Counting Carbon, Losing Productivity: The Unintended Operational Costs of Mandatory Carbon Disclosure

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Abstract

We examine the operational consequences of mandatory carbon disclosure (MCD) using the United Kingdom's 2013 carbon disclosure regulation as a quasi-natural experiment. We exploit differential exposure between manufacturing firms listed on the London Stock Exchange and comparable European-listed firms; and estimate difference-in-differences, event-study, and synthetic difference-in-differences models to identify causal effects on productivity-related outcomes. Our results show statistically and economically significant declines across a structured, multi-dimensional productivity system including total factor productivity, operational productivity, labour productivity, and inventory efficiency, following disclosure. Event studies show that effects emerge shortly after implementation, persist for several years, and propagate across interdependent production efficiency, labour deployment, and inventory execution. Consistent with an attention-based view, mandatory disclosure acts as a regulatory attention shock, shifting managerial focus and operational resources toward compliance and governance and away from productivity-enhancing investments. Effects are heterogeneous: productivity declines are more pronounced among firms with longer investment horizons and higher market valuations, consistent with strategic resource reallocation rather than reporting costs. Our findings show that MCD-based environmental regulation, often assumed operationally neutral, can create unintended productivity trade-offs. The study provides causal evidence that informational regulation reshapes firm operations and highlights the need for transparency-based policies to account for Unintended operational spillovers.

Keywords: Mandatory Carbon Disclosure, Productivity, Attention-based View, Quasi-natural Experiment

1 Introduction

Mandatory carbon disclosure has become a widely adopted regulatory instrument, premised on the idea that transparency can improve environmental outcomes without directly constraining firms' operations. Because such mandates do not prescribe production technologies, processes, or output levels, they are commonly assumed to be operationally neutral. Early firm disclosures following the UK's 2013 regulation, however, suggest that this assumption may be overly simplistic. Rolls-Royce plc, a major UK manufacturer, reported expanding sustainability reporting teams and investing in new emissions data systems, reallocating managerial attention and internal resources away from operational improvement (Rolls-Royce Holdings plc 2014). Similarly, BAE Systems and GKN Aerospace highlighted challenges in collecting and validating carbon data across complex production networks, with early disclosures pointing to higher administrative overheads and increases in working capital tied up in inventories during the initial compliance period (BAE Systems plc 2014). Industry bodies such as the Confederation of British Industries (CBI) and Make UK further warned that the regulation's rapid roll-out, which allowed only one reporting cycle for compliance, would impose substantial coordination and reporting costs on manufacturing firms (Make UK 2015). Taken together, these accounts suggest that even disclosure-based regulation can operate as an operational attention shock, with spillovers across production, labour deployment, and inventory management. Motivated by this tension, we examine productivity not as a single construct, but as a set of interrelated firm-level and operational outcomes through which mandatory carbon disclosure reshapes production efficiency, labour utilisation, and inventory execution.

Prior research has extensively examined environmental regulation and disclosure in relation to environmental outcomes, disclosure quality, financial performance, and capital market responses (e.g., Ilhan et al., 2023; Lee & Kaul, 2025; Jouvenot & Krueger, 2019). Related work in operations and economics studied productivity effects of regulation, but this evidence overwhelmingly concerns policies that directly constrain technologies, inputs, or output such as emissions caps, taxes, or technology standards (e.g., Lee & Kaul, 2025; Garicano et al., 2016; Wang & Wei, 2025). In contrast, mandatory climate disclosure mandates are typically treated as informational instruments and therefore presumed to be operationally neutral. As a result, we know little about whether and through what mechanisms mandatory carbon disclosure propagates through firms' operational systems and productivity-related outcomes. Existing studies rarely examine how disclosure affects internal resource allocation or operational decision-making, nor do they distinguish among different dimensions of productivity. Treating productivity as a single firm-level metric hinders potential spillovers across production efficiency, labour allocation, inventory execution, and overall firm-level outcomes. This leaves an important gap in understanding whether mandatory disclosure-based regulation can generate unintended operational trade-offs, even in the absence of direct technological constraints.

We address this gap by asking a focused but under-explored question: Does mandatory carbon disclosure causally affect firms' productivity-related operational outcomes, and through which operational channels do these effects arise? We exploit the United Kingdom's 2013 Mandatory Carbon Disclosure regulation as a quasi-natural experiment. The regulation imposed uniform disclosure requirements on UK's London Stock Exchange (LSE) listed public firms, while comparable manufacturing firms listed on European Economic Area (EEA) exchanges were not subject to the mandate, creating a clean treatment–control contrast (Lee & Kaul, 2025; Jouvenot & Krueger, 2019). We leverage this institutional setting and implement a differences-in-differences design using firm-level panel data for manufacturing firms over 2006–2019, comparing productivity trajectories of treated UK firms to matched non-treated EEA firms before and after the regulation. To improve identification, we implement coarsened exact matching on key pre-regulation firm attributes, estimate firm- and year-fixed-effects models (Iacus et al., 2012; Lee & Kaul, 2025; Schmidt & Raman, 2022). We also implemented the synthetic diff-in-diff (SDiD) to identify average treatment effects and to validate the parallel trends assumption by creating a weighted average of non-treated (EEA) firms that better resemble the LSE firms in the pre-regulation period (Arkhangelsky et al., 2021; Clarke et al., 2024; Lee & Kaul, 2025). Furthermore, we employ event-study and heterogeneity analyses to examine the timing, persistence, and cross-firm variation of these effects, providing insights on both the mechanisms and dynamics of operational adjustment. Importantly, we conceptualise productivity not as a single metric but as a multi-dimensional construct including total factor productivity, operational productivity, labour productivity, and inventory efficiency. This allows us to trace how a disclosure-induced regulatory attention shock propagates through firms' production systems, labour utilisation, inventory execution and overall firm related outcome.

Our analysis reveals that the introduction of mandatory carbon disclosure led to a statistically and economically meaningful deterioration in the productive performance of UK manufacturing firms subject to the regulation. We find consistent declines across multiple dimensions of productivity, including total factor productivity, operational productivity, and inventory efficiency, with effects emerging shortly after the regulation's introduction and persisting for several years. These productivity declines are driven primarily by reductions in output and value added rather than employment, indicating that firms absorbed the regulatory shock through lower operational efficiency rather than workforce contraction. Inventory performance also deteriorated, with slower inventory turnover and longer inventory holding periods, suggesting increased operational frictions in production and supply chain execution. While some effects attenuate over time, we find little evidence of a full recovery within the six-year post-regulation window. These patterns are robust across alternative productivity measures, estimation approaches, and extensive robustness checks, and are strongest in the early post-regulation years. Taken together, the findings suggest that mandatory carbon disclosure, though informational in intent, can generate sustained operational trade-offs that propagate through firms' operational systems.

This study makes three contributions. Substantively, we provide one of the first causal evidence that mandatory environmental disclosure—designed as an informational policy instrument—can materially affect manufacturing firms' productivity-related outcomes. Exploiting the UK's 2013 Mandatory Carbon Disclosure regulation, we move beyond financial and environmental metrics to examine how disclosure reshapes total factor productivity, operational productivity, labour utilisation, and inventory efficiency, revealing persistent operational trade-offs that remain hidden when productivity is treated as a single construct. Our findings highlight the unintended costs of disclosure mandates, suggesting that while such policies enhance transparency, they may also impose operational frictions that weaken firm's productivity-related outcomes. Regulators should therefore balance climate goals with operational realities, while managers must design compliance strategies that mitigate productivity losses without compromising long-term environmental commitments. Theoretically, we extend the attention-based view of the firm (Ocasio, 1997; Joseph & Wilson, 2018; Joseph et al., 2024) by showing how regulatory attention shocks propagate beyond managerial cognition into operational execution, reallocating resources across production, labour, and inventory systems in ways that generate heterogeneous responses across firms with different asset structures and investment horizons. Methodologically, we combine a quasi-natural experiment with multi-dimensional productivity measurement, event-study analysis, and synthetic difference-in-differences estimation, demonstrating a rigorous approach for identifying how disclosure-based regulation translates into firm-level operational performance effects.

2 Institutional background, mechanisms, and productivity outcomes

2.1 Institutional background and regulatory context

The UK's 2013 mandatory carbon disclosure regulation, which became effective on the 1st of October 2013 under the official title "Companies Act 2006 (Strategic Report and Directors' Report) Regulations 2013", is the first regulatory regime globally that requires mandatory greenhouse gas (GHG) emissions reporting by firms listed on the London Stock Exchange (LSE).[1] Under this regime, LSE firms are obliged to include in their annual directors' reports their Scope 1 emissions, Scope 2 emissions, and energy consumption in the corresponding financial year. Scope 1 emissions are the direct emissions of firms' own entities in the UK and abroad, while Scope 2 emissions are indirect emissions from purchased energy. Indirect emissions through firms' upstream and downstream linkages constitute a third category, Scope 3, but firms are not obliged by the regulation to report these.[2]

Crucially for identification, the regulation imposed uniform disclosure obligations on all affected firms, independent of their prior environmental performance, emissions intensity, or operational structure. Firms were not provided with financial compensation, carbon allowances, or flexibility mechanisms, and the regulation did not directly constrain production technologies, output levels, or input choices.

In their reporting, firms are obliged to provide detailed information about the data and methodology used. In particular, they must report the quality of data they rely on, the use of estimates or assumptions about emissions, the implementation of a recognised methodology for the calculation of emission-related metrics (e.g. GHG Protocol, ISO 14064), and how metrics are calculated (e.g. emission intensity calculated as the ratio of emissions per unit of activity, employee, or sales). Albeit optional, firms can also have the quality of their data

[1]Mandatory carbon disclosure reporting has also become an LSE listing requirement since the regulation came into effect. UK-incorporated firms listed on other stock exchanges, such as those of European Economic Area (EEA) countries and the New York Stock Exchange (NYSE), are also subject to the regulation, but constitute only a tiny fraction of the total number of firms.

[2]For more details on Scopes 1, 2, and 3, see the Greenhouse Gas (GHG) Protocol, which is one of the most common frameworks used by firms for the quantification and management of their GHG emissions and carbon footprint.

independently verified. In addition, this regulation obliges firms to report information in a prescriptive and standardised manner. These practices enhance transparency and facilitate comparisons by investors, analysts, and researchers of metrics over time for each firm but also across firms. Importantly, the costs of data collection, elaboration, and verification, methodology implementation, and report preparation are incurred by firms themselves. This design makes the regulation plausibly exogenous to firm-level operational decisions while remaining operationally salient through its compliance burden.

In this light, the regulation under study differs from previous regulations requiring the disclosure of non-financial information but not in a mandatory, prescriptive, and standardised way (Grewal et al., 2019) and regulations aiming at the allocation of carbon allowances across firms, provision of financial compensation to firms, or the creation of carbon pricing mechanisms (e.g. Lee & Kaul (2025) on California's Cap-and-Trade programme). The MCD goes beyond earlier non-financial disclosure or climate risk disclosure (Flammer et al., 2021) requirements by mandating standardized, auditable, and comparable emissions information, thereby increasing both internal coordination demands and external scrutiny. Importantly, the regulation was rolled out with limited transition time, requiring firms to comply within a single reporting cycle, which intensified short-run adjustment pressures.

We focus on manufacturing firms for both conceptual and empirical reasons. Manufacturing firms are among the most emissions-intensive sectors and thus faced substantial reporting and coordination demands under the regulation. More importantly, manufacturing settings allow us to observe multiple, well-defined margins of operational adjustment—production efficiency, labour deployment, and inventory management—that are central to productivity analysis. Restricting the sample to manufacturing firms enables the consistent estimation of operational productivity using Data Envelopment Analysis (DEA), which relies on tangible inputs such as labour, capital assets, and inventory, as well as the analysis of inventory efficiency outcomes that are not meaningful or comparable in service-based sectors. This focus strengthens internal validity by aligning the regulatory shock with observable operational responses, while ensuring that productivity-related outcomes are measured in a conceptually coherent and empirically consistent manner.

While the MCD regulation does not directly constrain production technologies or input choices, its compliance requirements introduce a salient organizational demand that may reallocate managerial attention and operational resources, an issue that we examine next by conceptualizing mandatory disclosure as an attention and resource allocation shock.

2.2 Regulation as an attention and resource allocation shock

We conceptualise mandatory carbon disclosure as an exogenous attention and resource allocation shock that re-configures managerial priorities and reshapes operational decision-making. Under the attention-based view of the firm (ABV), organisational actions reflect what decision-makers attend to, given limited cognitive and managerial capacity (Ocasio, 1997; Brielmaier & Friesl, 2023). Regulatory interventions that elevate new issues into managerial focus can therefore alter firm behaviour even in the absence of direct constraints on production technology, inputs, or demand.

Mandatory disclosure requirements systematically redirect attention toward compliance-related activities including but not limited to emissions measurement, data validation, reporting coordination, and governance oversight. These activities impose information-processing and coordination demands, which compete with ongoing operational priorities. Within the ABV, this reallocation operates through three reinforcing mechanisms linking attention shifts to productivity outcomes (Brielmaier & Friesl, 2023).

First, focus of attention at the individual level implies that managerial effort devoted to disclosure compliance shifts attention available for operational problem-solving. Time spent managing data, assurance, and regulatory scrutiny reduces cognitive bandwidth for decisions related to process optimisation, workforce deployment, and inventory coordination. Because firm-level productivity aggregates from individual actions, such micro-level reallocations can translate into observable changes in operational productivity, labour productivity, and inventory efficiency. Second, situated attention implies that disclosure regulation reshapes the contexts in which decisions are made. Carbon reporting introduces new temporal rhythms (e.g., reporting cycles), procedural routines (e.g., verification processes), and spatial interactions (e.g., coordination across plants and headquarters). These situational features elevate environmental metrics as salient decision criteria while rendering operational trade-offs less immediately visible, without requiring managerial intent to deprioritise productivity.

Third, structural distribution of attention at the organisational level implies that firms reallocate resources to support compliance. Investments in monitoring systems, reporting teams, assurance, and governance divert financial and human resources away from productivity-enhancing initiatives, particularly in the short run. For manufacturing firms, where productivity depends on tight coordination across production, labour, and inventory systems, such reallocations can disrupt routines and degrade execution efficiency even when technology and demand remain unchanged.

These effects arise because disclosure regulation is informational rather than prescriptive. Unlike technology or input regulations, disclosure mandates do not constrain how firms produce; instead, they impose attention and coordination burdens that absorb managerial time and organisational slack. As a result, productivity impacts operate through attention displacement and resource reallocation rather than factor substitution or capital vintage effects.

The productivity consequences of disclosure are also not expected to be to be uniform. Firms with higher capital intensity or longer investment horizons face greater coordination complexity and exposure to compliance-related attention shifts. Firms with higher market valuations may experience heightened external scrutiny, which might further amplify these effects. Over time, however, firms may internalise reporting routines, automate compliance processes, and reallocate attention back toward operations, implying dynamic adjustment rather than permanent productivity loss.

Taken together, the attention-based view suggests three testable predictions regarding how mandatory carbon disclosure affetcs firms' multidimensional productivity outcomes.

Hypotheses

H1 (Average Effect). Mandatory carbon disclosure reduces operational productivity, labour productivity, and inventory efficiency relative to comparable non-treated firms in the post-regulation period.

H2 (Heterogeneity). The negative productivity effects of mandatory carbon disclosure are more pronounced for firms with longer investment horizons, higher capital intensity, or higher market valuations.

H3 (Dynamics). The productivity effects of mandatory carbon disclosure are strongest in the early post-regulation years and attenuate over time as firms adapt to compliance requirements.

We test these hypotheses using a differences-in-differences framework comparing treated UK manufacturing firms subject to mandatory disclosure with matched European firms not subject to the regulation. Firm and year fixed effects identify within-firm changes relative to common shocks. To examine H2, we interact treatment exposure with firm characteristics capturing investment horizons, capital intensity, and market valuation. To test H3, we employ event-study specifications tracing productivity dynamics before and after the regulation.

2.3 Productivity as a multi-dimensional operational outcome

We conceptualise productivity not as a single metric, but as a system of interrelated operational outcomes that jointly reflect how firms transform inputs into outputs under changing constraints. In complex manufacturing settings, productivity emerges from multiple margins of operational adjustment including production efficiency, execution quality, labour deployment, and inventory coordination, each of which is governed by distinct processes and decision rules. Therefore, regulatory shocks that redirect managerial attention or operational resources are unlikely to affect all productivity dimensions uniformly.

At the firm level, total factor productivity (Olley & Pakes (1996); Levinsohn & Petrin (2003); Ackerberg et al. (2015)) captures changes in underlying production efficiency, reflecting how effectively labour, capital, and intermediate inputs are combined. TFP is sensitive to disruptions in process optimisation, learning-by-doing, and technology utilisation, and thus provides a benchmark measure of firm-wide efficiency. However, TFP alone may mask operational frictions that arise within specific functional domains.

To capture these frictions, we examine operational productivity (**?**) using Data Envelopment Analysis (DEA), which reflects execution efficiency given observable operational inputs including labour, tangible capital, and inventory. DEA-based productivity is particularly sensitive to short- to medium-run disruptions in operational coordination, scheduling, and workflow management, making it well-suited to detect compliance-related execution inefficiencies that may not immediately translate into changes in production technology.

We further consider labour productivity, which isolates the deployment and utilisation of human resources. Regulatory disclosure may alter managerial attention and employee time allocation toward reporting, verification, and coordination tasks, potentially reducing effective labour input devoted to production even if total employment remains unchanged. Labour productivity thus captures an important channel through which attention reallocation propagates into operational outcomes.

Finally, we examine inventory efficiency, measured through inventory turnover and average inventory days, to capture coordination and buffering responses. Inventory decisions often absorb shocks arising from upstream disruptions, reporting delays, or coordination failures, serving as a residual adjustment margin when production and labour cannot be flexibly re-optimised. Changes in inventory performance therefore reflect spillovers from regulatory attention shocks into supply chain execution and working capital management.

Together, these measures allow us to trace how mandatory carbon disclosure propagates through distinct but interconnected operational systems. A regulatory attention shock may first manifest in execution inefficiencies (DEA productivity), spill over into labour deployment and inventory coordination, and only subsequently affect firm-wide production efficiency (TFP). By jointly analysing these dimensions, we provide a more complete and operationally grounded assessment of how informational regulation reshapes productivity-related outcomes.

3 Data and variables

To empirically identify the effects of the UK's 2013 Carbon Disclosure Regulation (CDR) on productivity and other firm performance measures, we collect firm-level data from the LSEG database (previously named as Eikon). Relying on this database is appropriate for our empirical analysis for the following reasons. Firstly, it includes information for firms listed on stock exchange markets of numerous advanced countries, including the London Stock Exchange (LSE) in the UK and stock exchanges of other countries in the European Economic Area (EEA), which constitute the treatment and control groups, respectively, in the difference-in-difference (diff-in-diff) estimation strategy that we implement. Secondly, the detailed information available allows us to create measures of total factor productivity, labour productivity, and operational productivity, as well as other firm performance measures, such as the return on invested capital and inventory amount and days. Thirdly, the detailed information available allows us to consider throughout the econometric analysis a sufficiently high and roughly equal number of years before and after the regulation came into effect.

The treatment group comprises firms listed on the LSE, as those are the only ones that have been subjected to the regulation. To form the control group, we consider firms whose shares are also publicly traded but in stock exchange markets of EEA countries where the regulation has not applied, namely, Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Netherlands, Norway, Portugal, Spain, Sweden, and Switzerland. Although UK-based private firms could also serve as the control group, we opt for using firms listed on EEA stock exchanges for two main reasons. Firstly, publicly-listed EEA firms are, on average, closer to LSE firms in terms of key characteristics—most notably, scale of operations—than UK-based private firms. Secondly, UK-based private firms are more likely to be indirectly affected by the regulation than EEA firms through spillovers from the LSE firms. A similar approach has been made by other pertinent studies. For instance, within the context of California's Cap-and-Trade programme, Lee & Kaul (2025) have included Californian production facilities subjected to the regulation in the treatment group and similar facilities outside of California in the control group. The authors have also shown that their main results are robust when the control group includes instead only non-treated Californian facilities.[3]

As the productivity and other firm performance measures that we consider are more relevant or more precisely calculated for firms in manufacturing, we restrict the sample to firms whose primary industry is in this sector. That is, we keep only firms with primary industry codes 31–33 according to the 2017 version of North American Industry Classification System (NAICS). Besides, it is firms of this sector that are one of the main targets of the regulation, if not the primary one. In addition, we keep only firms that trade ordinary shares and operate in the main markets of the LSE and the EEA stock exchanges under consideration. For some EEA countries in which there is more than one stock exchange, identified by unique codes, we keep only the main one. Germany is the only country for which we keep two codes (XETR, XFRA), corresponding to the Frankfurt-based trading centres "Xetra" and "Frankfurt Stock Exchange". As the applicability of the regulation is conditional on whether firms are listed on the LSE or not, we also keep in the sample only LSE and EEA firms that were publicly listed in each year examined.

Our sample period starts in 2006 and ends in 2019. Although the bulk of information that we use in the analysis is available for more years, both before 2006 and after 2019, with this choice over the benchmark sample period, we consider (almost) balanced pre- and post-regulation periods in terms of the number of years covered in each, while excluding years in which the performance of the average firm has likely been adversely affected by the COVID-19 pandemic outbreak and the ensuing restrictive measures (e.g. social distancing, lockdowns) for its containment. Inevitably, given the proximity of the start date of the regulation to

[3] In addition to selecting firms for the control group that bear a closer resemblance to LSE firms in terms of various key characteristics, we implement econometric methods that ensure the close similarity between treated and non-treated firms (e.g. synthetic diff-in-diff, coarsened exact matching). Also, note that we refer throughout the paper to public and private firms not to distinguish between those in the public and private sector, but rather, between firms that are listed on stock exchanges and those that are not.

the global financial crisis of 2007–2009, the latter years are included in the sample period, but this should not be a concern, as we control for such shocks and their implications for productivity—and firm performance more broadly—with appropriate fixed effects throughout the econometric analysis. Regarding the lengths of the pre- and post-treatment periods, it it important to note that these vary slightly across LSE firms and according to their fiscal year ends. That is, firms whose fiscal year ends on the 30th of September were obliged to start to abide by the regulation in 2013, while firms whose fiscal year ends after that date were obliged to abide by it by 2014.

Table 1: Stock exchanges on which firms are listed

Stock exchange		Treatment		Control		Total	
Country	Code	No.	%	No.	%	No.	%
United Kingdom	XLON	48	100	0	0	48	13.9
Austria	WBAH	0	0	11	3.7	11	3.2
Belgium	XBRU	0	0	17	5.7	17	4.9
Denmark	XCSE	0	0	16	5.4	16	4.6
Finland	XHEL	0	0	21	7	21	6.1
France	XPAR	0	0	47	15.8	47	13.6
Germany	XETR	0	0	46	15.4	46	13.3
Germany	XFRA	0	0	11	3.7	11	3.2
Ireland	XDUB	0	0	2	0.7	2	0.6
Italy	MTAA	0	0	17	5.7	17	4.9
Netherlands	XAMS	0	0	14	4.7	14	4
Norway	XOSL	0	0	11	3.7	11	3.2
Portugal	XLIS	0	0	1	0.3	1	0.3
Spain	XMAD	0	0	12	4	12	3.5
Sweden	XSTO	0	0	29	9.7	29	8.4
Switzerland	XSWX	0	0	43	14.4	43	12.4
Total		48	100	298	100	346	100

Source: LSEG database. The statistics have been produced on the original (pre-matched) sample.

As a final step in the refinement of the sample, we eliminate a relatively small number of four-digit (NAICS) industries in which there exist non-treated firms but not treated ones. The final sample comprises 346 firms in total, of which 48 are listed on the LSE and 298 on EEA stock exchanges (Table 1). The countries in which they are listed largely coincide with the countries of their headquarters (Table A1). A relatively small fraction of EEA firms became publicly listed in the late 19th and early 20th centuries and a relatively small fraction of firms were listed on the LSE between the 1930s and 1970s, but the bulk of treated and non-treated firms obtained public status as of the mid-1980s (Table A2). Also, while industry coverage is quite broad, there are relatively high fractions of treated and non-treated firms in

Table 2: Principal (three-digit NAICS) industries of firms

Industry		Treatment		Control		Total	
NAICS code	Name	No.	%	No.	%	No.	%
311	Food Manufacturing	3	6.2	15	5.0	18	5.2
312	Beverage and Tobacco Product Manufacturing	2	4.2	15	5.0	17	4.9
313	Textile Mills	1	2.1	1	0.3	2	0.6
323	Printing and Related Support Activities	1	2.1	2	0.7	3	0.9
324	Petroleum and Coal Products Manufacturing	1	2.1	1	0.3	2	0.6
325	Chemical Manufacturing	7	14.6	71	23.8	78	22.5
326	Plastics and Rubber Products Manufacturing	2	4.2	9	3.0	11	3.2
327	Nonmetallic Mineral Product Manufacturing	1	2.1	6	2.0	7	2.0
331	Primary Metal Manufacturing	2	4.2	10	3.4	12	3.5
332	Fabricated Metal Product Manufacturing	4	8.3	11	3.7	15	4.3
333	Machinery Manufacturing	6	12.5	61	20.5	67	19.4
334	Computer and Electronic Product Manufacturing	10	20.8	58	19.5	68	19.7
335	Electrical Equipment, Appliance, and Component Manufacturing	3	6.2	16	5.4	19	5.5
336	Transportation Equipment Manufacturing	3	6.2	6	2.0	9	2.6
337	Furniture and Related Product Manufacturing	1	2.1	2	0.7	3	0.9
339	Miscellaneous Manufacturing	1	2.1	14	4.7	15	4.3
	Total	48	100.0	298	100.0	346	100.0

Source: LSEG database. The statistics have been produced on the original (pre-matched) sample.

computer and electronics, chemicals, and machinery manufacturing industries, and relatively low fractions of treated and non-treated firms in petroleum and coal product manufacturing, textile manufacturing, and printing and related support activities (Table 2).

In the main analysis, we consider creating primarily measures of total factor productivity (TFP), labour productivity (LP), and operational productivity (OP). We also consider using variables capturing additional aspects of firm performance, namely: the return on invested capital (ROIC), amount of inventory, inventory turnover, and average number of inventory days. Information on the last four variables is readily available in the LSEG database. ROIC is calculated as the ratio of net operating profit after tax to total capital investment and reflects the effectiveness of a firm's allocation of funds to productive investments. The amount of inventory is essentially the value of the inventory in British pound sterling (GBP). All other monetary variables that we employ in the analysis are also in GBP. Inventory turnover is calculated as the ratio of the cost of goods sold to the average inventory value (inventory amount) and signifies the frequency at which a firm sells and replaces its stock of goods. A higher (lower) inventory turnover implies that a firm has stronger (weaker) sales and/or more (less) efficient inventory management. Similarly, the average number of inventory days captures the duration a good is stored by a firm before it is sold. The higher (lower) this number is, the stronger (weaker) its sales are and/or more (less) efficient its inventory management is.

To obtain TFP measures, we implement the standard methods developed by Olley & Pakes (1996) and Levinsohn & Petrin (2003). Both methods involve two-step estimations of production functions while tackling the possible simultaneity bias (i.e., firms' input choices based on their productivity levels) and selection bias (i.e., firm entry and exit determined by productivity levels). The key difference, however, between the two methods is that the first proxies for the unobserved productivity of a firm by its decisions over capital investment, while the second by its choices over intermediate inputs (e.g. materials). Hence, we implement the two methods by estimating log of value added specifications with 50 bootstrap repetitions using the log of capital expenditure and log inventory amount as measures of capital investment and intermediate inputs, respectively. In both methods, we also use the log of full-time employment (number of employees) and log of the net stock of property, plants and equipment as measures of the amounts of labour and capital employed by firms. Information on value added is not readily available in the database and we thus calculate it by subtracting a firm's inventory amount (proxy for intermediate inputs) from its total sales. As labour and the proxy for capital investment or intermediate inputs are likely to be determined by the same productivity shock, we also implement the method proposed by Ackerberg et al. (2015) to correct for possible biases in the TFP measures obtained from the first two methods.[4] We then create the log of TFP measures based on the different methods by calculating the residual values from the estimation of the relevant log of value added specifications. Regarding LP, we calculate it as the ratio of a firm's total sales or value added to its full-time employment.

To obtain OP measures, we implement the input- and output-oriented versions of the Data Envelopment Analysis (DEA) method considering variable returns to scale. In contrast to constant returns to scale implying optimal scale of a firms' operations, variable returns to scale imply that a firm does not operate at optimal scale for various reasons. The latter is a more realistic assumption and thus preferred to the former one (Huguenin, 2013; Ji & Lee, 2010). Under the input-oriented approach, a firm aims at minimising inputs (e.g. labour, capital, materials) to maintain a certain level of output, while under the output-oriented approach, a firm aims at maximising output by utilising certain amounts of inputs. We use the log of total sales as a measure of a firm's output and the logs of full-time employment (number of employees), net tangible capital stock (property, plants and equipment), and inventory amount as measures of the amounts of labour, capital, and materials that a firm

[4]For robustness check purposes, we also use TFP measures that we obtain by implementing the methods developed by Wooldridge (2009), Robinson-Wooldridge, and Rovigatti & Mollisi (2018).

employs to produce output.[5] As firms in different industries, even when they pertain to the same sector (e.g. manufacturing), may have different production processes, we implement the DEA method by firms' primary four-digit (NAICS) industry.[6]

The method yields an efficiency score for each firm, ranging between 0 and 1, with higher (lower) values indicating that a firm is closer to (further from) the efficiency frontier. Hence, although the main advantage of the DEA method over TFP estimation is that it is non-parametric and thus requires no assumptions about production functions, the efficiency scores that it produces do not convey information about firms' actual productivity levels, but rather, their position relative to the efficiency frontier. On top of that, in a diff-in-diff research design like ours, a treatment like the carbon disclosure regulation may affect not only the position of firms relative to the efficiency frontier, but also the frontier itself. Therefore, although the effects that we identify on OP are consistent with those on TFP, LP, and the other firm performance measures, we are more cautious about their interpretation.

In a supplementary analysis, we employ a broad set of measures of firms' profitability and income, namely: the return on assets (ROA), operating profit margin, earnings before interest and taxes (EBIT), earnings before interest, taxes, depreciation, and amortisation (EBITDA), and pre- and after-tax incomes. A higher (lower) ROA indicates that a firm makes more (less) efficient use of its assets to generate profits, while a higher (lower) operating profit margin, calculated as the ratio of operating profit to total revenue, indicates that a firm has a greater (smaller) capacity to convert revenue into operating profit and thus higher (lower) operational efficiency. The interpretations for the other four measures are straightforward.

To prevent from introducing a bias in the empirical analysis due to possible outlier values of the main and additional variables, we winsorise them all by replacing their values below the 1st percentile and above the 99th percentile with the respective percentile values. Summary statistics for these variables produced on the unmatched sample are shown in Table 3, with the last two columns showing differences in mean values of the variables between treated and non-treated firms and the statistical significance (p-value) of these differences. Treated and non-treated firms differ in most dimensions that we consider. Treated firms have higher mean

[5] The implementation of DEA requires the ex-ante elimination of missing information on input and output variables.

[6] Although it would be feasible, we abstract from implementing the method for more disaggregated industries (e.g. five- or six-digit NAICS), as it would violate the sample size requirement for DEA to produce reliable efficiency scores. According to this requirement, the number of firm-year combinations per industry in our sample must be three times as large as the total number of input and output variables (4) that are used to implement the method (Huguenin, 2013).

and lower standard deviation values for most of the variables, and most differences in mean values are statistically significant. The summary statistics are very similar when the variables are not winsorised. As we will explain in detail in the next section, results produced on the unmatched sample will serve only as a robustness check; the main results will be produced after implementing sophisticated methods for the matching between firms in the treatment and control groups.

4 Econometric methodology

Taking advantage of the quasi-experimental setting generated by the introduction of the carbon disclosure regulation in the UK, we estimate the following diff-in-diff specification to identify the causal effects of the regulation on productivity and other firm performance measures:

$$FP_{ft} = \alpha + \beta_f \cdot D_f + \beta_t \cdot D_t + \beta_{CDR} \cdot CDR_t + \beta_{LSE,CDR} \cdot LSE_f \cdot CDR_t + \epsilon_{ft}. \tag{1}$$

Considering that the pre- and post-regulation periods vary across firms, this is in fact a staggered diff-in-diff research design (Athey & Imbens, 2017; Ilhan et al., 2023; **?**), where the identification of causal effects stems from comparisons of productivity between treated and non-treated firms (first difference) and between the (slightly different) periods before and after the regulation came into effect (second difference). The dependent variable, FP_{ct}, is one of the main measures of performance of firm f in year t, namely: TFP, labour productivity, OP, ROIC, the log of the inventory amount, inventory turnover, or the log of the average number of inventory days. In a supplementary analysis, we consider using as the dependent variable a profitability or income measure, namely: ROA, operating profit margin, log of EBIT or EBTIDA, or log of pre- or after-tax income.

The specification includes firm fixed effects, D_f, and year fixed effects, D_t. With the incorporation of the first set of fixed effects, we control for time-invariant unobserved heterogeneity across firms such as differences at each point in time in relative factor endowments and patterns of specialisation, levels of technological sophistication, organisational structure, management practices, and business culture. With the incorporation of the second set of fixed

Table 3: Summary statistics for main and additional variables

Variable	Firm group	No.	Mean	St. Dev.	Min	Max	Mean difference Value	Mean difference P-value
Log of TFP, OP	Treatment	647	9.412	0.474	6.723	10.719	-0.012	0.652
	Control	3791	9.424	0.640	4.409	12.237		
Log of TFP, OP with ACF	Treatment	647	9.165	0.464	6.629	10.500	-0.042	0.109
	Control	3791	9.207	0.634	4.333	11.987		
Log of TFP, LP	Treatment	647	11.025	0.582	7.770	12.391	0.096	0.002
	Control	3791	10.929	0.735	5.414	13.814		
Log of TFP, LP with ACF	Treatment	647	9.148	0.463	6.630	10.488	-0.046	0.080
	Control	3791	9.193	0.634	4.338	11.969		
Log of TFP, Wooldridge	Treatment	647	11.158	0.588	7.881	12.551	0.100	0.001
	Control	3791	11.058	0.741	5.526	13.942		
Log of TFP, Wooldridge-Robinson	Treatment	647	11.191	0.592	7.896	12.597	0.103	0.001
	Control	3791	11.088	0.746	5.539	13.975		
Log of TFP, Mollisi-Rovigatti	Treatment	647	11.127	0.590	7.839	12.522	0.102	0.001
	Control	3791	11.025	0.744	5.480	13.914		
Log ratio of sales to employment	Treatment	658	11.942	0.681	7.285	13.642	-0.106	0.000
	Control	3926	12.048	0.671	6.799	14.610		
Log ratio of value added to employment	Treatment	647	11.823	0.565	9.097	13.478	-0.014	0.603
	Control	3792	11.837	0.668	6.869	14.505		
Log of sales	Treatment	671	20.185	1.847	12.328	24.360	0.758	0.000
	Control	4166	19.427	2.227	12.328	24.360		
Log of value added	Treatment	660	20.125	1.651	13.797	24.272	0.778	0.000
	Control	3985	19.347	2.106	10.190	24.254		
Log of full-time employment	Treatment	659	8.262	1.516	3.611	11.838	0.727	0.000
	Control	3926	7.534	1.921	2.773	11.838		
Operational productivity (input-oriented)	Treatment	646	0.967	0.032	0.867	1	0.006	0.000
	Control	3862	0.961	0.036	0.729	1		
Operational productivity (output-oriented)	Treatment	646	0.978	0.022	0.884	1	0.005	0.000
	Control	3862	0.973	0.028	0.685	1		
ROIC	Treatment	643	0.508	1.966	-9.299	17.667	0.034	0.929
	Control	3508	0.474	9.688	-300.691	311.891		
Log of inventory amount	Treatment	660	18.265	1.693	11.785	22.608	0.495	0.000
	Control	4071	17.770	2.167	11.785	22.608		
Inventory turnover	Treatment	626	5.249	4.134	0.815	19.635	1.447	0.000
	Control	4012	3.802	2.897	0.249	19.635		
Log avg. # of inventory days	Treatment	626	4.461	0.634	2.857	6.108	-0.339	0.000
	Control	4009	4.800	0.732	2.857	7.239		
ROA	Treatment	667	0.001	0.001	-0.006	0.003	0.000	0.000
	Control	4155	0.000	0.001	-0.006	0.003		
Operating profit margin	Treatment	669	-0.000	0.008	-0.062	0.003	0.000	0.133
	Control	4139	-0.001	0.008	-0.062	0.003		
Log of EBIT	Treatment	640	18.152	1.792	12.612	22.543	0.815	0.000
	Control	3552	17.337	2.159	7.485	22.543		
Log of EBITDA	Treatment	648	18.474	1.773	13.225	22.897	0.794	0.000
	Control	3778	17.680	2.095	7.621	22.897		
Log of pre-tax income	Treatment	595	17.974	1.817	12.314	22.383	0.767	0.000
	Control	3385	17.207	2.183	7.569	22.383		
Log of after-tax income	Treatment	591	17.696	1.847	10.968	22.090	0.765	0.000
	Control	3369	16.931	2.190	9.028	22.090		
Tobin's Q	Treatment	670	4.231	3.604	0.977	48.637	-8.475	0.000
	Control	4147	12.706	52.598	0.087	1107.756		

Notes: The penultimate column displays the differences in the mean values of the variables between the treatment and control groups. The final column displays the statistical significance (p-value) of the differences in the mean values based on the relevant t-tests. Small differences in the number of observations of the treatment and control groups across variables are due to the existence of missing values. The statistics have been produced on the original (pre-matched) sample.
Source: LSEG database. The statistics have been produced on the original (pre-matched) sample.

effects, we control for time-varying shocks common to all firms, such as the global financial crisis of 2007–2009 and its consequences (e.g. restricted access to credit for business and households, contraction in aggregate outputs, incomes, and demand).

CDR_t is a dummy variable indicating the years after the mandatory carbon disclosure regulation came into effect (2013 – 2019 or 2014 – 2019) and the years before (2006 – 2012 or 2006–2013). Of particular interest to us is the interaction between this variable and the dummy variable, LSE_f, indicating whether a firm is listed on the LSE and thus pertains to the treatment group, or listed on one of EEA stock exchanges that we consider and thus pertains to the control group: $LSE_f \cdot CDR_t$. The estimation of the coefficient of the interaction term, $\beta_{LSE,CDR}$, captures the average treatment effect, that is, the causal effect of the regulation on the performance of the treated (LSE) firms. Note that the dummy variable indicating whether a firm pertains to the treatment or control group does not enter the specification individually as it is captured by the firm fixed effects.

To estimate Eq. 1, we rely primarily on the synthetic diff-in-diff (SDiD) estimator (Arkhangelsky et al., 2021; Clarke et al., 2024; Lee & Kaul, 2025) and, alternatively, on the simple diff-in-diff (DiD) estimator. For statistical inference in the SDiD method, we use bootstrap, while in DiD estimations, we cluster standard errors by stock exchange market to account for correlated shocks to firms listed on the same stock exchange. The main reason for relying primarily on the SDiD method is that it is better suited to guarantee the validity of the critical parallel trends assumption by considering a weighted average of EEA firms whose pre-regulation performance resembles more closely that of the LSE firms, thereby indicating a more accurate performance trajectory on which LSE firms would have been in the post-regulation period had the regulation not taken place. On top of that, similarly to the DiD estimator, the SDiD estimator guarantees that the estimates are insensitive to unobserved time-varying shocks to firms and thus unbiased, and that statistical inference of the estimates is valid. Also, in contrast to traditional synthetic control methods aiming at the minimisation of differences in pre-regulation productivity levels (Abadie, 2021), the SDiD estimator aims at minimising differences in pre-regulation productivity trends. In this way, we deal with concerns over a possible bias in the estimates due to the generation of an imperfect pre-regulation trend of productivity or the average treatment effect being confounded with other productivity drivers in the post-regulation period (Ferman & Pinto, 2021). Equally important is the fact that the SDiD method chooses firm and year weights itself for the creation of the counterfactual based on the actual values of the productivity measures and, therefore, requires no (subjective) choices to be made over which EEA firms would form the synthetic control group or over covariates (Arkhangelsky et al., 2021).

Unlike the SDiD that generates the counterfactual itself from the data in a way that ensures that the parallel trends assumption holds, the DiD method requires us to create as close as possible matches between treated and non-treated firms beforehand. To this end, we emulate pertinent studies in the literature in implementing coarsened exact matching (CEM) based on firms' total value of fixed assets and Tobin's Q (Iacus et al., 2012; Lee & Kaul, 2025; Schmidt & Raman, 2022). Information on the first variable is readily available in the LSEG database, while using information on additional variables from the same source, we calculate Tobin's Q as the ratio of the sum of a firm's market capitalisation and liabilities to the sum of its common equity and liabilities. Given that the treatment and control groups comprise publicly-listed firms and that we consider a large set of productivity, other firm performance, and firm profitability and income measures in the main and supplementary analyses, we deem the use of these two variables for matching purposes as appropriate, while at the same time, we ensure that we do not use in the matching procedure any of the variables on which we aim at identifying the causal effects of the regulation (Blackwell et al., 2009).

Considering how challenging a task is to match treated and non-treated firms on exact values of variables, especially continuous ones, the underlying idea of the CEM method is to coarsen the two selected variables into broader—yet substantively meaningful—groups, implement one-to-one matching between treated and non-treated firms based on these groups, and then retain the original (i.e., uncoarsened) values of the selected variables for the matched firms.[7] During the matching process, the method assigns zero weights to firms that are not matched and re-weights matched treated and non-treated firms of each broader group, so that each group contributes equally to the analysis, irrespective of the number of treated and non-treated firms that it consists of. After the completion of the matching process, we estimate weighed versions of Eq. 1 using the weights obtained from the CEM procedure.

In addition to the identification of average treatment effects, we are interested in identifying the year-by-year change in performance of LSE firms in the post-regulation period and checking whether the critical assumption about the existence of parallel trends in performance of LSE and EEA firms in the pre-regulation period holds. To this end, we also consider estimating the event-study version of Eq. 1:

[7] To prevent from making subjective choices over how the selected variables will be coarsened into broader groups, we do not do this task manually, but rather, allow the method to do it automatically based on the Sturge's rule.

$$FP_{ct} = \alpha + \beta_c \cdot D_c + \beta_t \cdot D_t + \sum_{\tau=2006}^{2019} \beta_{LSE,\tau} \cdot LSE_c \cdot D_t^\tau + \epsilon_{ct}. \quad (2)$$

The dummy variable, D_t^τ, is equal to 1 when $\tau = t$, and 0 otherwise. Estimating the coefficients of the interaction between this variable and LSE_c, which is defined as above, yields the annual effects on productivity in the pre- and post-regulation periods.

Similarly to Eq. 1, we estimate Eq. 2 implementing the SDiD and DiD methods.[8] In addition to these methods, we implement the imputation estimator that has been developed by Borusyak et al. (2024) and is more appropriate than the DiD method in a staggered diff-in-diff research design like ours, although, for reasons explained above, we again deem the SDiD as the most appropriate method. Before implementing this estimator, we match treated and non-treated firms by implementing the CEM method. The underlying idea of the imputation estimator is to estimate firm and year fixed effects among non-treated firms, impute the values of performance measures of non-treated firms for treated ones, and then produce efficient and unbiased estimates as weighted averages over the differences between actual values and imputed values of performance measures of treated firms.

5 Econometric results

In this section, we present and discuss the average and annual effects of the regulation that we identify from the estimation of Eqs. 1 and 2, respectively. We first focus on the effects on productivity and other firm performance measures, as these have been described in Section 3. In a supplementary analysis, we present and discuss the effects on firm profitability and income measures, which we have also described in that section. Then, we perform a battery of checks on the robustness of the results of the main and supplementary analyses.

[8]The Stata command "sdid_event" allows us to implement not only the first method, but also the second one. Hence, unlike the estimations aiming at the identification of average treatment effects, we implement the DiD method without previously implementing the CEM method.

5.1 Effects of the regulation on productivity and other firm performance measures

Table 4 displays the average treatment effects on TFP that we identify implementing the SDiD (Panel A) and DiD (Panel B) methods. The TFP measures in columns (1) and (3) have been created based on the Olley & Pakes (1996) and Levinsohn & Petrin (2003) methods, respectively, and those in columns (2) and (4) have been created based on the Ackerberg et al. (2015) method to correct for possible bias in the TFP estimates of the former two methods. The SDiD estimates in all four columns of Panel A are negative, but only that in column (3) is statistically significant, and only at 10%. Similarly, the DiD estimates in all four columns of Panel B are negatively signed. They have, however, higher statistical significance (5%). Results of the two panels, especially the second one, suggest that the regulation had a negative impact on the LSE firms' TFP.

Comparisons of the SDiD and DiD estimates in terms of size do not yield a clear pattern: The SDiD estimate is larger in column (3), smaller in columns (2) and (4), and roughly equal to the DiD estimate in column (1). To gain a better understanding of the magnitudes of the average treatment effects, we first calculate the means of the TFP measures among the treated (LSE) firms in the pre-regulation period and then divide the SDiD and DiD estimates by the respective pre-treatment means on the treated and multiply the ratios by 100 to calculate the percentage changes of the TFP measures in the post-regulation period. According to these statistics, shown at the bottom of Panels A and B, the average decline in TFP of LSE firms due to the regulation relative to the pre-regulation period ranged from 0.52% (Levinshon-Petrin, DiD) to 0.68% (Olley-Pakes with ACF correction, DiD).

Figure 1 displays the event-study results obtained from the SDiD (Panels (a), (c), (e), and (g)) and DiD (Panels (b), (d), (f), and (h)) methods, while Figure 2 displays those obtained from the Borusyak-Jaravel-Spiess (BJS) imputation method. Results are consistent with the average treatment effects and yield additional valuable insights. First of all, the three methods—especially SDiD—confirm that the critical assumption of parallel trends holds. In other words, there is very little or no evidence of pre-trends. As for the post-regulation period, although all panels in the two figures show TFP gains for the LSE firms in the year when the regulation came into effect, their TFP declined quite precipitously over the next two to four years, depending on the TFP measure used and the method implemented, and started recovering—yet only partially—thereafter. Again, the magnitudes of the gradual mitigation

Table 4: Average post-regulation effects on TFP

Panel A: Synthetic diff-in-diff (SDiD) method

	(1)	(2)	(3)	(4)
Dep. var: TFP	Olley-Pakes		Levinshon-Petrin	
	Original	ACF correction	Original	ACF correction
LSE companies * Post-regulation	-0.059	-0.056	-0.069*	-0.054
	[0.05]	[0.03]	[0.04]	[0.04]
Observations	3654	3654	3654	3654
Pre-treatment mean of outcome on treated	9.793	9.313	11.84	8.727
Post-treatment % ch. of outcome	-0.607	-0.598	-0.581	-0.621

Panel B: Simple diff-in-diff (DiD) method

	(1)	(2)	(3)	(4)
Dep. var: TFP	Olley-Pakes		Levinshon-Petrin	
	Original	ACF correction	Original	ACF correction
Post-regulation	-0.031	-0.026	-0.040*	-0.027
	[0.02]	[0.02]	[0.02]	[0.02]
LSE companies * Post-regulation	-0.059**	-0.059**	-0.059**	-0.058**
	[0.03]	[0.03]	[0.03]	[0.03]
Observations	4221	4221	4221	4221
R^2	0.757	0.740	0.838	0.742
Pre-treatment mean of outcome on treated	9.562	8.609	11.48	9.398
Post-treatment % ch. of outcome	-0.618	-0.681	-0.515	-0.615

Source: SDiD and DiD estimations in Panels A and B, respectively. DiD estimations have been made on matched samples based on coarsened exacting matching. The specifications in all columns of the two panels include firm fixed effects and year fixed effects. Asterisks denote significance at 1% (***), 5% (**), and 10% (*), based on bootstrapped standard errors (SDiD) or standard errors clustered by stock exchange market (DiD).

of the negative effects on the LSE firms' TFP vary by TFP measure and estimation method. Importantly, the negative effects on the LSE firms' TFP are mostly statistically significant in the first two to four years after the regulation came into effect and statistically insignificant in later years, according to the SDiD and DiD estimates, while they are statistically significant throughout the post-regulation period, according to the BJS estimates.

Having identified the average and annual effects of the regulation on the LSE firms' TFP, it is also interesting to look into the effects on their labour productivity (LP), whose construction relies, by definition, on a single factor of production (labour). As we calculate LP ourselves as the ratio of sales or value added to full-time employment, this exercise also allows us to look into the effects on each of its components. Table 5 shows the results obtained based on the SDiD (Panel A) and DiD (Panel B) methods. In each panel, the dependent variable is the log of the ratio of sales or value added to full-time employment (columns (1) and (2)), the log of sales (column (3)), the log of value added (column (4)), or the log of full-time employment (column (5)). The SDiD and DiD estimates in columns (1), (2), and (5) of the two panels are negative but statistically insignificant, suggesting that the regulation exerted no significant average effects on the LSE firms' LP and employment. Although the DiD estimates in columns (3) and (4) of Panel B are also statistically insignificant, the respective SDiD estimates in Panel A are negative and significant at 5% and 1%, respectively, suggesting that the regulation impacted negatively the LSE firms' sales and value added. The average decline of the two variables relative to the pre-regulation period was 0.56% (see relevant statistics at the bottom of panel).

Table 5: Average post-regulation effects on labour productivity and its components

Panel A: Synthetic diff-in-diff (SDiD) method	(1) Log of labour productivity	(2) Log of labour productivity	(3) Log of total revenue	(4) Log of total value added	(5) Log of full-time employment
LSE companies * Post-regulation	-0.051	-0.052	-0.11**	-0.11***	-0.054
	[0.04]	[0.04]	[0.05]	[0.04]	[0.05]
Observations	3906	3668	3668	3668	3668
Pre-treatment mean of outcome on treated	11.87	11.79	20.30	20.13	8.347
Post-treatment % ch. of outcome	-0.427	-0.443	-0.561	-0.563	-0.642

Panel B: Simple diff-in-diff (DiD) method	(1) Log of labour productivity	(2) Log of labour productivity	(3) Log of total revenue	(4) Log of total value added	(5) Log of full-time employment
Post-regulation	0.018	-0.034	-0.062*	-0.10***	-0.064*
	[0.03]	[0.03]	[0.03]	[0.03]	[0.04]
LSE companies * Post-regulation	-0.018	-0.039	0.019	-0.019	-0.013
	[0.03]	[0.03]	[0.05]	[0.04]	[0.04]
Observations	4362	4222	4608	4427	4363
R^2	0.794	0.775	0.966	0.964	0.976
Pre-treatment mean of outcome on treated	11.88	11.80	20.07	20.07	8.220
Post-treatment % ch. of outcome	-0.148	-0.333	0.0936	-0.0954	-0.163

Source: SDiD and DiD estimations in Panels A and B, respectively. DiD estimations have been made on matched samples based on coarsened exacting matching. The specifications in all columns of the two panels include firm fixed effects and year fixed effects. Asterisks denote significance at 1% (***), 5% (**), and 10% (*), based on bootstrapped standard errors (SDiD) or standard errors clustered by stock exchange market (DiD).

To better understand these results, we conduct an event-study analysis. Figure 3 shows the results obtained from the SDiD method. Results obtained from the DiD and Borusyak-Jaravel-Spiess (BJS) imputation methods are largely similar and we thus relegate them to the Appendix to save on space (Figures B1 and B2). The first important remark on these results is that none of the panels in the three figures raises concerns over the existence of pre-trends. Secondly, the annual effects on LP, sales, and value added in the post-regulation period bear a close resemblance to those on TFP. In particular, although the LSE firms experienced LP, sales, and value added gains in the year when the regulation came into effect and, in some cases, also smaller ones in the year after, all three measures started declining already from the start year of the regulation, leading to LP, sales, and value added losses for the LSE firms as of the first or second year of the post-regulation period. After its precipitous decline over the first three years of the regulation, LP moved rather erratically, as it recovered only partially until the fifth year, before declining again in the sixth year (Panels (a) and (b) of Figures 3, B1, and B2). Sales and value added started recovering only partially after the fourth and fifth year, respectively, according to the SDiD and DiD estimates (Panels (c) and (d) of Figures 3 and B1), while, according to the BJS estimates, they continued trending downwards even after the fourth year (Panels (c) and (d) of Figure B2).

In contrast to the productivity and output measures, the SDiD and DiD estimates point to no employment gains for the LSE firms at the start year of the regulation (Panel (e) of Figures 3 and B1). The BJS estimates also point to no employment gains in early post-regulation years, as, albeit positive, they are statistically insignificant (Panel (e) of Figure B2). Also in contrast to the productivity and output measures, employment remained almost unchanged (SDiD and DiD results) or fell only slightly (BJS results) until the second year of the post-regulation period. In fact, employment declines occurred mostly as of that year, they were less pronounced and reversed slightly after the fifth year according to the SDiD and DiD estimates, while they became more significant over the years according to the BJS estimates.

In sum, although the average effects of the regulation on the LP measures are statistically insignificant, the event-study analysis reveals some LP losses for the LSE firms that are concentrated mostly in the first two to four years after the regulation came into effect. Importantly, LP losses were driven primarily by output declines, especially when output is measured by value added. Although there is some evidence of employment losses for the LSE firms, these did not occur immediately, but rather, after the passage of three years since the regulation came into effect. This suggests that the LSE firms responded to the introduction of the regulation primarily by descaling production, rather than shedding employment, especially if they perceived this shock as transitory. However, the lag in employment declines for the LSE firms may also reflect labour market rigidities (e.g., labour laws prohibiting mass lay-offs or non-negligible costs associated with lay-offs).

In addition to TFP and LP, we consider using another firm-level productivity measure—namely, operational productivity—whose advantages and disadvantages relative to the other measures we have discussed in Section 3. Columns (1) and (2) of Table 6 portray the average treatment effects on the input- and output-oriented measures, respectively, of operational productivity based on the SDiD (Panel A) and DiD (Panel B) methods. Note that the

Table 6: Average post-regulation effects on operational productivity and other firm performance measures

Panel A: Synthetic diff-in-diff (SDiD) method						
	(1)	(2)	(3)	(4)	(5)	(6)
	Operational productivity		ROIC	Log of total	Inventory	Log of avg. # of
	Input-oriented	Output-oriented		inventory	turnover	inventory days
LSE companies * Post-regulation	-0.0041**	-0.0032**	-0.18	0.0092	-0.36*	0.091***
	[0.002]	[0.002]	[0.8]	[0.04]	[0.2]	[0.03]
Observations	2044	2044	2170	2170	2044	2044
Pre-treatment mean of outcome on treated	0.966	0.977	0.655	18.05	4.737	4.505
Post-treatment % ch. of outcome	-0.428	-0.328	-28.13	0.0508	-7.582	2.029

Panel B: Simple diff-in-diff (DiD) method						
	(1)	(2)	(3)	(4)	(5)	(6)
	Operational productivity		ROIC	Log of total	Inventory	Log of avg. # of
	Input-oriented	Output-oriented		inventory	turnover	inventory days
Post-regulation	-0.00027	0.00017	0.24	-0.026	0.0028	-0.017
	[0.001]	[0.0010]	[0.3]	[0.04]	[0.07]	[0.02]
LSE companies * Post-regulation	-0.0021	-0.0021	-1.23	0.021	-0.37***	0.080**
	[0.002]	[0.001]	[0.7]	[0.05]	[0.1]	[0.03]
Observations	4291	4291	3942	4513	4420	4417
R^2	0.797	0.733	0.181	0.965	0.823	0.840
Pre-treatment mean of outcome on treated	0.968	0.978	0.534	18.18	5.479	4.415
Post-treatment % ch. of outcome	-0.216	-0.211	-229.7	0.117	-6.793	1.811

Source: SDiD and DiD estimations in Panels A and B, respectively. DiD estimations have been made on matched samples based on coarsened exacting matching. The specifications in all columns of the two panels include firm fixed effects and year fixed effects. Asterisks denote significance at 1% (***), 5% (**), and 10% (*), based on bootstrapped standard errors (SDiD) or standard errors clustered by stock exchange market (DiD).

number of observations in the columns of this table is smaller than in the previous table showing effects on TFP because of higher fractions of missing values for these additional firm performance measures. Although the DiD estimates are negative and statistically insignificant, the SDiD estimates have the same sign but are significant at 5%. Hence, in line with the average treatment effects on TFP, the regulation led to declines in the LSE firms' operational productivity. Relative to the pre-regulation period, the input-oriented measure fell by 0.43% and the output-oriented measure fell by 0.33% (see relevant statistics at the bottom of Panel A). The event-study results obtained from the SDiD, DiD, and Borusyak-Jaravel-Spiess (BJS) imputation methods are also consistent with those obtained when we consider the TFP and LP measures and, on top of that, confirm the absence of pre-trends. Figure 4 portrays the event-study results obtained from the SDiD method. Given the similarity of the results obtained from the other two methods, we relegate them to the Appendix to save on space (Figures B3 and B4).

According to the event-study results, there were no operational productivity gains in the year when the regulation came into effect (Panel (a) of Figure 4) or only small ones relative to the TFP and LP gains observed in the previous exercises (Panel (b) of Figure 4 and Panels (a) and (b) of Figures B3 and B4). Depending on the productivity measure used and estimation method implemented, operational productivity declined steeply as of that year and for the next two or four years, after which its decline was slightly mitigated for three years or one, before becoming stronger again after the fifth post-regulation year. Despite the consistency of these effects with the effects on TFP and LP measures, we should stress that the effects on operational productivity do not reflect declines in their productivity *levels*, but rather shifts away from the productivity frontier. Also, the interpretation of these effects becomes harder in case the regulation changed not only the position of the LSE firms relative to the productivity frontier, but also the frontier itself (recall our relevant discussion in Section 3).

The rest of the columns of Table 6 and panels of Figures 4, B3, and B4 show average and annual effects of the regulation on the LSE firms' ROIC and inventory-related measures, namely, the log of inventory amount, inventory turnover, and the log of the average number of inventory days. The regulation exerted no statistically significant average effects on the LSE firms' ROIC, as indicated by the relevant SDiD and DiD estimates (column (3) in Panels A and B of Table 6). The negative signs of these estimates, however, are consistent with the negative average and annual treatment effects on the productivity measures identified earlier. Also, according to the event-study results, despite an increase in the LSE firms' ROIC in the year when the regulation came into effect, this measure declined in the next years, and increased again only after the fifth post-regulation year (Panel (c) of Figures 4 and B3) or declined more moderately after the fourth post-regulation year (Panel (c) of Figure B4). Similarly, the regulation exerted no statistically significant effects on the LSE firms' inventory amounts, despite the fact that the positive signs of the relevant SDiD and DiD estimates are also consistent with the negative average treatment effects on their productivity (column (4) in Panels A and B of Table 6). Relatedly, the event-study results point to increases in the LSE firms' inventory amounts in the first three years of the regulation (Panel (d) of Figures 4, B3, and B4). Despite some small fluctuations in the next years, there is some evidence that these increases persisted into the sixth post-regulation year (see SDiD and DiD estimates).

By contrast, the regulation exerted negative effects on the LSE firms' inventory turnover, which are significant at 10% (SDiD) or 1% (DiD), and positive effects on the LSE firms' average number of inventory days, which are significant at 1% (SDiD) and 5% (DiD) (columns (5) and (6) in Panels A and B of Table 6). According to the relevant statistics at the bottom of the two panels, inventory turnover fell by 7.6% (SDiD) or 6.8% (DiD) and the log of the average number of inventory days rose by 2% (SDiD) or 1.8% (DiD) relative to the pre-regulation period. Similarly, the event-study results point to declines in the LSE firms' inventory turnover and increases in their average number of inventory days throughout the post-regulation period (Panels (e) and (f) of of Figures 4, B3, and B4). These effects suggest that the frequency at which the LSE firms replaced their inventories to meet demand in the post-regulation period decreased, which is consistent with their productivity and output declines over that period. In fact, the decline in their inventory turnover and rise in their average number of inventory days might have well contributed to the deterioration in their productivity. Importantly, there is no evidence of pre-trends in Panels (c)–(f) of the three figures.

5.2 Effects of the regulation on firm profitability and income measures

To study the effects of the regulation on the LSE firms' profitability and income, we estimate Eqs. 1 and 2 while using each of the relevant measures described in Section 3 as the dependent variable. Table 7 displays the average treatment effects that we identify implementing the SDiD (Panel A) and DiD (Panel B) methods. Note that the number of observations in the columns of this table is smaller than in the previous tables because of higher fractions of missing values for the profitability and income measures. Despite a few differences in the patterns of these effects based on the two methods, the patterns emerging from the event studies are very similar across the three methods implemented. The event-study results obtained from the SDiD method are shown in Figure 5, while those obtained from the DiD and Borusyak-Jaravel-Spiess (BJS) imputation methods have been relegated to the Appendix (Figures B5 and B6).

Overall, the regulation did have negative effects on the LSE firms' profitability and income. The effects on the four profitability and two income measures took place immediately after the regulation came into effect and got stronger for the next two to five years, depending on the measure and estimation method used, before being gradually mitigated. In accord with the event-study results, the average effect of the regulation on the LSE firms' ROA was negative and significant at 1%, according to the relevant DiD estimate (column (1) of Panel B in Table 7). The measure declined by 34% relative to the pre-regulation period. The corresponding SDiD estimate is also negative but statistically insignificant (column (1) of Panel A in the same table). Similarly, the average effect of the regulation on the LSE firms' EBIT and EBITDA was negative and significant at 5% and 10%, respectively, according to the relevant SDiD

Table 7: Average post-regulation effects on firm profitability and income measures

Panel A: Synthetic diff-in-diff (SDiD) method	(1) ROA	(2) Operating profit margin	(3) Log of EBIT	(4) Log of EBITDA	(5) Log of pre-tax income	(6) Log of after-tax income
LSE companies * Post-regulation	-0.000085	-0.000066	-0.16**	-0.12*	-0.13	-0.11
	[0.00010]	[0.0001]	[0.07]	[0.07]	[0.1]	[0.2]
Observations	1022	1022	1022	1022	1120	1078
Pre-treatment mean of outcome on treated	0.00109	0.00141	18.43	18.72	18.41	17.93
Post-treatment % ch. of outcome	-7.847	-4.698	-0.874	-0.667	-0.696	-0.619

Panel B: Simple diff-in-diff (DiD) method	(1) ROA	(2) Operating profit margin	(3) Log of EBIT	(4) Log of EBITDA	(5) Log of pre-tax income	(6) Log of after-tax income
Post-regulation	-0.00014***	0.00033	-0.36***	-0.19***	-0.28***	-0.36***
	[0.00004]	[0.0004]	[0.05]	[0.05]	[0.06]	[0.07]
LSE companies * Post-regulation	-0.00024***	-0.00045	0.049	0.020	-0.064	-0.040
	[0.00005]	[0.0003]	[0.06]	[0.05]	[0.05]	[0.06]
Observations	4593	4588	3975	4202	3770	3752
R^2	0.678	0.690	0.923	0.941	0.906	0.899
Pre-treatment mean of outcome on treated	0.000693	-0.000262	18.02	18.35	17.89	17.60
Post-treatment % ch. of outcome	-34.26	170.6	0.272	0.111	-0.356	-0.228

Source: SDiD and DiD estimations in Panels A and B, respectively. DiD estimations have been made on matched samples based on coarsened exacting matching. The specifications in all columns of the two panels include firm fixed effects and year fixed effects. Asterisks denote significance at 1% (***), 5% (**), and 10% (*), based on bootstrapped standard errors (SDiD) or standard errors clustered by stock exchange market (DiD).

estimates, although the corresponding DiD estimates are positively signed and statistically insignificant (columns (3) and (4) of Panels A and B in the same table). These measures declined by 0.87% and 0.67%, respectively, relative to the pre-regulation period. The SDiD and DiD estimates in the rest of the columns of the two panels in Table 7 are negative but statistically insignificant.

5.3 Robustness checks

In this section, we check the robustness of the results presented in the previous two sections. To identify the main effects of the regulation on the LSE firms' TFP, we have relied on TFP measures created based on the Olley & Pakes (1996), Levinsohn & Petrin (2003), and Ackerberg et al. (2015) methods. In the first exercise of this section, we confirm that these effects are insensitive to the use of TFP measures created based on alternative methods—in particular, those developed by Wooldridge (2009), Robinson-Wooldridge, and Rovigatti & Mollisi (2018). Results are shown in Table B1 and Figures B7 and B8.

In a second exercise, we perform in-time placebo tests by setting as the start year of the regulation a year earlier than 2013 or 2014 (Abadie et al., 2015; Gilchrist et al., 2023). Choosing 2012 as the start year and estimating the main specifications based on the SDiD and DiD methods yields statistically insignificant effects on TFP (Table B2) and the other outcome variables that we have considered in the main analysis (available upon request). The same holds true when we consider 2010 or 2011 as the start year of the regulation (results available upon request). These results lend strong support to the suitability of our diff-in-diff research design in capturing the effects of the carbon disclosure regulation, rather than any other shock that might have occurred around its official start date, and also address concerns over the anticipation of the regulation by the LSE firms.

In a third exercise, we estimate the main specifications based on the SDiD and DiD methods after excluding iteratively from the sample non-treated firms listed on one of the EEA stock exchanges (Gilchrist et al., 2023; Klößner et al., 2018). Results, which are available upon request given their large volume, remain largely unchanged, suggesting that the main effects that we have identified are not driven by non-treated firms of a certain EEA stock exchange market.

In addition, we confirm that the main results obtained from the DiD method remain largely unchanged when we implement no matching between observations of the treatment and control groups. The effects on TFP are displayed in Panel A of Table B4 and those on the other outcome variables are available upon request. Similarly, results are largely unchanged when we incorporate the continuous variables in the specifications without winsorising them (see effects on TFP from the SDiD and DiD methods in Panels A and B of Table B3; effects on the other outcome variables are available upon request). In a final check on the DiD estimates, we confirm that they are insensitive to the incorporation of three-digit (NAICS) industry fixed effects and stock exchange market fixed effects in addition to firm and year fixed effects (effects on TFP are displayed in Panel B of Table B4 and those on the other outcome variables are available upon request). This also holds when we replace firm and year fixed effects with pairs of three-, four-, five, or six-digit industry and year fixed effects and stock exchange market fixed effects (results available upon request).

6 Discussion and conclusion

This paper studies the impact of mandatory environmental disclosure on productivity-related outcomes using the United Kingdom's 2013 mandatory carbon disclosure regulation as a quasi-natural experiment. We employ difference-in-differences and event-study designs on manufacturing firms listed on the LSE and comparable European firms over 2006–2019 and find that the regulation led to economically meaningful and persistent declines in productivity-related outcomes.

Using a multidimensional productivity framework comprising of total factor productivity, labour productivity, and operational productivity, we find that treated firms experience significant post-regulation declines, concentrated in the first two to four years after adoption and only partially attenuated thereafter. These productivity declines are accompanied by systematic operational adjustments: inventory turnover decreases, average inventory days increase, output growth weakens, and employment declines with a lag. Together, these results indicate that disclosure-based regulation affected not only aggregate efficiency but also the coordination of production, labour, and inventory decisions.

The findings challenge the assumption that disclosure mandates are operationally neutral. Although the regulation did not directly constrain technologies or output, it appears to have acted as a regulatory attention shock, reallocating managerial attention and organisational resources toward compliance and reporting activities. Consistent with the attention-based view of the firm, such reallocation can crowd out productivity-enhancing operational efforts. The persistence of the effects suggests that mandatory disclosure induces durable organisational and operational changes rather than temporary adjustment costs.

This study contributes to the operations and management science literature in three ways. First, it provides first causal evidence on how mandatory environmental disclosure regulation affects firm-level productivity, extending prior work that has focused primarily on environmental or financial outcomes [REF] or looked at the antecedents of climate disclosures. Second, it conceptualises disclosure as an attention and resource allocation shock, offering a mechanism through which informational regulation generates real operational consequences. Third, by treating productivity as a multi-dimensional outcome, the analysis reveals how regulatory shocks propagate across interdependent operational system.

From a policy perspective, the results suggest that the design and implementation of disclosure regulation matter: prescriptive and rapidly imposed mandates can impose nontrivial operational costs, particularly in manufacturing contexts. For managers, the findings highlight the importance of integrating compliance activities with operational decision-making to mitigate productivity losses during regulatory transitions.

AI Use Disclosure

The authors acknowledge the use of ChatGPT (OpenAI) as a language editing tool to improve clarity, structure, and presentation of the manuscript. All theoretical arguments, empirical data collection and analyses, interpretations, and conclusions were developed by the authors, who retain full responsibility for the content.

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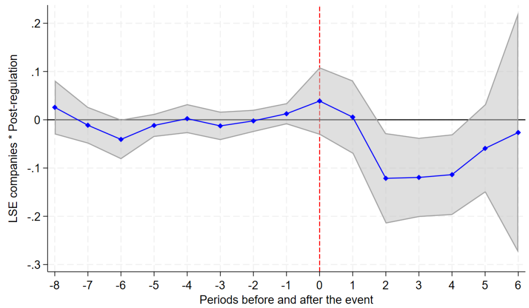
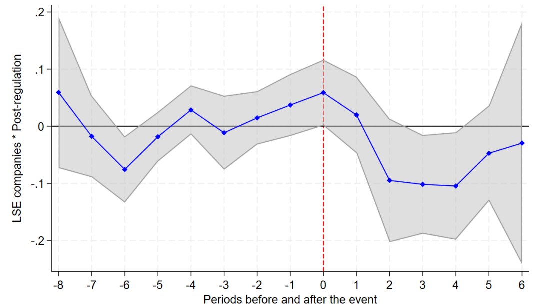
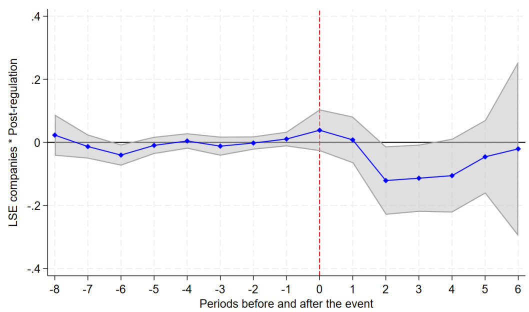
Figure 1: Event study on regulation effects on TFP, SDiD and DiD



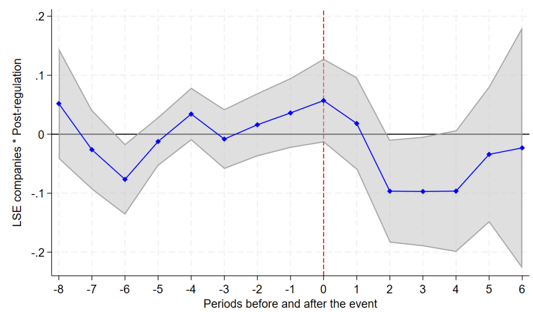
(a) Olley-Pakes, SDiD



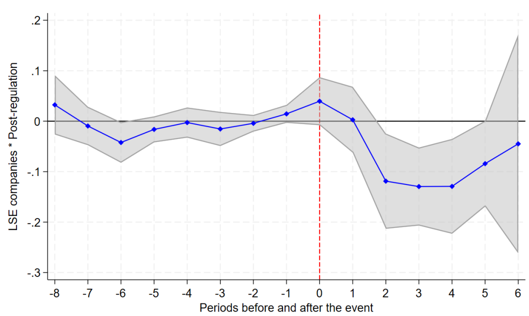
(b) Olley-Pakes, DiD



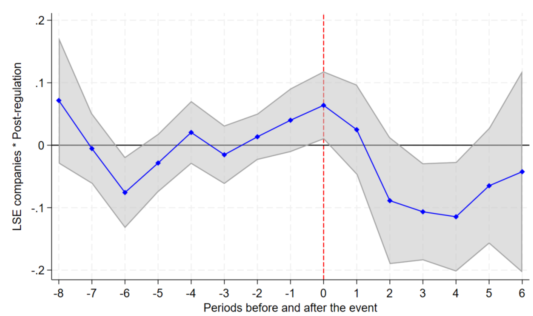
(c) Olley-Pakes, ACF, SDiD



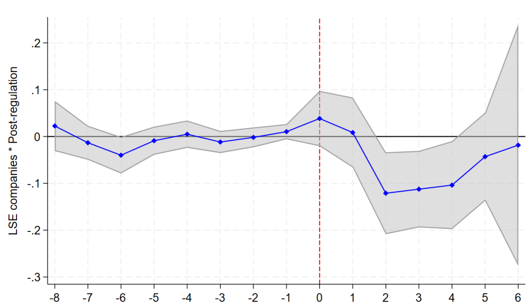
(d) Olley-Pakes, ACF, DiD



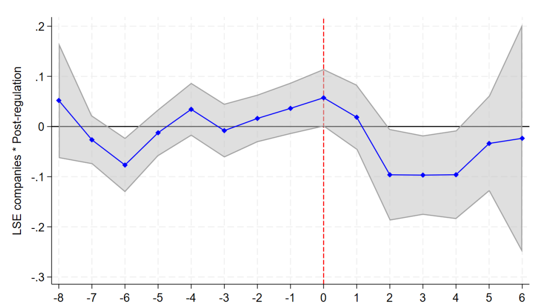
(e) Levinshon-Petrin, SDiD



(f) Levinshon-Petrin, DiD



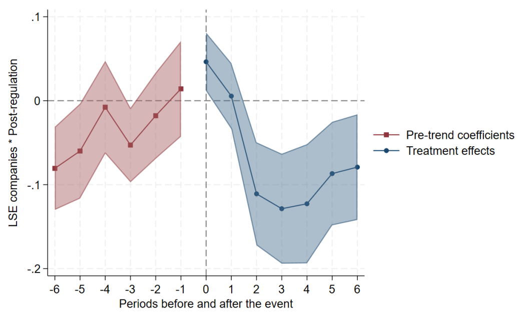
(g) Levinshon-Petrin, ACF, SDiD



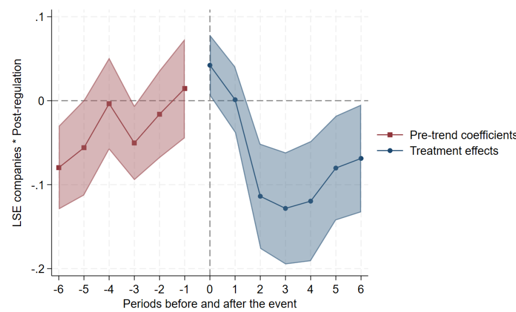
(h) Levinshon-Petrin, ACF, DiD

Notes: SDiD estimations with bootstrapped standard errors in Panels (a), (c), (e), and (g) and and DiD estimations with bootstrapped standard errors in Panels (b), (d), (f), and (h).

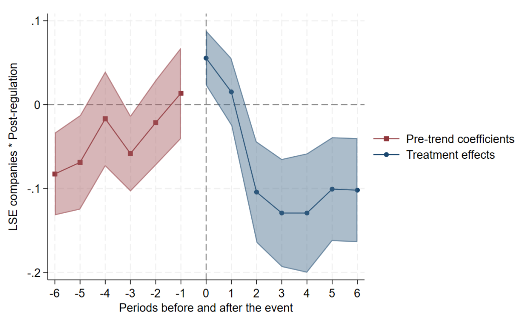
Figure 2: Event study on regulation effects on TFP, BJS imputation method



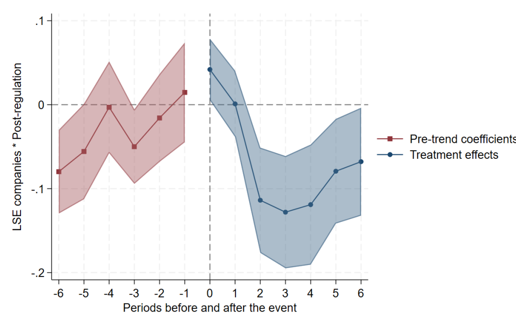
(a) Olley-Pakes



(b) Olley-Pakes, with ACF correction



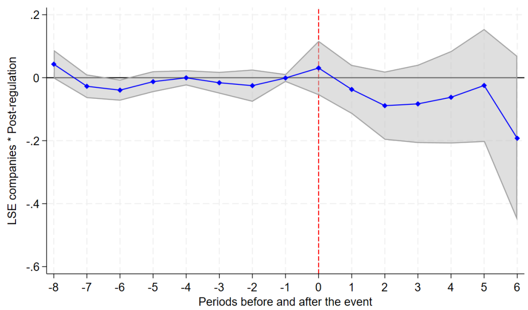
(c) Levinshon-Petrin



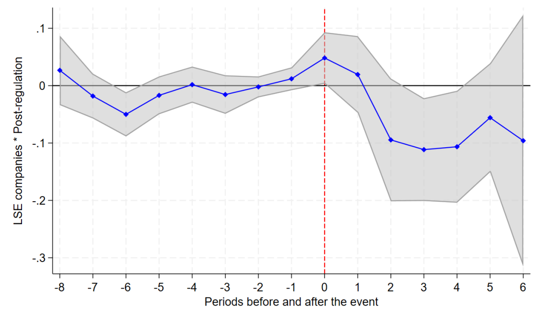
(d) Levinshon-Petrin, with ACF correction

Notes: Imputation estimations based on Borusyak et al. (2024) in all panels after implementing the coarsened exact matching method.

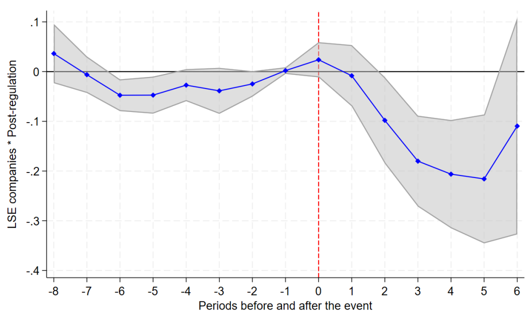
Figure 3: Event study on regulation effects on labour productivity and its components, SDiD



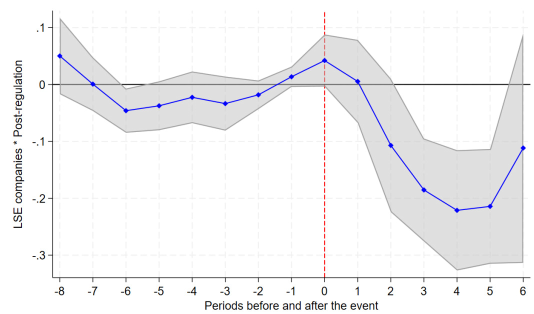
(a) Log of ratio of total revenue to full-time employment



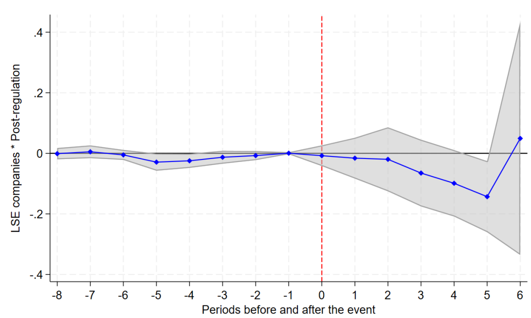
(b) Log of ratio of total value added to full-time employment



(c) Log of total revenue



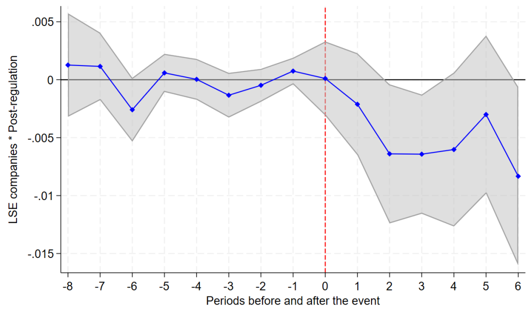
(d) Log of total value added



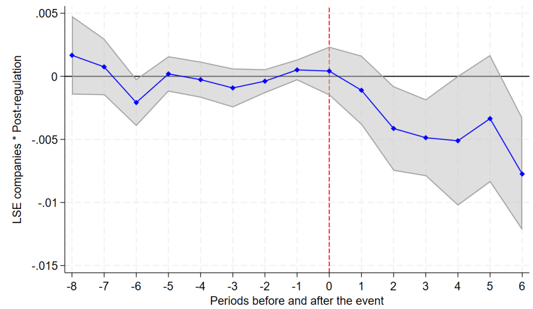
(e) Log of full-time employment

Notes: SDiD estimations in all panels with bootstrapped standard errors.

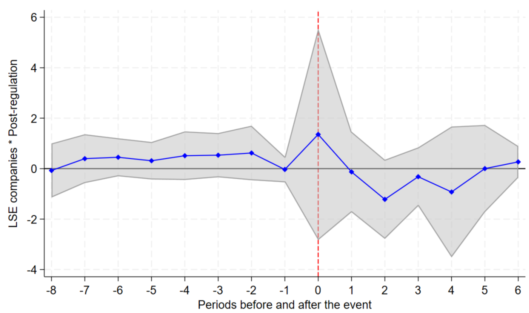
Figure 4: Event study on regulation effects on operational productivity and other firm performance measures, SDiD



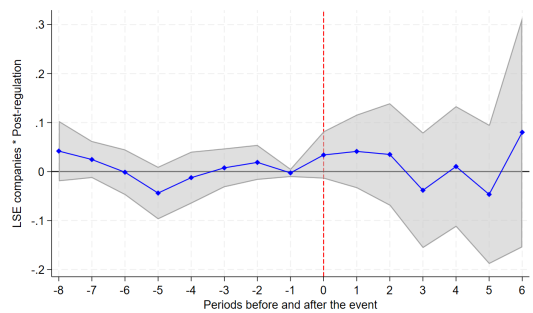
(a) Operational productivity, input-oriented



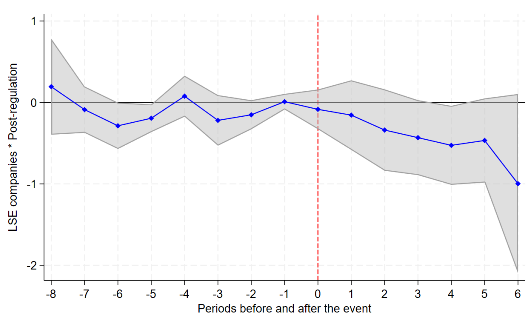
(b) Operational productivity, output-oriented



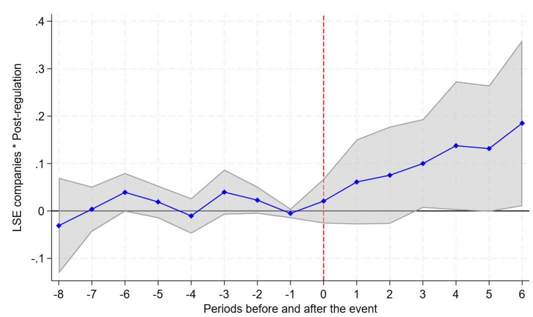
(c) ROIC - 2



(d) Log of total inventory



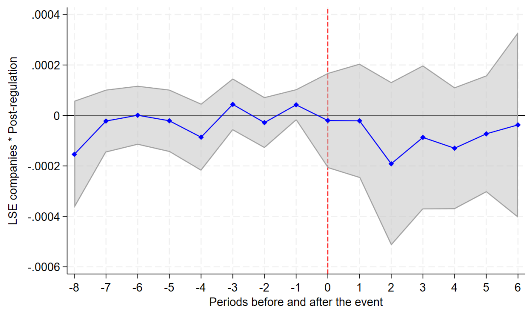
(e) Inventory turnover



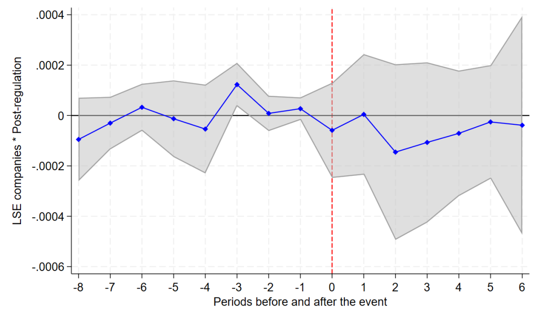
(f) Log of avg. # days of inventory

Notes: SDiD estimations in all panels with bootstrapped standard errors.

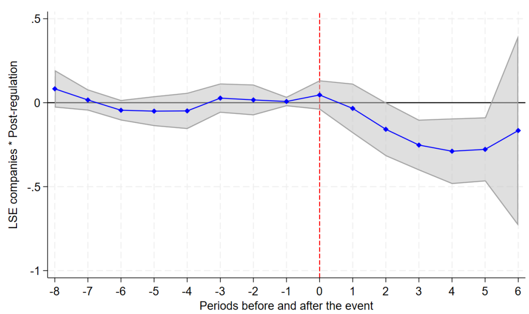
Figure 5: Event study on regulation effects on firm profitability and income measures, SDiD



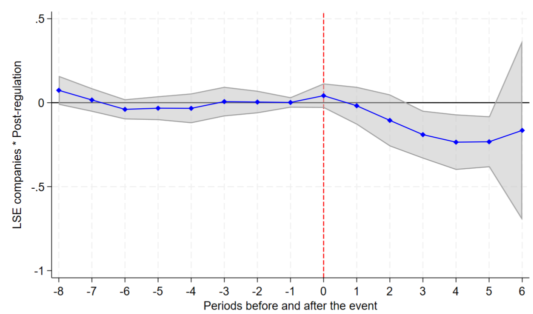
(a) ROA



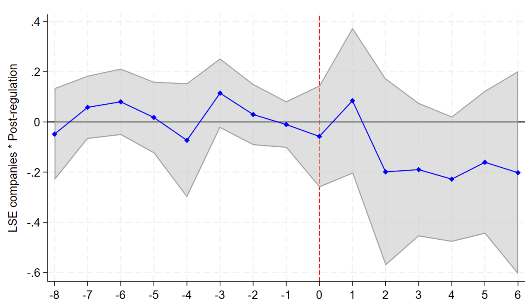
(b) Operating profit margin



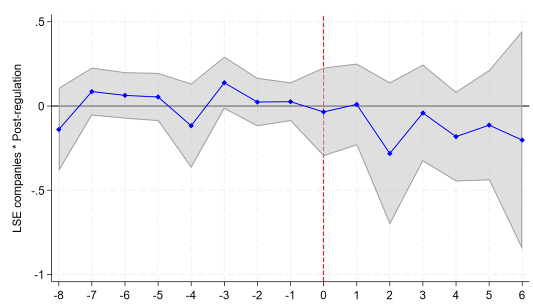
(c) Log of EBIT



(d) Log of EBITDA



(e) Log of pre-tax income



(f) Log of after-tax income

Notes: SDiD estimations in all panels with bootstrapped standard errors.

Appendix

A Additional descriptive statistics

Table A1: Countries of firms' headquarters

Country of HQs	Treatment		Control		Total	
	No.	%	No.	%	No.	%
United Kingdom	45	93.8	0	0	45	13
Austria	0	0	12	4	12	3.5
Belgium	0	0	16	5.4	16	4.6
Denmark	0	0	16	5.4	16	4.6
Finland	0	0	21	7	21	6.1
France	0	0	45	15.1	45	13
Germany	0	0	57	19.1	57	16.5
Ireland	1	2.1	2	0.7	3	0.9
Israel	1	2.1	1	0.3	2	0.6
Italy	0	0	17	5.7	17	4.9
Luxembourg	0	0	1	0.3	1	0.3
Netherlands	0	0	15	5	15	4.3
Norway	0	0	11	3.7	11	3.2
Portugal	0	0	1	0.3	1	0.3
Singapore	1	2.1	0	0	1	0.3
Spain	0	0	12	4	12	3.5
Sweden	0	0	29	9.7	29	8.4
Switzerland	0	0	42	14.1	42	12.1
Total	48	100	298	100	346	100

Source: LSEG database. The statistics have been produced on the original (pre-matched) sample.

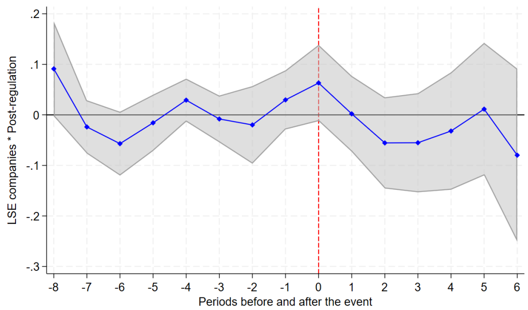
Table A2: Years in which firms became publicly listed

IPO year	Treatment		Control		Total	
	No.	%	No.	%	No.	%
1890	0	0	1	0.3	1	0.3
1895	0	0	1	0.3	1	0.3
1899	0	0	1	0.3	1	0.3
1905	0	0	5	1.7	5	1.4
1913	0	0	1	0.3	1	0.3
1920	0	0	1	0.3	1	0.3
1936	0	0	1	0.3	1	0.3
1937	0	0	1	0.3	1	0.3
1938	1	2.1	0	0	1	0.3
1939	1	2.1	1	0.3	2	0.6
1946	2	4.2	0	0	2	0.6
1948	1	2.1	0	0	1	0.3
1951	1	2.1	0	0	1	0.3
1953	0	0	1	0.3	1	0.3
1954	1	2.1	0	0	1	0.3
1961	0	0	1	0.3	1	0.3
1963	0	0	1	0.3	1	0.3
1964	2	4.2	0	0	2	0.6
1966	0	0	1	0.3	1	0.3
1968	1	2.1	1	0.3	2	0.6
1972	2	4.2	0	0	2	0.6
1973	0	0	1	0.3	1	0.3
1974	0	0	1	0.3	1	0.3
1975	0	0	2	0.7	2	0.6
1979	0	0	1	0.3	1	0.3
1980	0	0	1	0.3	1	0.3
1981	1	2.1	1	0.3	2	0.6
1983	1	2.1	0	0	1	0.3
1984	0	0	4	1.3	4	1.2
1985	0	0	15	5	15	4.3
1986	16	33.3	7	2.3	23	6.6
1987	1	2.1	14	4.7	15	4.3
1988	3	6.2	2	0.7	5	1.4
1989	0	0	9	3	9	2.6
1990	0	0	6	2	6	1.7
1991	0	0	7	2.3	7	2
1992	1	2.1	3	1	4	1.2
1993	1	2.1	2	0.7	3	0.9
1994	1	2.1	9	3	10	2.9
1995	1	2.1	12	4	13	3.8
1996	2	4.2	15	5	17	4.9
1997	1	2.1	26	8.7	27	7.8
1998	0	0	32	10.7	32	9.2
1999	0	0	21	7	21	6.1
2000	1	2.1	17	5.7	18	5.2
2001	0	0	21	7	21	6.1
2002	0	0	5	1.7	5	1.4
2003	0	0	3	1	3	0.9
2004	3	6.2	8	2.7	11	3.2
2005	2	4.2	13	4.4	15	4.3
2006	1	2.1	22	7.4	23	6.6
Total	48	100	298	100	346	100

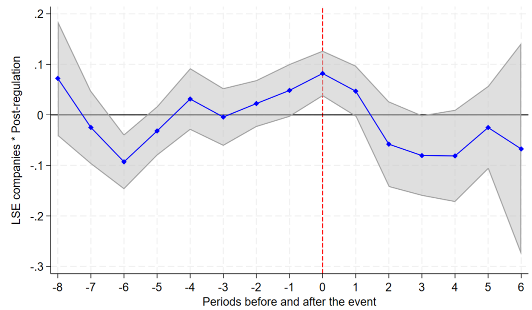
Source: LSEG database. The statistics have been produced on the original (pre-matched) sample.

B Additional econometric results

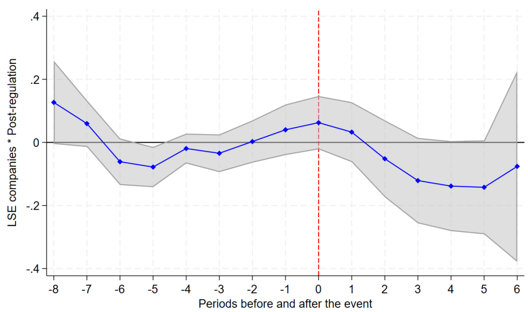
Figure B1: Event study on regulation effects on labour productivity and its components, DiD



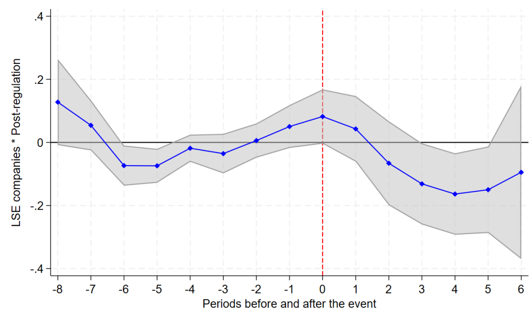
(a) Log of ratio of total revenue to full-time employment



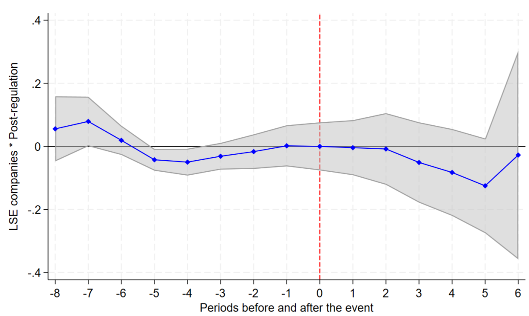
(b) Log of ratio of total value added to full-time employment



(c) Log of total revenue



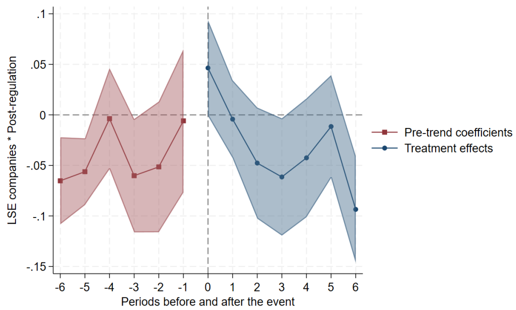
(d) Log of total value added



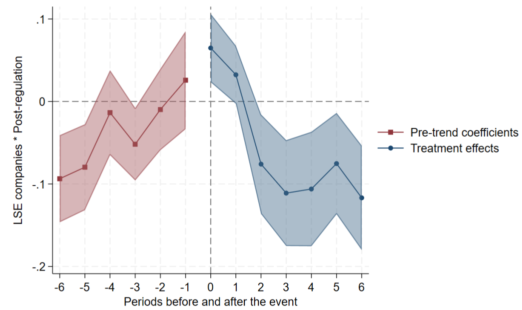
(e) Log of full-time employment

Notes: DiD estimations in all panels with bootstrapped standard errors.

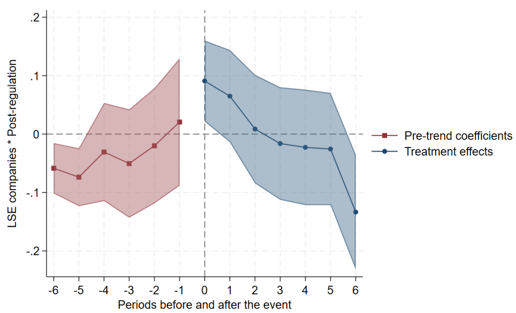
Figure B2: Event study on regulation effects on labour productivity and its components, BJS imputation method



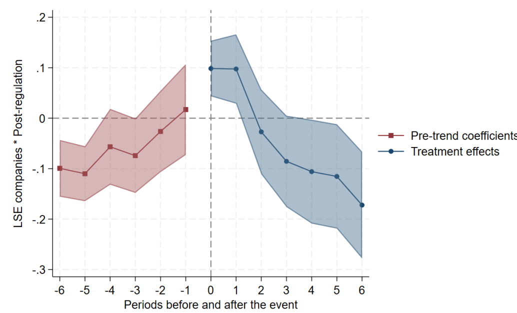
(a) Log of ratio of total revenue to full-time employment



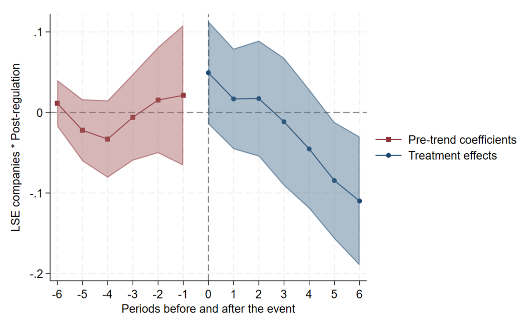
(b) Log of ratio of total value added to full-time employment



(c) Log of total revenue



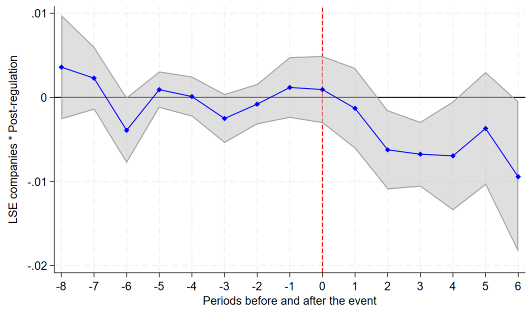
(d) Log of total value added



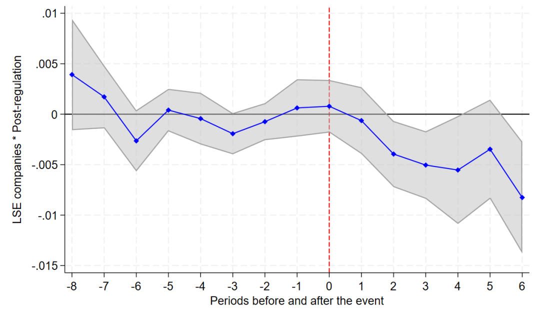
(e) Log of full-time employment

Notes: Imputation estimations based on Borusyak et al. (2024) in all panels after implementing the coarsened exact matching method.

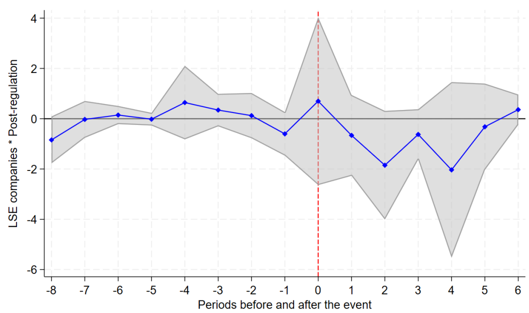
Figure B3: Event study on regulation effects on operational productivity and other firm performance measures, DiD



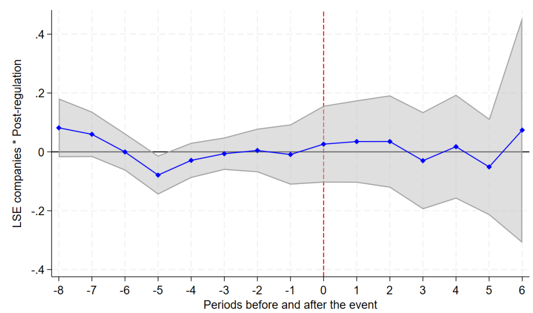
(a) Operational productivity, input-oriented



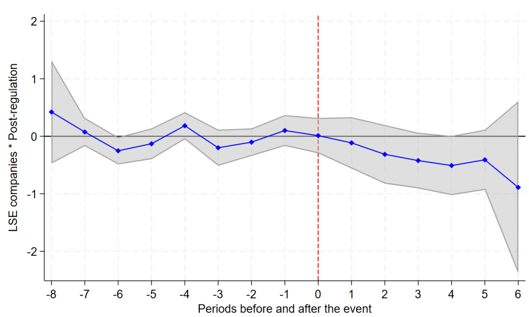
(b) Operational productivity, output-oriented



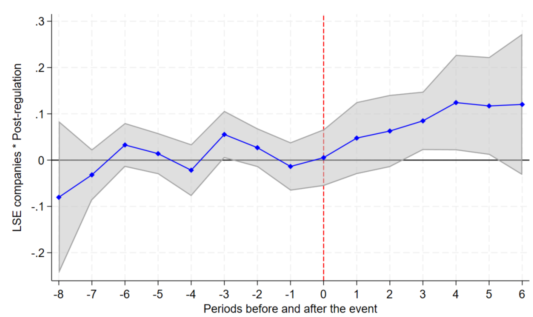
(c) ROIC - 2



(d) Log of total inventory



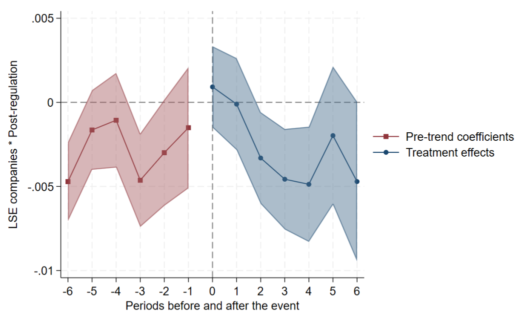
(e) Inventory turnover



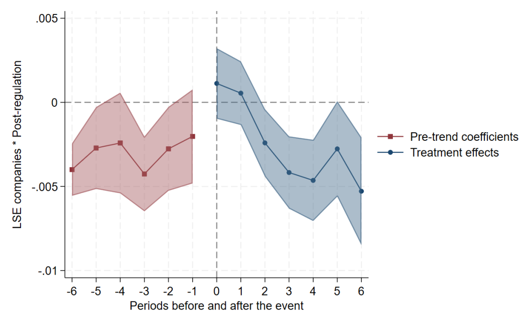
(f) Log of avg. # days of inventory

Notes: DiD estimations in all panels with bootstrapped standard errors.

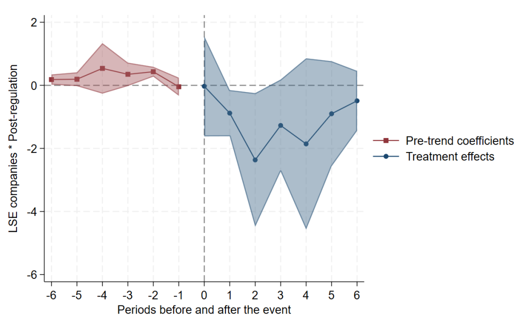
Figure B4: Event study on regulation effects on operational productivity and other firm performance measures, BJS imputation method



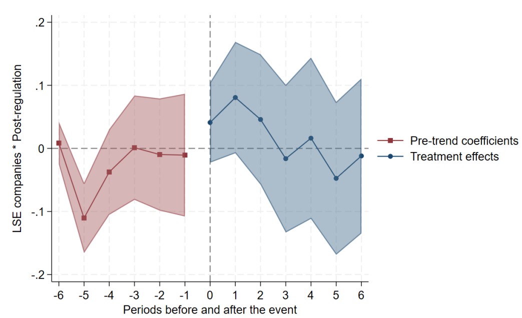
(a) Operational productivity, input-oriented



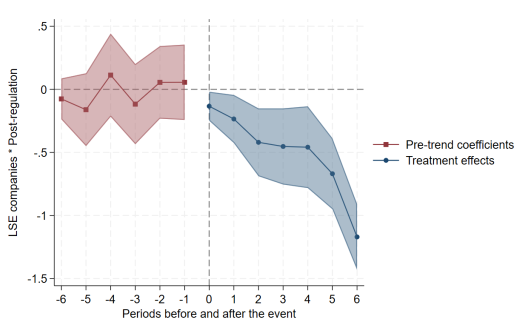
(b) Operational productivity, output-oriented



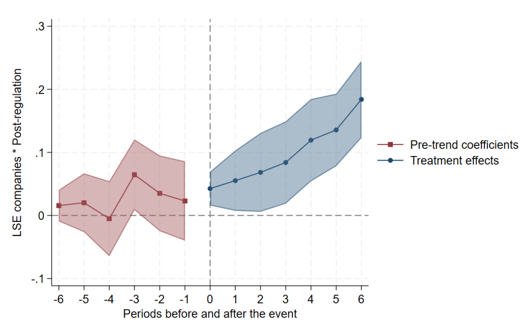
(c) ROIC - 2



(d) Log of total inventory



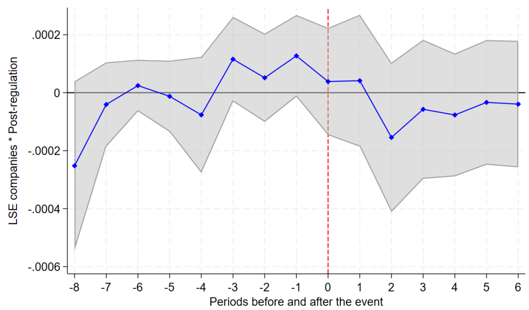
(e) Inventory turnover



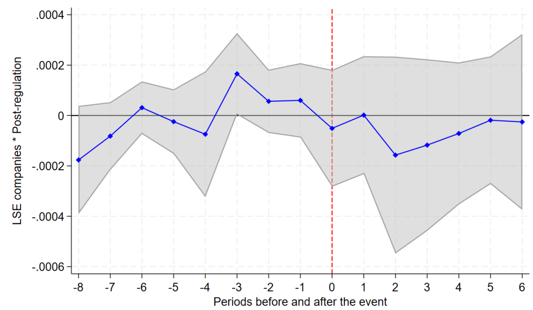
(f) Log of avg. # days of inventory

Notes: Imputation estimations based on Borusyak et al. (2024) in all panels after implementing the coarsened exact matching method.

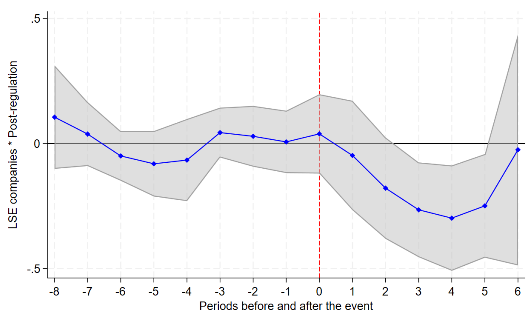
Figure B5: Event study on regulation effects on firm profitability and income measures, DiD



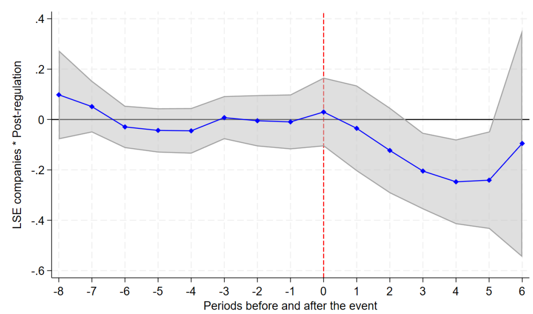
(a) ROA



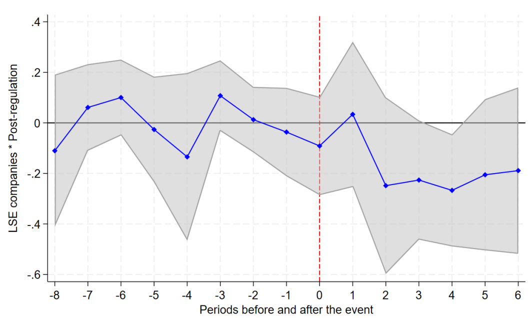
(b) Operating profit margin



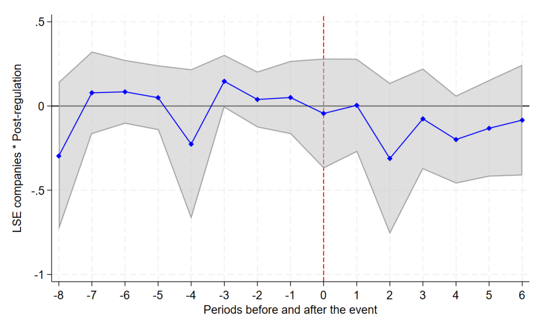
(c) Log of EBIT



(d) Log of EBITDA



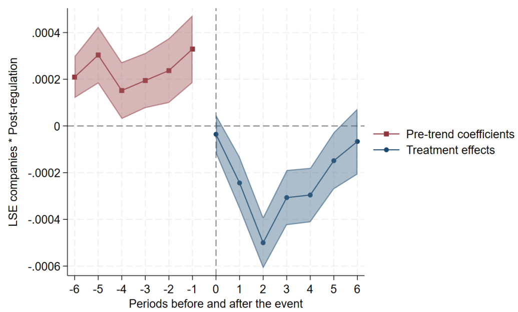
(e) Log of pre-tax income



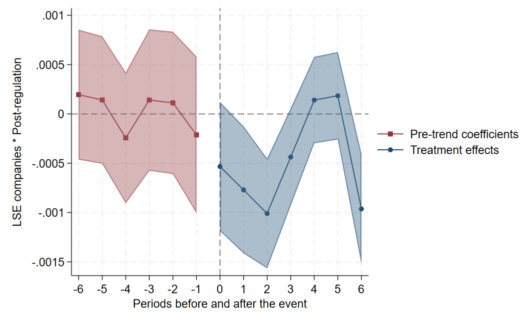
(f) Log of after-tax income

Notes: SDiD estimations in all panels with bootstrapped standard errors.

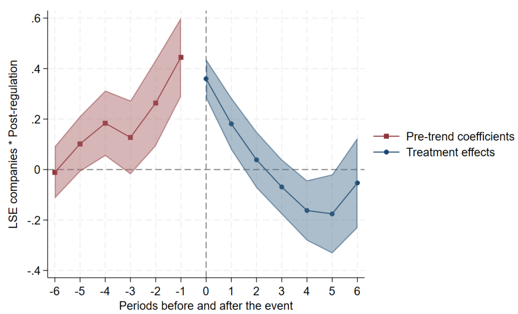
Figure B6: Event study on regulation effects on firm profitability and income measures, BJS imputation method



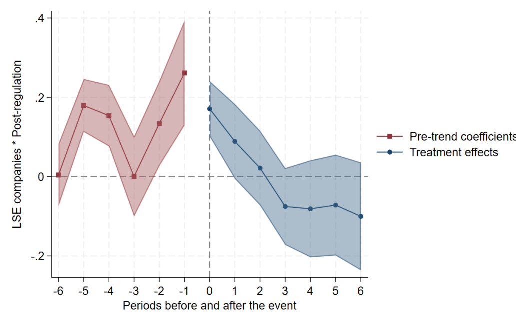
(a) ROA



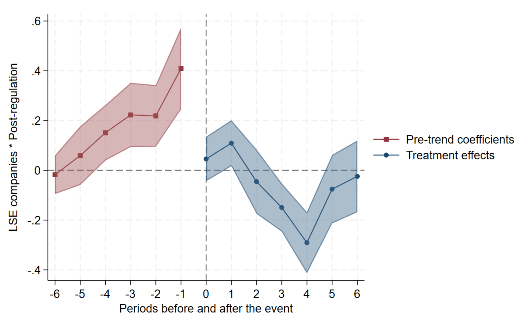
(b) Operating profit margin



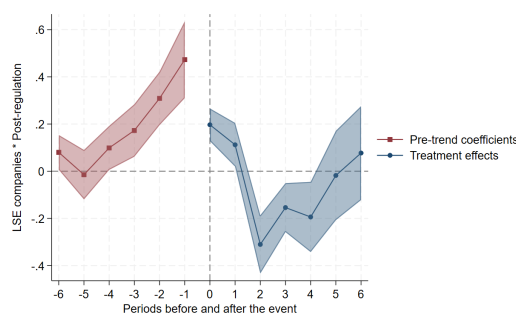
(c) Log of EBIT



(d) Log of EBITDA



(e) Log of pre-tax income



(f) Log of after-tax income

Notes: Imputation estimations based on Borusyak et al. (2024) in all panels after implementing the coarsened exact matching method.

Table B1: Average post-regulation effects on additional TFP measures

Panel A: Synthetic diff-in-diff (SDiD) method

	(1)	(2)	(3)
Dep. var: TFP	Wooldridge	Robinson-Wooldridge	Mollisi-Rovigatti
LSE companies * Post-regulation	-0.068*	-0.068*	-0.068*
	[0.04]	[0.04]	[0.04]
Observations	3654	3654	3654
Pre-treatment mean of outcome on treated	11.92	11.66	11.73
Post-treatment % ch. of outcome	-0.570	-0.586	-0.584

Panel B: Simple diff-in-diff (DiD) method

	(1)	(2)	(3)
Dep. var: TFP	Wooldridge	Robinson-Wooldridge	Mollisi-Rovigatti
Post-regulation	-0.041*	-0.041*	-0.041*
	[0.02]	[0.02]	[0.02]
LSE companies * Post-regulation	-0.058**	-0.059**	-0.058**
	[0.03]	[0.03]	[0.03]
Observations	4221	4221	4221
R^2	0.840	0.842	0.839
Pre-treatment mean of outcome on treated	11.58	11.63	11.57
Post-treatment % ch. of outcome	-0.505	-0.504	-0.505

Source: SDiD and DiD estimations in Panels A and B, respectively. DiD estimations have been made on matched samples based on coarsened exacting matching. The specifications in all columns of the two panels include firm fixed effects and year fixed effects. Asterisks denote significance at 1% (***), 5% (**), and 10% (*), based on bootstrapped standard errors (SDiD) or standard errors clustered by stock exchange market (DiD).

Table B2: Average post-regulation effects on TFP, in-time placebo test

Panel A: Synthetic diff-in-diff (SDiD) method

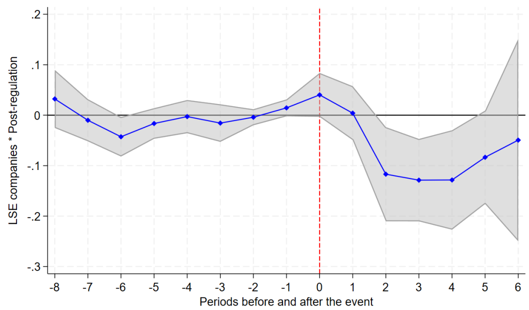
	(1)	(2)	(3)	(4)
Dep. var: TFP	Olley-Pakes		Levinshon-Petrin	
	Original	ACF correction	Original	ACF correction
LSE companies * Post-regulation	-0.0091	-0.0097	-0.0082	-0.0087
	[0.04]	[0.04]	[0.04]	[0.04]
Observations	3654	3654	3654	3654

Panel B: Simple diff-in-diff (DiD) method

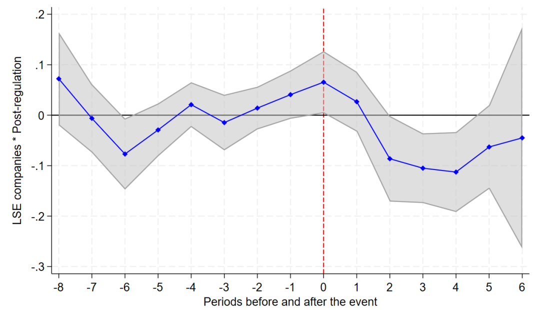
	(1)	(2)	(3)	(4)
Dep. var: TFP	Olley-Pakes		Levinshon-Petrin	
	Original	ACF correction	Original	ACF correction
LSE companies * Post-regulation	-0.042	-0.042	-0.040	-0.041
	[0.02]	[0.02]	[0.02]	[0.03]
Observations	4221	4221	4221	4221
R^2	0.757	0.741	0.838	0.740

Source: SDiD and DiD estimations in Panels A and B, respectively. DiD estimations have been made on matched samples based on coarsened exacting matching. The specifications in all columns of the two panels include firm fixed effects and year fixed effects. Asterisks denote significance at 1% (***), 5% (**), and 10% (*), based on bootstrapped standard errors (SDiD) or standard errors clustered by stock exchange market (DiD).

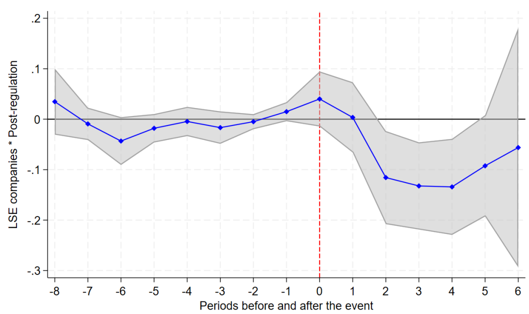
Figure B7: Event study on regulation effects on additional TFP measures, sDiD and DiD



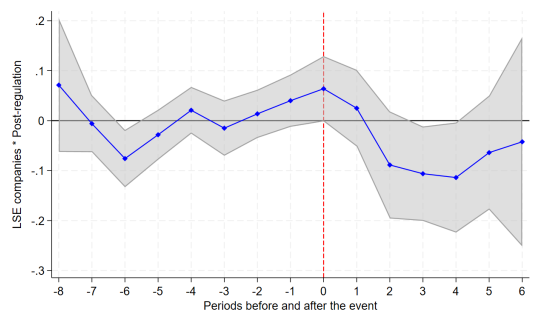
(a) Wooldridge, sDiD



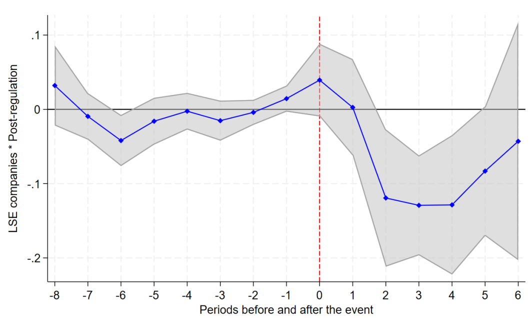
(b) Wooldridge, DiD



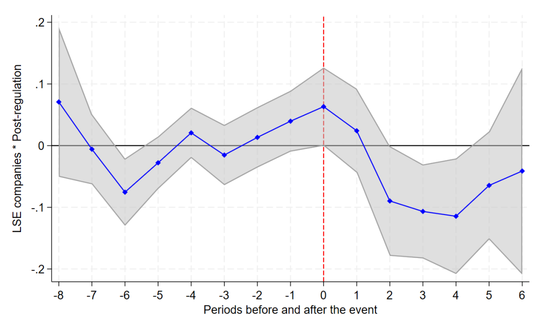
(c) Robinson-Wooldridge, sDiD



(d) Robinson-Wooldridge, DiD



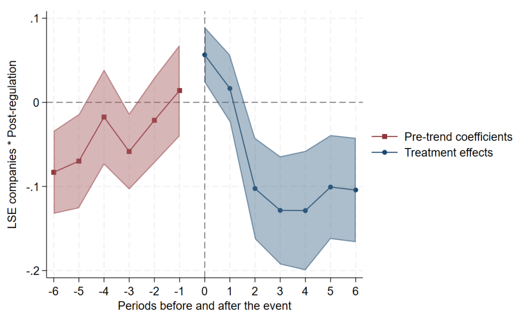
(e) Mollisi-Rovigatti, sDiD



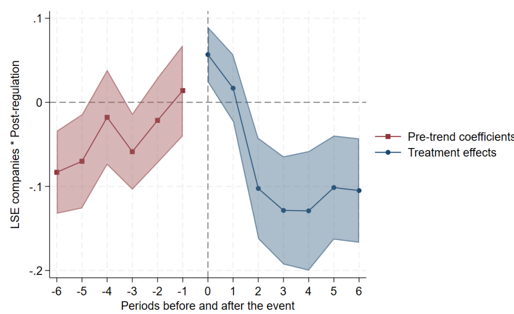
(f) Mollisi-Rovigatti, DiD

Notes: SDiD estimations with bootstrapped standard errors in Panels (a), (c), and (e) and DiD estimations with bootstrapped standard errors in Panels (b), (d), and (f).

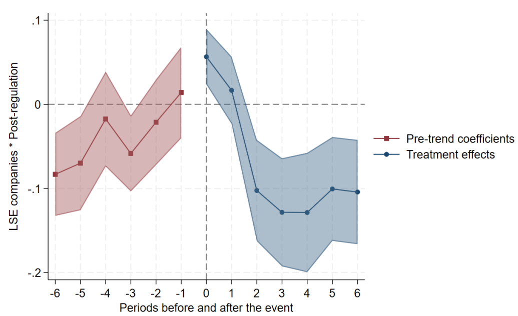
Figure B8: Event study on regulation effects on additional TFP measures, BJS imputation method



(a) Wooldridge



(b) Robinson-Wooldridge



(c) Mollisi-Rovigatti

Notes: Imputation estimations based on Borusyak et al. (2024) in all panels after implementing the coarsened exact matching method.

Table B3: Average post-regulation effects on TFP, non-winsorised variables

Panel A: Synthetic diff-in-diff (SDiD) method

	(1)	(2)	(3)	(4)
Dep. var: TFP	Olley-Pakes		Levinshon-Petrin	
	Original	ACF correction	Original	ACF correction
LSE companies * Post-regulation	-0.062	-0.053	-0.072**	-0.055
	[0.04]	[0.05]	[0.03]	[0.04]
Observations	3654	3654	3654	3654
R^2				
Pre-treatment mean of outcome on treated	9.883	9.166	11.84	9.021
Post-treatment % ch. of outcome	-0.627	-0.577	-0.610	-0.608

Panel B: Simple diff-in-diff (DiD) method

	(1)	(2)	(3)	(4)
Dep. var: TFP	Olley-Pakes		Levinshon-Petrin	
	Original	ACF correction	Original	ACF correction
Post-regulation	-0.023	-0.018	-0.030*	-0.019
	[0.02]	[0.02]	[0.02]	[0.02]
LSE companies * Post-regulation	-0.061**	-0.060**	-0.062**	-0.060**
	[0.03]	[0.03]	[0.03]	[0.03]
Observations	4379	4379	4379	4379
R^2	0.757	0.736	0.823	0.738
Pre-treatment mean of outcome on treated	9.669	9.318	11.06	8.780
Post-treatment % ch. of outcome	-0.634	-0.649	-0.563	-0.680

Source: SDiD and DiD estimations in Panels A and B, respectively. DiD estimations have been made on matched samples based on coarsened exacting matching. The specifications in all columns of the two panels include firm fixed effects and year fixed effects. Asterisks denote significance at 1% (***), 5% (**), and 10% (*), based on bootstrapped standard errors (SDiD) or standard errors clustered by stock exchange market (DiD).

Table B4: Average post-regulation effects on TFP, DiD, additional robustness

Panel A: No matching between treated and non-treated observations

	(1)	(2)	(3)	(4)
Dep. var: TFP	Olley-Pakes		Levinshon-Petrin	
	Original	ACF correction	Original	ACF correction
Post-regulation	-0.025	-0.022	-0.033*	-0.023
	[0.02]	[0.02]	[0.02]	[0.02]
LSE companies * Post-regulation	-0.065**	-0.063**	-0.067**	-0.062**
	[0.03]	[0.03]	[0.03]	[0.03]
Observations	4437	4437	4437	4437
R^2	0.750	0.741	0.817	0.743
Pre-treatment mean of outcome on treated	9.403	8.784	11.02	9.003
Post-treatment % ch. of outcome	-0.687	-0.712	-0.607	-0.689

Panel B: Incorporation of three-digit industry and year fixed effects and stock exchange fixed effects

	(1)	(2)	(3)	(4)
Dep. var: TFP	Olley-Pakes		Levinshon-Petrin	
	Original	ACF correction	Original	ACF correction
Post-regulation	-0.030	-0.028	-0.040*	-0.028
	[0.02]	[0.02]	[0.02]	[0.02]
LSE companies * Post-regulation	-0.060**	-0.058**	-0.059**	-0.058**
	[0.03]	[0.03]	[0.03]	[0.03]
Observations	4221	4221	4221	4221
R^2	0.751	0.743	0.838	0.743
Pre-treatment mean of outcome on treated	9.570	9.382	11.48	8.399
Post-treatment % ch. of outcome	-0.627	-0.619	-0.515	-0.687

Source: SDiD and DiD estimations in Panels A and B, respectively. DiD estimations have been made on matched samples based on coarsened exacting matching. The specifications in all columns of the two panels include firm fixed effects and year fixed effects. Asterisks denote significance at 1% (***), 5% (**), and 10% (*), based on bootstrapped standard errors (SDiD) or standard errors clustered by stock exchange market (DiD).