The Impact of Trade Credit Provision on Retail Inventory: An Empirical Investigation Using Synthetic Controls

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Trade credit is an important source of short-term financing and an integrated part in supply contracts. Although a number of theories have been proposed on how trade credit could improve supply chain efficiency, casual study on the impact of trade credit on operational decisions are scarce. In this study, we examine the impact of trade credit on inventory decisions using an empirical strategy that leverages: (i) an exogenous shock imparted by the French Government's intervention to impose a ceiling on trade credit duration; (ii) a triple difference-in-differences identification strategy; and (iii) Synthetic Controls (SC). By considering the 60-days ceiling coverage and SC construction requirements, we identify four French retail sectors as our main sample. Among them, in the post-regulation period, the hardware retail sector firms' on average exhibited a significant 16% decline in trade credit usage. Correspondingly, these firms also displayed a significant 11% decline in inventory level. In the remaining three sectors, we find mixed results in the main sample. All the four sectors, however, show consistent support for a casual link between trade credit and inventory in a sub-sample compiled using a stringent 90-days ceiling criterion. Collectively, our findings offer direct evidence that trade credit is an indispensable financing source for inventory procurement. Finally, in the post-regulation period, the hardware retailers exhibited a 15.5% decline in revenue and 3.5% reduction in gross profit. This cautions policy-makers that regulations limiting the use of trade credit may have unintended consequences on downstream firms, and may harm overall supply chain efficiency.

Key words: OM-Finance interface, trade credit, inventory, empirical OM, synthetic control
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1. Introduction

Trade credit, which allows buyers in supply chains to delay paying their suppliers for an agreed period after receiving goods or services, is an important source of short-term financing (Petersen and Rajan, 1997). A number of theories have been proposed to rationalize its prevalence (Chod et al., 2019b). Among them, several conjecture that trade credit has a unique advantage in achieving certain operational objectives, such as assuring product quality (Babich and Tang, 2012), curtailing buyers' opportunistic operational decisions (Chod, 2016), and sharing demand risk (Yang and Birge, 2018). Collectively, these theories suggest that trade credit plays a crucial role in operational decisions.

An operational variable that has been closely linked to trade credit is inventory. As an effective lever to manage demand risk and improve supply chain performance, inventory is an important investment decision. According to Gaur et al. (2005), inventory accounts for 36% of total assets among U.S. public retailers. Given its practical importance, inventory management has long been at the core of operations management (OM). The theoretical investigation of the relationship between trade credit and inventory can be traced back at least to Haley and Higgins (1973), which shows that, under an Economic Order Quantity (EOQ) model, trade credit terms and inventory decisions should be made jointly and that sub-optimal payment terms could lead to sub-optimal inventory stocking decisions. This view, combined with the aforementioned theories on trade credit, suggest that trade credit provision could have a material impact on firms' inventory decisions.

An alternative view, implied by the seminal work of Modigliani and Miller (1958), is that when the financial market is relatively efficient, trade credit, as a means of financing, does not necessarily have a significant impact on inventory, an operational decision. Under this view, trade credit does not play an essential role in financing inventory and can be readily substituted by other financing sources (for example, bank credit).

The above two competing schools of thoughts create ambiguity with regards to the impact of trade credit provision on retail inventory levels. Given the importance of trade credit as a financial instrument and inventory as an operation decision, this paper aims to empirically investigate the causal impact of trade credit provision on retail inventory and its magnitude, which is absent in the literature.

To achieve this, we overcome two empirical challenges. The primary challenge arises from the embedded simultaneity between the trade credit and the inventory decisions (Haley and Higgins, 1973). Presence of such simultaneity yields an endogeneity bias in estimates (Roberts and Whited, 2013). We overcome this challenge by exploiting a policy intervention by the French Government in 2008: The Law on the Modernisation of the Economy (LME). This policy imparted an exogenous shock to trade credit provision by imposing a 60-day maximum repayment period for supplier-provided trade credit. As this law only regulated trade credit, but not inventory, any change in inventory associated with this regulation allows us to identify the causal relationship between trade credit and inventory. Furthermore, to control for the confounding effect of other shocks that overlap with LME, for instance, the 2008 Global Financial Crisis (GFC), we implement a triple difference-in-differences (DDD) identification strategy that exploits variation in the LME effect on the two decisions across sectors in France and the rest of the European nations.

The second challenge relates to the central requirement in studies using policies as exogenous shocks: constructing a credible counterfactual estimate for the outcomes of policy-affected entities in the absence of the policy intervention. The wide heterogeneity among European firms on various dimensions complicates the construction of a control group that can yield such a counterfactual estimate. In particular, in our context, the commonly used approaches (such as the propensity score matching or coarsened exact matching) fail to eliminate the confounding impact of heterogeneity in group characteristics on the policy's impact on focal outcome variables. Specifically, under these approaches, we find strong empirical evidence towards violation of the central "parallel trends" assumption required for linear differences-in-difference (DD) estimation. We overcome the aforementioned challenge in constructing credible counterfactuals by using the relatively nascent *Synthetic Control* (SC) methodology (Abadie et al., 2010; Xu, 2017), which has been described as "*arguably the most important innovation in the policy evaluation literature in the last 15 years*" (Athey and Imbens, 2017). We adapt and extend the SC methodology to meet the demands of our large-size sample and DDD identification strategy. We validate our empirical strategy through two placebo analyses. One builds on selecting a pseudo treated nation among the control group nations and the other selects a pseudo treatment year. Jointly, these placebo analyses ratify our DDD SC-based estimation strategy.

By considering the coverage of LME and the number of control firms in other European countries that can be used for SC construction, we identified four French retail sectors, covering more than 5,000 firms, as our main sample. Across these four sectors, the retailers' pre-LME trade credit use varied considerably compared to the 60-day directive under the LME. As a result, in the post-LME period, the regulation constricted these retailers' trade credit usage to varying degrees, with some sectors showing a significant decline, and others continuing at the pre-LME levels. We find that as long as the LME leads to a significant drop in the retailers' trade credit usage in the post-LME period, there is consistent evidence of significant decline in these retailers' post-LME inventory level. For example, in the hardware retail sector, we observe a significant decline of trade credit usage in the post-LME period. In this sector, we find that the LME reduced firms' average accounts payable by 14.1 days (approximately 16% of the pre-LME level). Correspondingly, firms also exhibited a significant reduction of 15.9 days of inventory, which is more than 11% of their inventory days before the LME. Together, these estimates suggest that a 1% reduction in trade credit resulted in a 0.67% decrease in inventory levels, establishing a sizable causal link between trade credit and inventory. In the other three retail sectors of our main sample, we observe mixed results (weak or no effect) regarding the impact of LME on trade credit reduction.[1] Here, it is

[1] The absence of significant LME impact on the retailer's trade credit usage in these three sectors is likely due to a mixture of low trade credit level before the LME and poorer SC quality. See Section 6.3 for a more detailed discussion and Online Appendix D for the technical details.

important to note that for the identification of the causal relationship between trade credit usage and inventory level, it is imperative that the LME regulation imparts a significant exogenous shock on the retailers' trade credit usage. Not surprisingly, we observe mixed results regarding the change on inventory level following the LME when considering the full sample of the three remaining sectors. However, when focusing on the sub-sample of retailers who are likely to be significantly affected in trade credit usage due to LME (e.g., those with days of oustanding payables to be greater than 90 days in the pre-LME period), we find robust evidence of similar causal link between trade credit and inventory as in the hardware sector. We further validate this finding through a variety of robustness tests including alternate sub samples, alternate variable definitions, alternate econometric models, and alternate sample with an extended period.

Finally, we note that while the finding that restrictions on trade credit usage causes inventory decline is consistent with the extant theories on trade credit, there could exist alternative explanations to this phenomenon. For example, facing pressure from trade credit reductions, firms could invest in improving operational efficiency and thus require less inventory without significantly sacrificing key financial performance. To shed light on the validity of these competing theories, we applied the same DDD SC method to two key financial metrics: retail revenue and gross profit. We find that in the hardware retail sector, the LME-caused trade credit and inventory reduction led to a 15.5% reduction in sales revenue and a 3.5% decline in gross profit. This finding provides additional support to theories on trade credit, which imply that restricting trade credit usage is likely to result in inventory under-investment and poorer financial performance.

The contribution of this paper is two fold. First, to the best of our knowledge, this is the first empirical paper that establishes the causal relationship between trade credit provision and inventory, offering direct evidence that trade credit is an indispensable source of retail inventory financing. As such, the paper complements the existing theories on trade credit, and more generally, the OM-Finance Interface literature that emphasizes on the importance of joint operational and financial decision-making.

Second, our findings also bear important managerial and policy implications. By identifying the causal relationship between trade credit, inventory and the related financial metrics, our results suggest that when equipped with flexibility to design their trade credit granting policies, suppliers need to take the associated operational and financial consequences into consideration. Similarly, our study also caution policymakers that regulations which limit the use of trade credit, such as the LME in France, with the intention to alleviate suppliers' financial burdens by accelerating payment,[2] could have a negative impact on the downstream party in the supply chain. As an

[2] Similar regulations in the past decade or so includes two initiatives in the US, QuickPay and SupplierPay (Mandelbaum, 2011; White House, 2014), the European Union Directive on Payment and UK's Prompt Payment Code (Council of the EU, 2011; UK BEIS, 2016).

efficient supply chain requires coordination of different parties, such unintended consequences could possibly hurt the overall supply chain efficiency.

2. Literature and Theoretical Background

Our paper is closely related to two streams of literature: (i) the theoretical literature on trade credit on which our research question is grounded; and (ii) the empirical literature on trade credit and inventory, which our paper complements.

The theoretical literature on trade credit predominately studies the supplier's advantage in extending credit to their buyers, relative to financial institutions. We refer the readers to Peura et al. (2017) for a summary of such theories. Within this literature, our paper is most relevant to those that directly associate trade credit with inventory decisions. Based on the EOQ model, Haley and Higgins (1973) highlight the importance of coordinating trade credit terms and inventory decisions. They find that shorter trade credit terms lead to a less-than-optimal inventory level. Recently, Kouvelis and Zhao (2012) and Yang and Birge (2018) show that, in a selling-to-the-newsvendor model, trade credit serves as an effective demand risk-sharing mechanism and induces the retailer to carry an inventory level closer to the system-optimal one. The direct implication of this theory is that, if trade credit is made unavailable or restricted to a below-equilibrium level, the retailer will stock lower inventory, in turn hurting supply chain efficiency. This implication is also corroborated by some other theories of trade credit. For example, an implication of Babich and Tang (2012) is that limiting trade credit may induce downstream buyers to carry less inventory due to concerns over product quality. Similarly, Biais and Gollier (1997) also imply that restricting trade credit usage weakens its information role and thus results in downstream firms' lower investment in inventory. Combined, these theories suggest that trade credit creates value in the supply chain and plays an indispensable role in inventory decisions that cannot be completely replaced by other financial means (e.g., bank credit) and/or operational levers (e.g., wholesale price).

An alternative view follows the line of reasoning by the seminal work of Modigliani and Miller (1958): when the financial market is relatively efficiency, the usage of trade credit, a specific source of financing, should have a negligible impact on operational decisions such as inventory. Given the two possibilities, the causal relationship between trade credit and inventory is ambiguous. Thus, the existence of this relationship and its magnitude are best answered empirically.

In the empirical literature, our paper is related to both studies on trade credit and inventory. Earlier empirical research on trade credit, such as Petersen and Rajan (1997), use survey or observational data to identify the determinants of trade credit terms, and they indirectly support the various roles of trade credit. In OM, Cai et al. (2014) find that bank and trade credit can be either substitute or complement in inventory financing. Lee et al. (2018) examine the correlation between trade credit, competition, and firm performance. In the finance literature, Barrot (2016) exploits French legislation that limits trade credit terms in the trucking sector to show that shorter terms lower barriers for new entrants. Similarly, exploiting SupplierPay – the US Government initiative that expedited government payments to their suppliers – Barrot and Nanda (2016) find that paying firms faster improves supplier balance sheets and their willingness to hire more employees. Both papers focus on the financial impact of trade credit usage and its subsequent effect on the economy, in service industries. Despite their significant role in the economy, these industries differ from our setting as inventory considerations are absent when granting trade credit.

The paper that is most closely related to ours is Breza and Liberman (2017), which, to the best of our knowledge, is the only paper that empirically analyzes the impact of trade credit on the operational relationships between suppliers and buyers. Exploiting an agreement between the Chilean Government and a large retail chain, the authors find that restricting trade credit led to a drop in the probability of trade, the price, and total sales between the large retailer and their small suppliers. Our work complements Breza and Liberman (2017) along two dimensions. First, our focus is on the downstream firm's inventory level, which is of particular importance to the OM community (Cachon and Terwiesch, 2008). Second, our findings generalize beyond a single firm to a large number of retailers.

Our paper is also related to the empirical literature on inventory. Gaur et al. (2005), Rumyantsev and Netessine (2007), and Chen et al. (2007) offer comprehensive empirical analysis of inventory performance and its drivers in different industry sectors. Kesavan et al. (2010) focus on how inventory informs sales forecast. Jain et al. (2013) focus on inventory performance in a global sourcing setting. Hendricks and Singhal (2009) and Alan et al. (2014) show an association between inventory performance and equity returns. Wu et al. (2018) study the role of credit lines in inventory investment. Our paper contributes to this literature by identifying the causal relationship between trade credit and inventory.

Broadly speaking, our paper belongs to the literature of the interface of operations, finance, and risk management (Babich and Kouvelis, 2018). The OM-Finance interface literature has recently seen an increasing number of empirical works, including Osadchiy et al. (2015), Tunca and Zhu (2018), Serpa and Krishnan (2017), Wang et al. (2017), Xu et al. (2018), and Corsten et al. (2018). Methodologically, our paper complements this literature by introducing an adaptation of the SC methodology suited for large-size samples. Also, it emphasizes the importance of considering the operations-finance interaction in public policymaking.

3. Empirical Setting: Identification and Estimation Strategy

In this section, we first describe the French Government policy that imposes a ceiling on trade credit period. Next, we provide details of our empirical strategy that leverages this policy intervention to identify and estimate the impact of trade credit on inventory decisions.

3.1. French Government Policy Intervention to Restrict Trade Credit

In August 2008, the French Government enacted the Law on the Modernisation of the Economy (LME) with the aim of stimulating commerce by removing structural obstacles for SMEs (Grall and Lamy, 2009). Among other things,[3] the LME expanded an extant ceiling for payment terms of trade credit, introduced in 2006 for only the trucking industry (Barrot, 2016), to all sectors. Specifically, the law restricted payment of trade credit to a maximum of 60 days (net) or 45 days end-of-month for transactions post 1 January 2009 with pre-agreed terms between a supplier and buyer. The law stipulates that French suppliers and buyers should comply with the mandatory payment terms, irrespective of their counterparty's location–foreign or domestic (Vincent, 2009).[4]

Naturally, not all sectors were equally constrained by the 60-day trade credit restriction imposed by the LME. Sectors where firms engaged in longer payment terms as a norm were most affected. For example, in the Hardware Retail sector, the average Days Payable Outstanding (DPO, which equals Accounts Payables/Costs-of-Goods-Sold $\times$ 365) was 85.6 days before the LME.[5] Firms in some sectors were largely unaffected by the LME since their terms of trade were well below the 60-day ceiling. For example, firms in the Fruit and Vegetable Retail sector on average repaid trade credit within 36.9 days even before the LME.

In Figure 1, we present model-free evidence of the impact of the French LME policy intervention on the trade credit received by the retailers.[6] We examine trends in both sectors that were "affected" by the LME (LME-affected sectors, defined as sectors with the average DPO, in 2008, of that sector's firms in France as at least 60 days) and sectors that most likely remained "unaffected" (LME-unaffected sectors, those with the sector average DPO in 2008 of less than 45 days). Panels (a) and (b) respectively capture the trend in retailers' DPO. The vertical axis shows average DPO as a unit-less ratio, after normalizing each firm's DPO by its 2008 value. The horizontal axis covers an eight-year period spanning 2006 to 2013, which is split into the pre- and post-LME phases. The vertical-dashed line highlights the end of the pre-LME phase with 2008 and the first financial

[3] The LME also introduced guidelines on areas unrelated to trade credit, such as improving the standing of Paris as a financial center, simplifying the business environment for entrepreneurs, and incentivizing innovation.

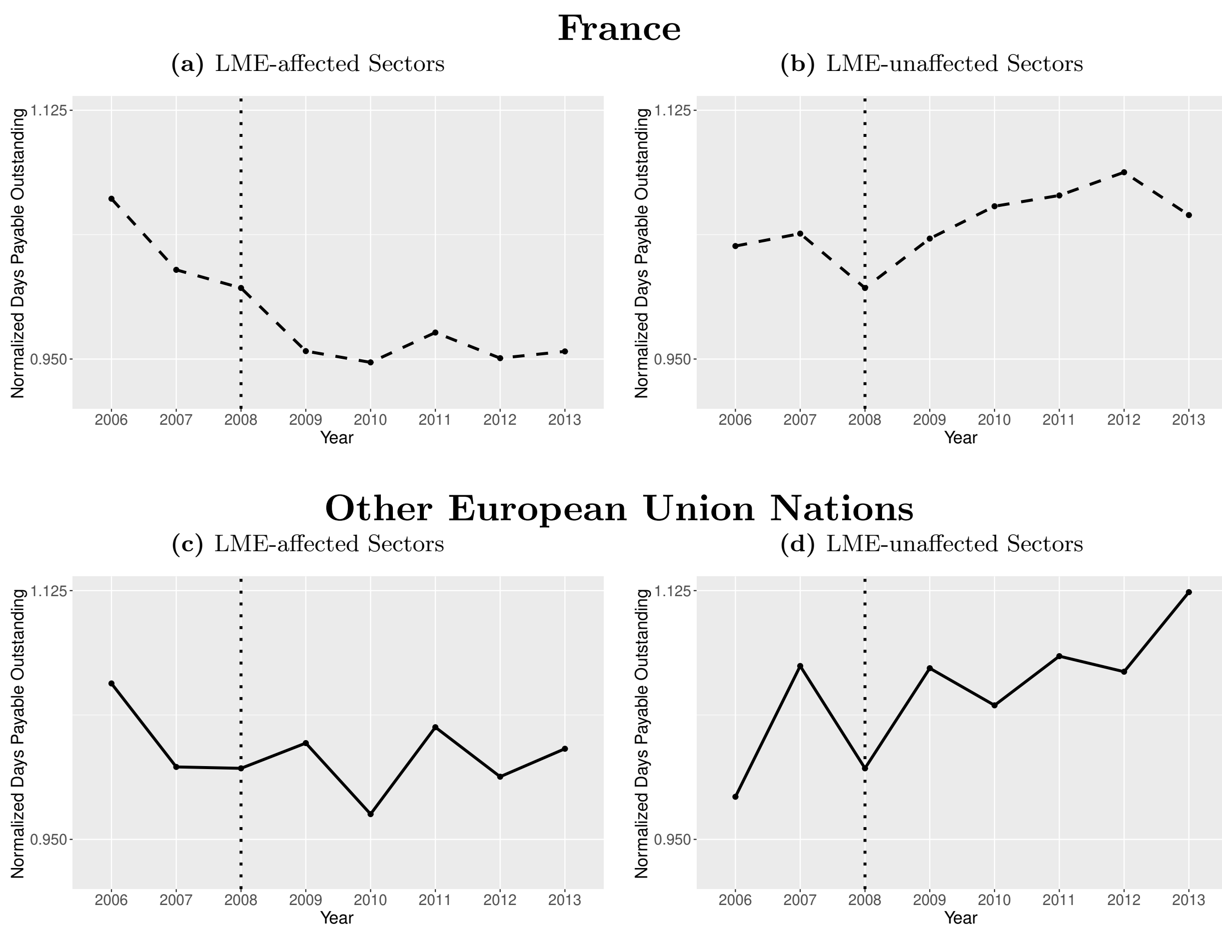
[4] Marolleau (2016) notes that administrative penalties for violation of the LME terms can be levied "when the entire business relationship takes place in France" even if the contract between two firms is governed by the law of a foreign state.

[5] The LME imposed varying ceilings on the following five sectors to account for their strong seasonality imprint: i) agricultural equipment (ceiling of 110 days EOM); ii) sporting goods – snow sports (90 days); iii) leather goods (54 EOM); iv) watches, jewellery and goldsmithing (74 days); and v) toys (75-95 days depending on time of year). See Decree n^{o} 2015-1484 of November 17, 2015.

[6] Figure A1 in Online Appendix A presents an equivalent model-free evidence of the LME impact on inventory levels.

Figure 1 Trends in Payment Terms: Pre and Post LME

This figure shows trends in average DPO received by the retailers in the LME-affected and LME-unaffected sectors over an eight-year period between 2006 and 2013. Panels (a) and (b) present these trends for France. Panels (c) and (d) present these trends averaged across six European nations. The vertical axis shows average DPO usage after normalizing each firm's DPO usage by its 2008 value.



year of the post-LME phase in 2009.[7] In panels (c) and (d), we present trends in trade credit usage in the rest of Europe by averaging respective trends in the aforementioned LME-affected and LME-unaffected sectors across six nations: Finland, Germany, Italy, Portugal, Spain, and Sweden.[8]

We find that French firms in the LME-affected sectors (panel (a)) experienced a persistent reduction in trade credit post-introduction of the LME (i.e, between 2009 and 2013). In contrast, similar firms in other European nations (non-French nations) experienced a small increase in trade

[7] We account for the in-year enactment of the LME by mapping financial reporting to the calendar year as follows: for firms with fiscal year ending in the period between July of year X to June of year X+1, the financial numbers are mapped to the year X.

[8] As detailed in §6, we find these six countries to be more comparable to French firms than the other European nations.

credit over this period (panel (c)). For the LME-unaffected firms, in both France and the non-French nations, we find, on average, an increase in DPO over time.

We next describe our identification strategy, which builds on the presence of the LME-affected and LME-unaffected sectors to identify the impact of trade credit on inventory decisions.

3.2. Identification Strategy

We face a key challenge in identifying the impact of trade credit on inventory levels due to the embedded *simultaneity* in making trade credit and inventory decisions, which induces endogeneity bias (Roberts and Whited, 2013). We exploit the exogenous shock imparted on trade credit provisioning decisions by the LME to overcome this simultaneity-in-decisions challenge. The law imposed an exogenous contraction in payment terms that, in turn, induced firms to make optimal adjustments to their inventory levels, which enables identification of the impact of trade credit on inventory levels.

In evaluating the impact of a policy intervention, a common concern is the presence of contemporaneous shocks that can affect focal variables of interests. For instance, in our context, the introduction of the LME overlaps with the aftermath of the 2008 GFC – a period with a severe credit crisis which also spilled over to firms' trade credit and inventory decisions (Alessandria et al., 2011). Henceforth, for brevity sake, we refer to all contemporaneous shocks as GFC – the prominent known shock that overlaps with LME. Next, we discuss our identification strategy to estimate LME impact while controlling for the GFC effect.

Identification Mechanism: A triple difference-in-differences (DDD). Our identification strategy rests on two observations. One, the LME constrained trade credit terms for France-based firms, but not for the other European firms.[9] Second, as highlighted above, there are sectors within France that were unaffected by the LME. Combining these observations, we implement a DDD strategy to isolate the effect of the LME on trade credit provisioning and resultant inventory decisions. To facilitate explanation, we illustrate our strategy using the following simplified setting.

Consider a two-period setting with comparable nations F and E. Nation F implemented the policy `LME`, which affected sector A, but not sector U. We use the short notation $s \in \{FA, FU, EA, EU\}$ to denote affected and unaffected sectors across the two nations. Let the dependent variable y of firm i in sector s be defined by

$$\mathbb{E}[y_{ist}|s,t] = \beta_s + \beta_t + \beta_{s,t} + \beta_s^{\texttt{LME}} \times \texttt{IS\_LME\_AFFECTED} \times \texttt{IS\_POST\_LME} + \beta_s^{\texttt{GFC}} \times \texttt{IS\_POST\_LME}, \qquad (1)$$

[9] We note that the EU introduced a similar trade credit-tightening directive to its members that had to be integrated into their respective national laws by March 16, 2013 at the latest (Directive 2011/7/EU, the Late Payment Directive (Council of the EU, 2011)). The actual implementation, however, stretched up to 2014 (e.g., Germany adopted the directive in July 2014).

where β_s captures the sector-specific time-invariant component, β_t denotes the sector-invariant common temporal component, $\beta_{s,t}$ captures the sector-specific temporal component, $\beta_s^{\mathtt{GFC}}$ captures the sector-specific impact of the confounding GFC shock that overlaps with `LME` commencement, $\beta_s^{\mathtt{LME}}$ captures the LME-policy's sector-specific impact, `IS_LME_AFFECTED` is a dummy variable that is set to 1 for the LME-affected sector A and 0 otherwise, and `IS_POST_LME` is a dummy variable that is set to 1 for the post-LME period (abbreviated by $Post$) and 0 for the pre-LME period (Pre).

Using Eq. (1), we derive the double-differences for sectors A and U (A: DD and U: DD respectively) and triple-difference (DDD) estimates as

$$\{\mathbb{E}[y_{ist}|s=FA, t=Post] - \mathbb{E}[y_{ist}|s=FA, t=Pre]\}$$
$$- \{\mathbb{E}[y_{ist}|s=EA, t=Post] - \mathbb{E}[y_{ist}|s=EA, t=Pre]\} = \beta^{\mathtt{LME}}_{s=FA} + \beta^{\mathtt{GFC}}_{s=FA} - \beta^{\mathtt{GFC}}_{s=EA}, \tag{A: DD}$$

$$\{\mathbb{E}[y_{ist}|s=FU, t=Post] - \mathbb{E}[y_{ist}|s=FU, t=Pre]\}$$
$$- \{\mathbb{E}[y_{ist}|s=EU, t=Post] - \mathbb{E}[y_{ist}|s=EU, t=Pre]\} = \beta^{\mathtt{GFC}}_{s=FU} - \beta^{\mathtt{GFC}}_{s=EU}, \tag{U: DD}$$

$$\Big[\{\mathbb{E}[y_{ist}|s=FA, t=Post] - \mathbb{E}[y_{ist}|s=FA, t=Pre]\} -$$
$$\{\mathbb{E}[y_{ist}|s=EA, t=Post] - \mathbb{E}[y_{ist}|s=EA, t=Pre]\}\Big]$$
$$- \Big[\{\mathbb{E}[y_{ist}|s=FU, t=Post] - \mathbb{E}[y_{ist}|s=FU, t=Pre]\}$$
$$\{\mathbb{E}[y_{ist}|s=EU, t=Post] - \mathbb{E}[y_{ist}|s=EU, t=Pre]\}\Big]$$
$$= \beta^{\mathtt{LME}}_{s=FA} + [\beta^{\mathtt{GFC}}_{s=FA} - \beta^{\mathtt{GFC}}_{s=FU}] - [\beta^{\mathtt{GFC}}_{s=EA} - \beta^{\mathtt{GFC}}_{s=EU}]\,. \tag{DDD}$$

From Eq. (A:DD), we see that if the GFC effect is constant across the two nations in the LME-affected sector (i.e, $\beta^{\mathtt{GFC}}_{s=FA} = \beta^{\mathtt{GFC}}_{s=EA}$), then the second difference estimate would suffice in identifying the impact of the LME on the focal variable y. In our setting, this assumption would imply that the GFC impact is constant across all European nations. In the absence of any concrete evidence, making such an assumption would be too restrictive.

Comparatively, in the case of the DDD estimate (Eq. (DDD)), a relatively less restrictive assumption of a constant relative GFC impact between the LME-affected (A) and LME-unaffected (U) sectors across the two nations *enables* identification of the LME effect. After controlling for the nation-level differences of the GFC impact, the relative difference in its impact between sectors seemingly would reflect the differences in these sectors' business environments (for example, the demand uncertainty of products being traded). Our placebo analyses in Section 7 provide support for the applied DDD strategy. Here, it is important to note that the DDD strategy retains the focal variable comparison (trade credit or inventory) between firms in alike sectors (i.e., between French and non-French firms in LME-affected and LME-unaffected sectors).

In summary, the combination of an exogenous shock due to the LME that resulted in trade credit tightening and the DDD framework enables us to identify the role of trade credit in inventory decisions while controlling for the aforementioned identification challenges.

3.3. Estimation Strategy: Synthetic Controls

An important step in estimating the causal effect of a policy intervention using a difference-in-differences (DD) framework is to construct a credible counterfactual path of policy-affected entities (treated firms) in the absence of policy intervention (Athey and Imbens, 2017). In our context, the counterparts of LME-affected sectors in non-French nations provide a natural pool of control entities to construct the counterfactual path. However, among these non-French nations, there is no clear choice that best emulates French firm characteristics and their business environment overall. For instance, consider Figure 2 that presents comparisons of key firm-level (panels (a)–(f)) and macro-level (panels (g)–(i)) variables between France and select non-French nations for the hardware, paints and glass retail sector (NACE code: 47.52).[10]

As shown, firms in non-French nations exhibit contrasting patterns across variables in comparison to French firms. For instance, Italian firms exhibit much higher levels of account payables (panel (a)) compared to the French firms, but lower fixed assets (panel (e)), which typically proxies for a firm's investment in supply chain information technology capabilities (Gaur et al., 2005; Jain et al., 2013). In contrast, we observe the opposite characteristics from German firms: much lower payables and higher investment in fixed assets.

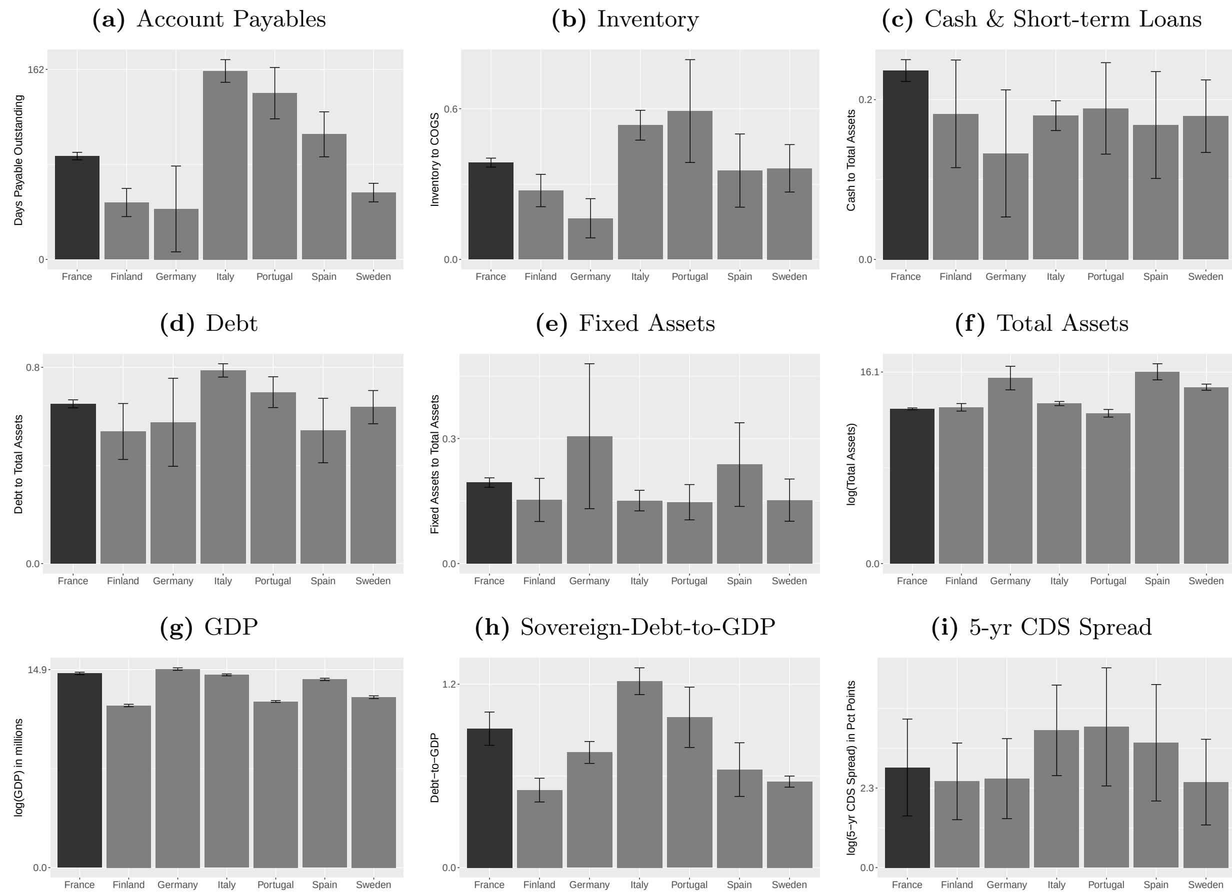
The aforementioned contrasting patterns of non-French nations suggest that, instead of an ad-hoc selection of firms in one of the non-French nations as control entities, a more appropriate choice would be a blend of firms in these nations. For instance, hypothetical firms with blended characteristics of German and Italian firms may better mirror French firms. The Synthetic Control (SC) estimation approach enables such a comparison by constructing an "optimally" blended entity based on pre-policy period characteristics (Abadie et al., 2010; Athey and Imbens, 2017). Moreover, in contrast to the linear DD approach that requires a constant difference in the impact of the unobservable confounders (commonly referred to as the "parallel trends" assumption), the SC estimation permits for time-varying effects of such confounders.

Given the above-cited benefits of the SC methodology in our context, we employ it as our primary estimation method. In Section 5, we provide an overview of the SC estimation and its adaptation to our context. As robustness, in Section G of the online appendix, we provide results of a restrictive linear DDD estimation.

[10] NACE (Rev.2) codes are created and maintained by Eurostat to classify economic activities of businesses Europe-wide.

Figure 2 Firm and Country characteristics

Bar chart of firm- and macro-level characteristics by country with their associated 99% confidence intervals. Firm characteristics (a, b, c, d, e, f) are based on reported financials in 2006. Inventory is scaled by Cost-of-Goods-Sold. Cash & Short-Term Loans, Debt, and Fixed Assets are scaled by total assets. Total Assets is reported in log form. Statistics of macroeconomics variables are computed using an eight-year period (2006–2013). GDP is reported as the log of GDP in millions, Debt-to-GDP is the ratio of total federal debt to GDP, and the Credit Default Swaps (CDS) spread is given as the log of the 5-yr sovereign CDS spread.



4. Data Description and Variable Construction

We construct the sample for this study by compiling data from three distinct sources: (i) European firm-level financials from the Bureau van Dijk (BvD) database; (ii) macroeconomic country-level data from the OECD; and (iii) credit default spreads of European sovereign bonds from the Markit CDS database.

4.1. Sample Data Construction

We extract European firm financials–public and private–from the BvD database, which is originally sourced from business registers maintained by local chambers of commerce. Kalemli-Ozcan et al.

(2015) notes that the BvD data covers up to 75-80 % of the economic activity reported in Eurostat,[11] making it a preferred dataset for coverage of public and private EU-based firms. The database provides information on a variety of balance sheet and income statement variables.

In this study, we focus on retail firms, which are identified by NACE codes 45.11, 45.19, 45.32, 45.40, and all codes within group 47. BvD covers a total of 66,758 firms across these retail sectors. The focus on retailers is due to two reasons. First, as the firms in the last tier of a supply chain, these firms receive trade credit from upstream suppliers, but do not extend trade credit to their buyers (end consumers). This enables us to isolate the effect of trade credit provision from upstream firms and avoid any confounding factors due to firms' own considerations for extending trade credit to downstream partners. Second, unlike other sectors such as manufacturing, retail firms largely carry finished goods inventory. This allows us to focus on the impact of trade credit on the inventory directly associated with it.

Our study covers an eight-year period between 2006 and 2013. This choice of period is driven by two reasons. First, BvD's pre-2006 coverage is quite limited. For example, the number of retailers covered in 2005 is half that of 2006. Second, the EU's Late Payment Directive mandated its members to bring their respective laws on trade credit management in line with Union regulations by 2013 (Council of the EU, 2011). To avoid any confounding effects from this EU directive, we omit data post 2013.

We make a few choices towards the inclusion of sectors/firms in the final sample. These choices are due to three reasons: (i) BvD's data coverage characteristics; (ii) minimizing the impact of outlier firms (Barrot, 2016); and (iii) improving the likelihood of constructing good-quality SCs.[12] First, we exclude firms that either have missing information or reported zero value for trade credit (accounts payable) and inventory levels before LME. We observed that small firms exhibit a high propensity for missing data (over 55%), potentially on the account of inconsistencies in data reporting of these small firms or in coverage efforts of intermediaries through whom BvD periodically complies data. To adjust for such data coverage issues, we focus on firms with total assets of at least US$50,000, yielding 40,464 firms.

Second, we deal with outlier firms as in Barrot (2016). We remove firms that (a) report having trade credit periods of longer than a year and, analogously for our study, (b) report having more than one year's worth of inventory. The former condition indicates that the concerned firm is most likely under financial duress, which may also spill over to its other operational decisions. The latter condition focuses on excessively high inventory levels, which again suggests operational decisions

[11] Eurostat provides economic activity statistics based on national censuses but does not provide the underlying firm level data itself.

[12] In Section 7, we show that none of our core findings are sensitive to the choices made in sample construction.

being undertaken under unusual firm-specific idiosyncratic business conditions. Finally, to limit the impact of extreme values, while accounting for sector- and country-specific characteristics, we winsorize all our variables at the country-sector level using a 5% threshold (Verbeek, 2008). The resultant sample has a total of 34,427 firms with an average DPO of 94.0 (respectively, 92.6) days among the French (respectively, non-French) firms.

Third, for enabling construction of well-fitted SCs, we focus on the LME-affected sectors that have a presence of at least 10 firms in at least five non-French nations. Note that the higher the SC's fit-quality with the treated entities, the better the credibility of the counterfactual estimates. The SC's fit-quality is naturally determined by the size and shape of the convex hull formed by the control entities. Thus, for firms in treated sectors that have a limited presence in non-French nations, the SC method may not be able to construct a well-fitted SC. We provide the count of non-French nations for each of the LME-affected sectors in Table A9 of Online Appendix F. Among all the LME-affected retail sectors, we find that the following four sectors meet the aforementioned criteria: (i) Retail trade of motor vehicle parts and accessories (NACE code: 45.32); (ii) Retail sale of hardware, paints, and glass in specialized stores (47.52); (iii) Retail sale of furniture, lighting equipment and other household articles in specialized stores (47.59); and (iv) Retail sale of clothing in specialized stores (47.71). We find that none of the LME-unaffected sectors by itself satisfy the aforementioned criteria; thus, we pool firms in the following sectors to define our LME-unaffected sector: (i) Retail sale of fruit and vegetables in specialized stores (47.21); (ii) Retail sale of meat and meat products in specialized stores (47.22); (iii) Retail sale of fish, crustaceans and mollusks in specialized stores (47.23); and (iv) Retail sale of automotive fuel in specialized stores (47.30).

The collective application of these inclusion choices yields the main analysis sample, with 7,103 firms in the four LME-affected sectors. Out of these, 5,494 firms are in France (treated firms) and 1,609 firms in non-French nations (control firms). Similarly, for the LME-unaffected sectors, the sample includes a total of 2,517 firms, among which 2,125 are French and 392 are non-French. We note that a higher representation of French firms in our sample is inline with extant studies (Dai, 2012). This seems to be driven by both industry-structure differences and BvD's varying coverage across the EU nations.[13] We cannot ascertain the extent of over representation due to BvD's varying coverage. In line with this data limitation, in this study, we interpret the obtained treatment estimates as the Average Treatment effect on the Treated entities (ATT) rather than Average Treatment effect on the population (ATE).

[13] Kalemli-Ozcan et al. (2015) reports that BvD covers 85.2% of total firms in French economy, 63.6% of firms in Germany, and 41.7% of firms in Spain.

4.2. Variable Construction: Dependent Variables

Trade Credit (DPO). Following the literature (Chod et al., 2019a), we use accounts payable (BvD variable: CRED) to measure trade credit provided to retailers. Formally, for a firm k in period t we measure the amount of trade credit received from its suppliers by Days Payable Outstanding (DPO) as:

$$\mathrm{DPO}_{kt} = \frac{\mathrm{CRED}_{kt}}{\mathrm{MATE}_{kt}} \times 365, \tag{2}$$

where MATE is the BvD variable for Cost of Goods Sold (COGS).

Inventory Level (DSI). We measure a firm's inventory stocking decision using a relative inventory measure. Similar to Rumyantsev and Netessine (2007), we normalize inventory (BvD variable: STOK) by COGS, deemed as a proxy of a firm's demand (Kesavan et al., 2010; Jain et al., 2013). Thus, one can interpret this relative inventory measure as a firm's optimal inventory stocking levels to expected demand. Formally, in line with the trade credit measure (DPO), we construct Days Sales of Inventory (DSI), as

$$\mathrm{DSI}_{kt} = \frac{\mathrm{STOK}_{kt}}{\mathrm{MATE}_{kt}} \times 365. \tag{3}$$

4.3. Variable Construction: Covariates

For each of the two dependent variables, we include explanatory variables that are commonly studied in the literature to populate covariate-vector Z of observables for SC construction (refer to Online Appendix B for details). The vector Z includes the following firm-level covariates: *log* of Total Assets (TA, BvD: TOAS), size-normalized measures of COGS, Gross Profit (GP = OPRE − MATE), Fixed Assets (FA, BvD variable: FIAS), and Long-Term Debt (DBT = TSHF − SHFD).[14] Note that variables on COGS, profit-margins, and fixed-assets have been studied in inventory-related studies respectively as a proxy for demand, cost of underage, and capital investment in supply chain information technology (Gaur et al., 2005). These variables are also used in the trade credit empirical literature, along with variables on long-term debt and total assets. In addition, to capture the embedded simultaneity between the trade credit and inventory decisions, we include DSI and DPO variables respectively in the Z vector of DPO and DSI models for SC construction.

We complement the aforementioned firm-level covariates by augmenting the Z vector with the following country-level macroeconomic variables: *log* of GDP (OECD: GDP), sovereign-debt-to-GDP SDGDP (General Government Debt), and the log of the 5-year CDS spread (Markit: spread5y). Collectively, these variables provide a proxy for the overall strength of the financial and business environment (Ang and Longstaff, 2013). In Table 1 we provide summary statistics of the above defined firm-level variables.

[14] BvD variables OPRE, TSHF and SHFD measure respectively operating revenue, total shareholder funds and liabilities, and total shareholder funds.

5. Synthetic Control Estimation with Multiple Treatment Units

The central principle of the SC method is that a linear combination of control entities provides a better comparison for a treated entity than any single control. An SC unit is constructed for each treated entity by assigning weights to the available control entities. The weights are derived using pre-policy intervention data and assigned to minimize the difference between the treated entity and the associated SC with respect to both the outcome variable and observed covariates. In Online Appendix B, we provide an overview of the SC estimation methodology using a simple example of a single treatment unit. Below, we discuss its conceptual extension to multiple treatment units setting like ours. The implementation details on how we compute the DDD estimate in a large sample setting are provided in Online Appendix C.

Multiple Treatment Units. Two approaches are adopted in the literature to extend the single treatment unit SC methodology to the setting with multiple treatment units. The first approach is to aggregate both the treated and control entities at a higher level, which reduces the multiple treated entities to a single treated entity (Abadie et al., 2010). In our setting, implementing this approach would entail aggregation at the country level which, in turn, would considerably reduce the analysis power due to a small country-level sample.

The second approach advocates for the construction of an SC for each treated entity and using it to compute the treatment effect $\widehat{\tau}$ for each of the treated entities. Specifically, we denote the individual treatment effect for firm k at period t as $\widehat{\tau}_{kt}$, where $t = 1, \ldots, T$ with the policy coming into force in $t = T_o + 1 > 1$ period. Next, we compute the average treatment effect $\overline{\tau}$ by taking the weighted average of these individual treatment effects. The weights for each treated entity can be computed in multiple ways, including: (i) a simple equal-weighted allocation scheme (Xu, 2017); (ii) entity-level characteristics (Kreif et al., 2016); and (iii) a 'goodness-of-fit' criterion for constructed SCs (Acemoglu et al., 2016), the last of which we follow in this paper. Specifically, we compute the average treatment effect $\overline{\tau}$ of the LME as:

$$\overline{\tau} = \frac{\sum\limits_{k \in \text{Treatment Group}} \widehat{\sigma}_k^{-1} \sum\limits_{t=T_0+1}^{T} \widehat{\tau}_{kt}}{\sum\limits_{k \in \text{Treatment Group}} \widehat{\sigma}_k^{-1}} \tag{4}$$

where $\widehat{\sigma}_k$ measures the goodness-of-fit using the Root Mean Square of Prediction Error (RMSPE) between the constructed SC and treated entity based on the pre-intervention period values of the outcome variable and covariates. Formally, $\widehat{\sigma}_k$ is defined as

$$\widehat{\sigma}_k = \sqrt{\frac{\sum_{t=0}^{T_0} \widehat{\tau}_{kt}^2}{T}}. \tag{5}$$

Intuitively, the $1/\widehat{\sigma}_k$ scaling factor provides larger weights to treatment effects that are estimated using better-fitted SCs. Following Acemoglu et al. (2016), we define a treated entity k as having a poor fit if $\widehat{\sigma}_k \geq \widehat{\sigma}\sqrt{3}$ where $\widehat{\sigma} = \sum_{k=1}^{K} \widehat{\sigma}_k / K$ is the average goodness-of-fit across all treated entities, and K is the number of firms in the treatment group. With this framework, we separately estimate the impact of the LME on trade credit and inventory levels for each of the four LME-affected sectors in our sample. This minimizes the effect of any sector-specific issues in using accounting variables to proxy for the actual contractual terms on trade credit repayment (Barrot, 2016). Our unit of analysis is the firm-year level, $k \times t$. To cater to the unique requirements of our empirical study, we implemented two modifications to the classic SC estimation methodology: (i) a sampling approach to manage the large sample size computational complexity and (ii) an extension to implement the DDD identification strategy. See Online Appendix C for the technical details.

6. Results

We estimate the LME impact on the two focal outcome variables–trade credit (DPO) and inventory level (DSI)–by constructing separate SCs with each of them. Below, we present the findings using the analysis of Sector 47.52 ("retail sale of hardware, paints, and glass in specialized stores"), which yields the best-fit SCs.[15] We provide the findings of the remaining three sectors in Section 6.3.

Table 1 presents summary statistics of key variables using the pre-LME period observations. The median DPO among the French firms is 82.7 days, much above the 60-days LME restriction. Compared to the all-sectors relative trend in average trade credit usage across the French and non-French firms (94.0 versus 92.6 days), we find this sector exhibits an opposite trend of lower average trade credit usage among the French firms relative to their non-French counterparts (85.6 versus 119.8 days). In contrast to the pool of all control entities, constructed SCs exhibit characteristics that are more comparable to that of the treated entities. For instance, the absolute difference in the average DPO (respectively, DSI) is only 2.6 (7.9) days between the treated entities and the trade credit-based (inventory-based) SCs, while the difference is 34.2 (28.7) days between the treated group and the pool of all control entities. In SC construction, we find that, on average, higher weights are assigned to firms from Germany, Finland, Italy, and Sweden for both outcome variables (see Panels (a), (c) in Figure A9 of Online Appendix F). Collectively, these four non-French nations account for, on average, 0.8 and 0.85 of the total weight respectively in constructing trade credit and inventory-based SCs.

[15] As evident from Figure A4 in Online Appendix D, Sector 47.52 stochastically dominates the three remaining sectors in the fit quality of the constructed SCs. In addition, as detailed later, Sector 47.52 experiences a significant drop in trade credit usage following the LME, a pre-requisite for our identification. The other three sectors, on the other hand, exhibit mixed evidence on this measure, weakening the basis of our identification.

Table 1 Summary Statistics: Treated versus Synthetic Controls (Sector 47:52)

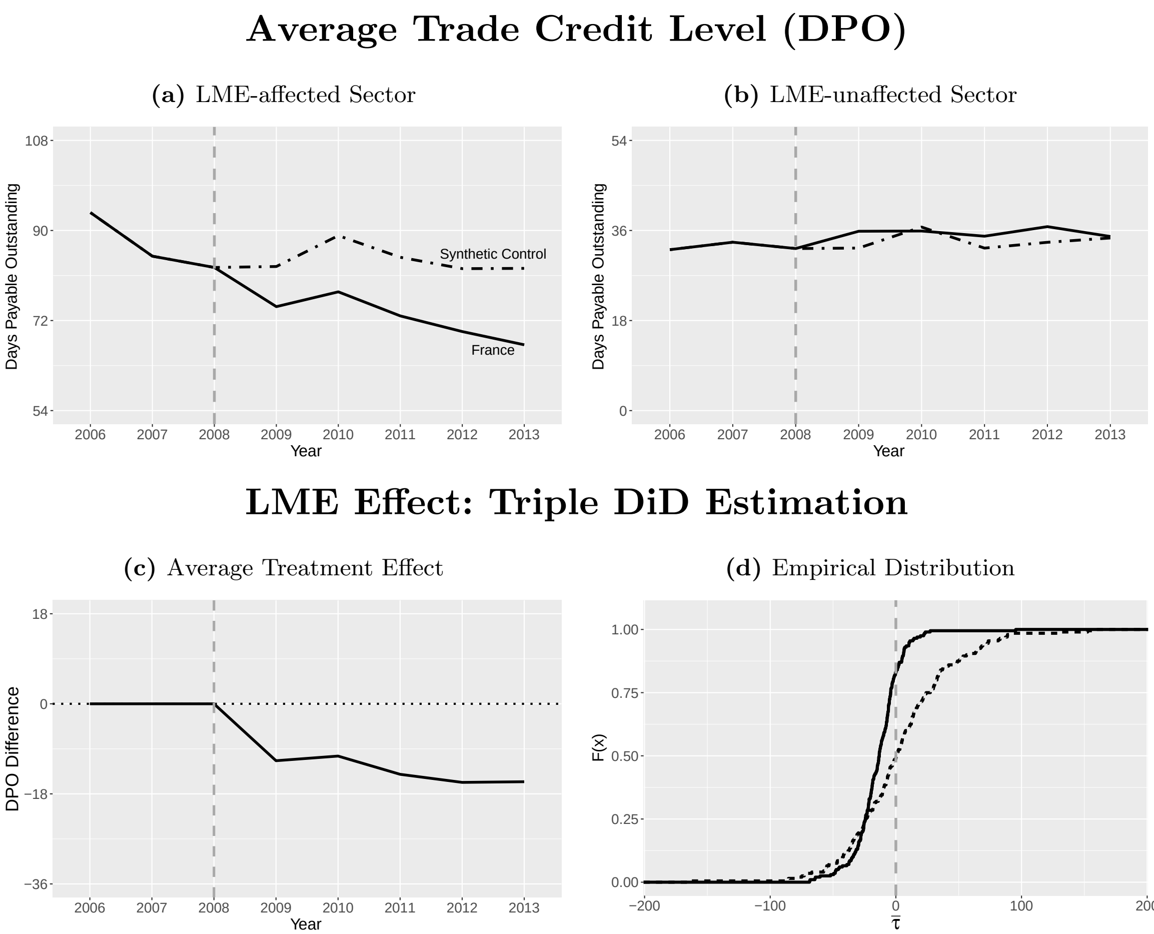
	French (# of Firms: 956)					Non-French (# of Firms: 459)				
	Mean	SD	25^{th}	50^{th}	75^{th}	Mean	SD	25^{th}	50^{th}	75^{th}
Trade Credit (DPO)	85.6	37.8	55.8	82.7	111.3	119.8	68.8	59.4	115.3	166.6
Inventory (DSI)	142.9	79.4	80.3	135.8	191.7	171.6	148.8	73.1	129.3	220.7
Cash to assets	0.18	0.17	0.03	0.12	0.28	0.10	0.12	0.01	0.06	0.15
Fixed assets to assets	0.19	0.14	0.08	0.16	0.28	0.16	0.16	0.04	0.10	0.25
Debt to assets	0.63	0.2	0.49	0.65	0.79	0.70	0.22	0.56	0.76	0.88
Gross profit to assets	0.84	0.36	0.59	0.76	0.99	0.49	0.34	0.27	0.39	0.61
COGS to assets	1.20	0.45	0.85	1.16	1.50	1.17	0.64	0.73	1.05	1.46
log(Total assets)	13.04	1.05	12.27	13.03	13.83	13.82	1.47	12.75	13.57	14.79
	Synthetic Controls: DPO					Synthetic Controls: DSI				
	Mean	SD	25^{th}	50^{th}	75^{th}	Mean	SD	25^{th}	50^{th}	75^{th}
Trade Credit (DPO)	88.2	20.2	76.42	86.3	99.0	-	-	-	-	-
Inventory (DSI)	-	-	-	-	-	150.8	52.2	117.4	145.8	180.8
Cash to assets	0.16	0.10	0.08	0.13	0.23	0.13	0.09	0.06	0.11	0.17
Fixed assets to assets	0.21	0.10	0.13	0.20	0.28	0.21	0.12	0.11	0.19	0.31
Debt to assets	0.61	0.14	0.51	0.61	0.73	0.65	0.12	0.56	0.67	0.85
Gross profit to assets	0.75	0.25	0.56	0.73	0.97	0.72	0.26	0.52	0.66	0.85
COGS to assets	1.33	0.46	1.01	1.28	1.50	1.32	0.43	0.98	1.32	1.63
log(Total assets)	13.78	0.78	13.20	13.73	14.22	13.85	0.76	13.27	13.87	14.40

6.1. Trade Credit

Figure 3, illustrates the interim results under the SC-methodology for Sector 47.52. Panel (a) of Figure 3 shows the average DPO of the treated (solid line) and corresponding SC (dashed line) entities in the pre- and post-LME periods. Likewise, panel (b) presents average DPO of the French firms (solid line) and SC entities (dashed line) in LME-unaffacted sectors. In panel (c), we present a calibrated difference in the average DPO of treated and corresponding SC entities by differencing out any difference in the LME-unaffected sectors between France and non-French nations on account of the GFC (as discussed in DDD extension on page 7). In other words, panel (c) presents the DDD estimate of the LME impact on trade credit provision. Finally, panel (d) shows the empirical cumulative distribution of estimated treatment and no-treatment effects using the DDD approach.

We find that, over the pre-LME period, the difference in average trade credit of treated and SC entities is close to zero. In the post-LME period, we find a persistent decrease in average trade credit of the treated entities. In comparison, the average trade credit of the SCs shows a steady trend in the post-LME period, which is consistent with the model-free evidence of European firms in LME-affected sectors (panel c of Figure 1). The average change in the trade credit usage of hardware retailers based on the DDD procedure is -14.1 days. This corresponds to 16% decrease on the average pre-LME trade credit level of 85.6 days. Further, we find this decline to be statistically

Figure 3 **Synthetic Control Estimation: Impact of the LME on Trade credit Provisioning in Sector 47.52**

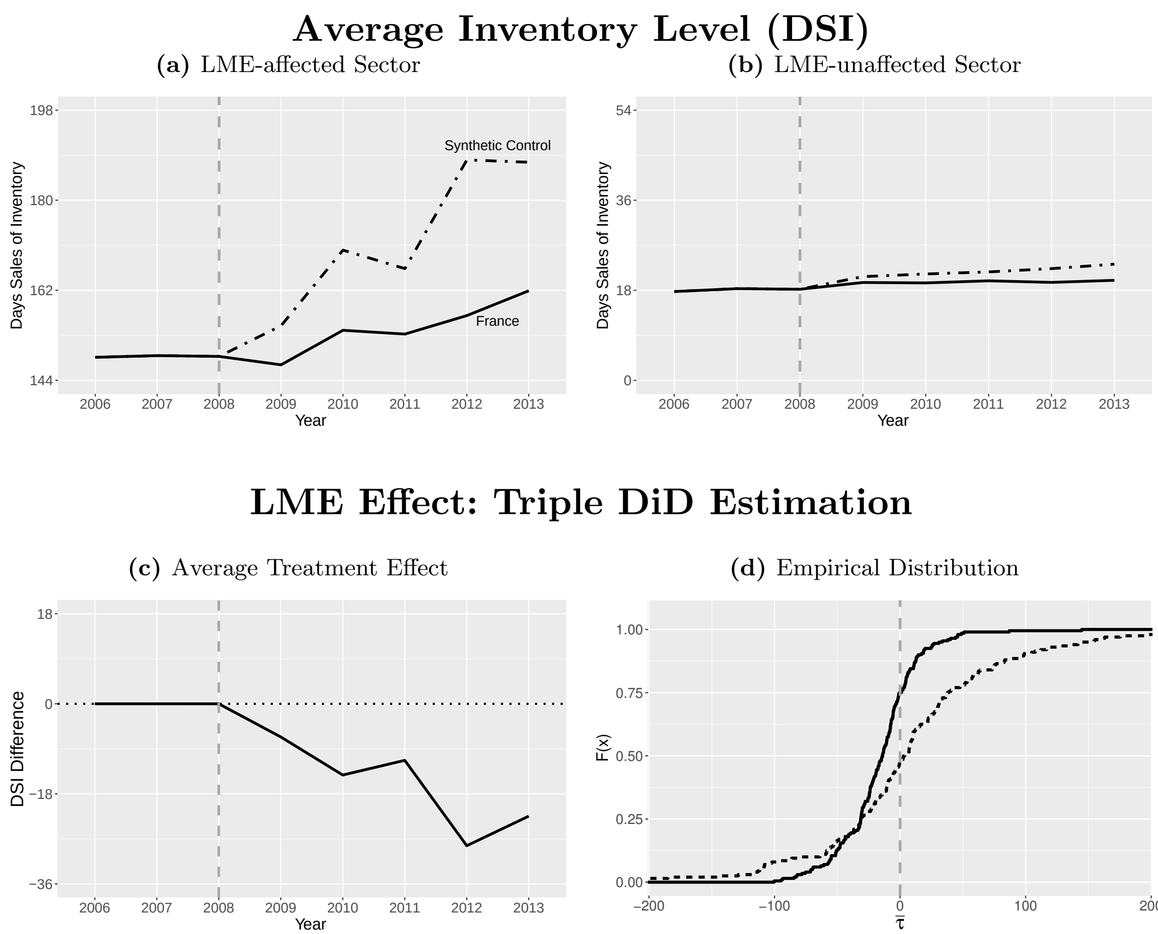


significant. The 99% CI based on the no-treatment effect distribution (see discussion on page 7 of the Online Appendix) is [−4.7, 10.7]. Further, the KS test rejects the null hypothesis of the treatment and no-treatment effect distributions being identical with a p-value less than 0.001.

6.2. Inventory Level

In Figure 4, similar to the analysis on trade credit, we provide an illustration of the SC estimation results. We find that, compared to the pre-intervention period, all firms exhibited an increase in average inventory levels during the post-LME period (panels (a) and (b)). This indicates that common macroeconomic factors over time resulted in a steady temporal increase in inventory levels. However, similar to the patterns observed in the model-free graphs (Figure A1 in the Online Appendix), the treated firms in France show a much smaller increase in inventory level compared to their counterpart SCs. In contrast, in the LME-unaffected sector, the increasing inventory trends are much more comparable between the French firms and the corresponding SCs. Together, these

Figure 4 Synthetic Control Estimation: Impact of the LME on Inventory Levels in Sector 47.52



observed asymmetries in the DSI usage trend suggest a spillover impact of LME on inventory decisions due to significant decline in trade credit usage of French hardware sector retailers. Specifically, panel (c) shows a significant impact of the LME in reducing average inventory levels of the treated entities. The DDD estimate is −15.9 days (99% no-treatment effect CI: [−7.0, 22.5]). This reflects a significant decrease of 11% on the average pre-LME inventory levels of 142.9 days. Since LME does not directly influence inventory, this decrease is attributed to the reduction of trade credit received by the retailers on account of the LME. The significance of these estimates is also corroborated by the KS test rejection of the null hypothesis that the treatment and no-treatment effect distributions are identical, with a p-value less than 1%.

In summary, the above analysis on the impact of the LME on trade credit and inventory levels provides strong empirical evidence for the causal role of trade credit in determining inventory level in the French hardware retail sector. Put differently, the exogenous shock by the LME enforcement resulted in these retailers experiencing a significant reduction in the amount of trade credit they receive from their suppliers. In response, these firms lowered their inventory level drastically. Combining the estimates of trade credit decline and inventory drop, our results suggest that a 1% decrease in trade credit leads to a 0.67% decline in inventory.

6.3. Results of the Three Remaining Sectors

So far, we focus on presenting the findings based on the hardware retail sector (NACE: 47.52). We also replicate the same DDD analysis on the other three remaining sectors that satisfy the basic inclusion criterion for the main sample (see Section 4.1), namely 45.32, 47.59, and 47.71. The detailed results of these three sectors are presented in Online Appendix D. In short, for sector 47.71 (specialized clothing retailers), we find that the LME leads to a significant drop in trade credit (-6.2 days, with a 99% CI: [3.7, 22.2]) and inventory levels (-17.4 days, with a 90% CI: [-17.3, -1.7]). For Sectors 45.32 (motor vehicle parts retailers) and 47.59 (future and specialized household stores), we do not observe a significant decline in trade credit. The apparent absence of significant LME impact on the retailers' trade credit usage in these sectors is likely due to a mixture of two reasons. First, compared to 47.52, we find that in these sectors a larger fraction of retailers were already complaint with the 60-days LME directive. In other words, for many retailers in these sectors the pre-LME trade credit usage was already below the 60-days threshold. Second, we find that the constructed SC counterfactuals of the retailers in these sectors are of a poorer quality. Motivated by these observations, for these sectors we replicated our analysis with sub-samples of retailers with longer DPO in the pre-LME period (e.g., > 60 days), and find support for a significant DPO and DSI decline in the post-LME period (see Table A6 in the Online Appendix). In summary, we find that across the retailers in our main sample, as long as the LME leads to a significant drop of trade credit usage due to LME restriction, there is consistent evidence that the decline of trade credit is associated to a decrease of inventory.

6.4. Synthetic Controls: Strengths and Limitations

Our analysis reveals a balanced view on the strengths and limitations of the SC methodology which is still in its nascent development stages. On the one hand, the SC methodology can provide a viable alternative to examine quasi-natural experiments. In such settings, the researchers prefer to apply the classical linear difference-in-differences estimation but are often constrained to find controls that satisfy the required parallel trends assumption. The SC methodology relaxes this requirement. On the other hand, the efficacy of SC methodology, as illustrated in our analysis, relies on the ability to construct good quality SCs which, in turn, depends on available observables and the length of pre-treatment period data. Yet other challenging aspect in using the SC methodology could be the sample size of treated and control units. As discussed over, the current packages may take hours to find optimal weights for SCs depending on control group size. Our proposed sampling approach could be effective in circumventing the sample size challenge. Finally, the SC methodology can also be limiting in obtaining heterogeneous treatment effects as it is onerous to replicate moderating variable analysis within the current SC methodology framework.

7. Robustness

In this section, we present robustness tests for our main findings using alternate criteria for sample creation, alternate variable definitions for outcome variables, alternate criteria for aggregation of multiple treatment effects, and alternate econometric models. In addition, we present results with alternate control groups to test the sensitivity of observed finding: (i) on the premise that the Stable Unit Treatment Value Assumption (SUTVA) (Abadie and Cattaneo, 2018) holds true in our context; and (ii) on the chosen vector of covariates. We further examine whether our findings that are based on the hardware retail sector (47.52) can be generalized to the other retail sectors. In addition to the result based on the three sectors summarized in Section 6.3, we replicate our analysis on an extended sample of six retail sectors obtained by modifying our main sample construction criteria.[16] Across these multiple robustness tests, we continue to find strong support for our findings, both within the hardware retail sector and across a range of other retail sectors. We conclude this section by describing the results of two placebo tests that supports the effectiveness of the proposed DDD framework in identifying the LME-caused effects on trade credit and inventory levels.

Alternate Sample Construction. We first test whether our findings are sensitive to the sample construction choices that were made to curtail the impact of data coverage and outlier issues (for details see Section 4). Specifically, in Rows 2 and 3 of Table 2, we present estimates using samples constructed respectively with alternate minimum thresholds of US$100,000 and US$25,000 on firm size, a measure that proxies for coverage extent and missing values in the BvD dataset. In Row 4, we show results with a sample that reflects changes in the winsorization level from 5% to 1% – a relaxed definition of extreme value. We reproduce estimates using the main sample in Row 1 for comparison.

Next, to enable construction of better fit-quality SCs, our main sample included the LME-affected sectors that have at least 10 firms in at least five non-French nations. We test robustness of the obtained finding to this criterion by reducing the minimum number of non-French nations to four. This expands our sample to 10 sectors, including the four sectors of our main sample. Row 5 reports results using a pooled sample of the six additional sectors: (i) Other retail sale in non-specialised stores (47.19), (ii) Retail sale of electrical household appliances in specialised stores (47.54), (iii)

[16] Another potential sector-specific concern is that maybe the hardware retail sector experience a unique spillover impact of the GFC, which is closely related to the housing sector. If true, it would cast doubt on whether the support for our finding in the 47.52 sector is on the account of best fit SCs or confounding GFC effect. In Online Appendix E, we show evidence that mitigates concern of such unique spillover impact.

Table 2 **Robustness Analyses** This table displays the Triple DiD SC estimates for the impact of the LME on trade credit provisioning (DPO) and inventory levels (DSI) across robustness tests. Statistical inference is based on the no-treatment distribution, with both the test of means and distribution shown.

No.	Means-Test DPO	Means-Test DSI	KS-Test DPO	KS-Test DSI	# Affected (Trt, Con)	Robustness Test Description
1	-14.1^{***}	-15.9^{***}	-14.1^{***}	-15.9^{***}	(956, 459)	Main Sample Estimates
2	-15.7^{**}	-13.9^{***}	-15.7^{**}	-13.9^{***}	(860, 435)	Min. Total Assets $\geq$\$100k
3	-13.8^{**}	-15.8^{***}	-13.8^{***}	-15.8^{***}	(992, 462)	Min. Total Assets $\geq$\$25k
4	-9.9^{***}	-19.0^{*}	-9.9^{***}	-19.0^{**}	(956, 459)	Winsorization at 1%
5	-3.9^{***}	-13.4^{***}	-3.9^{***}	-13.4^{***}	(5,571, 1,051)	Six sector pooled sample analysis
6	-16.3^{***}	-30.8^{***}	-16.3^{***}	-30.8^{***}	(2,734, 114)	Extended period pooled: 2005-2013
7	-11.7^{***}	-12.4^{***}	-11.7^{***}	-12.4^{**}	(947, 399)	Normalization by Sales
8	-13.1^{***}	-11.3^{***}	-13.1^{***}	-11.3^{***}	(956, 459)	SC Equal-weighted
9	-14.2^{***}	-16.1^{***}	-14.2^{***}	-16.1^{***}	(956, 459)	SC Poorly fitted threshold $> \sigma\sqrt{2}$
10	-10.9^{***}	-10.4^{***}	-10.9^{***}	-10.4^{***}	(956, 1,150)	SUTVA: Alternate Control Group

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Retail sale of newspapers and stationery in specialised stores (47.62), (iv) Retail sale of sporting equipment in specialised stores (47.64), (v) Retail sale of watches and jewellery in specialised stores (47.77), and (vi) Other retail sale of new goods in specialised stores (47.78).

Finally, as noted before, the BvD dataset coverage decreases sharply as we increase the pre-LME years in our study period. For example, our main sample covers 2006 to 2013 period with 7,103 firms. In comparison, the period starting with 2005 and 2004 respectively covers 4,084 and 827 firms. Intuitively, the SC-fit quality is dependent on the number of pre-treatment years. Thus, we examine sensitivity of our finding using the extended period of 2005 to 2013 period. Row 6 reports results of this test.

Alternate Variable Definition and Aggregation Criterion. In the main analysis, following the OM literature, we analyze relative inventory with respect to demand as measured using COGS (Gaur et al., 2005). Likewise, we measure trade credit provisioning by normalizing accounts payable using COGS. In Row 7, we present robustness to this normalization choice by using sales, commonly used in trade credit literature (Barrot and Nanda, 2016), as the normalizing factor. Next, we examine the sensitivity of estimates to the weighted-average method for collating multiple treatment effects to estimate an average treatment effect. In the main analysis, we determined weights using a goodness-of-fit scaling factor (Acemoglu et al., 2016). In Row 8, we present estimates using an equal-weighted approach (Xu, 2017). Row 9 shows the robustness of our findings towards the definition of poorly fitted SCs by changing the minimum quality threshold to $\sigma\sqrt{2}$ from $\sigma\sqrt{3}$.

7.1. Alternate Control Groups

Our main analysis builds on the premise of the SUTVA holding true in our context. It entails that trade credit usage by the non-French firms in the LME-Affected sector—the control entities in the main analysis—is not impacted by the LME. However, it is conceivable that these firms are indirectly affected; for example, French suppliers may demand similar short repayment terms or exploit benefits of early-payment by French buyers to gain a competitive edge in non-French markets by extending longer trade credit.[17] Thus, we test the robustness of our findings to the choice of non-French firms as control entities. Specifically, we construct SCs for the firms in sector 47.52 by using the non-French firms in the remaining three LME-Affected sectors (45.32, 47.59, 47.71) as control entities. In Row 10, we present the estimates using this alternate SCs construction approach.

Next, we test robustness of our findings to the choice of observables included in the covariate vector Z. Though we draw on the extant empirical literature focusing on inventory and trade credit decisions to select these variables, data constraints limit us from including all factors that influence inventory decisions in the covariate vector Z (for example, the demand uncertainty factor studied by Rumyantsev and Netessine (2007)). In Table A11 of the Online Appendix, we present estimates obtained with SCs constructed by excluding one covariate at a time. In all 16 tests, we find that results are consistent–both in direction and significance–with our main findings.

7.2. Alternate Econometric Models

Our main analysis focuses on the linear change in the focal outcome variables and uses the SC methodology to construct the counterfactual of treated entities. In the past, OM scholars have also used econometric models that capture non-linear changes in inventory decisions (Gaur et al., 2005). Below, we present analysis using proportional-growth based outcome variables to test the robustness of the observed findings to this modeling choice. Furthermore, in Online Appendix G, we present results with an alternate approach to estimating the counterfactual. Specifically, we obtain OLS estimates under the classic linear DDD approach (Gallino et al., 2016; Staats et al., 2016; Dhanorkar, 2017).

[17] However, we note that, according to Vincent (2009), under the LME, both the French suppliers and buyers are expected to comply with the mandatory payment terms irrespective of their business partner's location—foreign or domestic.

Similar to the main analysis, we implement a DDD strategy in three steps to identify the LME impact when modeling non-linear change in the outcome variables. First, we construct a measure of trade credit $\widehat{DPO}$ and inventory $\widehat{DSI}$ that captures the LME-induced effect after controlling for the firm and macro-level covariates Z that influence a firm's DPO and DSI decisions (see Section 4.3).[18] Second, following Tsoutsoura (2015), we measure the proportional growth in firm i's outcome variable y as the ratio of the average $\widehat{y}$ in the pre- and post-LME periods. Formally, $py_i = \frac{1}{5}\sum_{t=2013}^{2009}\widehat{y}_{it}/\frac{1}{3}\sum_{t=2006}^{2008}\widehat{y}_{it}$ where $y \in \{DPO, DSI\}$ and py denotes the proportional-growth in outcome variable y. Effectively, the measure py captures the first (non-linear) difference between the pre- and post-LME periods in variable $\widehat{y}$. Finally, we estimate the LME impact on the outcome variable by estimating the following linear model: $py_i = \alpha_0 + \alpha_A \mathbb{I}_{\texttt{LMA}} + \alpha_U \mathbb{I}_{\texttt{LMU}} + \alpha_{\texttt{LME}} \mathbb{I}_{\texttt{LMA}} \times \mathbb{I}_{\texttt{LMU}}$, where $\mathbb{I}_{\texttt{LMA}}$ (respectively, $\mathbb{I}_{\texttt{LMU}}$) denotes a dummy variable that is set to 1 for the LME-affected (respectively, LME-unaffected) sector, and $\alpha_{\texttt{LME}}$ captures the DDD estimate of the LME impact on $\widehat{y}$. We find $\alpha_{\texttt{LME}}$ equals -0.166^{***} and -0.183^{***} respectively for the pDPO and pDSI outcome variables. This implies that our results are robust irrespective of the modeling choice in outcome variables.

7.3. DDD Identification Strategy: Placebo Analyses

We perform two placebo analyses to validate the applied DDD framework. In particular, we examine whether GFC distorts identification of the LME-caused effects. The first test examines whether a *placebo-treated* nation (i.e., a non-French nation that enforced a pseudo-LME in 2008) exhibits significant treatment effects under our applied empirical strategy. We use the remaining non-French nations as *placebo-control* nations, which collectively constitute the pool of control entities for SC construction. Note that, since both the placebo-treated and placebo-control nations are affected by the common GFC shock, one would expect estimates of the pseudo-LME to be zero.

We perform the aforementioned placebo analysis on the following six non-French nations: Finland, Germany, Italy, Portugal, Spain and Sweden. In 9 out of 12 estimates (6 for trade credit and 6 for inventory, Table A10), we find no significant impact of the psuedo-LME passage on trade credit and inventory decisions at 10% level. The exceptional cases occur when either the outcome variables or the explanatory variables for the selected pseudo-treated nation are at the boundary of the convex hull of available control entities. For example, as shown in Figure 2, Italian firms exhibit the highest average trade credit levels; Portuguese firms have the highest average inventory levels and the Finland firms have the lowest average for the Debt, GDP and sovereign-debt-to-GDP predictors. These boundary characteristics naturally limit construction of well-fitted SCs. Intuitively, for a given treated entity, constructing a comparable weighted-average SC is not feasible

[18] $\widehat{y_{it}} = y_{it} - \overline{y}_{it}$ where $\overline{y}_{it}$ is the estimate of y_{it} obtained by estimating the following linear model: $y_{it} = \beta_0 + \beta_z Z + \beta_i \mathbb{I}_i + \beta_t \mathbb{I}_t + \epsilon_{it}$, here β_i and β_t denotes firm- and time-fixed effects.

if the heterogeneity among the available pool of control entities does not encompass the treated entity's characteristics. Not surprisingly, in these cases, we find that the quality of constructed SCs is considerably poorer and, hence, yields significant estimates for the impact of pseudo-LME.

The second test examines whether the French LME-Affected firms exhibit significant treatment effects with a *placebo-LME* year in the pre-LME period.[19] Specifically, we replicate our analysis using the 2005-2008 period observations and by setting 2007 as the placebo-LME year. We find insignificant DDD estimates for DPO (-9.0, 90% no-treatment CI [-34.8, 34.4]) and DSI (10.9, 90% no-treatment CI [-52.4, 48.4]).

8. Financial Implications of Inventory Reduction

Thus far, we have established that trade credit provision has a significant impact on retail inventory. While such evidence is consistent with extant trade credit theories, which predict that limiting trade credit below the equilibrium level should result in inventory under-investment, it may also be due to a different economic channel. For example, facing financing pressure due to a lack of trade credit, the conventional inventory financing source, retailers may invest in inventory efficiency and consequently require less inventory while maintaining similar levels of financial performance.

To shed light on such alternative explanations, we turn to two key financial performance metrics: revenue and gross profit. In general, trade credit theories predict that inventory under-investment caused by trade credit restriction would cause revenue to drop significantly (Yang and Birge, 2018). Similarly, such theories also predict that when trade credit is restricted to below the equilibrium level, especially when the supplier market is relatively competitive, the retailer's operational profit should also be negatively affected (Burkart and Ellingsen, 2004; Chod, 2016). On the other hand, if the reduction in inventory is caused by increased inventory efficiency, it should not lead to a simultaneous drop in both revenue and gross profit.

We put these competing hypotheses to empirical tests by using our main DDD Synthetic Control-based estimation methodology to investigate the impact of the LME-led trade credit and inventory reductions on revenue and gross profit. We normalize these two outcome variables by each firm's Total Assets. We find that after LME, the average change in the size-normalized revenue among French hardware retailers is -0.32^{***} (99% no-treatment CI [-0.07, 0.17]).[20] We note that in the pre-LME period, the average size-normalized revenue for French hardware retailers is 2.04. This implies that the LME resulted in a 15.5% decline in revenue.

[19] We thank the review team for suggesting this robustness check.

[20] We find these results to be robust across different sub-samples. Specifically, for retail sectors where LME causes a significant drop in trade credit, revenue and gross profit also experience sizeable decline. Table A13 reports results of these robustness tests.

Similarly, we find that the change in size-normalized gross profit is -0.032^{***} (99% no-treatment CI $[-0.02, 0.03]$), which translate to a 3.5% decline relative to the average pre-LME gross profit level (the pre-LME size-normalized gross profit is 0.85).[21] Collectively, the above results are consistent with existing trade credit theories, which imply that restricting trade credit could hurt financial performance. Further, by comparing the changes in revenue and gross profit, we note that the inventory reduction caused by trade credit restriction affects revenue more severely than gross profit. One possible explanation is that the retailer may use other means (such as renegotiating the wholesale price, or increasing the retail price) to mitigate the impact of limited inventory on profitability. However, such mitigating measures are likely to affect upstream firm profitability, consumer welfare, or both.

These results also bear important managerial and policy implications. On the managerial side, it suggests that suppliers, when possessing the flexibility to determine trade credit terms, need to take the operational, as well as the associated financial consequences into consideration. For policymakers, over the last decade or so, trade credit has become the target of several recent political and regulatory reforms around the world. In addition to the French regulation LME, the US Government launched two initiatives in recent years, QuickPay and SupplierPay, which aim to accelerate trade credit repayments from the government and companies to their contractors and suppliers (Mandelbaum, 2011; White House, 2014). The European Union (EU) and UK have imposed similar regulations (Council of the EU, 2011; UK BEIS, 2016). Mainly aiming to ease the financial burden of extending trade credit on small and medium enterprises (SMEs), these reforms are well-intended. However, our findings suggest that policies that restrict trade credit to a below-equilibrium level could have unintended negative consequences through operational channels.

9. Conclusion

Trade credit is an important external financing source for businesses. Related theoretical work proposes that trade credit affects not only the financial situations of firms offering and receiving it, but also operational decisions. In this paper, by using a French policy that restricts the trade credit period available to buyers, we offer a formal causal analysis of the impact of trade credit on inventory, a decision of vast importance in operations management. We find that among French hardware retailers, whose trade credit and inventory levels are significantly influenced by the LME,

[21] We note that the SC-fit quality for revenue and gross profit is poorer by an order of 20 or more compared to the analyses with DPO and DSI. Unfortunately, the limited BvD coverage of financial variables, such as short-term debt, hinders the ability to improve SC-fit quality by including well documented predictors of revenue and profitability in the extant literature. As a result, our estimates of the financial outcome variables are more prone to the disproportional impact of outliers (treated entities with poor SC-fit). For example, if we exclude outliers, from both sides, at the 5% level (resp., 10%), the average revenue reduction equals becomes 10.7% (resp., 8.3%) and profit reduction equals 5.3% (resp., 3.6%).

a 1% drop in trade credit leads to a 0.67% decline in inventory level. Further, we find a significant decrease in revenue and profitability of these retailers, suggesting that restriction in trade credit usage and, the concomitant retail inventory reduction can have considerable economic impact. Combined, our results provide strong evidence for the existing theories that emphasizes trade credit as an indispensable source in inventory financing and an important lever for supply chain efficiency. Further, it alerts managers and policymakers that a holistic approach is needed when designing trade credit related policies.

Our study can be extended along several dimensions. First, due to data limitations, the scope of our study is limited to aggregate level evidence among French retailers. Should more detailed data become available, further research that examines the exact mechanism (e.g., demand risk-sharing, or deterring opportunistic behaviors) through which trade credit leads to decline in inventory and profitability could better shed light on the validity of different theories on trade credit. On a related note, our quantification of trade credit impact on inventory is based on the French hardware retail sector. Studies using data from other countries and/or industries could help validate the generality of our findings. Second, this study focuses on the impact of the trade credit restriction on downstream inventory. Examining other operational metrics, such as service quality or upstream inventory, could offer a more comprehensive view of the impact of trade credit on operational decisions. Third, while using the LME as the policy intervention, the goal of this study is not to provide a formal policy evaluation. However, our empirical findings suggests that a comprehensive policy evaluation should include both the benefits of trade credit for retailers and its potential harm on suppliers' operations.

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Online Appendix

The Impact of Trade Credit Provision on Retail Inventory: An Empirical Investigation Using Synthetic Controls

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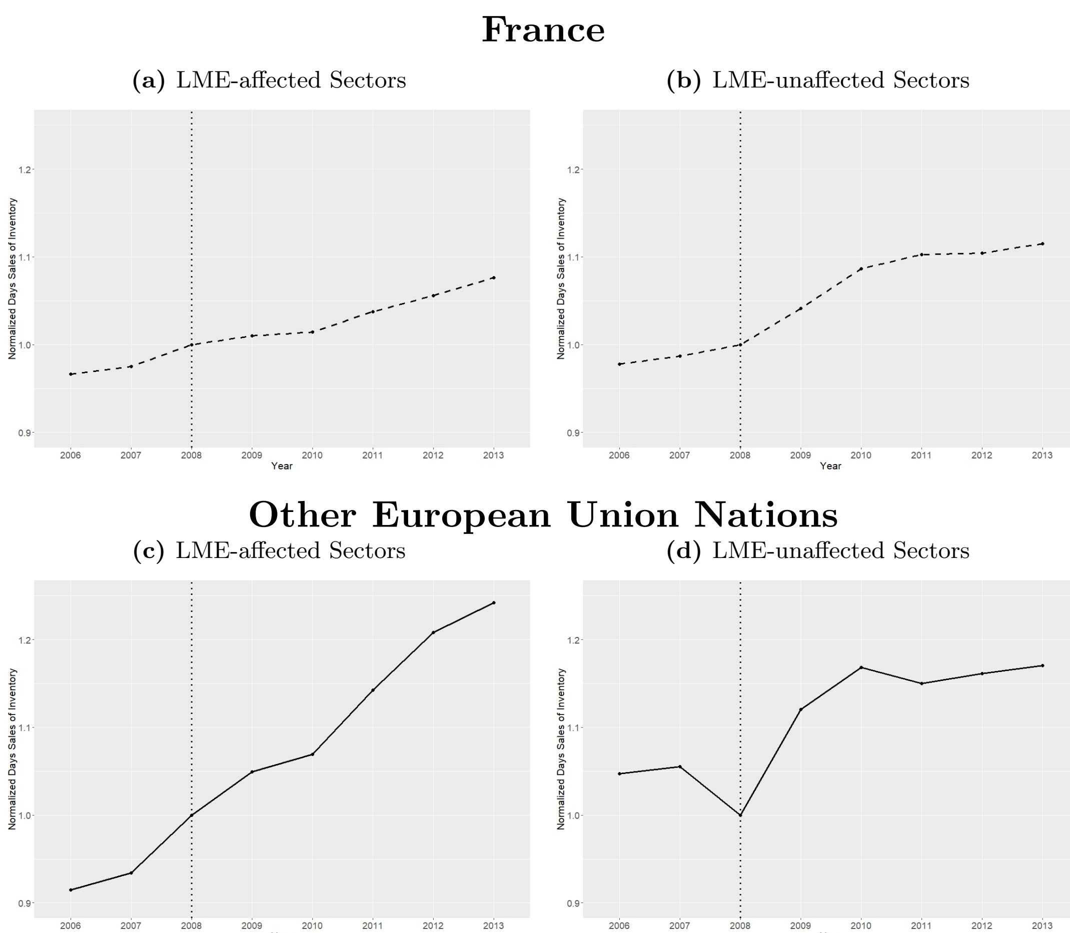
Appendix A: Model-Free Evidence: Trends in Inventory Levels in Pre-and Post-LME Periods

Similar to Figure 1, Figure A1 presents the model-free evidence of the trends in inventory levels, measured as $\text{DSI}_{kt} = \frac{\text{STOK}_{kt}}{\text{MATE}_{kt}} \times 365$. As shown, the model-free data supports a lessor proportional increase in the LME-Affected (LA) sectors in France relative to their European counterparts (8% vs. 25%) while the LME-Unafftected (LU) sectors had more comparable proportional changes in France and non-French countries (12% vs. 17%).

Figure A1 Trends in Inventory Levels: Pre and Post LME

This figure shows trends in average DSI received by the retailers in the LME-affected and LME-unaffected sectors over an eight-year period between 2006 and 2013. Panels (a) and (b) present these trends for France. Panels (c) and (d) present these trends averaged across six European nations. The vertical axis shows the average DSI after normalizing each firm's DSI usage by its 2008 value. In the LME-affected sectors, compared to the European firms, we find a muted increase in the French firms' DSI usage in post 2008 period. In contrast, the increasing trends are much comparable between French and European firms in LME-unaffected sectors.



Appendix B: Synthetic Control: Method Overview with Single Treatment Unit

We illustrate the Synthetic Control (SC) method using the following policy-intervention example. Suppose we observe $k = 1, \ldots, K$ firms with only one firm (without the loss of generality, $k = 1$) that has experienced the policy intervention. We observe $t = 1, \ldots, T$ periods of data with the policy coming into force in $t = T_o + 1 > 1$ period. We denote firm k's outcome during the non-intervention period by Y_{kt}^N and during the intervention period by Y_{kt}^I. Further, let D_{kt} and $\tau_{kt} = Y_{kt}^I - Y_{kt}^N$ denote respectively whether firm k experiences the intervention in period t and the corresponding impact of that intervention. Based on these notations, we can define the observed outcomes as

$$Y_{kt} = Y_{kt}^N + \tau_{kt} D_{kt}. \tag{1}$$

Note that, for firms $k = 2, \ldots, K$ that never experience the policy intervention, $D_{kt} = 0$ for all t's and thus, $Y_{kt} = Y_{kt}^N$.[1] For firm $k = 1$, $D_{kt} = 1$ for $t > T_o$ and 0 otherwise. The goal is to estimate $(\tau_{1T_o+1}, \ldots, \tau_{1T})$. For $t > T_o$, we have

$$\tau_{1t} = Y_{1t}^I - Y_{1t}^N = Y_{1t} - Y_{1t}^N. \tag{2}$$

Here, note that for $t > T_o$ while Y_{1t}^I is observed, Y_{1t}^N is never observed and has to be estimated using the pool of available control entities. Let Y_{kt}^N be defined by the following linear model

$$Y_{kt}^N = \theta_t \mathrm{Z}_k + \lambda_t \mu_k + \delta_t + \epsilon_{kt}, \tag{3}$$

where Z_k denotes a vector of observed pre-intervention covariates with time-varying coefficients θ_t, μ_k denotes a vector of unobserved covariates with time-varying coefficients λ_t, δ_t denotes the time-fixed effect, and ϵ_{it} is the idiosyncratic error term with mean zero. Here, note that the simple linear DiD model can be recovered from Eq. (3) by setting $\lambda_t = \lambda$. In other words, in comparison to the SC method, which permits time-varying coefficients of the unobservable confounders, the linear difference-in-difference (DiD) model restricts the coefficients of unobservable confounders to be time-invariant, which is also known as the parallel trends assumption.

A potential SC can be constructed by assigning relative weights to each of the available control entities. Consider a $(K-1) \times 1$ vector $\mathrm{W} = (w_2, \ldots, w_K)$, where $w_k \geq 0$ for $k = 2, \ldots, K$, and $\sum_{k=2}^{K} w_k = 1$. These restrictions on weights ensure that interpolation of an SC is within the convex hull of available control entities.[2] The value of the outcome variable for an SC with weights W is given by

$$\sum_{k=2}^{K} Y_{kt} = \delta_t + \theta_t \sum_{k=2}^{K} w_k \mathrm{Z}_k + \lambda_t \sum_{k=2}^{K} w_k \mu_k + \sum_{k=2}^{K} w_k \epsilon_{kt}. \tag{4}$$

Abadie et al. (2010) show that if there exists W^* such that

$$\sum_{k=2}^{K} w_k^* Y_{kt} = Y_{kt} \text{ for } t = 1, \ldots, T_o, \qquad \text{and} \qquad \sum_{k=2}^{K} w_j^* \mathrm{Z}_k = \mathrm{Z}_1. \tag{5}$$

[1] Following Rosenbaum (2007), we assume that no interference is made, that is, the treated entity outcomes do not interfere with the control entities' outcomes, or, the Stable Unit Treatment Value Assumption (SUTVA).

[2] A limitation of non-negative weights is that it may fail to generate a good-fit SC for a treated unit with characteristics not representative of the available control units. In a recent paper, Ben-Michael et al. (2018) present an extension of SC methodology that relaxes this restriction on weights.

then under general conditions, as the number of pre-intervention periods increase, we have:

$$\left| Y_{1t}^N - \sum_{k=2}^{K} w_k^* Y_{kt} \right| \longrightarrow 0. \tag{6}$$

This enables the construction of an unbiased estimator. Here, it is important to note that, depending on the structure of Y_{kt}^N model, the number of pre-intervention periods required for an unbiased estimator varies. For example, if Y_{kt}^N follows an AR(1) model, the SC method can yield unbiased estimates with just one pre-intervention data point. Building on Eq. (6), the treatment effect τ_{1t} for $t = T_o + 1, \ldots, T$ can be estimated using the following estimator

$$\widehat{\tau}_{1t} = Y_{1t} - \sum_{k=2}^{K} w_k^* Y_{kt}. \tag{7}$$

With real data, finding W^* that satisfies conditions Eq. (5) is not always possible. Instead, the SC method searches for optimal weights that best approximate these two conditions, or equivalently, minimizes the distance between the pre-intervention period values for the outcome variable and observed covariates of the treated unit. We refer readers to Abadie et al. (2010) for the implementation details of the SC method to compute optimal weights.

Appendix C: Synthetic Control Implementation: A Sampling Approach

In this section, we first present details of a sampling approach to SC estimation. We adapt this approach to manage the towering computational challenge due to large sample size in our setting. Next, we elaborate on our approach that extends the SC methodology to implement a DDD identification strategy.

We tailor the SC estimation procedure for multiple treatment settings (Acemoglu et al., 2016; Kreif et al., 2016) to meet the large sample-size challenge of our empirical setting. Although multiple packages have been developed over the past 15 years for SC estimation, most applications have been on samples with a small number of treated and control entities. To the best of our knowledge, in the extant literature, SC studies have examined settings where the number of treated entities is in a range of 22 to 100, and control entities is between 31 to 500. Table A1 provides a review of these papers. In contrast, in our sample, the four LME-affected sectors include between 555 and 2,419 treated entities, and between 203 and 474 control entities. As the complexity of the search for the optimal weights in the construction of a good-fit SC increases in the size of available control entities, applying the conventional SC estimation on such a large sample imposes a prohibitively high demand on computational time. We find a steep non-linear increase in computational time with the increase in the sample size of control entities. For instance, computing one SC for one of the 956 treated firms in sector 47.52 using all 459 control firms takes 95 minutes in R using the synth package on a dual E5-2650v3 CPU totalling 20 cores/40 threads for one dependent variable. In contrast, computing an SC using a random sample of 100 control firms require only 1.5 minutes.

Table A1 Multiple Treatment Literature. Literature review of studies with multiple treatment settings using the Synthetic Control (SC) methodology.

	Paper	Description	#Trt	# Ctrl	# Obs
1	Cavallo et al. (2013)	Natural disasters on GDP growth	∼27	∼192	7,644
2	Dube and Zipperer (2015)	Minimum wage on employment	29	31	7,344
3	Acemoglu et al. (2016)	Firm connections on stock performance	22	70	∼24,000
4	Kreif et al. (2016)	P4P payment scheme on mortality	24	132	1,872
5	Powell (2017)	Minimum wage on employment	29	31	7,344
6	Steffen (2017)	Construction ban on real estate prices	∼100	∼500	10,496
7	Jones (2018)‡	Invasive species on birth-weight	790 combined		13,430

‡ Jones (2018) analyzes a combined sample of 790 entities that are classified as a treated and/or control entity based on temporal rollout of treatment intervention.

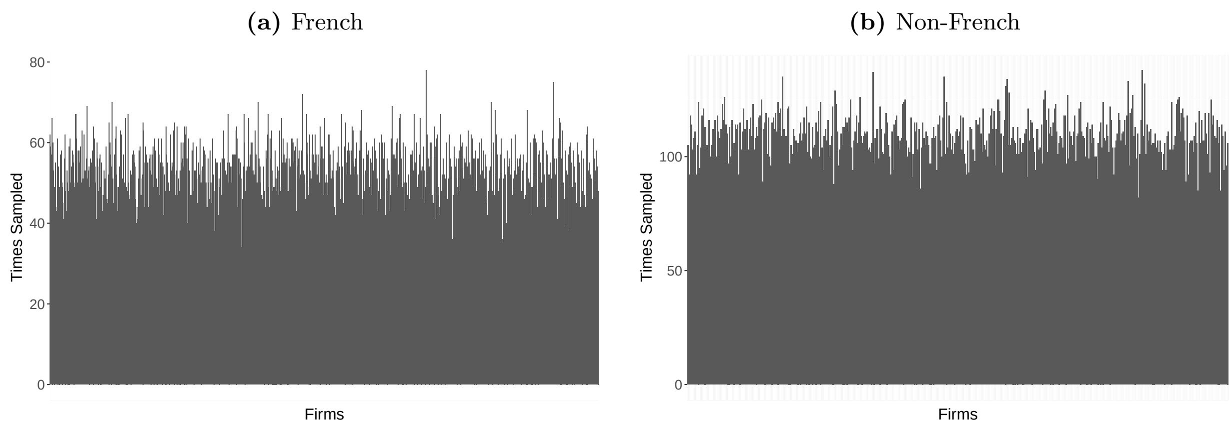
To overcome this computational challenge, we adopt a sampling approach for the SC estimation, which is based on the principle of deriving empirical findings through representative random sub-samples. Specifically, we apply SC estimation to study random sub-samples of up to 100 treated entities using a random sub-sample of up to 100 control units.[3]

[3] We note that computational time increases linearly with treated entities. Therefore, an alternate sampling approach could be to sample only control units for each treated entity. However, unlike our proposed sampling on both the treated and control entities that generates a one-to-one mapping between a treated entity and its SC, this approach would generate multiple SCs for each treated entity. Consequently, one faces an additional challenge of aggregating the multiple SCs into a single SC per treated entity. An intuitive choice is to pick the best-fitting SC; we find our results are robust to this alternate approach.

One concern about such a sampling approach could be that the findings are driven by a small subset of treated entities. To alleviate this concern, we draw our findings based on 500 iterations of such sub-sample analysis; this enables a fair representation of all treated and control entities, as detailed in Figure A2.

Figure A2 Sampling Frequency of Treated and Control Entities

This figure illustrates the fair representation of available treated and control entities in computing average treatment effect using the adapted SC sampling procedure in this study. The figure shows sampling frequency of firms in sector 47.52. Panels (a) and (b) respectively present sampling frequency of these firms for the French or Non-French nations.



A second potential concern could be the construction of poorly fitted SCs as, in each sampling iteration, the SC construction is restricted to a limited pool of control entities. A larger pool of control entities could enable construction of a better fitted SC. We test for this concern by studying the distribution of the goodness-of-fit of constructed SCs with an increasing number of control units where goodness-of-fit is measured using RMSPE, $\widehat{\sigma}_k$, as defined in Eq. (5). Below, in Section C.3 we show the distribution of the goodness-of-fit of constructed SCs with 100, 200, and 300 control entities. Using the Kolmogorov-Smirnov (KS) test (Gibbons and Chakraborti, 2011) for distribution comparison, we find no significant differences across the three distributions. This analysis suggests that, in our setting, a random sample of 100 control entities sufficiently represents the available heterogeneity across all the control entities and, thus, there is no significant improvement in the quality of the SCs with the increase in sampling size.

Implementation Steps. We compute the treatment effect, that is, the impact of the LME, at the sector level. In particular, we implement an iteration i of the above-described sampling approach in four steps. First, for a given sector, we draw a random sample of up to 100 firms from the pool of French firms (resulting in a sub-sample of treated entities) and from the collective pool of non-French firms (resulting in a sub-sample of control entities). Second, for each drawn treated entity, we generate an SC unit using the drawn pool of control entities. Next, following Acemoglu et al. (2016), we drop poorly fitted treated entities. We define a treated entity k as having a poor fit if $\widehat{\sigma}_k \geq \widehat{\sigma}\sqrt{3}$ where $\widehat{\sigma} = \sum_{k=1}^{K} \widehat{\sigma}_k / K$ is the average goodness-of-fit across all treated entities, and K is the number of firms in the treatment group of iteration i. Finally, using the remaining well-fitted treated entities, we compute an estimate of the average treatment effect $\bar{\tau}_i$ for iteration i as the quality-adjusted weighted-average of treatment effects, $\widehat{\tau}_{kt}$. We repeat the above steps for 500 iterations to generate an empirical distribution of $\bar{\tau}_i$ and calculate the LME's average treatment effect $\bar{\tau}$ as the average of this empirical distribution.

C.1. Synthetic Control: Inference Procedure for Significance Testing.

A commonly preferred inference procedure in the SC estimation literature tests the significance of the average treatment effect against the distribution of the no-treatment effect (Abadie et al., 2010; Acemoglu et al., 2016; Kreif et al., 2016).[4] Intuitively, the no-treatment distribution emulates the distribution of change in treated entities in the absence of treatment, that is, the counterfactual. If the estimated treatment effect is within the $1-\alpha\%$ Confidence Interval (CI) of the no-treatment distribution then one cannot reject the hypothesis that the change in treated entities equals that in the control entities at $\alpha\%$ significance level.

We construct an empirical no-treatment effect distribution in three steps, which retains the country-level treatment characteristic of our empirical setting (akin to the French Government's LME legislation). First, we select a *placebo-treated* nation through a random draw from the set of non-French nations. The remaining non-French nations constitute the pool of *placebo-control* entities. Second, we apply the sampling approach described in Section C to construct an estimate of the no-treatment effect in the selected placebo-treated nation. Finally, we perform 500 iterations of the preceding two steps to construct the empirical distribution of the no-treatment effect.

C.2. SC Sampling Approach: DDD Extension

We apply two extensions to the iteration step for implementing the DDD identification strategy (as discussed in Section 3.2 of the main paper). First, in each iteration i, we draw additional random sub-samples of up to 100 entities from the pool of firms in French and non-French nations' LME-unaffected sectors. By applying the SC estimation on these sub-samples, we obtain an estimate of only the GFC-related impact on the focal outcome variable. Next, we subtract the estimate from the SC estimate, computed using random sub-samples of the LME-affected sectors, to construct iteration i's revised DDD estimate for the treatment effect $\bar{\tau}_i$. Applying this modified iteration step, we construct our empirical distributions for the treatment and no-treatment effect.

C.3. Synthetic Control with Sampling: Supplementary Analyses

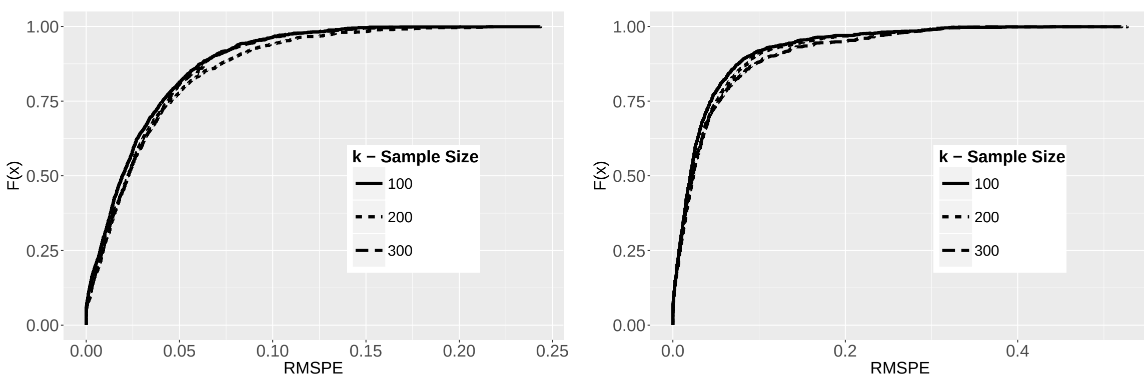
In a single iteration of the sampling approach used in our main analysis, we draw random sub-samples (with replacement) of up to 100 treated and control entities. It is conceivable that the higher the number of control entities, the better the quality of constructed SCs. Here, we examine whether restricting size of control entities' random sub-samples to 100 materially impacts the quality of constructed SCs. We implement the following three-step test. First, we draw a random sub-sample of 100 treated entities from the sample of firms' in Sector 47.52. Next, we draw 30 random sub-samples of size $k \in \{100, 200, 300\}$ control entities from the pool of all available non-French firms. Using each of these random sub-samples of control entities (120 in

[4] Alternatively, Campos et al. (2014) implement a DD regression framework using the actual data and its synthetic counterfactual to carry out statistical inference.

all), we construct SCs for the drawn sub-sample of 100 treated entities. Figure A3 shows three cumulative distribution plots of treated entities' (that satisfy the good-fit criterion) Root Mean Squared Prediction Error (RMSPE) (σ_k)–a plot each for three focal outcomes variables. We find that RMSPE distribution varies little by k. For all three variables, the Kolmogorov-Smirnov test fails to reject that the distribution with $K > 100$ is significantly different from the distribution with $k = 100$.

Figure A3 Root Mean Squared Prediction Error (RMSPE) by Sample Size. Empirical CDFs of the average RMSPE of 100 French firms matched against 100/200/300 randomly drawn control firms for accounts payable and inventory.

(a) Account Payables **(b)** Inventory



Appendix D: Results on Sectors 45.32, 47.59, and 47.71

In the main body of the paper, among the four retail sectors that satisfy the original selection criterion (sectors with at least five non-French European countries that have at least 10 firms in this sector), we focus on Sector 47.52, which generates the best SC-fit. For the remaining three LME-affected sectors, we find mixed evidence towards the impact of the LME on trade credit provision. Tables A2, A3, and A4 provide summary statistics for these sectors.

Compared to the pool of control entities, the constructed SCs continue to exhibit characteristics that are more comparable to that of the treated French firms in all dimensions. Figures A5 to A7 illustrate the SC estimation for these sectors. For firms in sector 47.71 (specialized clothing retailers), we find a significant decline in average trade credit level in the post-LME period: the average DDD treatment effect estimate is -6.2 days, with the 99% CI of the no-treatment effect being $[3.7, 22.2]$. Similarly, we find a significant reduction in average inventory level, the triple-difference estimate is -17.4 days (90% CI $[-17.3, -1.7]$). In Sectors 45.32 and 47.59, we find that the LME did not significantly impact firms' trade credit level: the average treatment coefficients for these sectors are not significantly different compared to the average no-treatment effect at 10% level.

We conjecture that the insignificant impact of the LME in these sectors is driven by a large number of firms that were likely to already comply with the LME (i.e., those repaying trade credit within the imposed 60-day ceiling). We find that the compliance rate (the fraction of firms whose DPO is less than 60 days before the LME) for these two sectors are 35% and 46% respectively, while the compliance rate of 47.52 is only 31%. Further, as illustrated in Figure A4, the quality of the SC-fit among firms in Sectors 45.32, 47.59, and 47.71 is considerably poorer than that in Sector 47.52. This low SC-fit quality also affects the quality of our treatment effect estimates as it impairs construction of counterfactual change in the treated entities in the absence of treatment.

Next, we test if the LME affects non-compliant firms in these sectors. Table A6 shows the results based on a sub-sample of non-compliant firms. Panels A, B, and C respectively report results with firms that had in 2008 DPO greater than or equal to 60, 75, and 90 days in 2008 respectively. We find that as the criterion of non-compliance becomes more stringent, the support for our finding — LME leads to both trade credit and inventory decline — becomes stronger in the remaining three sectors.

We also find consistent support for our finding across these sectors using the alternate econometric model, presented in the main paper, that models a non-linear change in the outcome variable. Specifically, we replicate Tsoutsoura (2015) methodology (for details see page 7.2 of the main paper) with proportional outcome variables pDPO and pDSI. Table A7 reports results of this analysis.

Finally, we also find support for our finding across these sectors with the alternate approach to constructing SCs using the non-French LME-affected firms, except those in the focal sector (for details, see description of this test on page 23 of the main paper). Table A5 reports results of this test. The DDD estimates, except for those of sector 47.59, are consistent with our main treatment estimates. Though these results provide a positive support to our finding, we would like to note that the estimated DPO effect is relatively small compared to our main finding based on Sector 47.52 (DPO: -14.1^{***} and DSI: 15.9^{***}). Exclusion of the most comparable set of firms—those belonging to the same focal sector in non-French nations—seems to have driven these differences in treatment estimates. Not surprisingly, we find that this exclusion results in a drop in SC-fit quality.

Table A2 Summary Statistics: Treated versus Synthetic Controls for Sector 47.71

	French (# of Firms: 2,419)					Non-French (# of Firms: 473)				
47.71	Mean	SD	25^{th}	50^{th}	75^{th}	Mean	SD	25^{th}	50^{th}	75^{th}
Trade Credit (DPO)	85.7	53.7	42.5	75.6	119.8	113.3	75.2	46.1	102.9	167.9
Inventory (DSI)	178.4	113.1	92.4	153.2	233.5	213.3	152.8	105.1	171.5	274.4
Cash to assets	0.18	0.17	0.03	0.12	0.28	0.13	0.14	0.02	0.07	0.21
Fixed assets to assets	0.35	0.21	0.17	0.34	0.52	0.20	0.18	0.05	0.14	0.31
Debt to assets	0.59	0.23	0.41	0.60	0.77	0.73	0.23	0.58	0.77	0.90
Gross profit to assets	0.72	0.29	0.5	0.67	0.89	0.66	0.40	0.36	0.57	0.86
COGS to assets	0.88	0.37	0.60	0.81	1.11	1.10	0.51	0.70	1.01	1.40
log(Total assets)	12.61	1.02	11.93	12.50	13.17	13.94	1.71	12.68	13.66	14.91
	Synthetic Controls: DPO					**Synthetic Controls: DSI**				
	Mean	SD	25^{th}	50^{th}	75^{th}	Mean	SD	25^{th}	50^{th}	75^{th}
Trade Credit (DPO)	77.9	22.3	63.8	75.5	89.8	-	-	-	-	-
Inventory (DSI)	-	-	-	-	-	208.6	71.2	163.8	198.7	244.5
Cash to assets	0.18	0.16	0.09	0.15	0.27	0.13	0.09	0.07	0.10	0.18
Fixed assets to assets	0.32	0.14	0.21	0.31	0.43	0.30	0.14	0.20	0.31	0.41
Debt to assets	0.59	0.15	0.47	0.59	0.72	0.63	0.15	0.51	0.63	0.76
Gross profit to assets	0.74	0.20	0.60	0.74	0.86	0.73	0.22	0.58	0.68	0.87
COGS to assets	1.02	0.26	0.84	0.99	1.20	0.98	0.28	0.79	0.95	1.16
log(Total assets)	14.00	0.97	13.22	13.90	14.58	13.74	0.95	13.11	13.71	14.41

Table A3 Summary Statistics: Treated versus Synthetic Controls for Sector 47.59

	French (# of Firms: 1,564)					Non-French (# of Firms: 474)				
47.59	Mean	SD	25^{th}	50^{th}	75^{th}	Mean	SD	25^{th}	50^{th}	75^{th}
Trade Credit (DPO)	76.7	42.1	43.0	69.3	102.3	96.8	66.9	41.8	81.6	140.2
Inventory (DSI)	154.7	104.8	74.4	134.8	207.4	216.9	170.3	104.5	166.8	272.0
Cash to assets	0.23	0.19	0.06	0.19	0.37	0.12	0.12	0.02	0.07	0.17
Fixed assets to assets	0.23	0.17	0.09	0.19	0.34	0.20	0.19	0.04	0.13	0.30
Debt to assets	0.62	0.21	0.47	0.63	0.79	0.67	0.24	0.49	0.73	0.87
Gross profit to assets	0.92	0.38	0.62	0.86	1.16	0.65	0.4	0.35	0.56	0.89
COGS to assets	0.99	0.40	0.68	0.95	1.26	1.12	0.59	0.67	1.02	1.48
log(Total assets)	12.88	0.95	12.22	12.86	13.49	14.06	1.54	12.88	13.94	15.11
	Synthetic Controls: DPO					Synthetic Controls: DSI				
	Mean	SD	25^{th}	50^{th}	75^{th}	Mean	SD	25^{th}	50^{th}	75^{th}
Trade Credit (DPO)	79.6	19.8	67.1	78.1	89.1	-	-	-	-	-
Inventory (DSI)	-	-	-	-	-	196.0	72.4	145.2	191.5	237.7
Cash to assets	0.19	0.10	0.10	0.18	0.26	0.15	0.09	0.08	0.13	0.21
Fixed assets to assets	0.23	0.11	0.14	0.21	0.30	0.20	0.11	0.12	0.16	0.26
Debt to assets	0.61	0.16	0.49	0.63	0.75	0.61	0.15	0.49	0.60	0.74
Gross profit to assets	0.81	0.24	0.61	0.80	0.98	0.80	0.23	0.63	0.82	0.95
COGS to assets	1.12	0.90	1.09	1.31	1.56	1.12	0.29	0.92	1.13	1.27
log(Total assets)	13.85	0.88	13.22	13.73	14.50	13.41	0.74	12.96	13.26	13.91

Table A4 Summary Statistics: Treated versus Synthetic Controls for Sector 45.32

	French (# of Firms: 555)					Non-French (# of Firms: 203)				
45.32	Mean	SD	25^{th}	50^{th}	75^{th}	Mean	SD	25^{th}	50^{th}	75^{th}
Trade Credit (DPO)	86.2	36.4	57.3	84.7	111.3	102.4	69.6	45.0	89.5	149.1
Inventory (DSI)	102.3	61.6	57.9	89.7	128.1	156.8	115.3	73.6	128.9	199.7
Cash to assets	0.23	0.19	0.06	0.19	0.37	0.12	0.12	0.02	0.07	0.18
Fixed assets to assets	0.22	0.16	0.08	0.17	0.32	0.16	0.16	0.04	0.09	0.23
Debt to assets	0.62	0.21	0.46	0.63	0.78	0.66	0.23	0.48	0.72	0.85
Gross profit to assets	1.02	0.40	0.72	0.94	1.26	0.57	0.32	0.33	0.47	0.78
COGS to assets	1.24	0.53	0.83	1.17	1.56	1.21	0.64	0.82	1.12	1.48
log(Total assets)	12.79	0.95	12.06	12.77	13.43	13.86	1.46	12.73	13.6	14.82

	Synthetic Controls: DPO					Synthetic Controls: DSI				
	Mean	SD	25^{th}	50^{th}	75^{th}	Mean	SD	25^{th}	50^{th}	75^{th}
Trade Credit (DPO)	78.7	22.0	64.6	76.5	90.9	-	-	-	-	-
Inventory (DSI)	-	-	-	-	-	133.8	45.0	108.3	127.4	154.3
Cash to assets	0.21	0.12	0.09	0.19	0.32	0.19	0.11	0.10	0.18	0.28
Fixed assets to assets	0.20	0.12	0.10	0.16	0.30	0.18	0.10	0.11	0.14	0.22
Debt to assets	0.57	0.15	0.45	0.58	0.69	0.60	0.14	0.46	0.63	0.72
Gross profit to assets	0.91	0.25	0.74	0.88	1.09	0.92	0.21	0.76	0.91	1.05
COGS to assets	1.37	0.48	1.02	1.31	1.72	1.35	0.45	1.03	1.22	1.56
log(Total assets)	13.48	0.78	12.95	13.44	13.93	13.46	0.61	13.05	13.50	13.88

Figure A4 Synthetic Controls Fit Quality: Sector-wise Comparison

This figure shows the relative fit of constructed SCs across the four sectors. The quality of constructed SCs is largely determined by the convex-hull characteristics of the available control units and available covariates. Figures (a) and (b) respectively compare the CDFs of the pre-LME period gaps Δ in the DPO and DSI outcome variables between the treated firms and corresponding SCs.

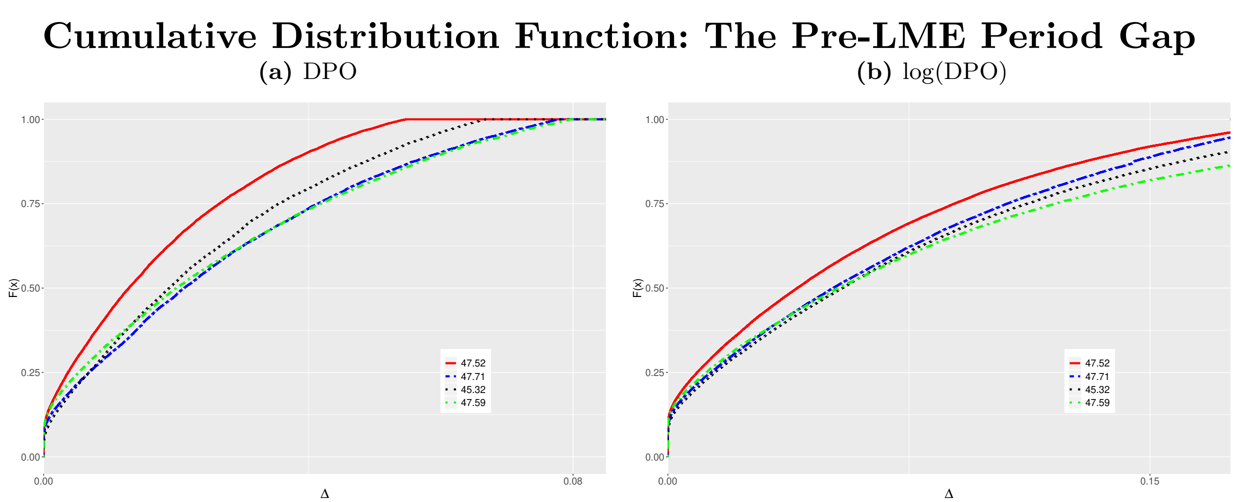


Figure A5 Triple Difference-in-Differences SC Estimation: Sector 47.71

Trade-Credit (DPO)

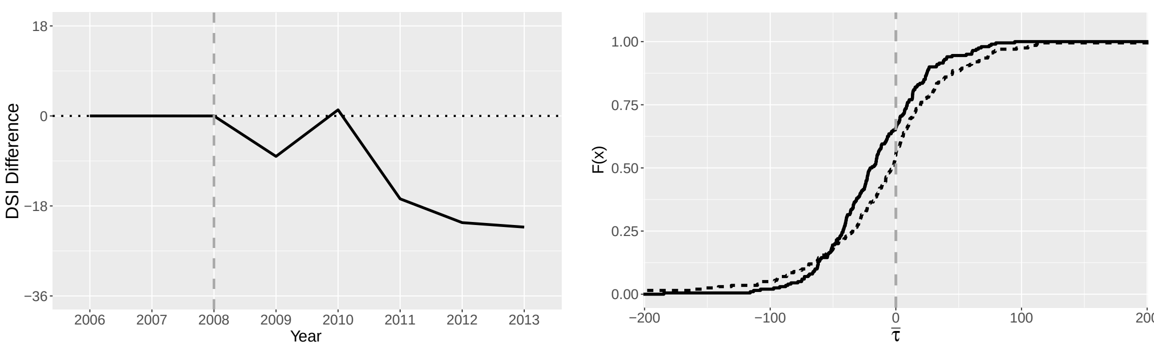
The average treatment effect is −6.2 and 99% CI is [3.7, 22.2]†.

(a) Average Treatment Effect **(b)** Empirical Distribution

Inventory (DSI)

The average treatment effect is −17.4 and 90% CI is [−17.3, −1.7]†.

(c) The average treatment effect **(d)** Empirical Distribution



†CI is based on no-treatment effect distribution, see discussion on page 7.

Figure A6 Triple Difference-in-Differences SC Estimation for Trade Credit (DPO): Sector 45.32

The average treatment effect is −5.8 and 90% CI is [−5.8, 3.9]†.

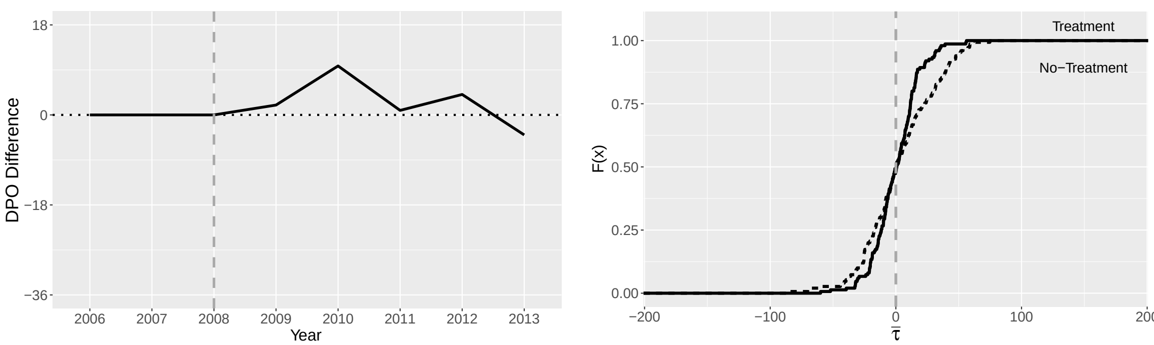
(a) Average Treatment Effect **(b)** Empirical Distribution

† CI is based on no-treatment effect distribution, see discussion on page 7.

Figure A7 Triple Difference-in-Differences SC Estimation for Trade Credit (DPO): Sector 47.59

The average treatment effect is 0.2 and 90% CI is $[-0.9, 6.4]^{\dagger}$.

(a) Average Treatment Effect **(b)** Empirical Distribution



† CI is based on no-treatment effect distribution, see discussion on page 7.

Table A5 SUTVA Analysis. The 99% no-treatment confident intervals are reported

	Dependent Variable	
	DPO	DSI
47.71	-1.0^{***} [−0.6, 9.6]	-16.2^{***} [−13.7, 4.0]
47.59	-3.0^{***} [−2.1, 8.0]	-6.2 [−13.3, 9.2]
45.32	-4.2^{***} [−0.1, 9.5]	-28.7^{***} [−10.2, 9.8]
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A6 Synthetic control triple differences estimates of the LME-effect on DPO and DSI by sector over a range of non-compliance cutoffs at the firm level. The 99% no-treatment confident intervals are reported in the brackets.

	Dependent Variable	
	DPO	DSI
Panel A. Days Payable Outstanding > 60 Days (2008)		
47.52	-11.3^{***} [−2.7, 9.5]	-13.0^{***} [−12.1, 9.5]
47.71	-7.0^{***} [−2.9, 7.7]	-15.8^{***} [−9.5, 11.0]
Panel B. Days Payable Outstanding > 75 Days (2008)		
47.52	-12.7^{***} [−2.7, 9.5]	-13.0^{***} [−12.1, 9.6]
47.71	-10.1^{***} [−2.9, 7.7]	-19.4^{***} [−9.5, 11.0]
45.32	-8.8^{**} [−8.9, 1.6]	-8.4^{**} [−9.8, 14.7]
Panel C. Days Payable Outstanding > 90 Days (2008)		
47.52	-15.0^{***} [−2.7, 9.5]	-12.5^{***} [−12.1, 9.6]
47.71	-10.4^{***} [−2.9, 7.7]	-22.0^{***} [−9.5, 11.0]
45.32	-10.4^{***} [−8.9, 1.6]	-10.8^{***} [−9.7, 14.6]
47.59	-3.4^{*} [−5.2, 5.9]	-19.6^{***} [−12.4, 13.0]
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A7

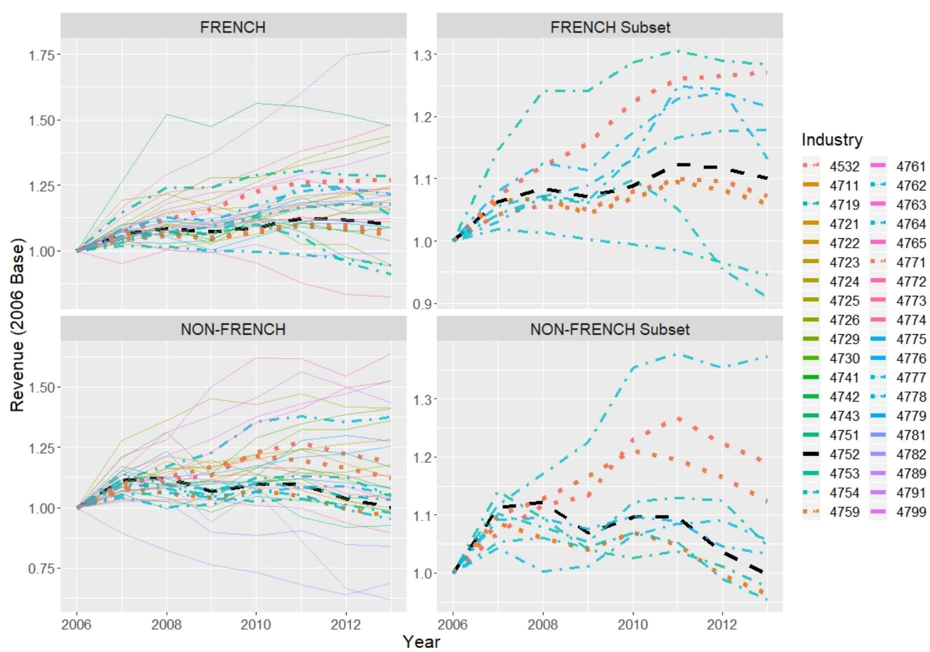
	Affected				Unaffected				Differences
	All (1)	French (2)	Non-French (3)	Mean DD (4)	All (5)	French (6)	Non-French (7)	Mean DD (8)	Mean DDD (9)
No. of Firms	1415	956	459		2517	2125	392		
Panel A. Annual Controls (47.52)									
Proportion in DPO	-0.096	-0.120	-0.046	-0.073***	0.054	0.068	-0.024	0.092	-0.166***
	(0.007)	(0.008)	(0.014)	(0.000)	(0.006)	(0.006)	(0.021)	(0.000)	(0.000)
Proportion in DSI	0.032	0.005	0.088	-0.082***	-0.018	-0.002	-0.103	0.100	-0.183***
	(0.007)	(0.007)	(0.016)	(0.000)	(0.008)	(0.008)	(0.024)	(0.000)	(0.000)
Panel B. Annual Controls (47.71)									
Proportion in DPO	-0.030	-0.020	-0.078	0.058***	0.034	0.049	-0.045	0.094***	-0.036***
	(0.007)	(0.008)	(0.017)	(0.000)	(0.006)	(0.006)	(0.021)	(0.000)	(0.000)
Proportion in DSI	0.009	-0.001	0.056	-0.057***	-0.010	0.008	-0.105	0.113***	-0.170***
	(0.005)	(0.005)	(0.013)	(0.000)	(0.008)	(0.008)	(0.025)	(0.000)	(0.000)
Panel C. Annual Controls (45.32)									
Proportion in DPO	-0.087	-0.080	-0.105	0.025***	0.026	0.040	-0.047	0.086***	-0.061***
	(0.010)	(0.011)	(0.024)	(0.000)	(0.006)	(0.006)	(0.021)	(0.000)	(0.000)
Proportion in DSI	0.001	-0.020	0.057	-0.076***	0.000	0.014	-0.080	0.094***	-0.170***
	(0.010)	(0.011)	(0.021)	(0.000)	(0.007)	(0.008)	(0.024)	(0.000)	(0.000)
Panel D. Annual Controls (47.59)									
Proportion in DPO	-0.049	-0.041	-0.077	0.037***	0.040	0.054	-0.038	0.092***	-0.056***
	(0.007)	(0.008)	(0.016)	(0.000)	(0.006)	(0.006)	(0.021)	(0.000)	(0.000)
Proportion in DSI	0.012	-0.009	0.081	-0.090***	-0.010	0.006	-0.097	0.103***	-0.193***
	(0.006)	(0.006)	(0.014)	(0.000)	(0.008)	(0.008)	(0.025)	(0.000)	(0.000)

Note: *p<0.1; **p<0.05; ***p<0.01

Appendix E: Did the GFC uniquely impact the hardware retail sector?

Figure A8 show temporal trends in revenue for various sectors in France and non-French nations. The x-axis covers the entire study period: 2006-2013. The y-axis shows the corresponding year's sector-wide average of revenue, measured as the ratio of 2006 revenue, across firms. The dash lines denote the four sectors in our main sample, with black color representing the 47.52 sector. The dot-and-dash lines represent the additional six sectors present in the extended sample obtained by relaxing the required number of non-French nations, for SC construction, to four from five. The solid lines denote all the other remaining sectors. The top and bottom panel respectively present sectors in France and non-French nations. Collectively, these graphs indicate that the hardware retail sector is among the sectors with lower revenue growth in the post-GFC period but is not the one that is extremely affected.

Figure A8 Temporal Trend in Revenues: 47.52 vs other sectors.



Next, we formally test for the relative change in revenues at the sector level to examine if the hardware retail sector is uniquely affected by the GFC crisis. Specifically, we test if the average proportional revenue change for 47.52 firms is significantly different from the that observed for firms in the remaining sectors. For a firm i, we measure the average proportional change in its revenue by $\Delta Rev_i = (Rev_{i,2006} + Rev_{i,2007})/(Rev_{i,2008} + Rev_{i,2009})$. We note that for French firms in the affected sectors, Rev_i reflects both the LME and GFC impact. In contrast, for non-French firms Rev_i only reflects GFC impact. Thus, we run separate tests for the sample of French and non-French firms. Table A8 shows mean comparison t-test results.

Row 1 of Table A8 shows results of test that compares mean of French firms in the 47.52 sector with that of the firms in the remaining three sectors of our main sample (i.e., 47.71/47.59/45.32). Row 2 shows comparison results with the nine sectors of our expanded sample that applies minimum four non-French nations criterion for SC construction.[5] Rows 3 and 4 respectively present analogous results of Rows 1 and 2 tests with non-French firms. Except for Row 1 in which mean difference is positive at p-value 0.09, all other tests report insignificant differences in Rev_i mean levels between 47.52 firms and the other remaining sectors. In summary, in each of the above described four tests, we fail to reject that the change in revenue for the firms in the hardware sector is comparable to other sectors.

Table A8 Revenue Comparison: Hardware Retail Sector (47.52) versus other sectors. p-value is reported in the parenthesis.

#	Test Description	Mean Difference	Comment
1	France: 47.52 vs Remaining 3 main sample sectors	0.012^{*} (0.09)	Within France comparison
2	France: 47.52 vs Remaining 9 extended sample sectors	-0.002 (0.79)	Within France comparison
3	Non-French Nations: 47.52 vs Remaining 3 main sample sectors	-0.012 (0.40)	Across EU comparison
4	Non-French Nations: 47.52 vs Remaining 9 extended sample sectors	-0.002 (0.88)	Across EU comparison

Note: *p<0.1

[5] 47.71/47.59/45.32/47.19/47.54/47.62/47.64/47.77/47.78

Appendix F: Supplementary Tables and Figures

Table A9 Country Count by Industry. Number of countries with at least 10 firms in a given industry.

NACE Code	# Countries	NACE Code	# Countries	NACE Code	# Countries
45.32	6	47.19	5	47.24	2
47.25	2	47.42	3	47.43	2
47.51	3	**47.52**	6	47.53	2
47.54	5	**47.59**	7	47.61	3
47.62	4	47.63	1	47.64	4
47.65	3	**47.71**	7	47.72	3
47.74	3	47.75	3	47.77	5
47.78	5	47.82	1	47.89	1
47.91	3	47.99	2		

Table A10 Placebo Results. This table shows the results of the placebo analysis with non-French nations individually designated as pseudo-LME treated. Coefficient estimates are given along with the 90% confidence intervals of the no-treatment distribution.

Country	DPO	DSI
Finland	-5.6 [-8.9, -0.2]	17.0^{***} [-14.4, -3.6]
Germany	1.4 [-2.9, 2.7]	-7.5 [-7.7, 6.5]
Italy	23.0^{***} [-7.6, -0.7]	-3.2 [-12.8, -3.1]
Portugal	-2.9 [-4.2, 2.8]	114.0^{***} [-26.1, -5.9]
Spain	-7.2 [-9.5, -2.2]	8.5 [-10.5, 9.8]
Sweden	-4.1 [-6.7, -0.2]	-4.9 [-13.5, -3.4]
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A11 Robustness Test: Sensitivity of Estimates to Inclusion/Exclusion of Covariates. Numbers in bracket report the 99% no-treatment confidence intervals.

(1) Variable	(2) DPO	(3) DSI
1) Cash-to-Assets	-9.0^{***} [1.2, 9.9]	-18.0^{***} [-10.3, 8.3]
2) Debt-to-Assets	-6.8^{***} [3.2, 13.2]	-12.3^{***} [-10.1, 4.6]
3) Fixed Assets-to-Assets	-7.6^{***} [2.8, 12.2]	-15.1^{***} [-11.5, 4.3]
4) Gross Profits-to-Assets	-9.6^{***} [2.5, 11.0]	-11.5^{***} [-8.3, 3.7]
5) log(Assets)	-9.2^{***} [-1.1, 9.3]	-12.6^{***} [-9.2, 9.5]
6) log(GDP)	-7.7^{***} [2.0, 10.9]	-18.0^{***} [-9.0, 6.3]
7) Debt-to-GDP	-9.1^{***} [3.6, 12.9]	-13.2^{***} [-5.6, 6.7]
8) log(5yr CDS)	-15.4^{***} [5.8, 13.9]	-14.7^{***} [-3.1, 13.2]

Figure A9 Relative Sampling Frequency of Treated and Control Entities

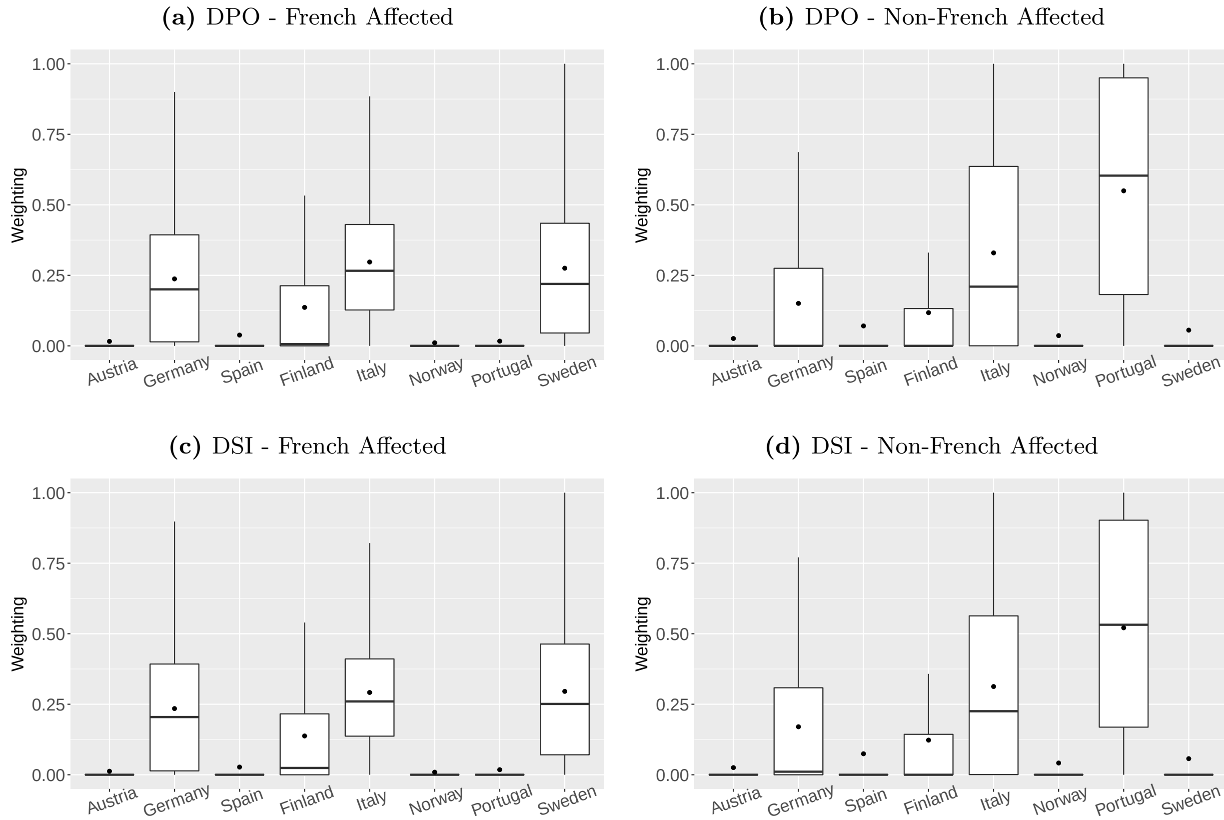


Table A12 GFC Placebo analysis (Sample period: 2005–2008, Placebo-LME Year: 2007). The 90% no-treatment confident intervals are reported in brackets.

	Dependent Variable	
	DPO	DSI
47.52	−9.0 [−34.8, 34.4]	10.9 [−52.4, 48.4]
47.71	18.8 [−34.4, 33.6]	4.5 [−52.4, 48.4]
45.32	10.8 [−34.9, 33.6]	15.9 [−52.4, 48.4]
47.59	−6.7 [−34.5, 34.3]	4.4 [−51.1, 48.4]
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A13 Financial Metrics Results: Using SC Methodology. Outcome variables normalized by Total Assets. The 99% no-treatment confident intervals are reported in the bracket.

	Revenue	Gross Profit
47.52	-0.32^{***} [−0.07, 0.17]	-0.03^{***} [−0.02, 0.03]
45.32 with DPO > 90 days	-0.50^{***} [−0.06, 0.19]	-0.05^{***} [−0.03, 0.04]
47.59 with DPO > 90 days	-0.34^{***} [−0.09, 0.18]	-0.05^{***} [0.01, 0.09]
47.71 with DPO > 90 days	-0.43^{***} [−0.15, 0.10]	-0.08^{***} [−0.00, 0.07]
6-sector pooled	-0.43^{***} [−0.14, 0.11]	-0.07^{***} [−0.03, 0.04]

Appendix G: Linear Triple Difference-in-Differences (DDD) with Parallel Trends Assumption

In Tables A16 and A17, we present results of the linear DiD analysis, along with that of parallel trend validation tests, of the LME impact on trade credit (DPO) and inventory level (DSI) respectively. In line with the main analysis, we identify the impact of the LME using the DDD identification strategy with firms in sector 47.52 as those that are LME-affected and firms in Sectors 47.21, 47.22, 47.23, 47.30 as those that are LME-Unaffected. Formally, we estimate the following specification (at the firm × year level)

$$\begin{aligned} DV_{it} = &\beta_1 Post_t \times France_i + \beta_2 Post_t \times Affected_i \\ &+ \beta_3 Post_t \times France_i \times Affected_i \\ &+ \gamma X + \delta_i + \eta_t + \epsilon_{it}, \end{aligned} \tag{8}$$

where $DV \in \{DPO, DSI\}$ is the outcome variable of interest, $Post$ is an indicator variable that is set to 1 for the post-LME period (from 2009 to 2013) and 0 otherwise, $France$ is an indicator variable that is set to 1 for French firms and 0 otherwise, $Affected$ is an indicator variable that is set to 1 for firms in the LME-affected sector and 0 otherwise, and X is a vector of control variables. In addition, we include firm- and time-fixed effects (δ and η, respectively). We allow error terms (ϵ_{it}) to be correlated for French and non-French observations and thus cluster standard errors for inference. The coefficient of interest is β_3, which captures the DDD estimate of the LME on the outcome variable.

Columns 1 and 2 (in both Tables A16 and A17) show the results estimated using matched samples constructed using the Propensity Score Matching (PSM) (Dhanorkar, 2017) and Coarsened and Exact Matching (CEM) (Iacus et al., 2012) methods respectively. PSM uses a specified model to compute propensity scores of an entity being treated. The estimated scores are then used to create matched pairs. For our analysis, we estimate the PSM-based model using all the covariates included in the main analysis and we implement the nearest neighbor algorithm for creating matched pairs (Campello et al., 2010). Statistics for the PSM matched sample are shown in Table A14.

In comparison to PSM, CEM excludes treated and control entities that do not meet the ex-ante specified matching criteria, with the benefit of bounding the imbalance between treated and control groups prior to matching. Consequently, the size of the matched sample is quite sensitive to the strictness of the matching criteria and the underlying heterogeneity of firm characteristics across the treated and control entities. For instance, in our context, CEM matching based on the complete list of covariates Z used in SC estimation yields a small sample of only six entities. Thus, we adopt a restricted list of matching variables comprised of the two outcome variables (DPO, DSI) and the total assets (TA) variable as a measure of firm size. We implement many-to-many matching and estimate the model using weighted OLS. Table A15 presents the sample statistics of the CEM-based sample.

Columns 3 to 6 (in both Tables A16 and A17) provide the results of tests validating the parallel trends assumption. In particular, we test whether outcome variables $DV \in \{DPO, DSI\}$ show any difference in trends between the LME-affected firms in France and non-French nations during the pre-LME period. We use two alternate specifications to test for the presence of such a difference.

Table A14 Summary Statistics of the Propensity Score Matching (PSM) Sample for Sector 47.52

	LME-affected Sector				LME-unaffected Sector			
	French ($N = 956$)		Non-French ($N = 221$)		French ($N = 2{,}125$)		Non-French ($N = 210$)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Trade credit (DPO)	77.8	36.1	80.1	55.8	42.2	25.8	43.3	35.9
Inventory (DSI)	148.4	83.1	135.0	131.6	18.5	23.0	28.5	31.8
Cash to assets	0.17	0.18	0.16	0.15	0.31	0.22	0.29	0.23
Fixed assets to assets	0.20	0.14	0.17	0.17	0.41	0.26	0.30	0.22
COGS to assets	0.61	0.21	0.62	0.23	0.64	0.24	0.68	0.28
Gross profit to assets	0.84	0.36	0.83	0.54	1.31	0.62	1.35	0.87
Long-term debt to assets	1.16	0.45	1.29	0.72	2.32	1.69	2.60	1.57
log(Total assets)	13.07	1.07	13.14	1.37	12.22	0.82	12.45	1.21

Table A15 Summary Statistics of the Coarsened Exact Matching (CEM) Sample for Sector 47.52

	LME-affected Sector				LME-unaffected Sector			
	French ($N = 770$)		Non-French ($N = 243$)		French ($N = 1{,}787$)		Non-French ($N = 249$)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Trade credit (DPO)	80.3	36.5	94.9	51.1	40.2	21.9	36.5	32.9
Inventory (DSI)	142.4	80.3	157.0	116.8	14.6	18.3	21.9	21.9
Cash to assets	0.18	0.18	0.12	0.13	0.31	0.22	0.22	0.23
Fixed assets to assets	0.20	0.14	0.15	0.15	0.42	0.25	0.29	0.24
COGS to assets	0.61	0.21	0.64	0.21	0.63	0.23	0.69	0.27
Gross profit to assets	0.85	0.36	0.54	0.36	1.30	0.61	0.82	0.56
Long-term debt to assets	1.18	0.45	1.17	0.60	2.42	1.73	4.17	2.37
log(Total assets)	13.15	1.00	13.49	0.96	12.2	0.79	13.13	0.87

First, following Gallino et al. (2016), we adopt a continuous time-indexed specification. Formally, we estimate the following specification using observations from the pre-LME period of the LME-affected firms:

$$DV_{it} = \alpha_1 t_{index} \times France_i + \gamma X + \delta_i + \eta_t + \epsilon_{it}, \tag{9}$$

where t_{index} denotes the order of the year in the pre-LME period (i.e, $t_{index} = 1$ for 2006 and $t_{index} = 3$ for 2008). The coefficient α_1 captures any trend in the differences between outcome variable DV of LME-affected firms in France and non-French nations. Columns 3 and 4 present the results using the PSM and CEM matched samples respectively.

The second test adapts the approach from Barrot (2016), which discretizes the short pre-treatment period and tests whether the differences in outcome variables between the treated and control entities differ significantly in pre-treatment sub-periods. Compared to the Gallino et al. (2016) approach, this approach avoids the over-fitting concerns of estimating a time-indexed linear trend with a short pre-treatment period. We execute this test by estimating a modified version of the full triple-difference specification (Eq. (8)) which is presented below in a concise manner for brevity:

$$\begin{aligned} DV_{it} = & \alpha_1' Year_{2008} \times France_i \times Affected_i + \alpha_2' Year_{2009} \times France_i \times Affected_i \\ & + \alpha_3' Year_{2010-13} \times France_i \times Affected_i + \gamma' X' + \delta_i + \eta_t + \epsilon_{it}, \end{aligned} \tag{10}$$

where $Year_{2008}$, $Year_{2009}$ and $Year_{2010-13}$ are dummy variables indicating year 2008, 2009, and the four-year span of 2010 to 2013, respectively. The covariate vector X' includes all the $Year_{\{.\}}$ interaction terms not shown in the above abbreviated specification. The coefficient α_1' tests whether the difference in outcome variables of treated and control entities differs in the pre-LME sub-period (specifically, in 2008) compared to the base pre-LME period of 2006–07. A significant value of α_1' indicates the presence of a non-constant difference in the outcome variables in the pre-LME period. In Columns 5 and 6, we present results of this specification with the PSM and CEM matched samples, respectively.

Table A16 Linear DDD estimate for the impact of LME on Trade Credit Provisioning

	Dependent variable: DPO					
	LME Impact		Parallel Trends Tests			
	(1)	(2)	(3)	(4)	(5)	(6)
Post × France × Affected	-13.361^{***}	-15.993^{***}				
	(0.018)	(2.328)				
t_{index} × Affected			-7.429^{***}	-5.261^{***}		
			(0.000)	(0.000)		
France × $Year_{2008}$ × Affected					-2.588^{***}	-2.882^{**}
					(0.000)	(1.250)
France × $Year_{2009}$ × Affected					-11.241^{***}	-14.663^{***}
					(0.018)	(1.590)
France × $Year_{2010-13}$ × Affected					-14.970^{***}	-17.527^{***}
					(0.018)	(2.951)
Post × Initial Controls	Yes	Yes	No	No	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,096	24,392	3,531	3,039	28,096	24,392
Adjusted R^2	0.775	0.777	0.774	0.778	0.776	0.778
Note:					*p<0.1; **p<0.05; ***p<0.01	

The results shown in the columns 1 and 2 of Tables A16 and A17 continue to support our main findings that the LME significantly reduced the average trade credit usage and the average inventory stocked by

Table A17 Linear DDD estimate for the impact of LME on Inventory Levels

	Dependent variable: DSI					
	LME Impact		Parallel Trends Tests			
	(1)	(2)	(3)	(4)	(5)	(6)
Post × France × Affected	-22.933^{***}	-26.459^{***}				
	(0.195)	(2.288)				
t_{index} × Affected			-4.753^{***}	-0.406^{***}		
			(0.000)	(0.000)		
France × $Year_{2008}$ × Affected					-7.829^{***}	-4.783^{***}
					(0.000)	(0.241)
France × $Year_{2009}$ × Affected					-6.269^{***}	-13.391^{***}
					(0.195)	(0.272)
France × $Year_{2010-13}$ × Affected					-30.361^{***}	-31.719^{***}
					(0.195)	(2.906)
Post × Initial Controls	Yes	Yes	No	No	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,096	24,392	3,531	3,039	28,096	24,392
Adjusted R^2	0.937	0.933	0.906	0.896	0.937	0.934

Note: *p<0.1; **p<0.05; ***p<0.01

the retailers in the sector 47.52. We note that these results, however, are subject to bias as we find strong evidence towards violation of the parallel trends assumption in our setting. Both the trend coefficient α_1 (in columns 3 and 4) and the marginal difference coefficient α_1' (in columns 5 and 6) are significant.

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