*** **(0.1202)**	0.0395 (0.1195)
2011	-0.0472 (0.1064)	-0.0136 (0.0926)	-0.0297 (0.1275)	**-0.1870*** **(0.1110)**	0.1006 (0.1086)
2012	-0.0100 (0.0949)	0.0382 (0.0846)	0.0119 (0.1167)	-0.0840 (0.0993)	0.1078 (0.0900)
2013	0.0633 (0.0701)	0.0629 (0.0650)	0.0158 (0.0943)	0.0957 (0.0918)	0.0666 (0.0773)
2016	-0.0520 (0.0872)	0.0020 (0.0729)	-0.0939 (0.0982)	-0.0804 (0.0949)	0.0116 (0.0939)
2017	0.0418 (0.1022)	0.0810 (0.0900)	0.0213 (0.0983)	**-0.1679*** **(0.1003)**	0.0695 (0.0970)
2018	0.0397 (0.0959)	0.1363 (0.0886)	0.0763 (0.1013)	-0.1198 (0.1001)	-0.0287 (0.1029)
2019	**0.2215**** **(0.1005)**	**0.2376***** **(0.0897)**	0.0398 (0.1091)	-0.0301 (0.1158)	0.1361 (0.1002)
2020	0.0750 (0.1085)	0.0605 (0.0975)	0.0246 (0.1169)	-0.0539 (0.1178)	0.0822 (0.1161)
Rajasthan × Year FE	Yes	Yes	Yes	Yes	Yes
Rajasthan × Treatment FE	Yes	Yes	Yes	Yes	Yes
Year × Treatment FE	Yes	Yes	Yes	Yes	Yes
Industry x Year FE	-	-	-	-	Yes
Factory FE	Yes	Yes	Yes	Yes	Yes
Observations	118,443	119,796	116,597	112,693	109,835
R^2	0.877	0.875	0.862	0.491	0.850

Note: Standard errors clustered at the factory level are reported in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: event study description for model 2

Adding factory fixed effects in Version 2, we observe that nearly all statistically significant pre-reform coefficients are removed relative to Version 1. Under Version 2, labour productivity and machine intensity are statistically insignificant in every year, pre- and post-reform. Management premia remains significant in three years [2010

(−0.1987) and 2011 (−0.1870), and 2017 (−0.1679), significant at 10%] which means that only one of its three significant coefficients falls in the post-reform period. Plant intensity and capital intensity are both statistically insignificant in every pre-reform year and in every post-reform year except 2019, where plant intensity (0.2215) is significant at 5% and capital intensity (0.2376) is significant at 1%.

The pre-reform estimates are mostly close to zero and statistically insignificant which is consistent with the absence of systematic differential trends before the reform. The post-reform estimates provide evidence for the timing and persistence of the reform's effects, which is useful for differentiating between slow and fast adjustments in production.

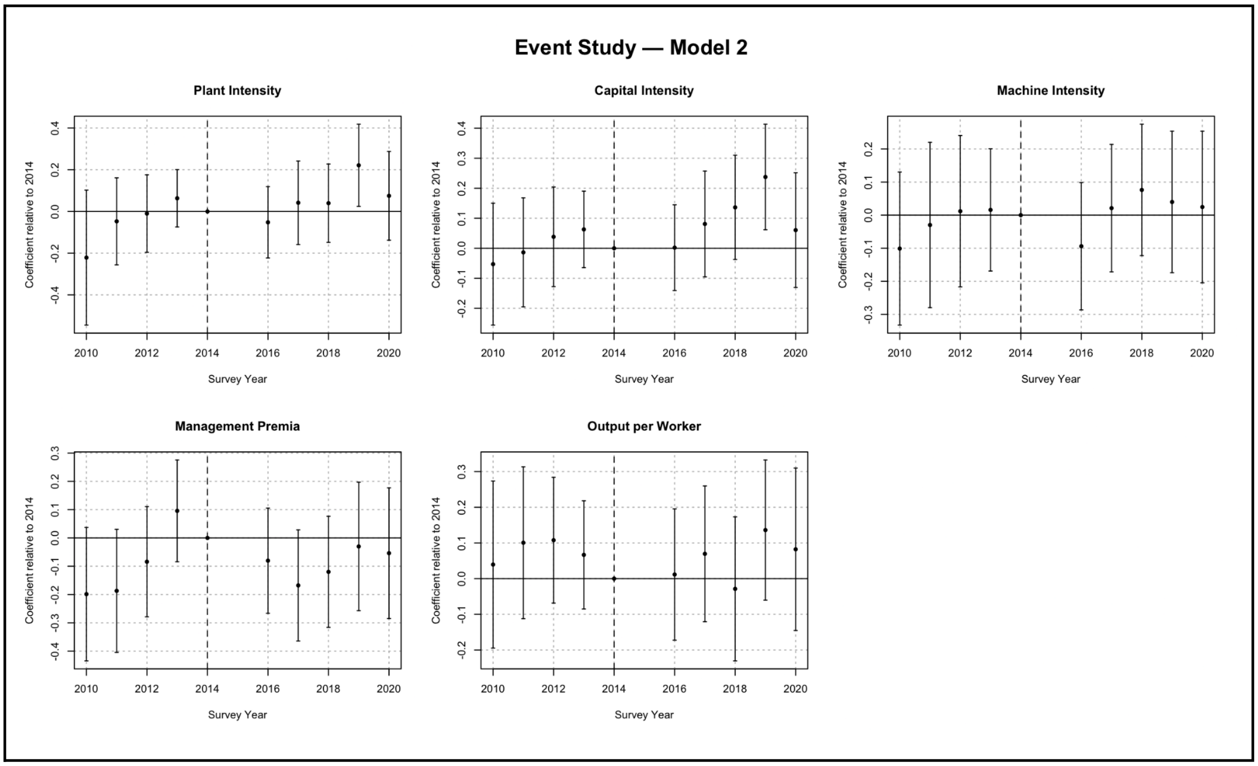

Figure 2: Event study for key outcome variables as per model 2

From figure 2, we see the results from version 2 (also at 95% confidence interval):

- *Plant Intensity:* All the bars (except 2019 – it lies above zero without the bar intersecting it) cross zero across the whole timeline.
- *Capital Intensity*: This is similar to plant intensity above. Here too, 2019 lies above zero without intersecting zero.
- *Machine Intensity:* All bars cross zero, both pre- and post-reform, with no outliers.
- *Management Premia:* Like version 1, here too, the outcome with the largest pre-2014 gap between point and zero (2010, 2011).
- *Output per Worker:* Here, every point is close to zero with wide, zero-crossing bars in every single year and is visibly flatter than its Model 1 counterpart.

To conclude, the event study tells us:

- Before the reform (2010–2013), most outcomes have coefficients close to zero with confidence intervals (at 95%) that cross zero. This means that we don't see evidence of pre-existing differences between the treatment and control groups, which supports our identification strategy. Management premia is the one exception, which is below zero even before the reform (for 2010 and 2011) in both models. These pre-reform differences indicate that treatment and control factories differed in management premia before the reform, so the parallel-trends assumption is less clearly supported for this outcome.
- When we add factory fixed effects, as in Model 2, most outcomes shift closer to zero compared to Model 1. This is most noticeable for plant intensity and output per worker. After this adjustment, only 2019 stands out, where both capital intensity and plant intensity have their confidence intervals at 95% that don't cross zero. As there is no consistent pattern before or after it, so it is insufficient on its own to establish a reform effect.
- Overall, after controlling for factory-level differences, we don't find a consistent or lasting effect of the reform on any of the five outcomes. The only signal is a single-year spike in plant and capital intensity in 2019, which by itself isn't strong evidence that the reform caused a real effect.

4.2 — The regression equation

This study has two distinct models that will be analysed. Note that, factory fixed effects absorb time-invariant factory heterogeneity. Furthermore, using the interaction of 'rajasthan and year', 'year and treatment' and 'rajasthan and treatment' as controls for this study will account for state-specific time shocks, treatment-specific time shocks, and time-invariant differences between Rajasthan and the treatment group, respectively.

The two key models in this study have been illustrated in tables 7 and 8. These tables answer the main empirical question: did the Rajasthan reform differentially affect factories in the 100–299 worker treatment group relative to larger factories, compared with the corresponding difference in other states?

<u>MODEL 1:</u>

The specification being used here is:

$$Y_{ist} = \beta(Rajasthan_s \times Small_i \times Post_t) + \zeta_{st} + \Delta_{si} + \gamma_{\mathrm{it}} + \epsilon_{ist}$$

where:

- $Small_i = 1$ if baseline employment is 100–299; and $Small_i = 0$ if baseline employment is ≥300;
- $Post_t = 1$ from 2016 onward;
- ζ_{st} = Rajasthan × year FE
- Δ_{si} = Rajasthan × treatment FE
- γ_{it} = year× treatment FE
- SE clustered at factory level.

	Plant Intensity	Capital Intensity	Machine Intensity	Management Premia	Labour Productivity
Rajasthan × Treatment × Post	**-0.217*** **(0.119)**	-0.121 (0.093)	-0.133 (0.094)	-0.056 (0.0722)	**−0.1634*** **(0.0909)**
Rajasthan × Year FE	Yes	Yes	Yes	Yes	Yes
Rajasthan × Treatment FE	Yes	Yes	Yes	Yes	Yes
Year × Treatment FE	Yes	Yes	Yes	Yes	Yes
Industry x Year FE	-	-	-	-	Yes
Observations	118,443	119,796	116,597	112,693	109,835
Adjusted R^2	0.02230	0.02081	0.00864	0.00138	0.20681
Within R^2	0.00002	0.000006	0.00001	0.000008	0.00003

Note: Standard errors clustered at the factory level are reported in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 7: regression output for model 1

When we add state x industry x Year FE to the labour productivity variable, the coefficient is −0.1784 (0.0981) which is again statistically significant at 10% significance level.

MODEL 2:

The specification being used here is:

$$Y_{ist} = \beta(Rajasthan_s \times Small_i \times Post_t) + \zeta_{st} + \Delta_{si} + \gamma_{it} + \lambda_j + \epsilon_{ist}$$

where:

- $Small_i$ = 1 if baseline employment is 100–299; and $Small_i$ = 0 if baseline employment is ≥300;
- $Post_t$ = 1 from 2016 onward;
- ζ_{st} = rajasthan × year FE
- Δ_{si} = rajasthan × treatment FE
- γ_{it} = year× treatment FE
- λ_j = factory FE
- SE clustered at factory level.

	Plant Intensity	Capital Intensity	Machine Intensity	Management Premia	Labour Productivity
Rajasthan × Treatment × Post	0.0894 (0.0863)	0.0889 (0.0741)	0.0253 (0.0746)	-0.0340 (0.0706)	−0.0069 (0.0761)
Rajasthan × Year FE	Yes	Yes	Yes	Yes	Yes
Rajasthan × Treatment FE	Yes	Yes	Yes	Yes	Yes
Year × Treatment FE	Yes	Yes	Yes	Yes	Yes
Industry x Year FE	-	-	-	-	Yes
Factory FE	Yes	Yes	Yes	Yes	Yes
Observations	118,443	119,796	116,597	112,693	109,835
Adjusted R^2	0.859	0.857	0.841	0.417	0.829
Within R^2	0.00002	0.00002	0.000003	0.000006	0.0000002

Note: Standard errors clustered at the factory level are reported in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: regression output for model 2

When we add state x industry x Year FE to the labour productivity variable, the coefficient is −0.0393 (0.0804) which is again statistically insignificant.

The above tables (table 7 and table 8) report the DDD coefficient of interest, β on Rajasthan × Treatment × Post, for the five main outcomes under both specifications. Taken together, both tables 7 and 8 show that the effects on plant intensity and labour productivity in Model 1 do not survive the inclusion of factory fixed effects. The preferred Model 2 specification does not provide any statistically significant evidence of reform-induced changes in these outcomes.

The 2014 Rajasthan labour-law reform relaxed the restrictions on adjusting regular employment. This may affect factories through other adjustment margins as well, such as composition of labour (like contract and regular workers), or through changes in digital capital (measured through IT assets).

	(1) Model 1	(2) Model 2
Panel A: Contract Worker Share		
Rajasthan × Treatment × Post	**−0.1101*** (0.0331)**	**-0.1278*** (0.029)**
Observations	89,415	89,415
Adjusted R^2	0.1100	0.7756
Panel B: Regular Worker Share		
Rajasthan × Treatment × Post	**0.0944*** (0.0308)**	**0.0739*** (0.027)**
Observations	116,001	116,001
Adjusted R^2	0.0132	0.7493
Factory FE	-	Yes
Rajasthan × Year FE	Yes	Yes
Rajasthan × Treatment FE	Yes	Yes
Year × Treatment FE	Yes	Yes

Note**:** Standard errors are clustered at the factory level and reported in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9: regression output for worker types

The coefficient for share of contract workers is consistently negative, −0.11 in model 1, −0.13 in model 2 and it is statistically significant in both specifications. As this is a share, it can be said that as per model 2, there is an approximately 13 percentage-point reduction in the share of workers employed through contractors, relative to the comparison group, following the reform. Also, it is visible that the estimate is stable across both specifications so adding factory fixed effects doesn't make the result disappear but the adjusted R squared has increased. The coefficient for share of regular workers is consistently positive, 0.094 in model 1 and 0.074 in model 2 and it is statistically significant across both specifications. As this is a share, it can be said that as per model 2, there is an approximately 7.4 percentage-point increase in the share of regularly employed workers, relative to the comparison group, following the reform. This suggests that if the reform induced any adjustment at treated Rajasthan factories, the adjustment operated through labour composition (substituting regular for contract workers) rather than through the other mentioned channels in this paper.

To see if post-reform, the factories change the importance of IT capital in its overall capital stock, IT asset share is analysed.

	(1) Model 1	(2) Model 2
Rajasthan × Treatment × Post	0.0020 (0.0022)	0.0014 (0.0022)
Observations	111,894	111,894
Adjusted R^2	0.0012	0.4401
Factory FE	-	Yes
Rajasthan × Year FE	Yes	Yes
Rajasthan × Treatment FE	Yes	Yes
Year × Treatment FE	Yes	Yes

Note: Standard errors are clustered at the factory level and reported in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 10: regression output for IT asset shares

The estimated coefficient of interest is positive and close to zero for both the specifications, ranging from 0.0014 to 0.0020, but is statistically insignificant. Thus, there is no statistically significant evidence that the 2014 Rajasthan labour-law reform altered the share of IT assets in firms' capital.

RESULTS

The results are presented in three stages. First, the main adjustment margins are reported (plant intensity, capital intensity, machine intensity, management premia and labour productivity). Second, labour composition through contract and regular worker shares is reported along with IT asset share as an additional technology-related margin. Third, the robustness of these findings are assessed with the help of alternative comparison groups, donut-hole specifications and permutation inference checks.

In Model 1, the coefficient on plant intensity is negative (at -0.217) and statistically significant at the 10% significance level and labour productivity is negative and statistically significant at the 10% level (at −0.163). Capital intensity (−0.121), machine intensity (−0.133), and management premia (−0.056) are all negative but statistically insignificant in this model. As the outcomes are in logs, these coefficients are approximately interpretable as percentage effects. For example, the Model 1 estimate tells us that labour productivity in the treated Rajasthan factories fell by approximately 16% relative to the comparison groups after the reform, though this estimate does not yet control for time-invariant factory heterogeneity (we control for this via factory fixed effects in model 2).

After adding factory fixed effects in Model 2, there is a substantial difference in our results. All the coefficients are now statistically insignificant, and three of the outcomes [plant intensity (from -0.217 to 0.0894), capital intensity (from -0.121 to 0.0889), and machine intensity (from -0.133 to 0.0253)] change sign or move towards zero relative to Model 1. Management premia (changes from -0.056 to −0.0340) remains small and insignificant in both specifications, and it is noted that its sign is consistent across both models. Labour productivity changes from -0.163 to −0.0069 and becomes statistically insignificant.

This pattern suggests that the significant Model 1 estimates for plant intensity and productivity are not robust to controlling for unobserved, time-invariant factory characteristics. These are likely capturing cross-sectional differences between treated and comparison factories rather than a within-factory response to the reform. Under our preferred specification (Model 2), we do not find statistically significant evidence that the 2014 Rajasthan reform has altered capital intensity, plant intensity, machine intensity, management premia, or labour productivity at the factory level.

This null result for capital-related margins is consistent with the theoretical ambiguity mentioned in the introduction. The results from this study suggest that capital and plant intensity and machine intensity were not among the adjustment margins the Rajasthan reform activated, at least using this sample and variables. These estimates indicate that the main observed adjustment following the reform was in the composition of the workforce rather than in the measured capital, productivity, or management-related margins.

The results from Table 9 suggest a reduction in factories' reliance on contract labour along with an increase in the share of regular workers following the reform. This indicates that lower restrictions on employment adjustment may have been associated with a shift in workforce composition toward directly employed workers rather than contract workers. This finding is important as it suggests that the reform may have affected the form in which labour was employed, even in the absence of detectable effects on the other adjustment margins examined.

These results differ from Chaudhary and Sharma (2022), who find greater substitution towards contract workers and away from regular workers. This difference in results does not by itself invalidate either result, as the studies use different empirical designs, samples, and measures of labour composition. The current estimates should therefore be interpreted as evidence from the ASI factory panel and the identification strategy used rather than as establishing a universal direction of labour substitution following the reform.

The paper's introduction frames plant intensity and machine intensity as proxies for technology adoption; IT assets share offers a third test of whether the reform changed factories' digital capital stock. Based on the above results from Table 10, it is observed that there is no evidence the reform shifted factories toward greater IT investment. Taken together with the null results on plant and machine intensity, this reinforces the paper's overall finding that the technology-adoption channel does not appear to have been affected by the reform.

In conclusion, it can be said that: The magnitude of these estimates is economically meaningful and indicates that the main observed adjustment following the reform was in the composition of the workforce rather than in the measured capital, productivity, technology or management-related margins.

Robustness Checks

To assess the robustness of the main results discussed above, there are several additional exercises that have been conducted. They include variation in the comparison groups, the factories included around the regulatory thresholds, and the inference procedure. The variation in comparison groups are described below. The other checks namely the alternate control group, the donut hole tests and the permutation-based inference to assess the sensitivity of statistical significance to the presence of a single treated state have been explained in the appendix. Together, these exercises examine whether the estimated effects are sensitive to the choice of comparison group, observations close to the treatment thresholds, or the conventional inference procedure.

Round 1: Here the sample changes to states with similar GDP to Rajasthan. These include Andhra Pradesh, Chhattisgarh, Jharkhand, Madhya Pradesh and West Bengal (taken from Chaudhary and Sharma, (2022), these states were shortlisted based on average real GDP per capita from 2012 to 2015). Subsequently, this new sample is compared to the treatment, Rajasthan.

Outcome	Coefficient	SE	Obs	Adj R^2
Plant intensity	-0.155	0.135	18,633	0.01754
Capital intensity	0.021	0.106	18,885	0.01745
Machine intensity	**-0.196***	0.107	18,511	0.00909
Management premia	-0.058	0.079	17,625	0.00707
Labor Productivity	-0.081	0.099	17,414	0.2275
Contract worker share	**-0.137*****	0.036	14,605	0.09042
Regular worker share	**0.089*****	0.033	18,228	0.01925
IT assets share	0.001	0.003	17,857	0.0023

Table 11: MODEL 1 with interaction FE only and labour productivity also includes industry x year FE. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

When state × year × industry FE are added to the productivity, the coefficient is −0.13 (0.109) which is also statistically insignificant.

Outcome	Coefficient	SE	Obs	Adj R^2
Plant intensity	0.157	0.098	18,633	0.87635
Capital intensity	**0.181****	0.086	18,885	0.85648
Machine intensity	-0.0002	0.085	18,511	0.85064
Management premia	-0.036	0.077	17,625	0.49293
Labor Productivity	0.026	0.085	17,414	0.7864
Contract worker share	**-0.127*****	0.032	14,605	0.7742
Regular worker share	**0.085*****	0.029	18,228	0.72422
IT assets share	0.001	0.003	17,857	0.44602

Table 12: MODEL 2 with interaction and factory FE and labour productivity also includes industry x year FE. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

When state × year × industry FE are added to the productivity, the coefficient is 0.005 (0.089) which is again, statistically insignificant.

The robustness analysis re-estimates the baseline specifications using a more comparable control group consisting of Andhra Pradesh, Chhattisgarh, Jharkhand, Madhya Pradesh and West Bengal, selected on the basis of similar levels of per-capita income to Rajasthan. These results indicate that the main findings regarding labour composition are fairly robust to this alternative choice of control states.

Most notably, the coefficient on the contract worker share remains negative and statistically significant across both specifications, with estimates of −0.137 and −0.127 respectively. At the same time, the coefficient on the regular worker share remains positive and statistically significant, with estimates of 0.089 and 0.085. Thus, the results consistently indicate a shift away from contract worker share and towards regular worker shares following the reform. The persistence of these effects across specifications suggests that the observed change in labour composition is not driven by the choice of the original control group.

For the other outcomes, the evidence is considerably weaker. Capital intensity is positive and statistically significant only in the specification with factory fixed effects, while it is statistically insignificant in the interaction fixed-effects only specification. Hence, the evidence for an increase in capital intensity is suggestive but not robust across specifications. Management premia, productivity and IT asset share remain statistically insignificant in both specifications, providing no robust evidence of a differential effect of the reform on these outcomes. While machine intensity is statistically significant at the 10% significance level but when factory fixed effects are added, this effect disappears.

Overall, this robustness exercise supports the evidence for a change in the composition of labour following the Rajasthan labour reforms, particularly the decline in contract-worker share and increase in permanent-worker share. In contrast, the results provide limited evidence of systematic changes in capital intensity, mechanisation, management premia, productivity or IT asset intensity.

Round 2: The exercise asks whether the results are sensitive to restricting the comparison group to Rajasthan's neighbouring states. The sample includes Gujarat, Haryana, Madhya Pradesh, Punjab and Uttar Pradesh.

Outcome	Coefficient	SE	Obs	Adj R^2
Plant intensity	**-0.211***	0.123	39,997	0.02460
Capital intensity	-0.071	0.096	40,292	0.02354
Machine intensity	-0.111	0.097	39,147	0.00844
Management premia	-0.042	0.074	38,145	0.00660
Labor Productivity	**-0.162***	0.094	36,913	0.2237
Contract worker share	**-0.123*****	0.034	30,605	0.10996
Regular worker share	**0.090****	0.032	39,161	0.01021
IT assets share	0.001	0.002	38,631	0.00167

Table 13: MODEL 1 with interaction FE; Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.10.

When state × year × industry FE are added to the productivity, the coefficient is -0.159 (0.101) which is statistically insignificant in this model.

Outcome	Coefficient	SE	Obs	R^2
Plant intensity	0.073	0.089	39,997	0.87411
Capital intensity	0.090	0.076	40,292	0.87252
Machine intensity	0.050	0.077	39,147	0.80328
Management premia	-0.022	0.073	38,145	0.49066
Labor Productivity	-0.015	0.078	36,913	0.8160
Contract worker share	**-0.134*****	0.030	30,605	0.79962
Regular worker share	**0.077*****	0.028	39,161	0.76713
IT assets share	0.001	0.002	38,631	0.50209

Table 14: MODEL 2 with interaction and factory FE; Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.10.

When state × year × industry FE are added to the productivity, the coefficient is -0.019 (0.0822) which is statistically insignificant in this model.

The results from Round 2 show that the estimated effect of the reform on contract worker share is negative and statistically significant in both specifications, with coefficients of −0.123 and −0.134. Similarly, the estimated effect on regular worker share is positive and statistically significant in both specifications, with coefficients of 0.090 and 0.077. Thus, across both models, the results consistently indicate a decline in the share of contract workers and an increase in the share of regular workers. For the remaining outcomes, there is no statistically significant effect. Capital intensity, machine intensity, productivity, management premia and IT assets share are statistically insignificant in both specifications. While plant intensity is statistically significant at the 10% level in Model 1, the effect disappears when factory fixed effects are added.

Overall, the most consistent finding from this particular robustness exercise is the negative effect on contract worker share and positive effect on regular worker share. The results from our robustness checks are consistent with our main results and do not show any significant effects on plant intensity, machine intensity, management premia, productivity, or IT assets share. While capital intensity shows a significant increase as per the similar GDP states specification for our preferred model. To summarise, the main results are not entirely dependent on the choice of comparison states as shown above. Other robustness checks are detailed in the appendix and discussed in the conclusion below.

CONCLUSION

This study evaluates the firm-level adjustments in capital intensity, technology adoption, and labour composition following the 2014 Rajasthan reform. Under the preferred fixed-effects specification (Model 2), the empirical estimates do not have a statistically significant impact on capital intensity, plant intensity, machine intensity, management premia, or labour productivity. This empirical null result diverges from established international evidence, such as the Italian benchmark established by Cingano et al. (2016), which documented a capital-deepening effect alongside a decline in total factor productivity. Within standard theoretical frameworks, these findings suggest that there is no evidence that the regulatory reform led to any adjustments in capital intensity, managerial shifts and technology adoption.

Instead, the key margin of firm adjustment operated through workforce composition. Following the policy change, affected factories demonstrated a statistically significant reduction in contract worker share (falling by approx 13 percentage points) alongside an increase in regular worker share (rising by approx 7 percentage points). This rise in the regular worker share contrasts with prior empirical evaluations of the Rajasthan reform. Specifically, Chaudhary and Sharma (2022), and Goswami and Paul (2021) observed an expansion of contract employment paired with a decline in directly employed workers. As mentioned before, this difference may reflect differences in the samples, measures of labour composition and capital, and empirical specifications used across studies.

In Section B of the Appendix, the alternative control group (50–99 workers) is considered. This specification produces a null result pattern across nearly all of the outcomes and for both models. It mirrors the pattern in the preferred DDD specification once factory fixed effects are included for all outcomes except the contract and regular worker shares. The one exception is labour productivity, which becomes significant at the 10% level only in Model 1 with state×year×industry FE, but loses significance again in Model 2. This suggests that this single result is not robust to the inclusion of factory fixed effects and should be treated cautiously rather than as evidence of any real effect. This alternative control group specification does not deviate from the main null results of the study but it also does not support the results for labour composition shares.

Across both donut specifications given in Section C of the Appendix, the contract and regular worker share results are the only outcomes that hold up consistently across Model 1 and Model 2, suggesting this substitution pattern (away from contract labour and toward regular labour) is a more robust finding across control group choices, unlike the level outcomes (plant, capital, machine intensity, and productivity), which lose significance once factory FE are included in both donuts. The labour productivity results, by contrast, are significant only in Model 1 and disappear in Model 2 in both donuts. This indicates that this particular result depends on the absence of factory fixed effects and should not be treated as a stable finding.

In section D of the Appendix, the permutation-based inference provides mixed evidence on the statistical strength of the baseline results (Conley and Taber (2011) was used as reference). For contract-worker share, the estimated negative effect is not statistically significant in the full placebo pool, but becomes significant when the placebo pool is restricted to states with at least 750 or 1,000 observations, with the latter remaining significant in the factory fixed-effects specification. In contrast, the regular-worker-share effect is not statistically significant in the full placebo pool and remains insignificant under the restricted pools as well. Furthermore, the other outcomes do not show consistent evidence of statistically significant effects. These results therefore provide stronger support for the contract-worker-share finding than for the regular-worker-share result under the more conservative permutation-based inference sample. Additionally, for the labour productivity outcome, this check reports a statistically significant result in the without factory fixed effects specification for the restricted pools but this effect disappears when the factory fixed effects are added.

The evidence from this study indicates that easing employment protection legislation (a pro-employer reform) in this context did not appear to induce physical capital deepening, active machine utilization, or digital technology adoption. Rather, lowering firing costs for these medium-sized (100 to 299 worker size) establishments reduced the regulatory risk associated with regular headcount, prompting firms to restructure their human capital toward direct, regular employment rather than contract workers.

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APPENDIX

Section A: Worker Distributions

Year	<50 workers	50–99 workers	100–299 workers	300–999 workers	1,000–5,000 workers	>5,000 workers
2010	24,583	4,407	8,038	3,787	833	52
2011	25,162	4,880	8,389	3,939	927	58
2012	24,827	5,342	8,768	4,190	984	63
2013	27,360	5,661	9,012	4,317	989	65
2014	28,854	5,606	9,335	4,421	1,050	68
2016	28,832	6,763	10,156	4,854	1,145	86
2017	33,515	6,958	10,686	4,977	1,213	88
2018	31,256	7,001	11,301	5,323	1,286	90
2019	29,721	7,215	11,713	5,633	1,352	101
2020	29,687	7,386	11,860	5,664	1,384	106

Table A1. Distribution of factories by employment size

The distribution of factories by employment size was one of the main factors in the choice of the control group in the empirical analysis. The 100–299 worker category is sizable throughout the sample, making it a meaningful group for studying the 2014 reform, which altered the employment threshold from 100 to 300 workers. Factories with 300 or more workers provide a natural comparison group because they remained subject to the stricter firing regulations after the reform, whereas factories with 100–299 workers experienced a change in their regulatory status.

Year	Mean workers	Median workers	Minimum workers	Maximum workers	No. of factories
2010	146	29	0	44,266	41,700
2011	151	31	0	43,300	43,355
2012	158	34	0	46,814	44,174
2013	149	30	0	45,749	47,404
2014	150	29	0	54,236	49,334
2016	157	35	0	65,463	51,836
2017	148	29	0	70,100	57,437

2018	159	34	0	71,587	56,257
2019	169	40	0	69,398	55,735
2020	171	41	0	65,166	56,087

Table A2. Mean, median and range of factory employment by year for the whole sample

Year	Mean workers	Median workers	Minimum workers	Maximum workers	No. of factories
2010	410	231	0	21,538	9,169
2011	418	232	0	30,068	10,327
2012	424	232	0	23,800	11,459
2013	436	225	1	45,749	13,490
2014	442	226	100	54,236	14,874
2016	461	234	0	65,463	13,413
2017	472	236	0	70,100	12,952
2018	493	246	0	71,587	12,398
2019	520	259	0	69,398	11,797
2020	534	262	0	65,166	11,278

Table A3. Mean, median and range of factory employment by year, using 2014 as the baseline employment level

A possible robustness check is constructed below using factories with 50–99 workers, which are closer in employment size to the treated factories and therefore potentially more comparable along dimensions related to firm size. However, these factories were already below the regulatory threshold before the reform and did not experience the same change in regulatory status. Additionally, according to the literature as well, the 300+ group is chosen as the primary control group as it provides the more direct counterfactual to the 100–299 treated factories. The 50–99 group represents a potentially useful alternative comparison, but is not used as the main specification (in this study). The below tables show how using the 50-99 factory size group as the control has impacted the study.

SECTION B: Alternative Control Group (50-99 workers)

This section answers if the main results are affected by a particular employment threshold that has been used for the control group.

Outcome	Coefficient (SE)	Obs	Adj R^2
Plant intensity	-0.2311 (0.1515)	101,701	0.00792
Capital intensity	-0.0591 (0.1242)	103,182	0.01205
Machine intensity	-0.0953 (0.1116)	99,947	0.0065
Management premia	0.0540 (0.0690)	94,133	0.0044
Labor Productivity	−0.1302 (0.0963)	92,418	0.2301
Contract worker share	0.0102 (0.0377)	73,292	0.14807
Regular worker share	0.0195 (0.0350)	98,267	0.01475
IT assets share	-0.0025 (0.0037)	95,111	0.00214

Table B1: MODEL 1 with interaction FE only and labour productivity also includes industry x year FE. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Across this alternative control group specification (treatment = 100–299 workers, control = 50–99 workers), none of the eight outcomes show a statistically significant result at conventional levels (10%, 5%, or 1%), as seen in Table B1. Coefficients are mixed in sign and small relative to their standard errors, with plant intensity showing the largest point estimate (-0.2311) and labour productivity the second largest (-0.1302), though are statistically insignificant. When state × year × industry fixed effects are added to the labour productivity specification, the coefficient becomes -0.157 (0.095), which is significant at the 10% level.

Outcome	Coefficient (SE)	Obs	Adj R^2
Plant intensity	0.0186 (0.0901)	101,701	0.86859
Capital intensity	0.0104 (0.0743)	103,182	0.89074
Machine intensity	-0.0718 (0.0753)	99,947	0.87688
Management premia	0.0439 (0.0650)	94,133	0.53174
Labor Productivity	−0.0789 (0.0786)	92,418	0.8454
Contract worker share	-0.0525 (0.0372)	73,292	0.81539
Regular worker share	0.0340 (0.0321)	98,267	0.77021
IT assets share	0.0006 (0.0035)	95,111	0.52983

Table B2: MODEL 2 with interaction and factory FE and labour productivity also includes industry x year FE. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In Table B2, factory fixed effects are added, but all eight core outcomes continue to be statistically insignificant. Point estimates are smaller in magnitude than in Table B1 (e.g., plant intensity moves from -0.2311 to 0.0186; contract worker share moves from 0.0102 to -0.0525), and adjusted R^2 values rise substantially across all outcomes (e.g., plant intensity from 0.008 to 0.869), which is consistent with factory fixed effects absorbing most of the cross-sectional variation. When state × year × industry fixed effects are added to the labour productivity specification here, the coefficient is -0.035 (0.076), which remains insignificant, and this is directly in contrast to the corresponding Model 1 result.

SECTION C: Donut Specifications

As an additional robustness check, factories close to the employment thresholds are used to define treatment and control groups. This addresses the possibility of self-selection around the regulatory thresholds, i.e., factories might have incentives to remain just below a threshold in order to avoid employment-protection requirements, or to adjust employment to move across a threshold following the reform. To reduce the influence of such factories, a donut-hole specification is implemented. The restricted sample compares treated establishments with 100–299 baseline workers to smaller control factories of 50–90 workers and larger control factories of 320 and more workers. This exercise answers the question if factories close to the legal thresholds drive the results.

Donut A (Control = 320+)

Model 1: No Factory FE			
Outcome	**Coefficient (SE)**	**Obs**	**Adj R^2**
Plant intensity	**-0.2236* (0.1218)**	115,355	0.02253
Capital intensity	-0.1282 (0.0935)	116,646	0.02407
Machine intensity	-0.1388 (0.0956)	113,578	0.00891
Management premia	-0.0634 (0.0715)	109,745	0.00171
Labor Productivity (Industry×Year FE)	**-0.1752* (0.0925)**	106,971	0.20997
Labor Productivity (Industry×State×Year FE)	**-0.1900* (0.1007)**	106,971	0.41591
Contract worker share	**-0.1067*** (0.0338)**	87,077	0.11086
Regular worker share	**0.0898*** (0.0313)**	112,971	0.01344
IT assets share	0.0020 (0.0023)	108,971	0.00153
Model 2: Factory FE			
Outcome	**Coefficient (SE)**	**Obs**	**Adj R^2**
Plant intensity	0.0809 (0.0877)	115,355	0.87751
Capital intensity	0.0734 (0.0716)	116,646	0.89437
Machine intensity	0.0198 (0.0747)	113,578	0.86254
Management premia	-0.0422 (0.0699)	109,745	0.50802
Labor Productivity (Industry×Year FE)	-0.0099 (0.0776)	106,971	0.85225
Labor Productivity (Industry×State×Year FE)	-0.0472 (0.0822)	106,971	0.86586
Contract worker share	**-0.1275*** (0.0295)**	87,077	0.81108
Regular worker share	**0.0721** (0.0277)**	112,971	0.78097
IT assets share	0.0014 (0.0023)	108,971	0.51574

Table C1: MODEL 1 and 2, model 1 with only interaction FE and model 2 with interaction and factory FE when the control group is large sized factories (320+). Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In Model 1 (interaction FE only), plant intensity and labour productivity (both industry×year and industry×state×year FE) are significant at the 10% level, with negative coefficients of -0.2236, -0.1752, and -0.1900 respectively. Capital intensity, machine intensity, management premia, and IT assets share are all statistically insignificant. Contract worker share (-0.1067) and regular worker share (0.0898) are both significant at the 1% level, with contract share falling by approx 11 percentage points and regular share rising by approx 9 percentage points.

In Model 2 (factory FE added), all outcomes except contract and regular worker share become insignificant (also, plant, capital, and machine intensity coefficients shrink substantially and flip toward zero or positive). Contract worker share remains significant at the 1% level (-0.1275) and regular worker share remains significant, now at the 5% level (0.0721), both similar in sign and magnitude to Model 1.

Results from the preferred specification, Model 2 in Donut A, support the results from the main outcomes mentioned in the study.

Donut B (Control: 50-90)

Model 1: No Factory FE			
Outcome	**Coefficient (SE)**	**Obs**	**Adj R^2**
Plant intensity	-0.2485 (0.1594)	95,871	0.00716
Capital intensity	-0.1084 (0.1265)	97,211	0.01212
Machine intensity	-0.1943 (0.1185)	94,228	0.006
Management premia	0.0457 (0.0714)	88,956	0.00442
Labor Productivity (Industry×Year FE)	**-0.1819* (0.1009)**	87,150	0.22644
Labor Productivity (Industry×State×Year FE)	**-0.1842* (0.1007)**	87,150	0.4303
Contract worker share	0.0104 (0.0388)	68,978	0.14952
Regular worker share	0.0326 (0.0352)	92,727	0.01556
IT assets share	-0.0025 (0.0041)	89,787	0.00236
Model 2: Factory FE			
Outcome	**Coefficient (SE)**	**Obs**	**Adj R^2**
Plant intensity	0.0383 (0.0935)	95,871	0.86647
Capital intensity	0.0325 (0.0767)	97,211	0.88828
Machine intensity	-0.0937 (0.0774)	94,228	0.87492
Management premia	0.0415 (0.0663)	88,956	0.52984
Labor Productivity (Industry×Year FE)	-0.1062 (0.0822)	87,150	0.86136
Labor Productivity (Industry×State×Year FE)	-0.0649 (0.0812)	87,150	0.87679
Contract worker share	**-0.0761** (0.0364)**	68,978	0.81476
Regular worker share	**0.0525* (0.0317)**	92,727	0.77037
IT assets share	0.0013 (0.0038)	89,787	0.52898

Table C2: MODEL 1 and 2, model 1 with only interaction FE and model 2 with interaction and factory FE when the control group is small sized factories (50-90). Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

From Table C2, in Model 1, only the two labour productivity specifications are significant, both at the 10% level (-0.1819 with industry×year FE; -0.1842 with industry×state×year FE). Plant intensity, capital intensity, machine intensity, management premia, contract worker share, regular worker share, and IT assets share are all statistically insignificant. While in Model 2, both labour productivity specifications lose significance entirely, while contract worker share becomes significant at the 5% level (-0.0761), and regular worker share becomes significant at the 10% level (0.0525).

Results from the preferred specification, Model 2 in Donut B, also support the results from the main outcomes mentioned in the study, while Model 1 in both Donut A and B only partially does so.

SECTION D: Placebo/Permutation Inference with Inverse Testing

Standard difference-in-differences inference relies on asymptotic approximations that assume the number of policy-changing units grows large. In this study, only one state (Rajasthan) changed policy status, so conventional factory-clustered standard errors understate the true sampling uncertainty (Conley and Taber, 2011).

The authors show that when the number of treated groups N_1 is small and fixed, the treatment effect estimator remains unbiased but is no longer consistent, since the noise term does not disappear as the number of control groups grows; standard errors computed under the usual large N_1 asymptotics are invalid. Their proposed solution is to use the empirical distribution of coefficients from the (large) pool of untreated control groups to approximate the finite-sample distribution of the treatment effect estimator under the null, and to use this distribution to conduct hypothesis tests and construct confidence intervals.

This study implements a permutation-based analog of this idea. Each control state is, in turn, treated as if it had received the policy change, generating an empirical placebo distribution against which Rajasthan's actual coefficient can be judged. Where the actual coefficient falls within this placebo distribution provides a more credible basis for inference than clustered standard errors when $N_1 = 1$, i.e., Rajasthan is the only treated state.

The 750 and 1000 observations pool was chosen as it was found that a handful of states (13, 14, 4, 17) had very few observations (48, 51, 115, and 188 respectively) compared to a typical state in the data (roughly 1,000–19,000 observations). These thin states show the most extreme values in the placebo distribution across the multiple outcomes and are extremely noisy. As the states need enough observations to reliably estimate a coefficient across both the pre- and post-treatment years, 750 and 1000 were chosen as a reasonably large sample on both sides.

Outcome	Actual Coeff	p-val (Full N)	90% CI, Full	p-val (N≥750)	90% CI, Pool 1	p-val (N≥1000)	90% CI, Pool 2
Plant Intensity	−0.2166	0.323 (31)	[−0.5365, 0.5562]	0.208 (24)	[−0.4568, 0.4489]	0.227 (22)	[−0.4610, 0.4894]
Capital intensity	−0.1211	0.581 (31)	[−0.3674, 0.4055]	0.583 (24)	[−0.3323, 0.1538]	0.545 (22)	[−0.3337, 0.1140]
Machine intensity	−0.1331	0.613 (31)	[−0.3473, 0.7026]	0.500 (24)	[−0.3638, 0.4012]	0.455 (22)	[−0.3234, 0.4418]
Management Premia	−0.0561	0.548 (31)	[−0.3792, 0.1689]	0.458 (24)	[−0.3509, 0.1582]	0.409 (22)	[−0.3640, 0.1060]
Labour Productivity	−0.1634	0.226 (31)	[−0.4430, 0.2417]	0.083 (24)	[−0.3250, −0.0073]	0.091 (22)	[−0.3219, −0.0067]
Contract Share	−0.1101	0.133 (30)	[−0.2416, 0.0590]	0.043 (23)	[−0.2077, −0.0381]	0.048 (21)	[−0.2096, −0.0375]
Regular share	0.0944	0.258 (31)	[−0.1629, 0.3036]	0.125 (24)	[−0.0029, 0.1764]	0.136 (22)	[−0.0051, 0.1822]
IT share	0.002	0.645 (31)	[−0.0017, 0.0142]	0.583 (24)	[−0.0015, 0.0133]	0.545 (22)	[−0.0015, 0.0134]

Table D1: Placebo tests without factory FE

From Table D1, across the Full placebo distribution (31 (and 30) state sample), no outcome is significant at conventional levels. Labor productivity (p=0.226) and contract share (p=0.133) are close but remain above the 10% significance level.

Restricting to Pool 1 (N ≥ 750, 24 (and 23) states) and Pool 2 (N ≥ 1000, 22 (and 21) states) changes this: labour productivity becomes significant at 10% in both (p=0.083 and p=0.091), with 90% CIs that exclude zero ([-0.3250, -0.0073] and [-0.3219, -0.0067]). Contract share follows the same pattern, becoming significant at 5% in both Pool 1 and Pool 2 (p=0.043 and p=0.048), with 90% CIs that also exclude zero ([-0.2077, -0.0381] and [-0.2096, -0.0375])

Contract share and labour productivity already had the two lowest p-values among all outcomes in the full placebo pool (0.133 and 0.226, respectively), so restricting the pool sharpened inference for the two outcomes that already showed the strongest evidence of an effect once thinly-sampled placebo states were removed. All other outcomes (plant intensity, capital intensity, machine intensity, management premia, permanent share, and IT share) remain insignificant across all three placebo pools, with 90% CIs that include zero throughout.

Outcome	Actual Coeff	p-val (Full N)	90% CI, Full	p-val (N≥750)	90% CI, Pool 1	p-val (N≥1000)	90% CI, Pool 2
Plant Intensity	0.0894	0.710 (31)	[−0.1805, 0.7721]	0.667 (24)	[−0.1243, 0.6629]	0.636 (22)	[−0.1266, 0.6700]
Capital Intensity	0.0889	0.613 (31)	[−0.1424, 0.9374]	0.542 (24)	[−0.0957, 0.3871]	0.545 (22)	[−0.0966, 0.4033]
Machine Intensity	0.0253	0.903 (31)	[−0.2465, 0.4760]	0.958 (24)	[−0.2788, 0.1943]	0.955 (22)	[−0.1976, 0.1979]

Management Premia	−0.0340	0.645 (31)	[−0.3182, 0.3465]	0.542 (24)	[−0.2609, 0.2836]	0.500 (22)	[−0.2752, 0.1466]
Labour Productivity	−0.0069	1.000 (31)	[−0.2117, 0.2425]	1.000 (24)	[−0.1974, 0.1591]	1.000 (22)	[−0.1877, 0.1586]
Contract Share	−0.1278	0.200 (30)	[−0.3652, 0.0300]	0.087 (23)	[−0.2213, 0.0053]	0.048 (21)	[−0.2280, −0.0366]
Regular Share	0.0739	0.258 (31)	[−0.0601, 0.3629]	0.125 (24)	[0.0111, 0.1936]	0.136 (22)	[0.0155, 0.1956]
IT share	0.0014	0.806 (31)	[−0.0019, 0.0153]	0.792 (24)	[−0.0016, 0.0149]	0.773 (22)	[−0.0017, 0.0154]

Table D2: Placebo tests with factory FE

In Table D2, when factory FE are included, labour productivity is now statistically insignificant across all three pools (p = 1 for all three). Contract share follows a pattern of strengthening significance as the sample is restricted: insignificant at Full N (p=0.200), significant at 10% in Pool 1 (p = 0.087), and significant at 5% in Pool 2 (p = 0.048). It is important to note here that the Pool 2 CI ([-0.2280, -0.0366]) is the only one of the three that excludes zero. All remaining outcomes (plant intensity, capital intensity, machine intensity, management premia, permanent share, and IT share) are insignificant across all three pools by the p-value criteria.

For permanent share, the two-sided p-values (0.258, 0.125, 0.136) do not cross the 10% threshold, while the corresponding 90% confidence intervals in the restricted pools exclude zero. This divergence arises as the p-value is computed by comparing absolute deviations, implicitly treating the placebo distribution as symmetric around zero, whereas the confidence interval is constructed from the empirical quantiles of the placebo distribution and does not impose symmetry.

Since the placebo distribution for permanent share is right-skewed (with a longer upper tail, as several placebo states produced unusually large positive coefficients) the two-sided p-value, which folds both tails together by absolute value, is inflated by these large-magnitude values even though they lie in the same direction as the actual coefficient. The confidence interval, by contrast, uses the signed quantiles directly and is less affected by this asymmetry. As a result, the two statistics can disagree near the margin of significance. This is illustrated in the 1000-observation, factory-FE specification, where Rajasthan's coefficient exceeds every one of the 22 placebo coefficients, yet the two-sided p-value remains 0.136 because the placebo pool contains a large negative outlier (min = −0.339) whose absolute magnitude matches Rajasthan's positive deviation, despite pointing in the opposite direction.