# Prudential Capital Controls, Credit Supply and International Trade: Evidence from Greece

**by**

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**Abstract**

This paper investigates the credit channel of prudential capital controls on international trade. For identification, we use a policy experiment of controls on capital outflows transactions in Greece in June 2015 and across-sectors variation of credit dependence within the same firm. Although the effect is theoretically ambiguous, we find that, for the same firm, exports of goods belonging to sectors with higher dependence on short-term credit drop relatively more compared to sectors with lower dependence. We provide evidence of the bank lending channel by exploiting the sudden stop in the provision of the Emergency Liquidity Assistance (ELA) that triggered the imposition of capital controls as a natural experiment, Greek banks' pre-determined exposure to the ELA and novel matched bank-firm credit exposures data. We find that the weak banks' balance sheets cause a reduction in the overall credit, which in turn causes a reduction in the overall exports of firms. Overall, our findings suggest that, despite the nature of macro-prudential capital controls as a crisis management tool, there is a credit crunch with negative real effects that crucially depends on the health of the banking system.

**JEL Classification Codes**: F15, F23, F3, G21, G28
**Keywords**: Capital Controls, Macroprudential Regulation, Bank Lending Channel, International Trade, Multinational Firms.

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## 1. Introduction

Systemic risk has been at the center of policy making since the global financial crisis. To deal with it, policy makers use macroprudential tools to mitigate domestic financial instability and capital controls to discipline international capital flows. In case capital flows are the source of domestic financial instability, capital controls and macroprudential policy are substitutes and thus hard to distinguish (Korinek and Sandri, 2016). Macro-prudential controls have been recently used in Brazil in 2009 to reduce capital inflows, as well as in Cyprus and Greece in 2013 and 2015, respectively, to stop deposit outflows from domestic banks and help restore financial stability. Even the IMF, which was a strong advocate of capital account liberalization, recently reviewed its stance on introducing capital management measures to restrict increasing capital inflows or disruptive capital outflows (IMF, 2012). Given the recent policy experience and the renewed interest of capital controls in theory (e.g. Jeanne and Korinek, 2010; Bianchi, 2011; Korinek and Sandri, 2016), evaluating the intended and unintended consequences of capital controls on the real economy as well as the channels in place is of first order importance.

We contribute to the current debate by investigating the credit channel of prudential capital controls on international trade. Theoretically, as discussed in Tamirisa (1999), banks may enjoy substantial market power or credit and other trade-related banking products may be either costly or not available to use during capital controls, which in turn may hurt trade. On the other hand, capital controls may prevent a weak banking system from collapse when they serve prudential purposes, increasing banks' capacity to provide financial products and services and, as a result, foster trade indirectly[1]. For identification, we exploit a policy experiment of prudential capital controls in Greece in June 2015. Greece introduced a series of controls on capital outflows transactions to stabilize the domestic banking system following a period of increased economic and political uncertainty and an extensive bank run. After the Greek government's decision to bring the terms of a new bailout agreement with the troika of international lenders (European

[1] Gallego and Hernandez (2003) provide evidence of lower borrowing costs for firms during controls on capital outflows in Chile.

Commission, European Central Bank and International Monetary Fund) into a referendum, which was announced on June 27, 2015 and ECB's decision to cap its loan limit for the provision of the Emergency Liquidity Assistance (ELA) to Greek banks in response to the announcement of the referendum on the same day, a series of measures were initiated to stabilize the banking system and prevent outflows of funds abroad. Although there was no institutional restriction on the provision of credit by banks during capital controls, it is *ex ante* ambiguous whether credit supply declined because of banks' exposure to the sudden stop of the ELA or increased because of the prudential regulation to contain the liquidity shock by preventing the rapid decline of bank assets and liabilities and limiting cash withdrawals from banks.

Credit is particularly important for export activities, which entail significant fixed and variable costs, such as the maintenance of an international distribution network, payment of salaries abroad or compliance with the foreign regulatory frameworks, which require considerable upfront expenses (e.g. Manova, 2013; Foley and Manova, 2015). A firm can cover these expenses both by using internal liquidity (e.g. revenues generated from ongoing sales) and external liquidity (e.g. bank credit, trade credit). The ability of a firm to cover trade-related costs is particularly impaired during controls on capital outflows, when much of a firm's *internal* liquidity is trapped within the banking system. All else equal, compared to multinational companies, domestic firms do not enjoy access to a parent's internal capital market, which in turn may reduce or terminate existing export flows during a liquidity crisis[2]. We identify the credit channel of capital controls on trade by interacting a firm's *domestic* ownership structure with the sector's credit conditions, such as (i) *External Finance Dependence* to proxy for the sector's need for long-term bank credit (Rajan and Zingales, 1998; Kroszner, Laeven and Klingebiel, 2006), (ii) *Asset Tangibility* to proxy for the sector's ability to raise bank credit (Braun, 2003), (iii) *Liquidity Needs* to proxy for the sector's need for short-term bank credit (Raddatz, 2006) and (iv) *Trade Credit* to proxy for the sector's alternative sources to bank credit (Fisman and Love, 2003). Our approach allows us to exploit variation *within the same domestic*

[2] Manova et al (2015) document the importance of accessing an internal capital market in international trade.

*firm* by employing firm fixed effects and effectively control for all observed (e.g. internally generated liquidity by ongoing sales) and unobserved (e.g. management practices of the firm[3]) heterogeneity that matters in international trade. We, thus, isolate the credit channel by exploiting a sector's credit conditions, which are plausibly exogenous to any individual firm within the sector and alleviate endogeneity concerns related to the patterns of multinational activity. In other words, we identify the credit channel of capital controls on trade by asking how much a *domestic* firm's exports differentially drop based on exogenous industry credit conditions.

Our findings can be summarized as follows: At the intensive margin, *for the same domestic firm*, which exports a number of different products, exports decline more for products belonging to sectors with high dependence on credit. In particular, we find strong evidence of the short-term credit channel of capital controls on trade, which is proxied by a sector's *Liquidity Needs* ratio (Raddatz, 2006). In terms of economic magnitude, compared to the pre-controls period, the exports of a sector at the 75$^{th}$ percentile of the distribution of short-term credit dependence drop by 26 percentage points compared to a sector at the 25$^{th}$ percentile of the distribution for the same domestic firm. We find weaker results (statistically significant at the 10% level and at the 5% level, respectively) for the long-term and trade credit channel of capital controls. Interestingly, a sector's ability to raise external funds, which is proxied by the *Asset Tangibility* ratio, enters statistically insignificant in all models. Note that these estimates reflect the adjustment of exports within the *same* firm. At the extensive margin, we find no evidence of termination of an existing export flow across all measures of sector's financial conditions. One concern with these findings is that exports are jointly determined by supply and demand. We identify the supply-side effect of the credit channel of capital controls by including a comprehensive set of destination*sector fixed effects in all

[3] Bloom et al (2017) have recently shown both theoretically and empirically the importance of management practices in international trade.

models. These dummies capture all heterogeneity (e.g. demand, transportation costs, tariffs) that is destination*sector specific[4].

In order to identify the effect, we assume that exports of products belonging to sectors with different credit needs within the *same* firm would not have differentially adjusted absent the imposition of capital controls. To shed light on the validity of our assumption, we assume a placebo episode of capital controls a year before the actual restrictions, i.e. in June 2014. We fail to find any statistically significant effect, which supports our underlying identifying assumption that trade adjusts because of capital controls and not because of any systematic pre-existing differences across sectors within the same firm. Our findings remain robust across a number of robustness checks that could bias our estimates, such as alternative channels in place both at the firm and the sector level, alternative proxies of sector's credit needs and alternative assumptions of the distribution of the errors. Further, we complement our results with a dynamic analysis to examine the interplay of credit constraints with the gradual resolution of economic and political uncertainty following the imposition of capital controls. We show that the effect of the short-term credit channel of capital controls on trade was short-lived (i.e. 2015Q3), which is likely associated with the resolution of uncertainty and domestic banks' recapitalization in 2015Q4. Taken together, our results suggest that, although the effect is *ex ante* ambiguous, credit constraints become more, not less, binding during prudential capital controls. The impact works primarily through short-term working capital needs, rather than long-term access to finance. Our findings also indicate that binding credit constraints affect the intensive, rather than the extensive, margin of exports, which is line with the theoretical models of Feenstra et al (2014) and Manova (2013).

Although our baseline cross-sectoral within-firm specification provides robust evidence of a decline in exports in sectors with high dependence on short-term credit needs following the imposition of capital controls, an important question related to the

[4] For example, it is not sufficient to assume that Italy has the same demand for Greek olive oil and vegetables or Greek olive oil is equally demanded in Italy and Spain. In both cases, it would be sufficient to control for destination and sector fixed effects, respectively.

underlying mechanism remains open. Our hypothesis is that banks enter the capital controls period with weak balance sheets. Intuitively, absent any prudential controls, the domestic banking system would have collapsed, so restrictions on capital outflows would have proved successful as a crisis containment tool (Demirguc-Kunt and Serven, 2009). To formally test this hypothesis, we exploit the sudden stop of ELA as a natural experiment that triggered the imposition of capital controls and exploit pre-determined dependence on ELA of Greek banks along with a novel dataset on matched bank-firm credit exposures data. That will help us identify the bank lending channel of capital controls, isolating credit supply from demand, by employing firm fixed effects (Kwaja and Mian, 2008). To put it differently, *for the same firm*, which borrows from multiple banks with differential exposure to ELA before the shock, we compare the adjustment in credit from more compared to less exposed banks. We find that 10% higher exposure of a bank to ELA is related to 0.37 percentage points lower credit to the *same* firm after capital controls[5].

As a final step to explore further the credit channel of capital controls at the firm level, we follow closely Paravisini et al (2015) by matching trade data with our credit data. First, although firms can theoretically substitute credit from banks less exposed to the sudden stop of the ELA, we find that this is not the case and that 10% higher exposure of firms (through their banks) to the liquidity shock reflects 2pp lower *overall* credit at the firm level. Matched with the trade data and employing an instrumental variables approach, we find that a decline in the overall firm's credit of 1% implies a reduction in exports by 0.17% following capital controls.

The remainder of the paper is structured as follows. In section 2, we provide a review of the related literature. In section 3, we discuss the institutional background of capital controls in Greece. Section 4 discusses our identification strategy and describes the data used in our empirical analysis, while section 5 presents our baseline results. In section 6,

[5] Compared to other countries, our results are free from selection bias, because Greek firms traditionally retain active lending relationships with multiple banks.

we present estimates of the bank lending channel of capital controls and in section 7 we provide further evidence on the mechanism at the firm level. Section 8 discusses potential threats to our identification strategy. Section 9 concludes.

## 2. Related Literature

Our paper contributes to the existing literature in several ways. First, to the best of our knowledge, this paper is the first to explore the impact of the credit channel of capital controls on international trade relying on micro data. Although the different microeconomic channels have been theoretically discussed in Tamirisa (1999), previous literature has focused on aggregate data across countries, which makes it difficult to establish causal links and pin down specific channels through which capital controls affect trade. Tamirisa (1999) studies the effects of capital and exchange restrictions on trade for a sample of 40 countries in 1996 and concludes that capital account liberalization is beneficial for trade. However, the empirical model did not include importer and exporter fixed effects as required by the theory (e.g. Anderson and van Wincoop, 2003) and, thus, suffered from misspecification. Wei and Zhang (2007) extend the analysis of Tamirisa (1999) and find that FX controls and stricter controls on trade payments reduce trade. In contrast to these studies, we exploit a policy experiment of prudential capital controls in Greece along with an extensive micro data set of trade and bank credit data. This allows us to deal effectively with endogeneity concerns surrounding the imposition of capital controls and to identify a specific credit channel in place.

Second, our paper contributes to a new literature that focuses on the real effects of capital controls. Alfaro, Chari and Kanczuk (2017) evaluate the impact of capital controls on inflows on firms' investment decisions in Brazil. Employing an event study methodology, they document a drop in cumulative abnormal returns of firms following the announcement of prudential capital controls measures. Keller (2018) studies the impact of controls on carry trade inflows in Peru and shows that banks switch lending to local

firms from local currency to dollars in order to hedge dollar deposits. Following a depreciation shock, the dollarization of firms' debt has a negative effect on employment as firms become more financially constrained. In contrast to these studies, we study how the *ex ante* ambiguous effect of prudential capital controls on credit supply affected trade at both the sector and the firm level. Our paper also belongs to a wider literature that provides micro-evidence on the effects of capital controls on different outcome variables. For example, Forbes (2003) studies the impact of Chilean encaje controls and documents increased financing costs for small traded firms. A more recent paper by Forbes, Fratzscher, Kostka and Straub (2016) shows how investors re-allocate their portfolios away from Brazilian assets following a tax on foreign investments in bonds, as well as away from countries that seem more likely to impose similar restrictions[6].

Third, we add to a small but rapidly growing literature that studies the intended and unintended consequences of macroprudential regulation. The focus of this literature has been on evaluating different macroprudential instruments both across and within countries. For example, Cerutti et al (2015) show that borrower-based and bank-based measures reduce credit growth in a sample of 119 countries in the period 2000-2013. Similar results on the impact of macroprudential policies on credit growth are recently obtained in Ayyagari et al (2017)[7]. In contrast to this literature, we study capital flow management measures as a form of macroprudential regulation, where international capital flows are the source of domestic financial instability. Capital controls are used to restore financial stability, but may also have unintended consequences on the real economy.

Fourth, we add to the literature that explores the impact of credit constraints on international trade[8]. Earlier studies that employ micro data have relied on balance-sheet variables, credit scores and credit rationing surveys to proxy for financial constraints (e.g. Muuls, 2008; Minetti and Zhu, 2011). However, the main empirical challenge with this approach is that firm's financial health is endogenous to its exporting activity (e.g. Farre-

[6] A survey of this literature is provided by Forbes (2005a).
[7] Claessens (2014) provides an overview of this literature.
[8] A survey of this literature that uses macro- and micro-data is provided by Foley and Manova (2015).

Mensa and Ljungqvist, 2016). More recently, papers started exploiting exogenous shocks to the available financial capital to establish causal a link from credit to trade. Two important papers that document the importance of credit in international trade are Amiti and Weinstein (2011), who study the Japanese crisis in the 1990s along with matched bank-firm data and Paravisini et al (2015), who employ matched customs-credit registry data for Peru and the global financial crisis as a natural experiment. Manova et al (2015) also show that multinational firms export more in sectors with higher dependence on credit, because of access to parent's internal funds. In contrast to these studies, we exploit a different source of friction, which is the imposition of capital controls to stabilize the domestic banking system. In contrast to a credit crisis, because of the prudential nature of the regulatory measures, the impact of capital controls on credit and, as a result, on trade is *ex ante* ambiguous.

## 3. Institutional Background

Following the failure of the Greek parliament to elect a President of the Republic in December 2014, general elections in January 2015 brought into power a coalition government of the radical left party of SYRIZA with the smaller right-wing party of "Independent Greeks". Both parties had fiercely opposed the economic adjustment program, which had been agreed by previous governments with the troika of international lenders (European Central Bank, International Monetary Fund and EU member countries) following the bailout of the Greek sovereign in May 2010. Lengthy negotiations and increased uncertainty over a new bailout plan and Greece's future within the Eurozone drove depositors to withdraw 48.6bn euros during the first six months of 2015, accounting for more than one quarter of deposits of the Greek banking system. At the same time, Emergency Liquidity Assistance (ELA) provided by the Bank of Greece to Greek banks increased drastically from 45bn to 127bn euros (including Eurosystem funding). At the time of imposition of capital controls in June 2015, deposits accounted for less than 50%, while central bank funding (Eurosystem and ELA) accounted for more than 50% of total bank liabilities. Figure 1 presents the evolution of funding for Greek banks during this period.

The new bailout agreement between the Greek government and the troika was brought into a referendum, which was unexpectedly announced on June 27, 2015[9]. As a response, the ECB refused to increase its loan limit for the provision of ELA to Greek banks on the same day, triggering the shutdown of banks (bank holiday) and the imposition of capital controls on June 28 2015[10]. Capital controls can be broadly characterized as restrictions on capital transactions and comprised of three pillars: (a) measures to prevent outflows of funds abroad, (b) measures limiting cash withdrawals from banks and (c) measures to prevent the rapid decline of bank assets and liabilities (e.g. repayment of the remaining capital on bank loans). Despite these measures, there was no explicit restriction on the provision of credit by financial institutions. In addition, during the first phase of restrictions, all credit institutions operating in Greece, including branches of foreign banks, were forced to close until July 20, 2015, the Athens Stock Exchange remained closed and daily cash withdrawals were limited to a maximum of 60 euros per depositor per bank. No capital restrictions were applied to credit cards issued by foreign banks. During this phase, a special Banking Transactions Approval Committee (BTAC) was established to approve transfers of funds abroad and transactions deemed as necessary for public interest (e.g. imports of pharmaceutical products).

Following the end of the bank holiday, certain transactions were approved, such as early redemption of time deposits and payment of tuition fees to foreign universities. To reduce the burden of documentation requirements submitted to BTAC, special subcommittees were established in each financial institution to approve or reject the transfer of funds abroad. The special subcommittees were responsible for the approval of transfers abroad under a certain threshold. Transfers larger than this threshold had to be approved by BTAC (the central committee). The threshold of the total amount of transfers abroad by any individual firm was set initially at 100,000 euro per working day. This limit has been gradually increased to 250,000 euros by September 2015 and to 350,000 by July 2016. Following the agreement over the new bailout plan between the Greek government and

[9] Although the new bailout plan was rejected in the referendum of July 5, the Greek government came to an agreement with lenders for a new bailout program which envisaged financing of up to 86bn euros over a three year period in exchange of a programme of fiscal austerity measures and structural reforms.

[10] Although Bank of Greece is responsible for ELA funding, it is ECB's decision to extend or restrict the ELA ceiling, i.e. the maximum amount of ELA available to Greek banks

the troika in August 2015 and a new round of elections in September 2015, capital restrictions were gradually relaxed and the Athens Stock Exchange re-opened in December 2015.

Nevertheless, during this early period of capital controls, external trade adjusted and, in particular, the export performance of firms deteriorated. Figure 2 presents the evolution of exports growth on a yearly basis. Anecdotal evidence suggests many factors contributing to the reduction of exports. For example, the establishment of a central committee to gather an enormous amount of requests regarding transfer of funds abroad increased considerably the cost of accessing available liquidity. Along with any application, firms were required to submit a significant amount of information and documentation that most of the times were not easily available. The purpose of this centralized process was to minimize the probability of firms over-invoicing imports or under-invoicing exports and, thus, evade capital restrictions. A second major impediment was that (short-term) credit and specialized banking products, such as letters of credit, documentary collection and bank guarantees became costly or even unavailable. Anecdotal evidence suggests that concerns over the solvency of the Greek banks as well as the uncertain prospects of the economy reduced the availability of credit and of similar products. Figure 3 presents the evolution of total and short-term credit to the manufacturing sector in the period before and after capital controls. During the six months following the imposition of capital controls, short-term credit to manufacturing companies declined by 4.9% and total credit by 1.9%[11].

## 4. Identification Strategy and Data

### *4.1 Identification*

We identify a specific channel of prudential capital controls on trade, *the credit channel*, by interacting a firm's domestic ownership structure with a sector's credit needs. In other words, we look at the change in exports before and after capital controls within the *same domestic* firm, which exports products belonging to multiple sectors with different degree

[11] Rousakis and Priftis (2017) provide an analytical overview of these later stages of the Greek crisis.

of dependence on external finance. Our strategy allows us to control for firm fixed effects, which absorb all observed and unobserved firm heterogeneity that matters in international trade, including a firm's available internal liquidity. To further isolate the supply-side effect of the credit channel of capital controls, we include a comprehensive set of destination*sector dummies to account for demand for Greek exports as well as other unobservables across destination-sector markets, such as tariffs and transportation costs. To make sure we do not introduce any bias related to Greek banks' recapitalization in December 2015, we exploit the monthly frequency of our trade data and exclude December 2015 (and us such December 2014) from our baseline regressions. Because trade data are highly seasonal, we compare performance of exports before and after capital controls during the same calendar months. In other words, our pre-post sample periods are defined as *pre*={June 2014 – November 2014} and *post*={June 2015 – November 2015}. We proceed by collapsing the exports time-series into two periods, because of a large number of intermittent export flows and consider an active flow if it registered positive exports at any time during the respective period. We then estimate the intensive and extensive margins of trade, using a difference-in-differences methodology, as follows:

$$\Delta\log(exports)_{fds} = \beta_1 \times Domestic_f \times Credit\ Constrained_s + \alpha_{ds} + \alpha_f + \varepsilon_{fds},$$

$$Pr(exit)_{fds} = \beta_1 \times Domestic_f \times Credit\ Constrained_s + \alpha_{ds} + \alpha_f + \varepsilon_{fds},$$

Our dependent variable, $\Delta\log(exports)_{fds}$, is exports' growth of firm $f$ of products belonging to sector *s* going to destination *d* after imposition of controls and is winsorized at the 1st and 99th percentiles to minimize the probability that outliers drive our findings. $Pr(exit)_{fds}$ denotes the probability of terminating an existing export flow of firm's $f$ products belonging to sector *s* to destination *d*. We define the termination of the relationship as export flow active in the *pre*, but not in the *post* period. *Domestic* is a dummy variable that equals 1 if the firm has more than 50% equity stakes owned by domestic investors. Because foreign investors tend to buy domestic firms at fire-sales

prices during a liquidity crisis (Aguiar and Gopinath, 2005), it is important to measure a firm's ownership structure *before* capital controls. As a result, *Domestic* reflects a firm's ownership structure as of December 2013. The ownership structure of a firm is our preferred measure of access to finance, because the geographic allocation of a firm's internal liquidity matters more during capital controls on outflows rather than a normal credit crisis[12]. We employ four measures to proxy for sector's credit conditions ($Credit\ Constrained_s$), which include External Finance Dependence, Asset Tangibility, Liquidity Needs and Trade Credit Intensity. *External Finance Dependence* is defined as capital expenditures not financed by internally generated cash flow for the median firm averaged over the 1980s. It reflects a sector's need to obtain long-term external funds. *Asset Tangibility* is defined as fixed assets to total assets and reflects a sector's ability to obtain outside funds. *Liquidity Needs* is defined as the ratio of inventories over sales for the median firm over the 1980s[13] and reflects a sector's dependence on short-term working capital. As Kroszner et al (2006) discuss, *External Finance Dependence* is a broader measure of sectos's financial needs and may better capture reliance on long-term bank credit, compared to the *Liquidity Needs* ratio that captures very short-term working capital needs. Finally, *Trade Credit Intensity* is defined as the ratio of accounts payable over total assets and is a substitute for bank credit when this is scarce or unavailable. To remain consistent, the first three measures are from Kroszner, Laeven and Klingebiel (2006), while the latter is from Fisman and Love (2003). The terms $\alpha_{ds}$ and $\alpha_f$ are destination*setor and firm fixed effects respectively. Standard errors allow for correlation at the destination-sector level.

The coefficient of interest is $\beta_1$. The coefficient has a different meaning for the intensive and extensive margins of exports. At the intensive margin, it measures the change in exports of sectors with different credit needs for the same domestic firm compared to the pre-controls period and compared to multinational companies. At the extensive margin, it measures the change in the probability of termination of an existing export flow of goods

[12] Ownership structure is extensively used in the literature to proxy for liquidity constraints. See among others, Manova et al (2015) and Kalemli-Ozcan et al (2016).

[13] In our baseline regressions, both *External Finance Dependence* and *Liquidity Needs* are measured over the 1980s. Later on, we show that our conclusions are not sensitive on how we calculate sector-level measures of credit constraints.

belonging to sectors with different credit needs for the same domestic firm compared to the pre-controls period and compared to multinational companies. Following the cap in the provision of the ELA, under the fear that Greek banks would turn illiquid, capital controls were announced to contain the liquidity shock. If capital controls contained the shock, then there should be no differential adjustment in exports of sectors with different credit needs suggesting that credit constraints become less binding. In that case, $\beta_1$ should be statistically indistinguishable from zero. If, despite the restrictions on capital outflows transactions, banks' balance sheets are weak, then there should be a differential adjustment in exports of sectors with different credit needs suggesting that credit constraints become more binding. In that case, $\beta_1$ should be statistically distinguishable from zero.

### *4.2 Data*

For our baseline regressions, we combine administrative firm-product-destination level data along with detailed confidential firm level reports. We obtain administrative data on exports (FOB) of goods for the universe of Greek firms at annual and monthly frequency in the period 2005-2015 from the Hellenic Statistical Authority (ELSTAT). Data report export values (in euros) and volumes (in kgr) at the firm-product (5-digit SITC Rev.4)-destination level. We restrict our attention to the period before (*pre*) and after (*post*) capital controls as described above, which returns just above 1,000,000 observations. The monthly frequency of our data allows identifying precisely different financial events, addressing potential endogeneity issues. First, we drop a handful of destinations that are not named for confidentiality reasons. Second, we aggregate 5-digit SITC (Rev.4) products in 3-digit ISIC (Rev.2) sectors to exploit variation on credit dependence at the sector level. Since there is no concordance table to map directly, we first map 5-digit SITC Rev.4 to 6-digit HS 2007 products[14] and then 6-digit HS 2007 products to 4-digit ISIC Rev.2 industries[15]. We then aggregate at the 3-digit level. This leaves us with 905,649 observations. We subsequently restrict our attention to the manufacturing sector, as defined by the ISIC classification (code 3). Data report export flows for both intra-EU

[14] Table is from UN (https://unstats.un.org/unsd/cr/registry/regdnld.asp?Lg=1).
[15] Table is from WITS (http://wits.worldbank.org/product_concordance.html).

(Intrastat) and extra-EU (Extrastat) transactions. Intrastat refers to the trading of goods between EU Member States, while Extrastat refers to the trading of goods with third countries. Firms that perform intra-EU transactions are liable for providing statistical information to ELSTAT, while firms that perform extra-EU transactions fill the Single Administrative Document (SAD) and submit it to Customs Authorities. Documents are then transmitted to ELSTAT, which is responsible for compiling the total trade data within and outside the EU[16]. Although Extrastat system records virtually all flows, EU National Authorities impose statistical thresholds for intra-EU trade, below which Intrastat declarations are not submitted by firms[17]. ELSTAT has set exports' exemption reporting thresholds at 90,000 euros in 2014 and 2015. Data below the statistical threshold are still included in the Intrastat database and are estimates based on the Recapitulative Statements of intra-EU Deliveries and Acquisitions that all firms submit for fiscal purposes to the Ministry of Finance. Essentially, these are administrative documents that *all* firms are obliged to submit and thus can be considered of high quality. A downside of our data is that firms are marked with a unique numerical (anonymized) ID. We retrieve tax identifiers, by exploiting a second dataset that reports export values (in euros) and volumes (in kgr) at the firm-product (5-digit SITC)-destination level, and overlaps with our entire database in years 2005-2009[18]. Crucially, export flows in both datasets (anonymized and identified) are administrative and include the *same* information at the *same* level of disaggregation (i.e. firm-product (5-digit SITC)-destination level), however, data have been extracted from ELSTAT's intra-EU and extra-EU databases at different points in time. Thus, the first and more recent (anonymized) dataset is richer than the second (identified) dataset, because of subsequent revisions by ELSTAT. To conserve space here, we describe how we recover tax identifiers and discuss selection bias concerns in the Appendix.

We proceed by matching with detailed confidential firm level reports that provide an overview of the company's ownership structure. The reports are provided by ICAP,

[16] Since 2005, it is mandatory for member states to check the quality of the declared information.
[17] On an annual basis, ELSTAT sets and releases imports and exports exemption thresholds in order to reduce the reporting burden on firms.
[18] We access these data through the Bank of Greece.

which is the largest business registry in Greece and the primary source of information for ORBIS in Greece. Although the ORBIS database is considered to provide high-quality ownership information and, as a result, has been extensively used in the literature (e.g. Franks et al, 2012; Fons-Rosen et al, 2013), there are data gaps, missing information and double entries for many companies[19]. Although it is data-intensive, we overcome these shortcomings by collecting ownership structure information directly from the reports. A standard report provides a detailed overview of firm's operations, such as financial data, bankers and branches, suppliers, shareholders and board of directors, as well as their management histories as of today. Crucially for our purposes, as-of-today corporate control information is complemented by detailed changes in the ownership structure, their timing and the precise share of stakes owned by each individual shareholder[20]. This allows us to track acquisitions that took place after the end of 2013 and, most importantly, after the imposition of capital controls and minimize any endogeneity concerns regarding ownership structure changes during a liquidity crisis (Aguiar and Gopinath, 2005).

### *4.3 Summary Statistics*

We start by providing some basic summary statistics surrounding our experiment in Table 1. Our baseline sample includes 2,767 firms, of which 2,421 are domestic and 346 are multinational companies. The share of total exports is balanced, in particular 50.3% and 49.7% respectively. We also distinguish between exports of domestic vis-a-vis multinational companies in sectors with different credit conditions before and after the imposition of capital controls. Some interesting patterns arise. First, domestic firms reduce the fraction of total exports in sectors with high dependence on long- and short-term credit following capital controls. In contrast, domestic firms increase the fraction of total exports in sectors with more tangible assets and, thus, ability to borrow and high trade credit following capital controls. Interestingly, we observe the opposite pattern for

[19] An important step to clean and update ORBIS's corporate control data has been made by Aminandav and Papaioannou (2017).

[20] To give an example, we see the ownership structure (i.e. names of sharehoders and precise stake ownership of each shareholder) of firm X, until it was fully acquired by a foreign company in December 2016. As of today, firm X is part of a multinational company but back at the time of the imposition of capital controls in June 2015, it was a domestic company. Thus, firm X is classified as a domestic firm.

multinational firms. These patterns suggest that access to internal capital markets may be particularly beneficial for financially vulnerable sectors and less important for sectors with high ability to borrow and alternative sources of bank credit.

## 5. Results

### *5.1 Baseline*

Table 2 presents our baseline estimates for both margins of exports. At the intensive margin, we find that exports decline more in sectors high external finance dependence, i.e. high long-term credit needs (column 1) for the same firm compared to the pre-capital controls period. Economically, compared to the exports of a sector at the 25$^{th}$ percentile, the exports of a sector at the 75$^{th}$ percentile of the distribution of long-term credit dependence across industries drop by 8pp for the same domestic firm after capital controls. Statistically, however, the effect is marginally significant providing little support for the long-term credit channel of capital controls. In column 3, we find both statistically and economically strong evidence for the short-term credit channel of capital controls on trade. The coefficient -3.265*** of the interaction term *domestic*Liquidity needs* says that compared to the exports of a sector at the 25$^{th}$ percentile, the exports of a sector at the 75$^{th}$ percentile of the distribution of short-term credit dependence across industries drop by 26pp for the same domestic firm after capital controls. In column 4, we find that trade credit substitutes for bank credit when this is costly or not available. The economic magnitude is approximately 12pp higher exports of a sector at the 75$^{th}$ percentile compared to the exports of a sector at the 25$^{th}$ percentile of the distribution of trade credit intensity across industries for the same domestic company following the imposition of controls. Interestingly, although the coefficient for sector's higher ability to borrow is positive for the same domestic firm, it is not statistically significant (column 2). This suggests that the ability to borrow during capital controls may not alleviate credit constraints. At the extensive margin (columns 4-8), we fail to find statistically significant effects across all four measures of a sector's credit conditions. This finding suggests that a firm does not terminate existing export flows to destination-sector markets, because it has already paid the fixed costs to enter. These costs are considerably higher than the trade-related variable costs, which are associated with the intensive margin of exports.

Taken together, these results suggest that despite the prudential nature of controls on capital outflows transactions, exports drop more in sectors with higher credit needs within the same firm, but they do not terminate. This finding indicates that credit constraints become more, not less, binding during capital controls. Our results echo the findings of the previous literature that capital controls are associated with binding financial constraints (e.g. Forbes, 2007). We find stronger evidence, both statistically and economically, for the short-term credit channel of capital controls, which is in line with the findings of Paravisini et al (2015) that credit shocks restrict access to short-term working capital, rather than long-term access to finance, which in turn affects the intensive margin of international trade.

### *5.2 Robustness Checks*

These findings are obtained assuming that exports of the same firm of products belonging to sectors with differential credit needs behave similarly before capital controls. If this assumption is not valid, i.e. if a firm differentiates its exports depending on products' credit intensity, our results may simply capture pre-existing differences in export trends rather than the impact of capital controls. To formally test whether our underlying assumption is valid, Table 3 presents the results of a hypothetical policy experiment as if capital controls were imposed in June 2014, where *placebo-pre*={June 2013 – November 2013} and *placebo-post*={June 2014 – November 2014}. We fail to find any statistical significant evidence of differential export behavior within the same firm suggesting that our baseline estimates indeed capture the effect of the credit channel of capital controls on trade.

Next, we account for the possibility that bigger firms self-select themselves in sectors with different credit needs. It could well be the case that a big firm exports products of high financing needs in order to enjoy a comparative advantage within the sector. If the firm's size channel is at place, then our estimates may capture more than access to finance, as this is proxied by the ownership structure. Measuring a firm's size as the (log of) total assets before capital controls and interacting with the respective sector level

financial measure, we effectively control for the size channel. Table 4 shows that our conclusions remain unchanged for both margins of trade. The economic magnitude of the short-term credit channel of capital controls on trade drops from 26pp to 24pp, while for the long-term credit and trade channels it is 8pp and 9.5pp, respectively. This suggests that the firm's size channel is not a confounding factor of our baseline estimates.

We further control for additional channels at the sector level in Table 5. Capital controls imply increased uncertainty and a breach of existing contracts, so the impact on trade may go beyond a credit channel. It could well be the case that a domestic firm cuts exports of less contract intensive sectors. For these sectors, intermediate inputs are sold on thick markets and require less relationship-specific investments (Nunn, 2007). Similarly, following capital controls, exports of sectors with higher physical capital intensity may drop more within the same domestic firm compared to sectors with a higher share of intangible assets. If these channels are at play, our baseline estimates might be inflated and give a distorted picture of the credit channel of capital controls on trade. We account for these explanations by interacting the domestic ownership structure dummy with a sector's contract and capital intensity. The coefficient of the short-term credit channel drops to 2.860** suggesting that the differential exports behavior of sectors at the $25^{th}$ and $75^{th}$ percentile of the distribution is about 22pp.

Next, we repeat our regressions by excluding the period of the bank holiday (July 2015) and including the period of banks' recapitalization (December 2015). The bank holiday month restricted completely the provision of credit, so we want to ensure that our findings are not driven by the closure of banks. The recapitalization month increased considerably the capacity of banks to provide credit, so we want to understand how the credit channel of capital controls on trade adjusted. Table 6 presents the results when we exclude the bank holiday month. The estimated effect of the short-term and trade credit channel become smaller, suggesting that the complete shutdown of the banking system as part of prudential capital controls measures increases the negative effect on the real economy. Interestingly, the long-term credit channel at the intensive margin enters statistically insignificant and the short-term credit channel at the extensive margin enters

statistically significant at the 5%. Table 7 presents the results when we include the banks' recapitalization month. The increased capacity of banks to provide credit makes our estimated effect of capital controls on trade slightly smaller. Interestingly, a sector's dependence on trade credit enters statistically significant at the 11% level, suggesting that when banks' balance sheet capacity improves, trade credit is of less importance (e.g. Fisman and Love, 2003). Also, a sector's ability to obtain credit (*Asset Tangibility*) enters marginally statistically significant, suggesting that a well capitalized system can mitigate the negative effect of capital controls on the real economy.

One concern is that our sector level proxies of financial characteristics, which reflect credit dependence during the 1980s, may poorly capture credit conditions today. This could well be a concern if sectors' credit characteristics vary over time. Although what matters is the relative ranking of sectors (Rajan and Zingales, 1998), we address this concern by considering alternative measures for a sector's long- and short-term credit dependence measured over the period 1980-1999. Table 8 confirms the weak support for the long-term access to finance channel (it becomes statistically significant at the 11%) and the strong support for the short-term working capital channel of capital controls on trade.

As a final sensitivity test, we question our assumption on the distribution of the errors. Although standard errors allow for correlation at the destination-sector level, we re-run our regressions performing double clustering at the destination-sector and firm level, as well as triple clustering at the destination, sector and firm level. We confirm that the results (not shown, but available upon request) remain unchanged and our conclusions hold under alternative assumptions for errors.

### *5.3 Dynamic Effects*

Our central result is that exports of products belonging to sectors with higher short-term credit needs within the same domestic firm drop more compared to sectors with lower short-term credit needs. In this section, we try to relate the differential drop in exports to political and financial events during capital controls. Following the agreement over the

terms of a new bailout program between the Greek government and troika in August 2015, capital controls were partly relaxed in September 2015, a new round of elections partly resolved political uncertainty in September 2015 and banks were successfully recapitalized in December 2015. Broadly, much of the uncertainty was resolved in 2015Q4 and gradually deposits started flowing back to the banking system (Figure 1).

Motivated by these events, we re-run our baseline models on a yearly basis quarter-by-quarter. In other words, for both margins of trade, we compare the exports of sectors with different credit dependence in 2015Q3-2014Q3 and 2015Q4-2014Q4 within the same domestic firm. Figure 4 plots the estimated effect of the short-term credit channel of capital controls on the intensive margin of trade. Interestingly, for the same domestic firm, the coefficient turns negative when the first signs of political uncertainty emerge in December 2014 and remains negative, but not statistically significant, in 2015Q1 and 2015Q2, which was marked by increased economic uncertainty. It turns statistically significant only in 2015Q3, which marked the bank holiday and the imposition of capital controls. The estimated effect of the short-term credit channel of capital controls on trade, although negative, turns statistically insignificant in 2015Q4, which is in line with the gradual resolution of uncertainty and Greek banks' recapitalization at the end of 2015.

Figure 5 plots the estimated effect of the short-term credit channel of capital controls on the extensive margin of trade. The probability of an existing export flow to terminate in sectors with higher credit dependence increases as economic uncertainty increases in 2015Q1. In all quarters of 2015, the estimated parameters imply that, for the same domestic company, the probability of exiting an export flow gets higher the more credit dependent the sector is. However, the coefficient is not statistically significant and, interestingly, it increases less in 2015Q4.

Note that in both margins of exports, the estimated coefficients are statistically insignificant in our baseline *pre* period (i.e. compared to *pre-1* period), which adds to the validity of our identifying assumption of no pre-existing trends in exports of sectors with differential credit conditions within the same firm.

Overall, in line with our previous findings when we include the banks' recapitalization month in our regressions (Table 7), the dynamic analysis suggests that a well capitalized banking system, as well as the resolution of economic and political uncertainty, alleviates the negative consequences of capital controls on the real economy.

## 6. Why Is There a Credit Crunch During Capital Controls on Outflows?

In our analysis, we have provided causal evidence that, for the same domestic firm, exports of sectors with higher credit needs drop more after capital controls. Because exports data are seasonal, we compared exports during capital controls to exports one year earlier, where capital controls were plausibly unanticipated. Also, a sector's credit conditions are plausibly exogenous to any individual firm within the sector and, to further push for causality, we measured a firm's ownership structure as of end 2013, which is well before the imposition of capital controls. In this section, we want to understand the mechanism behind the binding credit constraints during prudential capital controls.

As of June 2015, deposits accounted for less than 50% of the four systemic banks' balance sheets and Eurosystem funding, of which most was in the form of emergency liquidity (ELA), accounted for more than 50% of total liabilities. The decision of the Greek government to bring the terms of a new bailout agreement with troika into a referendum triggered the sudden stop in the provision of the ELA and, on the same day, the imposition of capital outflows restrictions. Although capital controls were designed to contain the liquidity shock, our hypothesis is that the sudden stop in the provision of the ELA affected domestic banks differently based on their pre-determined exposure to the emergency liquidity mechanism, which weakened considerably their balance sheets.

We test for the bank lending channel by exploiting the sudden stop of the ELA provision as a natural experiment, pre-determined exposure of the Greek banks to the ELA and novel matched bank-firm credit exposures data. The data are similar to a credit registry, in the sense that we observe the outstanding credit at the bank-firm level. However, there are two downsides. One, data capture all loans above 1 million Euros, which implies that

the distribution of our sample firms may be negatively skewed. Two, data are of semi-annual frequency, which implies that we must include December 2015 (i.e. Greek banks' recapitalization month) in our model. As such, the estimates should be seen as a lower bound of the total effect. Formally, we estimate:

$$\Delta\log(credit)_{bf} = \beta_1 \times ELA_{b,12/2013} + \beta_2 \times Z_{b,12/2013} + \alpha_f + \varepsilon_{bf},$$

where $\Delta\log(credit)_{bf}$ is the growth rate of credit in December 2015-December 2014 and is winsorized at the 1st and 99th percentiles to minimize the probability that outliers drive our findings, $ELA_b$ is a bank's pre-determined exposure to the ELA, $Z_b$ is a matrix of pre-determined bank level controls and $\alpha_f$ is a set of firm fixed effects. To overcome endogeneity issues associated with the banks' exposure to the ELA, we measure dependence before the shock. However, the level of the ELA in all Greek banks was zero in both December 2014 and June 2014 (i.e. the end and start of our *pre* period), which further adds to the "normality" of our *pre* period. As a result, we exploit variation in the exposure of the banks to the ELA as of end 2013. Importantly, the ranking of the four systemic banks in terms of exposure as of end 2013 and as of the time of the shock was the same. We also include pre-determined (as of end 2013) bank level controls, such as bank's size and capital, and to isolate credit supply from concurrent changes in credit demand during capital controls, we include a set of firm fixed effects (Kwaja and Mian, 2008). Note that by collapsing the data into a *pre* and a *post* period, we effectively control for all time invariant heterogeneity at the bank level.

Table 9 presents our results. In column 1, we obtain an estimate -0.033***, which implies that a 10% higher dependence of a bank on the ELA lowers available credit for all firms by 0.33pp. The effect becomes larger when we account for standard controls at the bank level and credit demand by employing firm fixed effects (column 2). In terms of magnitude, we find that 10% higher exposure on ELA lower credit by 0.37pp. Restricting our attention to domestic firms only, the coefficient becomes 0.041*** and jumps to 0.050** when we include firm fixed effects to account for credit demand (column 4). In

other words, for the same domestic firm, 10% higher exposure of a bank to the ELA implies half percentage point decline in credit.

These findings are in line with our hypothesis of weak balance sheets of banks during prudential capital controls. A remaining concern is whether our estimates capture any systematic pre-existing differences in credit supply among banks. In that case, the coefficient would be biased and capture more than the liquidity shock. As in our baseline regressions, we assume a placebo episode of capital controls in June 2014 and compare the change in credit in the period December 2014-December 2013 among banks with differential exposure to ELA, as of December 2013, for the same (domestic) firm. The Appendix Table 5 confirms that there were no such systematic differences in the provision of credit one year before the actual imposition of capital controls.

## 7. Further Evidence on the Credit Channel at the Firm Level

In this section, we provide further evidence on the credit channel of capital controls on trade at the (domestic) firm level by matching our trade data at the firm-sector-destination level and our credit data at the bank-firm level with banks' pre-determined exposure to the sudden stop in the provision of the ELA as of December 2013. We follow closely Paravisini et al (2015) and employ an instrumental variables approach, where in the first stage we regress the change in a firm's *total* credit on its banks' dependence on ELA and in the second stage we regress the change in the firm's *total* exports on the change in a firm's *total* credit. In other words, we first ask whether the sudden stop in the provision of the ELA to Greek banks that triggered the imposition of capital controls is associated with the provision of overall credit to firms through their banks' exposure to the shock. It could well be the case that a firm substitutes credit by a more exposed bank from a less exposed bank. In that case, even though credit supply may decline, credit constraints would not bind at the firm level. We find no evidence of credit substitution across lenders. Subsequently, we test whether the reduction in firm's *overall* credit is associated with firm's *overall* exports. Formally, for the first stage regression, we estimate:

$$\Delta\log(credit)_f = \delta_1 \times ELA_{f,12/2013} + \delta_2 \times Z_{f,12/2013} + \mu_{ds} + \mu_p + \eta_f,$$

whereas, for the second stage regression, we estimate the intensive and extensive margins of exports as follows:

$$\Delta\log(exports)_{fsd} = \beta_1 \times \Delta\log(credit)_f + \beta_2 \times Z_{f,12/2013} + \alpha_{ds} + \alpha_p + \varepsilon_{fsd},$$

$$Pr(exit)_{fds} = \beta_1 \times \Delta\log(credit)_f + \beta_2 \times Z_{f,12/2013} + \alpha_{ds} + \alpha_p + \varepsilon_{fsd},$$

where $\Delta\log(credit)_f$ is the growth rate of the overall credit of firm in December 2015-December 2014 and is winsorized at the 1st and 99th percentiles to minimize the probability that outliers affecting our results, $ELA_f$ is the total pre-determined firm's exposure to the ELA shock through its banks, $Z_f$ is a matrix of pre-determined firm level controls[21] and $\alpha_{ds}$ and $\alpha_p$ are a set of destination*sector and firm's province fixed effects. Importantly, in all models, we include an importer dummy to control for the decline in exports due to a drop in imports, which would constrain firms' exports due to lack of imported raw materials and intermediate goods[22].

Table 10 presents the results of the first stage regression. The estimated parameter in column 1 is statistically significant at the 1% level and implies that 10% higher exposure of a firm to the sudden stop in the provision of the ELA lowers firm's *total* credit by 2pp. When we restrict the sample to domestic firms (column 2), the effect becomes larger (-0.227***) and remains statistically significant at the 1% level. In both regressions, the F-statistic is well above the critical values provided by Stock and Yogo (2005), suggesting a strong first stage regression. Taken together, our findings suggest that (domestic) firms face tighter credit conditions during prudential capital controls and this is the result of weak balance sheets of their banks.

Table 11 presents the results of the second stage regression for both margins of exports. In column 1, we obtain a coefficient 0.017 (statistically significant at the 12%), which

[21] Balance sheet data for firms are from ICAP.
[22] Note that about half of Greek imports consist of raw materials, mainly crude oil, and intermediate goods.

implies that a 10% reduction in firm's overall credit lower exports by 0.17%. We fail to find any significant effect at the extensive margin. When we restrict our sample to domestic firms, the effect of credit availability on exports becomes larger (0.027**). The coefficient implies that 10% reduction in domestic firm's overall credit lower exports by 0.27%. Overall our results suggest that firms adjust their exports in light of tighter credit condition during capital controls.

There is a remaining concern that needs to be addressed. In case more exposed (domestic) firms export systematically differently than less exposed (domestic) firms, then our estimates are biased. As before, we re-run our regressions as if controls were imposed in June 2014 in order to explore this possibility. The Appendix Table 6 confirms that our coefficients do not capture any systematic pre-existing differences and trends and, thus, are free from such bias.

## 8. Potential Threats to Identification

We have provided extensive evidence on the credit channel effect of capital controls on international trade. The central message of the paper is that, despite the goal of prudential capital controls regulation to restore domestic financial stability, credit supply drops, because of weak banks' balance sheets, which in turn hurts trade.

A number of political and financial events during our *post* period could pose a threat to our identification. First, the agreement between the Greek government and troika in August 2015 on the terms of a new bailout program, which marked the gradual resolution of economic uncertainty. Second, a new round of elections in September 2015 and the formation of a new government with a clear mandate to implement the economic adjustment program agreed with international lenders. If the resolution of economic uncertainty induced banks to provide more credit or firms to export more, both of these channels would work against us finding an effect or, to put it differently, they would bias our estimates downwards. Hence, our estimates can be considered as a lower bound of the effect of capital controls on trade.

## 9. Conclusion

This paper is, to the best of our knowledge, the first to study the impact of capital controls on international trade using micro data and pinning down a specific channel in place, *the credit channel*. For identification, we use a policy experiment of controls on capital outflows transactions in Greece in June 2015 following a period of increased economic and political uncertainty. Exploiting across-sectors dependence on credit within the same firm, we find that exports of sectors with higher credit needs drop *relatively* more after capital controls. To further inspect the mechanism, we exploit the sudden stop in the provision of Emergency Liquidity Assistance (ELA) as a natural experiment that triggered the imposition of capital controls, Greek banks' pre-determined exposure to the ELA and novel matched bank-firm credit exposures data. We find that, for the same firm, as opposed to banks with low exposure to ELA, banks with higher exposure cut credit *relatively* more to the same firm. We show that firms do not substitute the credit losses by borrowing from less exposed lenders, suggesting that firms' overall credit reduces during capital controls. By using matched trade-credit data, we show that lower overall credit leads to lower overall exports of firms.

Our paper aims to inform the theoretical literature. The renewed interest on capital controls as a form of macroprudential regulation should take into account heterogeneous effects at the firm and sector level. The adverse effects of capital controls are not spread uniformly across firms and sectors, which implies that there are distributional effects from such policies.

Our paper aims to inform policymakers as well. Our central message is that, despite the nature of prudential capital controls as a crisis management tool, yet there is a credit crunch with real effects that crucially depends on the health of the banking system. A better understanding of the costs and benefits associated with the imposition of prudential capital controls is clearly an important area for future research.

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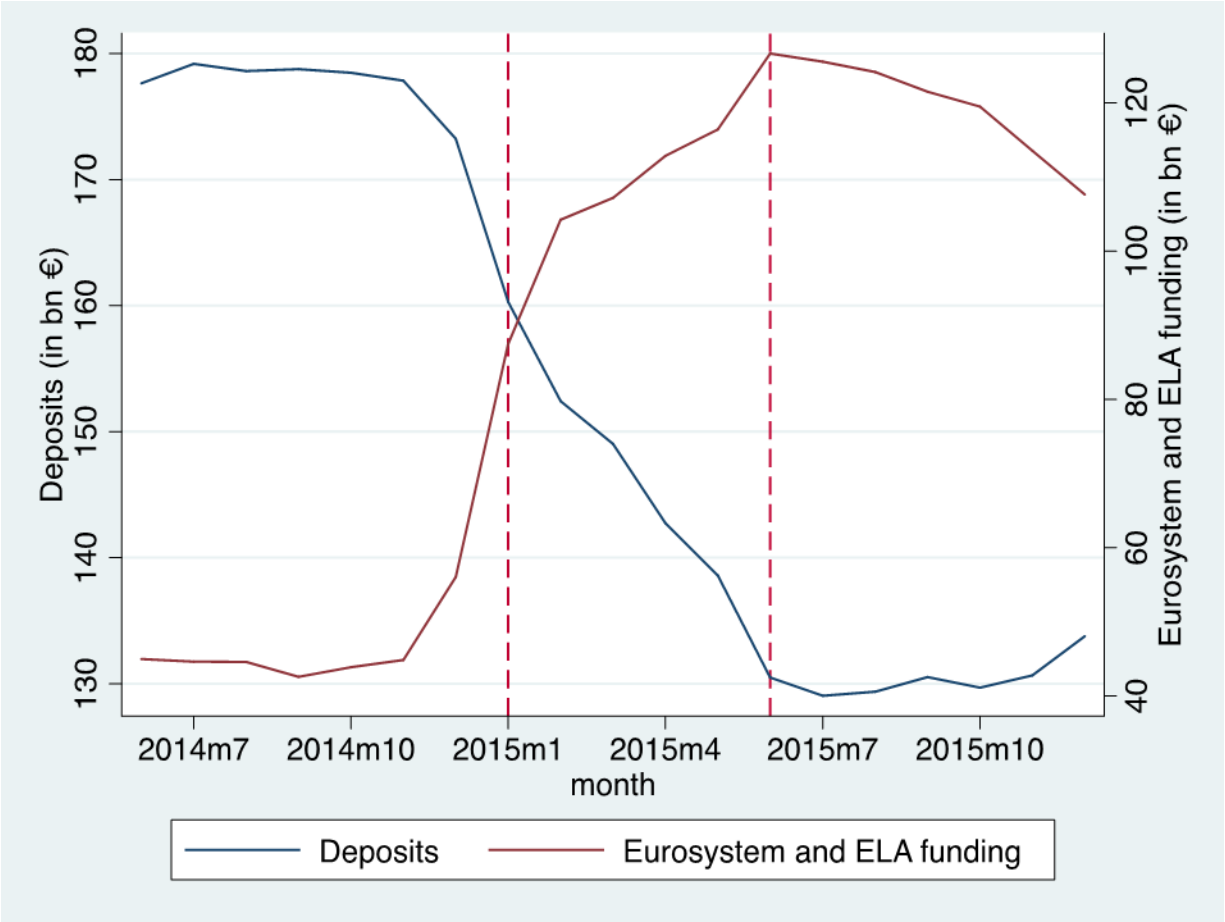
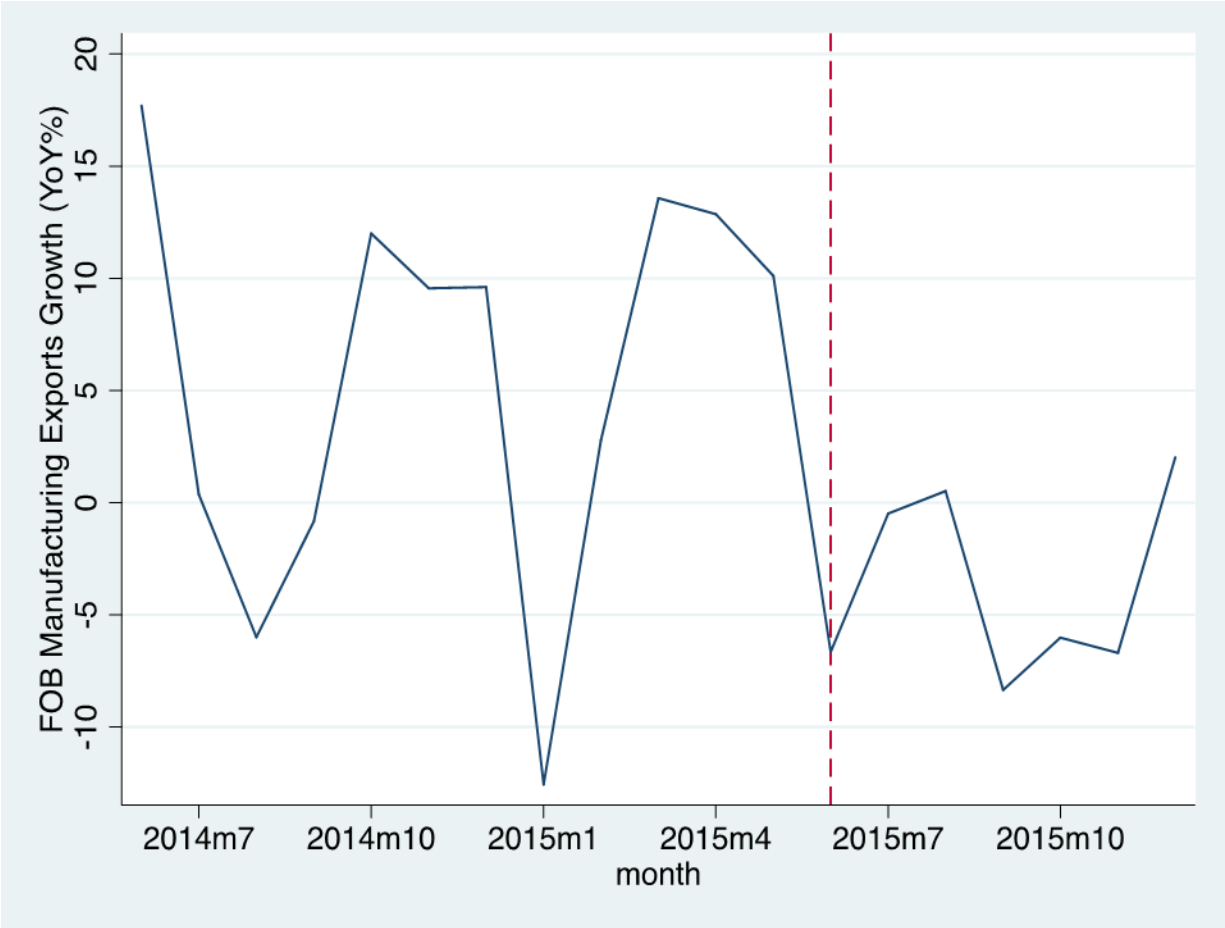
**Figure 1: Evolution of Funding of Greek Banks**



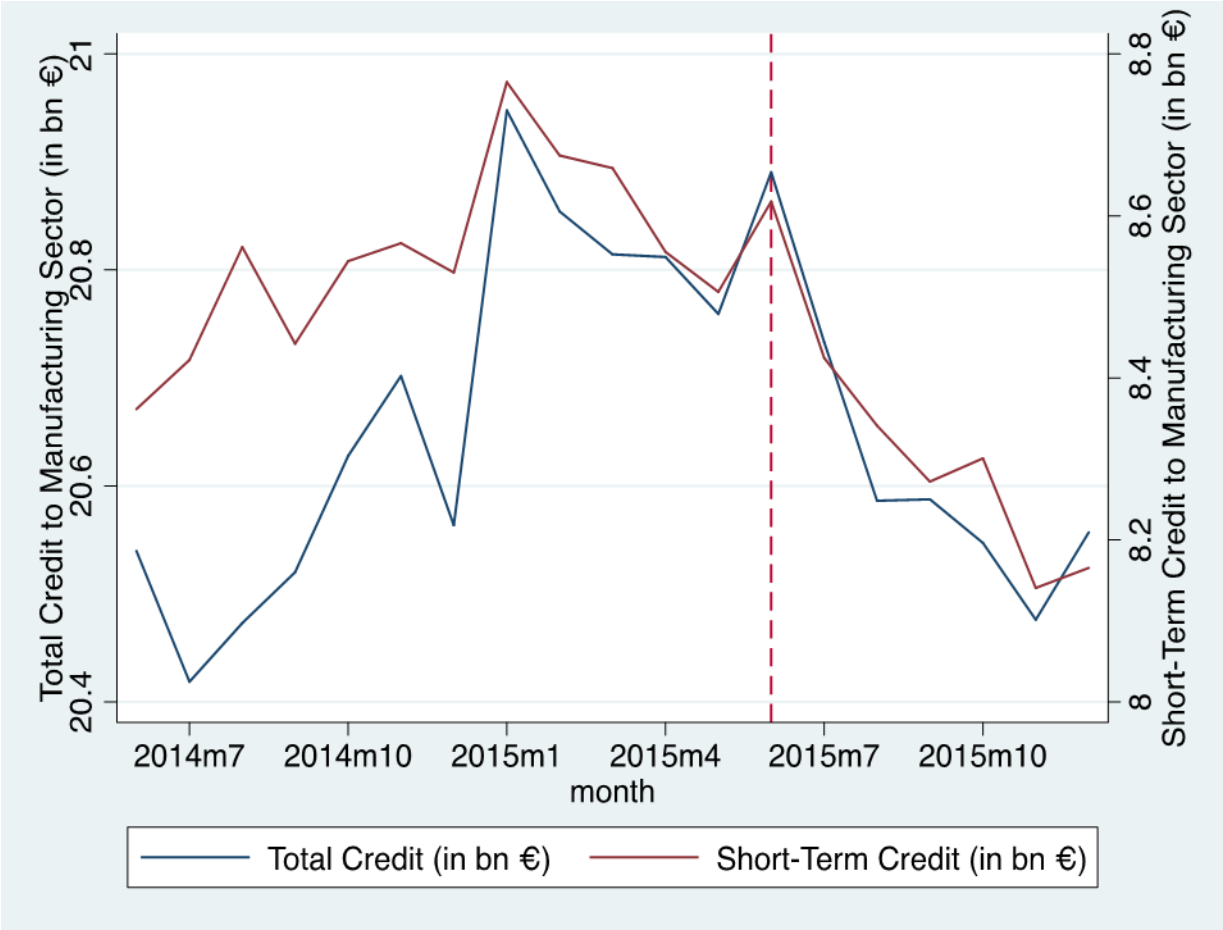

*Note:* The figure reports the evolution of funding of Greek banks before and after Syriza election win (January 2015) and capital controls (June 2015). The left axis displays funding from deposits. The right axis displays Eurosystem and ELA funding. Source: Bank of Greece

**Figure 2: FOB Manufacturing Exports Growth (YoY%)**




*Note:* The figure reports FOB manufacturing exports growth (3-digit ISIC Rev.2) on a yearly basis in period 2014M6-2015M12. Source: Author's calculations

**Figure 3: Total and Short-Term Credit to Manufacturing Sector**

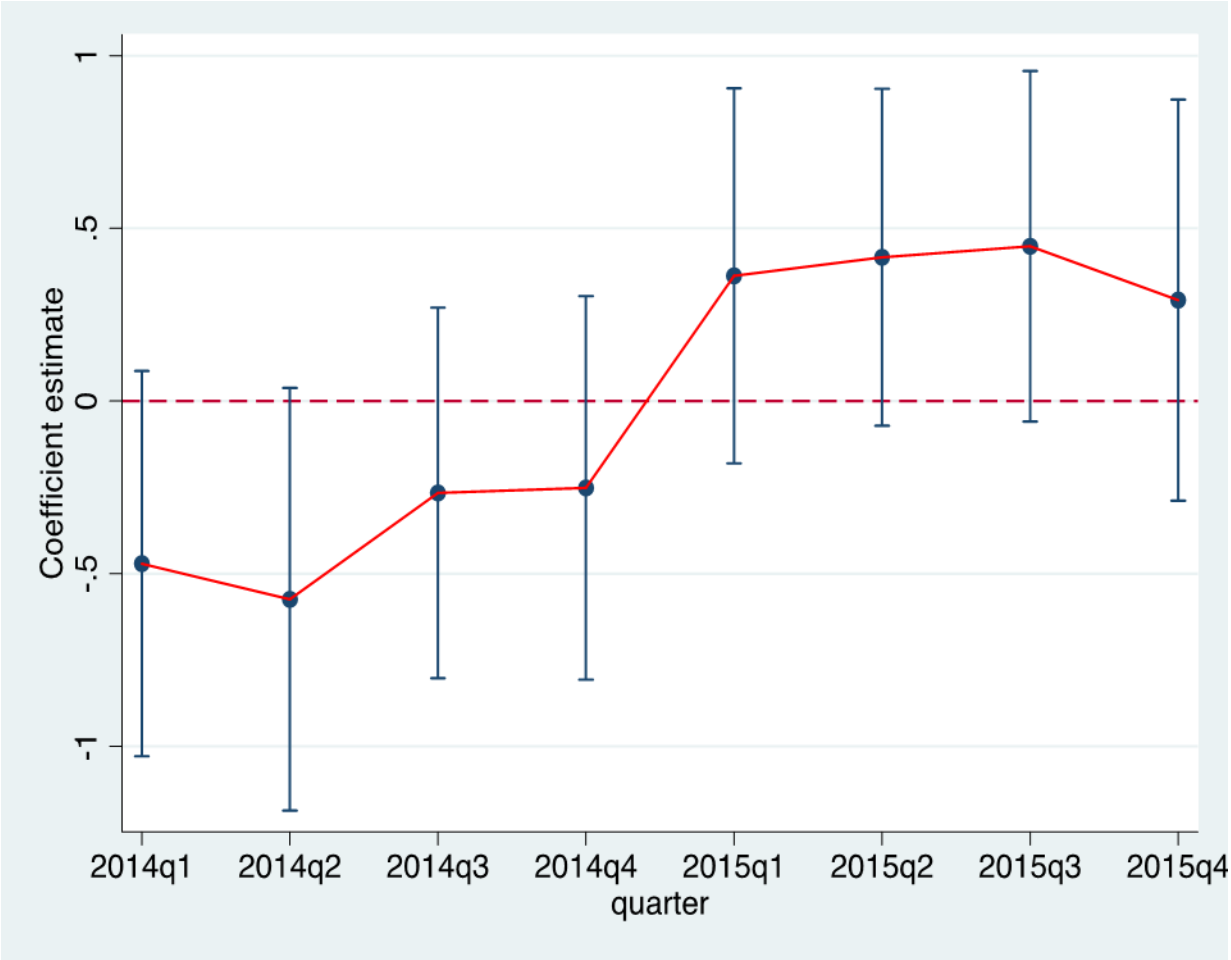



*Note:* The figures report the evolution of total and short-term credit to the manufacturing sector before and after capital controls (June 2015). The left axis displays the evolution of total credit and the right axis displays the evolution of short-term credit. Source: Bank of Greece

**Figure 4: Dynamic Effects of Capital Controls on Trade: Intensive Margin**

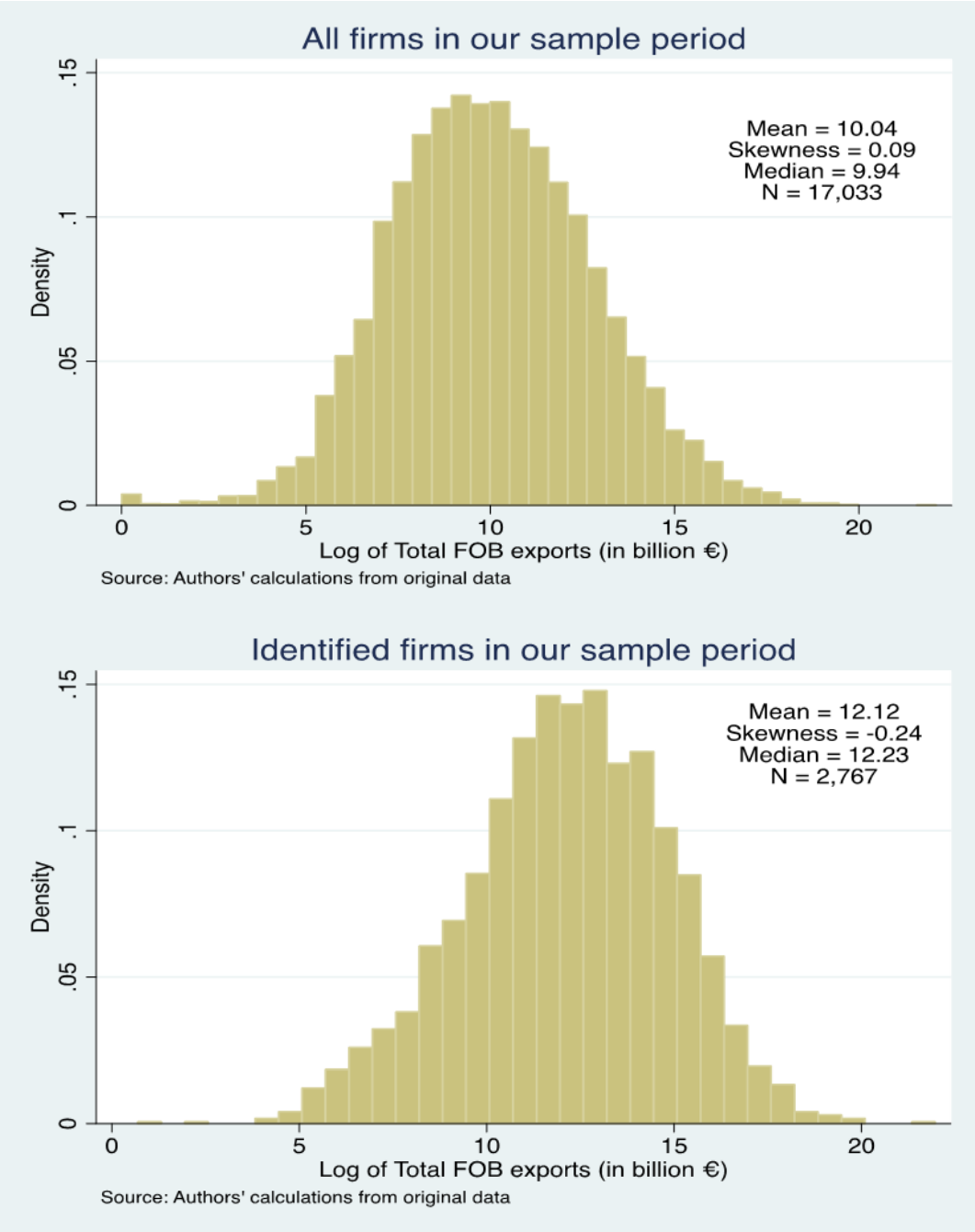

*Note:* The figure presents the time-varying estimates of capital controls on the intensive margin of trade (ΔlogExports) for the interaction Domestic*Liquidity Needs independent variable. Estimates are obtained by running the basseline regression on a quarterly basis compared to the respective quarter a year earlier. The plot uses 90% confidence intervals, where the errors have been clustered at the destination-sector level.

**Figure 5: Dynamic Effects of Capital Controls on Trade: Extensive Margin**




*Note:* The figure presents the time-varying estimates of capital controls on the extensive margin of trade (probability of exit) for the interaction Domestic*Liquidity Needs independent variable. Estimates are obtained by running the basseline regression on a quarterly basis compared to the respective quarter a year earlier. The plot uses 90% confidence intervals, where the errors have been clustered at the destination-sector level.

**Figure 6: Selection Bias**




*Note:* Compared to all 17,033 active firms in the market during our sample period, we are able to identify 2,767 firms or 16.24% of them. These firms account for 55.44% of the total value of exports and the distribution of their exports is presented in the bottom figure. Essentially, we are left with a trade-off. Given the extremely detailed structure of our trade data, by exploiting both

the cross-section and the time dimension, we feel confident about the quality of our matching. The obvious cost of this process is a slightly negative skewed distribution of exports. Strictly speaking, since extremely small exporters are not included in our baseline regressions, our evidence provides a lower bound estimate of the adverse effect of the credit channel of capital controls on trade.

**Table 1: Summary Statistics**

| | | | Domestic | Multinational |
|---|---|---|---|---|
| | # Firms | | 2.421 | 346 |
| | % Exports | | 50,30% | 49,70% |
| External Finance Dependence | High | before | 76,51% | 23,49% |
| | | after | 75,14% | 24,86% |
| | Low | before | 70,73% | 29,27% |
| | | after | 87,10% | 12,90% |
| Asset Tangibility | High | before | 58,86% | 41,14% |
| | | after | 84,84% | 15,16% |
| | Low | before | 87,15% | 12,85% |
| | | after | 76,55% | 23,45% |
| Liquidity Needs | High | before | 91,86% | 8,14% |
| | | after | 80,29% | 19,71% |
| | Low | before | 64,35% | 35,65% |
| | | after | 81,76% | 18,24% |
| Trade Credit | High | before | 73,59% | 26,41% |
| | | after | 87,31% | 12,69% |
| | Low | before | 70,99% | 29,01% |
| | | after | 73,49% | 26,51% |

*Note*: The table presents the distribution of exports of domestic vis-a-vis multinational companies in manufacturing sectors with high/low External Finance Dependence, Asset Tangibility, Liquidity Needs and Trade Credit before and after the imposition of capital controls. External Finance Dependence, Asset Tanbility and Liquidity Needs are from Kroszner, Laeven and Klingebiel (2006). Trade Credit is from Fisman and Love (2003). High denotes dependence above the sector's median, while low denotes dependence below the sector's median. Before refers to the period June 2014-November 2014 and after refers to the period June 2015-November 2015.

## Table 2: The Credit Channel of Capital Controls on Trade

| | Δlog(exports) | | | | Probability of Exit | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| **Domestic * External Finance Dependence** | -0.256* | | | | -0,012 | | | |
| | 0,141 | | | | 0,031 | | | |
| **Domestic * Asset Tangibility** | | 0,612 | | | | -0,063 | | |
| | | 0,438 | | | | 0,115 | | |
| **Domestic * Liquidity Needs** | | | -3.265*** | | | | 0,266 | |
| | | | 1,099 | | | | 0,26 | |
| **Domestic * Trade Credit** | | | | 6.759** | | | | 0,155 |
| | | | | 3,21 | | | | 0,823 |
| **Destination * Sector FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **Firm FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **N** | 17238 | 17238 | 17238 | 17238 | 26340 | 26340 | 26340 | 26340 |
| **R2** | 0,062 | 0,061 | 0,062 | 0,062 | 0,201 | 0,201 | 0,201 | 0,201 |

*Note*: The table presents results from FE difference-in-differences regression models, where Pre={June 2014-November 2014} and Post={June 2015-November 2015}. Sector-level measures External Finance Dependence, Asset Tanbility and Liquidity Needs are from Kroszner, Laeven and Klingebiel (2006). Trade Credit is from Fisman and Love (2003). For definitions, see main text. Domestic is dummy equal to 1 if firm has more than 50% equity stakes owned by domestic investors and is measured as of end 2013. Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .01***; .05**; .1*.

**Table 3: The Credit Channel of Placebo Capital Controls on Trade**

| | Δlog(exports) | | | | Probability of Exit | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| **Domestic * External Finance Dependence** | 0,073 | | | | -0,006 | | | |
| | 0,128 | | | | 0,037 | | | |
| **Domestic * Asset Tangibility** | | -0,239 | | | | 0,071 | | |
| | | 0,499 | | | | 0,123 | | |
| **Domestic * Liquidity Needs** | | | 1,673 | | | | 0,049 | |
| | | | 1,214 | | | | 0,282 | |
| **Domestic * Trade Credit** | | | | 0,972 | | | | 0,285 |
| | | | | 3,594 | | | | 0,848 |
| **Destination * Sector FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **Firm FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **N** | 16668 | 16668 | 16668 | 16668 | 25312 | 25312 | 25312 | 25312 |
| **R2** | 0,065 | 0,065 | 0,065 | 0,065 | 0,207 | 0,207 | 0,207 | 0,207 |

*Note:* The table presents results from FE difference-in-differences regression models.We assume placebo capital controls imposed in June 2014, where Pre={June 2013-November 2013} and Post={June 2014-November 2014}. Sector-level measures External Finance Dependence, Asset Tangibility and Liquidity Needs are from Kroszner, Laeven and Klingebiel (2006). Trade Credit is from Fisman and Love (2003). For definitions, see main text. Domestic is dummy equal to 1 if firm has more than 50% equity stakes owned by domestic investors and is measured as of end 2013. Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .01***; .05**; .1*.

## Table 4: The Firm's Size Channel

| | Δlog(exports) | | | | Probability of Exit | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| **Domestic * External Finance Dependence** | -0.259* | | | | 0,021 | | | |
| | 0,139 | | | | 0,034 | | | |
| **Domestic * Asset Tangibility** | | 0,402 | | | | -0,075 | | |
| | | 0,457 | | | | 0,124 | | |
| **Domestic * Liquidity Needs** | | | -2.999** | | | | 0,36 | |
| | | | 1,163 | | | | 0,28 | |
| **Domestic * Trade Credit** | | | | 5.484* | | | | 0,15 |
| | | | | 3,241 | | | | 0,872 |
| **Size * Sector Dependence** | yes | yes | yes | yes | yes | yes | yes | yes |
| **Destination * Sector FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **Firm FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **N** | 16801 | 16801 | 16801 | 16801 | 25454 | 25454 | 25454 | 25454 |
| **R2** | 0,063 | 0,063 | 0,063 | 0,063 | 0,191 | 0,191 | 0,191 | 0,191 |

*Note:* The table presents results from FE difference-in-differences regression models, where Pre={June 2014-November 2014} and Post={June 2015-November 2015}. Sector-level measures External Finance Dependence, Asset Tanbility and Liquidity Needs are from Kroszner, Laeven and Klingebiel (2006). Trade Credit is from Fisman and Love (2003). For definitions, see main text. Domestic is dummy equal to 1 if firm has more than 50% equity stakes owned by domestic investors and is measured as of end 2013. All regressions control for the interaction of firm's size, measured by the log of total assets as of end 2013, with the respective sector-level dependence measure. Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .01***; .05**; .1*.

## Table 5: Controlling for Alternative Sector-Level Channels

| | Δlog(exports) | | | | Probability of Exit | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| **Domestic * External Finance Dependence** | -0.281* | | | | -0,001 | | | |
| | 0,15 | | | | 0,033 | | | |
| **Domestic * Asset Tangibility** | | -0,064 | | | | -0.285* | | |
| | | 0,633 | | | | 0,163 | | |
| **Domestic * Liquidity Needs** | | | -2.860** | | | | 0.471* | |
| | | | 1,195 | | | | 0,282 | |
| **Domestic * Trade Credit** | | | | 6.061* | | | | 0,014 |
| | | | | 3,191 | | | | 0,828 |
| **Domestic * Capital Intensity, Contract Intensity** | yes | yes | yes | yes | yes | yes | yes | yes |
| **Destination * Sector FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **Firm FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **N** | 17238 | 17238 | 17238 | 17238 | 26340 | 26340 | 26340 | 26340 |
| **R2** | 0,062 | 0,061 | 0,062 | 0,062 | 0,201 | 0,202 | 0,202 | 0,201 |

*Note:* The table presents results from FE difference-in-differences regression models, where Pre={June 2014-November 2014} and Post={June 2015-November 2015}. Sector-level measures External Finance Dependence, Asset Tanbility and Liquidity Needs are from Kroszner, Laeven and Klingebiel (2006). Trade Credit is from Fisman and Love (2003). For definitions, see main text. Domestic is dummy equal to 1 if firm has more than 50% equity stakes owned by domestic investors and is measured as of end 2013. All regressions control for the interaction of Domestic with sector's Capital Intensity (Kroszner, Laeven and Klingebiel, 2006) and sector's Contract Intensity (Nunn, 2007). Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .01***; .05**; .1*.

## Table 6: Excluding Bank Holiday

| | Δlog(exports) | | | | Probability of Exit | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| **Domestic * External Finance Dependence** | -0,137 | | | | -0,023 | | | |
| | 0,155 | | | | 0,032 | | | |
| **Domestic * Asset Tangibility** | | 0,739 | | | | -0,198 | | |
| | | 0,486 | | | | 0,122 | | |
| **Domestic * Liquidity Needs** | | | -2.759** | | | | 0.667** | |
| | | | 1,183 | | | | 0,27 | |
| **Domestic * Trade Credit** | | | | 6.623* | | | | -0,98 |
| | | | | 3,63 | | | | 0,884 |
| **Destination * Sector FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **Firm FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **N** | 14110 | 14110 | 14110 | 14110 | 21862 | 21862 | 21862 | 21862 |
| **R2** | 0,07 | 0,07 | 0,071 | 0,07 | 0,199 | 0,199 | 0,199 | 0,199 |

*Note:* The table presents results from FE difference-in-differences regression models, where Pre={August 2014-November 2014} and Post={August 2015-November 2015}. Sector-level measures External Finance Dependence, Asset Tanbility and Liquidity Needs are from Kroszner, Laeven and Klingebiel (2006). Trade Credit is from Fisman and Love (2003). For definitions, see main text. Domestic is dummy equal to 1 if firm has more than 50% equity stakes owned by domestic investors and is measured as of end 2013. Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .01***; .05**; .1*.

## Table 7: Including Banks' Recapitalization

| | Δlog(exports) | | | | Probability of Exit | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| **Domestic * External Finance Dependence** | -0.235* | | | | 0,002 | | | |
| | 0,127 | | | | 0,028 | | | |
| **Domestic * Asset Tangibility** | | 0.865* | | | | -0,02 | | |
| | | 0,447 | | | | 0,113 | | |
| **Domestic * Liquidity Needs** | | | -3.013*** | | | | 0,187 | |
| | | | 1,158 | | | | 0,25 | |
| **Domestic * Trade Credit** | | | | 5,11 | | | | 0,722 |
| | | | | 3,245 | | | | 0,79 |
| **Destination * Sector FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **Firm FE** | yes | yes | yes | yes | yes | yes | yes | yes |
| **N** | 18496 | 18496 | 18496 | 18496 | 27991 | 27991 | 27991 | 27991 |
| **R2** | 0,067 | 0,067 | 0,067 | 0,067 | 0,2 | 0,2 | 0,2 | 0,2 |

*Note:* The table presents results from FE difference-in-differences regression models, where Pre={June 2014-December 2014} and Post={June 2015-December 2015}. Sector-level measures External Finance Dependence, Asset Tanbility and Liquidity Needs are from Kroszner, Laeven and Klingebiel (2006). Trade Credit is from Fisman and Love (2003). For definitions, see main text. Domestic is dummy equal to 1 if firm has more than 50% equity stakes owned by domestic investors and is measured as of end 2013. Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .01***; .05**; .1*.

**Table 8: Alternative Sector-Level Measures of Credit Needs**

| | Δlog(exports) | | Probability of Exit | |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| **Domestic * External Finance Dependence ('80-'99)** | -0.239+ | | -0,039 | |
| | 0,15 | | 0,034 | |
| **Domestic * Liquidity Needs ('80-'99)** | | -3.372*** | | 0,223 |
| | | 1,232 | | 0,301 |
| **Destination * Sector FE** | yes | yes | yes | yes |
| **Firm FE** | yes | yes | yes | yes |
| **N** | 17238 | 17238 | 26340 | 26340 |
| **R2** | 0,061 | 0,062 | 0,201 | 0,201 |

*Note:* The table presents results from FE difference-in-differences regression models. Sector-level measures External Finance Dependence and Liquidity Needs are from Kroszner, Laeven and Klingebiel (2006). For definitions, see main text. Domestic is dummy equal to 1 if firm has more than 50% equity stakes owned by domestic investors and is measured as of end 2013. Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .01***; .05**; .1*, .15+.

**Table 9: The Effect of Capital Controls on Credit Supply**

| | Δlog(credit) | | | |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| | *All Firms* | *All Firms* | *Domestic Firms* | *Domestic Firms* |
| **ELA Exposure** | -0.033*** | -0.037* | -0.041*** | -0.050** |
| | 0,009 | 0,021 | 0,01 | 0,022 |
| **Bank Controls** | no | yes | yes | yes |
| **Firm FE** | no | yes | yes | yes |
| **N** | 2542 | 2141 | 2313 | 1935 |
| **R2** | 0,007 | 0,137 | 0,011 | 0,141 |

*Note:* The table presents results from OLS and FE difference-in-differences regression models around the cap on ELA funding shock in June 2015. Δlog(credit) denotes the change in bank-firm

credit in December 2015-December 2014. ELA Exposure denotes bank's dependence on Emergency Liquidity Assistance as of December 2013. Bank Controls include bank's size, which is proxied by total liabilities, and capital. Robust standard errors are reported. Statistical significance is denoted as .01***; .05**; .1*.

**Table 10: Capital Controls, Weak Banks' Balance Sheets and Trade**

| *First-Stage of 2SLS: from credit shock to credit supply* | | |
|---|---|---|
| | Δlog(credit) | |
| | 1 | 2 |
| | *All Firms* | *Domestic Firms* |
| **ELA Exposure** | -0.203*** | -0.227*** |
| | 0,005 | 0,005 |
| **Firm Controls** | yes | yes |
| **Destination * Sector FE** | yes | yes |
| **Province FE** | yes | yes |
| **F-statistic** | 194,24 | 234,02 |
| **N** | 9434 | 8450 |
| **R2** | 0,504 | 0,576 |

*Note:* The table presents results from the first-stage regression from credit shock to credit supply. Δlog(credit) denotes the change of firm credit in December 2015-December 2014. Firm Controls include firm's age, age squared, size, inventories, trade payables, sales growth, reserves, short-term debt and an importer dummy as of December 2013. In all models, destination*sector and firm's province fixed effects are included. Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .01***; .05**; .1*.

**Table 11: Capital Controls, Weak Banks' Balance Sheets and Trade**

| *Second-Stage of 2SLS: from credit supply to trade* | | | | |
|---|---|---|---|---|
| | Δlog(exports) | Probability of Exit | Δlog(exports) | Probability of Exit |
| | 1 | 2 | 3 | 4 |
| | *All Firms* | *All Firms* | *Domestic Firms* | *Domestic Firms* |
| **Δlog(credit)** | 0.017+ | -0,003 | 0.027** | -0,003 |
| | 0,011 | 0,003 | 0,011 | 0,003 |
| **Firm Controls** | yes | yes | yes | yes |
| **Destination * Sector FE** | yes | yes | yes | yes |
| **Province FE** | yes | yes | yes | yes |
| **N** | 9434 | 13556 | 8450 | 12207 |
| **R2** | 0,021 | 0,092 | 0,019 | 0,092 |

*Note:* The table presents results from the second-stage regression from credit supply to trade. Δlog(credit) denotes the change of firm credit in December 2015-December 2014. Firm Controls include firm's age, age squared, size, inventories, trade payables, sales growth, reserves, short-term debt and an importer dummy as of December 2013. In all models, destination*sector and

firm's province fixed effects are included. Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .01***; .05**; .1*, .15+

## Appendix Table 1: Hypothetical Example of Firms' Identification

| tax identifier | anonymized firm | year | product | destination | value | volume |
|---|---|---|---|---|---|---|
| 1 | A | 2007 | 63311 | IT | 1000 | 10 |
| 2 | B | 2007 | 63311 | IT | 1000 | 10 |
| 3 | C | 2007 | 58221 | DE | 500 | 20 |
| 3 | D | 2007 | 72511 | UK | 300 | 5 |
| 4 | E | 2007 | 23222 | US | 250 | 40 |
| 4 | E | 2007 | 55351 | CY | 400 | 100 |

In this section, we describe how we recover tax identifiers of firms, exploiting our access, through the Bank of Greece, to a second administrative dataset that reports export flows at the firm-product-destination level. Both datasets (anonymized and identified) have been extracted by ELSTAT's intra-EU and extra-EU databases, but at different points in time. Importantly, identified dataset reports the *same* information at the *same* level of disaggregation (i.e. firm-product (5-digit SITC)-destination level) and overlaps with the anonymized dataset until year 2009. We restrict our attention to years 2007-2009 to avoid discontinuities related to product revisions. In both databases (anonymized and identified), we observe unique firm-product (5-digit SITC Rev.4)-destination-value-volume export entries. Starting in year 2007, we match anonymized firms from the first dataset with their tax identifiers from the second dataset by, first, dropping *all* duplicate entries with respect to product-destination-value-volume and, second, by merging the two datasets with respect to product-destination-value-volume. Put differently, we first drop firms that export the *same* product to the *same* destination at *same* values *and* volumes from *both* datasets (i.e. anonymized and identified), and then we merge the remaining unique export entries with respect to these characteristics. The table above presents an example with imaginary data for year 2007. With no further information, it is not possible to map tax identifier 1 (from the identified dataset) to numerical codes A or B (from the anonymized dataset), and thus we drop *both* transactions from *both* datasets. As one would expect, this accounts for just 0.56% of observations in the anonymized and 0.08% of observations in the identified dataset. Duplicate rates are at similar or lower levels for years 2008 and 2009. This leaves us with *unique* product-destination-value-volume export entries in year 2007, which we proceed to matching them. An obvious concern is whether the matched tax identifier corresponds to the "true" anonymized firm as given by its numerical code.

Theoretically, it could well be the case that, although unique entries match, firms are different. Rows 3 and 4 give an example. Firm 3 from identified dataset matches with numerical codes C and D from anonymized dataset, simply because unique transaction

characteristics (product-destination-value-volume) are the same. Indeed, we detect two such observations in 2007 (0.003% of total observations), where the same tax identifier corresponds to different numerical codes. We check for the opposite (i.e. same numerical code matches to different tax identifiers) as well. We drop these two observations. No such case was detected in years 2008 and 2009. The detection of only two observations in three years form the universe of export flows data is no surprise given the very detailed structure of reporting. We are left with unique matched pairs of tax identifiers-anonymized firms in year 2007 (rows 5 and 6 in Table 1). We repeat the process for each year separately and end up with unique matched pairs in years 2008 and 2009. On average, our matching rate is between 60-63% of all export flows in the anonymized and 94-96% of all export flows in the identified dataset in years 2007-2009. To increase the quality of our matching process, we exploit the time dimension of our data. For our baseline regressions, we keep matched pairs common to all three years. In our imaginary example, this would mean that firm 4 matches to firm E in years 2007, 2008 and 2009. In other words, we keep all identified firms with at least one transaction per year across all years. Finally, we conduct additional sanity tests, by checking whether (identified) firms, which have been dropped from our baseline sample, jump between different numerical codes from year to year. In other words, we check whether tax identifier 4 matches to anonymized firm E in 2007, K in 2008 and back to E in 2009 (and vice versa). We find zero such cases. Our results are robust to three alternative identifying procedures. These are i) matching firms with at least two transactions per year across all years, ii) matching firms that transact at least once in two out of three years and iii) matching firms that transact at least twice in two out of three years. Our conclusions remain unchanged.

**Appendix Table 2: Top 15 Greek Export Destinations**

| Destination | Number of exporters | Value (in billion €) |
|---|---|---|
| Cyprus | 160201 | 2,31 |
| Bulgaria | 129865 | 2,14 |
| Germany | 56594 | 2,91 |
| Romania | 48493 | 1,09 |
| Italy | 40199 | 4,11 |
| Albania | 38188 | 0,63 |
| United Kingdom | 27603 | 1,72 |
| Spain | 27358 | 1,02 |
| France | 23451 | 1,05 |
| Netherlands | 20159 | 0,66 |
| FYROM | 19990 | 1,13 |
| United States | 19067 | 1,72 |
| Belgium | 15627 | 0,4 |
| Poland | 13219 | 0,52 |
| Turkey | 12010 | 4,27 |

*Note:* The table presents the top 15 export destinations in 2014 and 2015.

**Appendix Table 3: Top 15 Greek Exported Products**

| Product | 5-digit SITC Rev.4 | Value (in billion €) |
|---|---|---|
| Petroleum oils | 33460 | 14,76 |
| Medicaments | 54293 | 1,37 |
| Aluminium plates | 68423 | 1,05 |
| Virgin oil | 42141 | 0,74 |
| Other vegetables | 05679 | 0,69 |
| Other cheese | 02499 | 0,65 |
| Tubes and pipes | 68271 | 0,63 |
| Portable automatic data processing machines | 75220 | 0,6 |
| Apricots, cherries and peaches | 05895 | 0,48 |
| Cigarettes containing tobacco | 12220 | 0,45 |
| Aluminium alloys | 68412 | 0,45 |
| Aluminium foil | 68424 | 0,41 |
| Aluminium bars, rods and profiles | 68421 | 0,38 |
| Polypropylene | 57511 | 0,33 |
| Articles of furskin | 84831 | 0,3 |

*Note:* The table presents the top 15 exported products in 2014 and 2015. Product names were shortened.

## Appendix Table 4: Sector Characteristics of Financial Conditions

| Code | Sector | External Finance | Liquidity Needs | Asset Tangibility | Trade Credit |
|---|---|---|---|---|---|
| 311 | Food products | 0,14 | 0,1 | 0,37 | 0,112 |
| 313 | Beverages | 0,08 | 0,11 | 0,4 | 0,091 |
| 314 | Tobacco | -0,45 | 0,25 | 0,19 | 0,066 |
| 321 | Textiles | 0,4 | 0,17 | 0,31 | 0,101 |
| 322 | Wearing apparel | 0,03 | 0,21 | 0,15 | 0,111 |
| 323 | Leather products | -0,14 | 0,27 | 0,12 | 0,055 |
| 324 | Footwear | -0,08 | 0,23 | 0,13 | 0,093 |
| 331 | Wood products | 0,28 | 0,12 | 0,32 | 0,088 |
| 332 | Furniture | 0,24 | 0,15 | 0,28 | 0,092 |
| 341 | Paper and products | 0,18 | 0,12 | 0,42 | 0,081 |
| 342 | Printing and publishing | 0,2 | 0,08 | 0,21 | 0,075 |
| 351 | Industrial chemicals | 0,25 | 0,15 | 0,43 | 0,083 |
| 352 | Other chemicals | 0,22 | 0,15 | 0,27 | 0,097 |
| 353 | Petroleum refineries | 0,04 | 0,07 | 0,62 | 0,118 |
| 354 | Misc. petroleum and coal | 0,33 | 0,12 | 0,46 | 0,096 |
| 355 | Rubber products | 0,23 | 0,14 | 0,36 | 0,088 |
| 356 | Plastic products | 1,14 | 0,13 | 0,38 | 0,099 |
| 361 | Pottery, china, earthenware | -0,15 | 0,17 | 0,28 | 0,067 |
| 362 | Glass products | 0,53 | 0,15 | 0,42 | 0,089 |
| 369 | Other non-metallic mineral | 0,06 | 0,14 | 0,48 | 0,064 |
| 371 | Iron and steel | 0,09 | 0,16 | 0,44 | 0,094 |
| 372 | Non-ferrous metal | 0,01 | 0,17 | 0,32 | 0,078 |
| 381 | Fabricated metal products | 0,24 | 0,18 | 0,28 | 0,088 |
| 382 | Machinery, except electrical | 0,45 | 0,22 | 0,22 | 0,086 |
| 383 | Machinery, electric | 0,77 | 0,2 | 0,21 | 0,082 |
| 384 | Transport equipment | 0,31 | 0,19 | 0,23 | 0,105 |
| 385 | Professional & scientific | 0,96 | 0,21 | 0,16 | 0,072 |
| 390 | Other manufactured | 0,47 | 0,22 | 0,18 | 0,087 |

*Note:* The table presents measures of External Finance Dependence, Asset Tangibility, Liquidity Needs and Trade Credit for 28 3-digit ISIC (Rev.2) sectors used in baseline regressions. For definitions, see main text.

## Appendix Table 5: The Effect of Placebo Capital Controls on Credit Supply

| | Δlog(credit) | |
|---|---|---|
| | 1 | 2 |
| | *All Firms* | *Domestic Firms* |
| **ELA Exposure** | -0,016 | -0,001 |
| | 0,021 | 0,022 |
| **Bank Controls** | yes | yes |
| **Firm FE** | yes | yes |
| **N** | 2141 | 1935 |
| **R2** | 0,103 | 0,105 |

*Note:* The table presents results from FE difference-in-differences regression models around the *placebo* cap on ELA funding shock in June 2014. *Δlog(credit)* denotes the change in bank-firm credit in December 2014-December 2013. *ELA Exposure* denotes bank's dependence on Emergency Liquidity Assistance as of December 2013. *Bank Controls* include bank's size, which is proxied by total liabilities, and capital. Robust standard errors are reported. Statistical significance is denoted as .01***; .05**; .1*.

## Appendix Table 6: Placebo Capital Controls, Weak Banks' Balance Sheets and Trade

| | Δlog(exports) | Probability of Exit | Δlog(exports) | Probability of Exit |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| | *All Firms* | *All Firms* | *Domestic Firms* | *Domestic Firms* |
| **Δlog(credit)** | -0,003 | -0,002 | -0,001 | -0,001 |
| | 0,012 | 0,003 | 0,011 | 0,003 |
| **Firm Controls** | yes | yes | yes | yes |
| **Destination * Sector FE** | yes | yes | yes | yes |
| **Province FE** | yes | yes | yes | yes |
| **N** | 9074 | 12699 | 8135 | 11459 |
| **R2** | 0,017 | 0,083 | 0,020 | 0,082 |

*Note:* The table presents results from a placebo credit supply shock to trade. Δlog(credit) denotes the change of firm credit in December 2014-December 2013. Firm Controls include firm's age, age squared, size, inventories, trade payables, sales growth, reserves, short-term debt and an importer dummy as of December 2013. In all models, destination*sector and firm's province fixed effects are included. Standard errors are clustered at the destination-sector level. Statistical significance is denoted as .15+, .01***; .05**; .1*.