Venting Out: Exports during a Domestic Slump*

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Abstract

We exploit plausibly exogenous geographical variation in the reduction in domestic demand caused by the Great Recession in Spain to document the existence of a robust, within-firm negative causal relationship between demand-driven changes in domestic sales and export flows. Spanish firms whose domestic sales were reduced by more during the crisis observed a larger increase in their export flows, even after controlling for firms’ supply determinants (such as labor costs). This negative relationship between demand-driven changes in domestic sales and changes in export flows illustrates the capacity of export markets to counteract the negative impact of local demand shocks. We rationalize our findings through a standard heterogeneous-firm model of exporting expanded to allow for non-constant marginal costs of production. Using a structurally estimated version of this model, we conclude that the firm-level responses to the slump in domestic demand in Spain could well have accounted for more than two-thirds of the spectacular increase in Spanish exports (the so-called ‘Spanish export miracle’) over the period 2009-13.

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1 Introduction

The Great Recession of the late 2000s and early 2010s shook the core of many advanced economies. Few countries experienced the consequences of the global downturn as intensively as Spain did. From its peak in 2008, Spain’s real GDP fell by an accumulated 9.2% in the following five years, until bottoming out in 2013. During the same period, private final consumption contracted by 14.0%, and the unemployment rate shot up from 9.6% to 26.9%. Despite this pronounced domestic slump, Spanish exports demonstrated a remarkable resilience during these years. After tumbling by 19.2% during the global trade collapse of late 2008 and early 2009, Spanish merchandise exports quickly recovered and grew by 39.8% between 2009 and 2013. Overall, Spanish exports grew by an accumulated 12.9% during the 2008-2013 period, while merchandise exports in the rest of the euro area decreased by 0.7% during the same years. As a result, as shown in Figure 1, the share of euro area merchandise exports to non-euro area countries accounted for by Spain increased markedly during this period, despite the contemporaneous decline in the relative weight of Spain’s GDP in the euro area’s GDP.\footnote{In Appendix C.1, we provide similar figures for two other countries whose relative GDP dropped drastically during the Great Recession (Portugal and Greece) and for a country whose relative GDP increased (Germany). In all three cases, we observe a negative relationship between these countries’ GDP shares in the euro area and their shares in euro area goods exports to other countries. See Section 3 and Appendix B for a description of the data sources underlying the figures mentioned in this Introduction.}

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{figure1.png}
\caption{The Spanish Export Miracle}
\end{figure}

Two leading explanations of this so-called Spanish ‘export miracle’ have been offered (see Eppinger et al., 2017, De Lucio et al., 2018). First, this remarkable export performance has been attributed to the internal devaluation undergone by the Spanish economy since 2010. Indeed, a process of wage moderation in the post-crisis period, together with a labor market reform in 2012, led to a fall in unit labor costs. According to this explanation, this decline in labor costs allowed firms to reduce their export prices and increase their market shares abroad. Still the improvement
in competitiveness was modest: unit labor costs in Spanish manufacturing, relative to those in other countries in the euro area, declined by a mere 3.8% between 2008 and 2013.²

A second explanation relates the growth in exports directly to the collapse in domestic demand. According to this hypothesis, the unexpected demand-driven reduction in Spanish firms’ domestic sales, in combination with the irreversibility of their investments in certain inputs, freed up capacity that these firms used to serve customers abroad.³ More precisely, this explanation posits that, as domestic demand dropped, Spanish firms were able to cut their short-run marginal costs by reducing their usage of flexible inputs (e.g., temporary workers and materials) relative to their usage of fixed inputs (e.g., physical capital and permanent workers). This fall in short-run marginal costs translated into a gain in competitiveness in foreign markets and, consequently, to an increase in firms’ exports.⁴ This explanation resonates with the “vent-for-surplus” theory of the benefits of international trade, which has a long tradition in Economics dating back to Adam Smith.⁵ Nevertheless, the link between a domestic slump and export growth is hard to reconcile with modern workhorse models of international trade. The reason for this is that these canonical models – including those emphasizing product differentiation and economies of scale – assume that firms face constant marginal costs of production, an assumption that implies that firms’ domestic and export sales decisions can be studied independently from each other.

In this paper, we leverage Spanish firm-level data from 2002 to 2013, and geographic variation across Spanish regions in the reduction in domestic demand caused by the financial crisis, to study the empirical relevance of the “vent-for-surplus” mechanism. To do so, we divide our sample into a “boom” period (2002-08) and a “bust” period (2009-13) and measure the extent to which a decline in the domestic sales in the bust period relative to the boom period is associated with an increase in export sales in the bust period (again relative to the boom period). When measuring this association, we control for “boom-to-bust” changes in observed marginal cost shifters; i.e., measures of factor prices and productivity. To further isolate demand-driven changes in domestic sales, we exploit the fact that the financial crisis and the Great Recession affected different geographical areas in Spain differentially. More specifically, we rely on municipality-level registration data on a major household durable consumption item, vehicles, and use the change in the municipality-level

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²Generally, one can interpret the “internal devaluation” explanation as encompassing all factors that caused a downward shift in Spanish firms’ marginal cost or supply curves (e.g., reductions in the price of factors or materials, or increases in productivity).

³See “La exportación como escape” in El País, 1/16/2016, for a journalistic account in Spanish with some specific case studies (https://elpais.com/economia/2016/01/14/actualidad/1452794395_894216.html). Further firm-level examples are provided in the more recent “El milagro exportador español” in El País, 5/27/2018 (http://elpais.com/economia/2018/05/25/actualidad/1527242520_600876.html), a newspaper article which was inspired by an early version of our paper.

⁴Generally, one can interpret this explanation as encompassing any mechanism that makes firms’ short-run marginal cost curves increasing and that, thus, links the drop in firms’ domestic demand to a downward movement along their supply curves.

⁵In The Wealth of Nations (1776) Book II, Chapter V, Adam Smith writes “When the produce of any particular branch of industry exceeds what the demand of the country requires, the surplus must be sent abroad, and exchanged for something for which there is a demand at home. Without such exportation, a part of the productive labour of the country must cease, and the value of its annual produce diminish.” The term “vent-for-surplus” was introduced by John Stuart Mill in his Principles of Political Economy (1848) and popularized by Williams (1929) and Mynt (1958).
stock of vehicles per capita between 2002-08 and 2009-13 as a proxy for the extent to which the Great Recession affected demand across municipalities. Armed with this measure of changes in local demand, we use it as an instrument for the reduction in the domestic sales of firms located in different parts of Spain.

To understand the properties of our estimates of the causal impact of demand-driven changes in a firm’s domestic sales on its exports, we first base our analysis on a commonly used model of firms’ export behavior: a model à la Melitz (2003). For our purposes, this framework serves the role of identifying several empirical challenges that one encounters when measuring the relevance of the “vent-for-surplus” mechanism; i.e., when measuring the causal impact of changes in a firm’s domestic sales on exports that work exclusively through changes in the firm’s domestic demand.\(^6\)

We draw three main conclusions from our theoretical analysis. First, as long as firms’ marginal cost shifters (i.e., firms’ productivity and production factor costs) are not perfectly observable – and their unobserved component is not fully captured by various fixed effects – there will tend to be a positive spurious correlation between domestic sales and exports that does not reflect a causal impact of the former on the latter.\(^7\) Second, the fact that firm-level domestic sales are computed as the difference between firm-level total sales and exports leads to non-classical error-in-variables biases that, under plausible conditions, tend to generate a negative spurious correlation between exports and domestic sales (see also Berman et al., 2015). Third, an instrumental variable approach that exploits a potential proxy for local demand as an instrument for the changes in domestic sales of the firms producing in a given locality will identify the causal impact of demand-driven changes in domestic sales on exports as long as it satisfies three conditions: (i) it is indeed a useful proxy for ‘local demand’ (i.e., the overall propensity to consume), (ii) ‘local demand’ is a good predictor of the domestic sales of Spanish firms producing in a given locality, and (iii) it is not correlated with unobserved covariates that have an independent effect on Spanish firms’ exporting decisions (i.e., unobserved marginal cost or export-demand shifters). We discuss each of these three conditions in turn.

Although, given available data, we cannot directly test that our measure of the “boom-to-bust” changes in the stock of vehicles per capita in the municipality of location of a firm satisfies conditions (i) and (ii), prior work has provided empirical evidence supporting the independent validity of each of these two conditions. First, an extensive literature in empirical macroeconomics has documented that consumption of durable goods (such as vehicles) is strongly procyclical (see, for instance, the survey by Stock and Watson, 1999). Second, a significant impact of highly localized demand shocks on Spain-wide firm sales would be consistent with the findings of Hillberry and Hummels (2008),

\(^6\)The Melitz (2003) model assumes that firms face constant marginal costs of production, implying the null hypothesis of a zero effect of demand-driven changes in domestic sales on exports. However, as we show below, the lessons we learn from this model in terms of the econometric challenges one faces when evaluating the “vent-for-surplus” mechanism are also applicable to more general models that feature firms with increasing marginal costs of production.

\(^7\)Unobserved supply factors would lead to a positive co-movement between domestic sales and exports. Unobserved export-demand shocks would also lead to a positive correlation between domestic sales and exports in the plausible case in which firm-level demand shocks are positively correlated across domestic and foreign markets.
who document that U.S. manufacturers’ shipments are extremely localized, with shipments within their 5-digit zip code of location being three times as large as shipments outside their zip code. Consistent with this prior literature, our first-stage results indicate that our instrument is indeed relevant, in the sense that the change in the municipality-level stock of vehicles per capita between 2002-08 and 2009-13 has significant predictive power for the domestic (i.e., Spain-wide) sales of firms producing in that municipality.

Armed with these first-stage results, we show that a larger predicted drop in domestic sales in the bust period relative to the boom period is associated with significantly higher export sales (conditional on exporting) during the domestic slump (relative to the boom years). Furthermore, these IV estimates are significantly larger in absolute value than the OLS ones. This is consistent with the biases predicted by our baseline Melitz (2003)-type model in the plausible scenario in which our specification only imperfectly controls for a firm’s supply and export demand determinants, and their unobserved components are positively correlated with its domestic sales but orthogonal to the stock of vehicles per capita in the municipality in which the firm is located. Specifically, our IV estimates point at an intensive-margin elasticity of exports to domestic sales in the neighborhood of $-1.6$, while the OLS one is around $-0.2$.

As indicated by condition (iii) above, a potential challenge to our identification approach is that the “boom-to-bust” changes in the stock of vehicles per capita in the municipality of location of a firm may be correlated with the extent to which unobserved shifters of the firm’s marginal cost curve changed in the bust period relative to the boom period. Although, by definition, we cannot test this identification assumption, we provide several additional pieces of evidence that are consistent with the empirical relevance of the “vent-for-surplus” hypothesis and that address some specific sources of endogeneity that could affect the validity of the instrument in our baseline specification.

First, an identification threat arises if differences in the severity of the contraction in vehicle purchases across Spanish municipalities are not exclusively a reflection of differences in demand shocks, but also partly a reflection of unobserved production costs affecting car manufacturers. According to this hypothesis, if vehicles sales are local, municipalities that concentrate a significant share of firms operating in the auto industry could observe a correlation in the boom to bust changes in production costs and purchases of new vehicles. Our results are however robust to this identification threat. Both the relevance of our instrument as well as the finding of a sizeable negative elasticity between domestic sales and exports are robust to excluding from the estimating sample: (a) all firms in the auto industry, no matter where they are located; (b) all firms located in any zip code that hosts at least one auto-maker employing more than 20 workers; (c) all firms located in any zip code that is geographically close to a zip code in which a significant share of manufacturing employment is in the auto industry; and (d) all firms producing in sectors that are either leading input providers or leading buyers of the vehicles manufacturing industry.

When estimating the effect of a predicted drop in domestic sales on the probability of exporting, we again estimate a negative elasticity, but this elasticity is not statistically different from zero.
Second, the “vent-for-surplus” hypothesis suggests that the elasticity of a firm’s Spain-wide sales with respect to changes in local demand is likely to vary across firms in ways that can be verified. For instance, firms will naturally differ in their exposure to demand changes in their municipality of location depending on the share of their total domestic sales that is earned in that municipality. While we do not observe firms’ sales distribution across different Spanish municipalities, it seems plausible that small firms will be more likely to concentrate their sales in their municipality of location than large firms. We indeed find that the first-stage elasticity of domestic, Spain-wide, sales with respect to our demand proxy is larger for smaller firms. Moreover, because different geographic areas in Spain were affected by the Great Recession in very heterogeneous degrees, it is conceivable that for many firms the “vent-for-surplus” mechanism would have operated largely at the intranational level. Rather than being pushed towards export markets, certain firms located in areas where local demand decreased by more could have redirected their sales largely towards other regions within Spain in which local demand decreased less (or increased). Thus, one would expect changes in local demand to have had a smaller impact on Spain-wide sales in inward-oriented firms.

We present evidence consistent with this hypothesis: a reduction in the municipality-level stock of vehicles per capita was associated with a smaller reduction in Spain-wide sales for firms with a low propensity to export, as measured by the pre-sample average export share of the firm’s sector in the province in which the firm is located.

Third, the possibility of the “vent-for-surplus” mechanism operating at the intranational level via increased sales to less affected municipalities also implies that we should observe a larger elasticity of firms’ Spain-wide sales with respect to proxies that capture changes in demand at the province level (instead of the municipality level), as they preclude firms from redirecting their sales across municipalities belonging to the same province. Conversely, if one were to hypothesize that our measure of changes in the stock of vehicles per capita is purely operating as a proxy for changes in unobserved marginal cost shifters (e.g., unobserved factor prices), then any dispersion in these unobserved shifters across municipalities located in the same province would imply that a firm’s domestic sales elasticity with respect to our province-level instrument should be smaller than that with respect to our municipality-level instrument, as the province-level instrument would naturally be a worse proxy for the unobserved marginal costs shifters relevant to the firm. Our results in fact feature more than twice as large a response of domestic sales to a change in the instrument when this one is measured at the province level than when it is measured at the municipality level.

Fourth, consistently with the hypothesis that firms face increasing marginal costs of production and that the slope of these costs is related to the importance of inputs whose investment is predetermined and irreversible, we document that the estimated causal effect of demand-driven changes in domestic sales on exports is larger for firms in capital-intensive sectors, suggesting the importance

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9While there are over 8,000 municipalities in Spain, there are only 50 provinces. Provinces are therefore significantly larger than municipalities.

10Consistently with changes in the stock of vehicles per capita capturing demand changes, we also find that a firm’s domestic sales react to a distance- and population-weighted average of the changes in the stock of vehicles in all municipalities other than the municipality in which the firm is located, even after controlling for the changes in the stock of vehicles in the firm’s municipality.
of fixed factors and capacity utilization in explaining this causal linkage.

Fifth, while our baseline instrumentation approach exploits a proxy for demand changes and is thus agnostic about the underlying causes of the differential impact of the Great Recession in Spain, we also explore alternative instrumentation strategies that focus instead on the deep roots of the differential fall in demand across Spanish regions. More specifically, we show that, relative to the boom years, firm-level domestic sales fell by more in municipalities with lower housing supply elasticities (in which house prices grew disproportionately during the boom years), in zip codes with a larger pre-crisis contribution of the construction sector to the total labor income, and in provinces that experienced larger declines in tourism during the bust years.\footnote{The construction and tourist sectors are among the ones that experienced the largest reduction in total sales and employment in the bust relative to the boom. Regions more exposed to these sectors are likely to have experienced a larger drop in demand for manufacturing goods.} Reassuringly, the second-stage elasticities of exports to domestic sales associated with these instruments are similar in magnitude to those obtained with our benchmark instrument.

Sixth, although we control for firm-specific average wages in all of our specifications, compositional changes in the firm’s workforce may have caused changes in effective labor costs that our wage measure does not capture correctly. An important feature of the Spanish labor market is the division of the workforce into permanent and temporary workers, the later group being typically less productive than the former. We do indeed observe that firms whose share of temporary workers dropped by more in the bust relative to the boom experienced a smaller drop in their exports, consistently with the hypothesis that an increase in the ratio of permanent to temporary workers had an effect equivalent to a positive supply shock. The elasticity of exports with respect to domestic sales remains however largely unaffected when we control for the firm’s change in the share of temporary workers. Similarly, controlling for the change in financial costs experienced by the firms does not change the second-stage estimate of the elasticity of exports with respect to domestic sales.

Seventh, and finally, we address the possibility that the correlation between boom to bust changes in the firm’s domestic sales and boom to bust changes in the stock of vehicles per capita is spurious and due to the presence of cross-municipality correlation in these variables’ time trends. To rule out this possible explanation for our results, we perform a placebo exercise in which we break each of the boom and the bust periods into two subperiods, and evaluate whether our instrument (changes in demand between the boom and bust periods) predicts changes in domestic sales between the two boom subperiods (i.e., between 2005-07 and 2002-04) and between the two bust subperiods (i.e., between 2012-13 and 2009-11). In both cases, we find that it does not.

Having established a causal link between changes in domestic demand and exports that operates through firms’ changes in domestic sales, we generalize our baseline model à la Melitz (2003) to allow for non-constant marginal costs of production. We rationalize this cost structure by including a predetermined and fixed factor into the firm’s production function, and show that the curvature of the marginal cost function is related to the elasticity of output with respect to this factor. Furthermore, we demonstrate how to estimate the curvature of the marginal cost function using a simple variant
of our IV estimator, and employ the resulting estimate to quantitatively evaluate the importance of the “vent-for-surplus” mechanism in explaining the 2009-13 observed export miracle in Spain. More specifically, we compute counterfactuals for different types of “representative” firms in which we remove the contribution of demand factor in explaining their fall in domestic sales, and assess the implications of this more moderate slump for export growth. Our quantitative results indicate that the contribution of the “vent-for-surplus” is substantial. For instance, for a representative average manufacturing firm exporting in the boom and in the bust, export growth would have been 73% lower (a reduction in export growth from 11.6% to 3.2%) when halving their drop in domestic sales (which corresponds to an agnostic 50-50 split of demand versus supply factors in explaining the Spanish domestic slump).

Our paper connects with several branches of the literature. As mentioned above, we relate the Spanish export miracle to Adam Smith’s “vent-for-surplus” theory. The international trade literature has largely ignored this hypothesis as exemplified by the fact that we have only found one mention (in Fisher and Kakkar, 2004) of the term “vent-for-surplus” in all issues of the Journal of International Economics. Nevertheless, there has been an active recent international trade literature focused on relaxing the assumption of constant marginal costs in the canonical (Melitz) model of firm-level trade, and has shown that, in the presence of increasing marginal costs, there is a natural substitutability between domestic sales and exports for which there is supporting empirical evidence. This literature includes the work of Vannoorenberghe (2012), Blum et al. (2013), Soderbery (2014), and Ahn and McQuoid (forthcoming). The results in those papers very much resonate with the OLS results using yearly data we describe in Appendix E. Relative to this prior literature, our paper provides a more explicit discussion of the endogeneity concerns associated with simple OLS reduced-form regressions. More importantly, our paper also attempts to identify the causal effect of a domestic slump on exporting by exploiting plausibly exogenous variation in domestic sales during a particularly salient episode. Relatedly, in contemporaneous work, Fan et al. (2018) exploit variation in the extent to which Chinese authorities enforce the collection of value-added taxes to establish a negative causal link between the profitability of domestic sales and firm-level exports. Conversely, Berman et al. (2015) document a positive (reverse) causal effect of changes in firm-level exports on firm-level domestic sales, using French data over the period 1995-2001. Their identification strategy (based on exogenous variation in foreign demand conditions) is quite distinct from ours and so is their setting, since 1995-2001 was a tranquil period of sustained economic growth in France. For these reasons, even if one takes their findings at face value, it would be unreasonable to interpret them as questioning the empirical relevance of the “vent-for-surplus” mechanism.

Our identification strategy is inspired by the influential work of Mian and Sufi (and collabo-

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12 A broader search to include top general-interest journals identified Neary and Schweinberger (1986), who provide a neoclassical rationale for the “vent-for-surplus” idea.

13 Our paper also relates to previous work documenting the behavior of firm-level exports in Spain around the Great Recession, including Antràs (2011), Myro (2015), Eppinger et al. (2017), and De Lucio et al. (2017, 2018). This literature is largely descriptive and has not attempted to test the relative contribution of different mechanisms in explaining the patterns observed in the data.
rators) on the causes and consequences of the Great Recession in the United States. Specifically, Mian and Sufi (2009) identify important differences in the extent to which the mortgage default crisis affected household wealth in different areas of the United States. In subsequent work, Mian, Rao and Sufi (2013) and Mian and Sufi (2014) study how the unequal geographic distribution of household wealth losses resulting from the housing crisis gave rise to a geographically unequal decline in consumption across U.S. counties. Our finding that geographical variation in the change in the stock of vehicles per capita is a significant predictor of variation in the change in local manufacturing sales in Spain is very much consistent with the findings in Mian and Sufi (2013), who also explore the link between household housing wealth and auto sales. Illustrating this link in the Spanish case would be interesting, but this is complicated by the sluggish adjustment of house prices in Spain during the financial crisis, as documented among others by Akin et al. (2014).  

The rest of the paper is structured as follows. In Section 2, we lay out a baseline model of firm behavior in the spirit of Melitz (2003) and discuss its implications for our approach to the estimation of the causal impact of demand-driven changes in domestic sales on exports. In Section 3, we introduce our Spanish firm-level data and, in Section 4, we develop our core instrumental variable estimation approach. The results of this instrumental variable approach are presented in Section 5. We present additional evidence in favor of the “vent-for-surplus” mechanism in Section 6. In Section 7, we generalize the baseline model à la Melitz (2003) to allow for non-constant marginal costs, and use this framework to quantify the importance of the domestic-sales channel in linking the slump in domestic sales to the growth in Spanish exports. We offer some concluding remarks in Section 8.

2 Benchmark Model: Estimation Guidelines

As indicated in the Introduction, we aim to estimate the causal impact of within-firm demand-driven changes in domestic sales on firm-level exports. To guide our empirical analysis and our choice of an adequate estimator, we first consider the implications for this question of a model of exporting with heterogeneous firms along the lines of Melitz (2003), which is the canonical model of firm-level exports in the recent international trade literature. This model features the standard assumption of constant marginal costs. After presenting our evidence contradictory with this assumption, in Section 7 we will develop an extension of this benchmark model that allows for non-constant marginal costs. Crucially, the lessons we learn in this section about the properties of different estimators will also apply in the more general model.

2.1 Benchmark Model: Estimating Equation

We index manufacturing firms producing in Spain by \( i \), firms’ production locations within Spain by \( \ell \), the sectors to which firms belong by \( s \), and the two potential markets in which they may sell

\[ \text{More specifically, the fact that the housing market adjustment following the bursting of the housing bubble in Spain was largely made through quantities rather than prices implies that standard measures of housing wealth in Spain are not as good predictors for household consumption as they are in other countries.} \]
by \( j = \{d, x\} \), with \( d \) denoting the domestic market and \( x \) denoting the export market. At a given point in time, firm \( i \) faces the following isoelastic demand in market \( j \),

\[
Q_{ij} = \frac{P_{ij}^{1-\sigma}}{P_{sj}^{1-\sigma}} E_{sj} \xi_{ij}^{\sigma-1}, \quad \sigma > 1,
\]

(1)

where \( Q_{ij} \) denotes the number of units of output of firm \( i \) demanded in market \( j \) if it sets a price \( P_{ij} \), \( P_{sj} \) is the sectoral price index in \( j \), \( E_{sj} \) is the total sectoral expenditure in market \( j \) expressed in units of the numeraire; and \( \xi_{ij} \) is a firm-market specific demand shifter.

Firm \( i \)'s total variable cost of producing \( Q_{ij} \) units of output for market \( j \) is given by

\[
c_{ij} Q_{ij} \quad \text{with} \quad c_{ij} \equiv \tau_{sj} \frac{1}{\varphi_i} \omega_i,
\]

(2)

where \( c_{ij} \) denotes the marginal cost to firm \( i \) of selling one unit of output in market \( j \), \( \tau_{sj} \) denotes an iceberg trade cost, \( \varphi_i \) is a measure of firm-specific productivity, and \( \omega_i \) is the firm-specific cost of a bundle of inputs.\(^{15}\) Additionally, we assume that firm \( i \) needs to pay an exogenous fixed cost \( F_{ij} \) to sell a positive amount in market \( j \).

Firm \( i \) chooses optimally the quantity offered in each market \( j \), \( Q_{ij} \), taking the price index, \( P_{sj} \), and the size of the market, \( E_{sj} \), as given. As the marginal production cost is independent of the firm’s total output and the per-market fixed costs are independent of the firm’s participation in other markets, the optimization problem of the firm is separable across markets. Specifically, conditional on selling to a market \( j \), firm \( i \) solves the following optimization problem

\[
\max_{Q_{ij}} \left\{ Q_{ij}^{\sigma-1} P_{sj}^{\frac{1-\sigma}{\sigma}} E_{sj}^{\frac{1}{\sigma}} \xi_{ij}^{\sigma-1} - \tau_{sj} \frac{1}{\varphi_i} \omega_i Q_{ij} \right\},
\]

and sales by firm \( i \) to market \( j \) are thus: \( R_{ij} = P_{ij} Q_{ij} = \kappa ((\xi_{ij} \varphi_i)/(\tau_{sj} \omega_i))^{\sigma-1} E_{sj} P_{sj}^{\sigma-1} \), where \( \kappa \) is a function of \( \sigma \). For the case of exports (\( j = x \)), and taking logs, we can rewrite this expression as:

\[
\ln R_{ix} = \ln \kappa + (\sigma - 1) (\ln \xi_{ix} + \ln \varphi_i - \ln \omega_i) - (\sigma - 1) (\ln \tau_{sx} - \ln P_{sx}) + \ln E_{sx}.
\]

(3)

The bulk of our empirical analysis will compare firm-level export behavior in a bust period, relative to a boom period.\(^{16}\) With that in mind, and letting \( \Delta \ln X \) denote the log change in the cross-year average value of \( X \) from boom to bust, we can express the log change in exports from boom to bust as

\[
\Delta \ln R_{ix} = (\sigma - 1) [\Delta \ln \xi_{ix} + \Delta \ln \varphi_i - \Delta \ln \omega_i] - (\sigma - 1) (\Delta \ln \tau_{sx} - \Delta \ln P_{sx}) + \ln \Delta E_{sx}.
\]

(4)

In order to transition to an estimating equation, we model the change in firm-specific foreign

\(^{15}\)Since our econometric specifications below include some location fixed effects, it would be straightforward to let the iceberg trade cost \( \tau_{sj} \) also be a function of the production location \( \ell \).

\(^{16}\)In Appendix E, we theoretically develop and empirically test specifications that use yearly data; these results facilitate the comparison of our estimates with those in the previous literature.
demand, productivity and cost levels as follows:

\[
\begin{align*}
\Delta \ln(\xi_{ix}) &= \xi_{sx} + \xi_{lx} + u_{ix}^\ell, \\
\Delta \ln(\varphi_i) &= \varphi_s + \varphi_\ell + \delta_\varphi \Delta \ln(\varphi_i^*) + u_i^\varphi, \\
\Delta \ln(\omega_i) &= \omega_s + \omega_\ell + \delta_\omega \Delta \ln(\omega_i^*) + u_i^\omega.
\end{align*}
\]

(5)

Note that we are decomposing these terms into (i) a sector fixed effect, (ii) a production location fixed effect, (iii) an observable part of these terms for the case of productivity (\(\varphi_i^*\)) and for input bundle costs (\(\omega_i^*\)), and (iv) a residual term.\(^\text{17}\) We can thus re-write equation (4) as:

\[
\Delta \ln R_{lx} = \gamma_{sx} + \gamma_{lx} + (\sigma - 1) \delta_\varphi \Delta \ln(\varphi_i^*) - (\sigma - 1) \delta_\omega \Delta \ln(\omega_i^*) + \varepsilon_{ix},
\]

(6)

where \(\gamma_{sx} \equiv (\sigma - 1) [\xi_{sx} + \varphi_s - \omega_s - \ln \tau_{sx} + \ln P_{sx}] + \ln E_{sx}, \gamma_{lx} \equiv (\sigma - 1) [\xi_{lx} + \varphi_\ell - \omega_\ell],\) and

\[
\varepsilon_{ix} = (\sigma - 1) [u_{ix}^\ell + u_i^\varphi - u_i^\omega].
\]

(7)

Following analogous steps as above, we derive an expression for the change in domestic sales:

\[
\Delta \ln R_{id} = \gamma_{sd} + \gamma_{ld} + (\sigma - 1) \delta_\varphi \Delta \ln(\varphi_i^*) - (\sigma - 1) \delta_\omega \Delta \ln(\omega_i^*) + \varepsilon_{id},
\]

(8)

where \(\gamma_{sd} \equiv (\sigma - 1) [\xi_{sd} + \varphi_s - \omega_s - \ln \tau_{sd} + \ln P_{sd}] + \ln E_{sd}, \gamma_{ld} \equiv (\sigma - 1) [\xi_{ld} + \varphi_\ell - \omega_\ell],\) and

\[
\varepsilon_{id} = (\sigma - 1) [u_{id}^\ell + u_i^\varphi - u_i^\omega].
\]

(9)

We use equations (6) through (9) to generate predictions for the asymptotic properties of several estimators of the response of log exports to demand-driven changes in log domestic sales. The assumption of constant marginal costs implies that, according to this baseline model, the parameter of interest is zero: changes to \(\xi_{id}\) that are independent of changes in the other model fundamentals (i.e. \(\xi_{ix}, \varphi_i,\) and \(\omega_i\)) have no effect on \(\ln R_{lx}.\) However, many estimators of the impact of log domestic sales on log exports based on observational data will yield estimates that differ from zero, even in large samples. We discuss here the asymptotic properties of different OLS and IV estimators.

Consider first using OLS to estimate the parameters of the following regression, which includes the change in log domestic sales as an additional covariate in equation (6):

\[
\Delta \ln R_{ix} = \gamma_{sx} + \gamma_{lx} + (\sigma - 1) \delta_\varphi \Delta \ln(\varphi_i^*) - (\sigma - 1) \delta_\omega \Delta \ln(\omega_i^*) + \beta \Delta \ln R_{id} + \varepsilon_{ix}.
\]

(10)

From equations (7), (9), and (10), the probability limit of the OLS estimator of the coefficient on

\(^{17}\)More precisely, we assume that \(\Delta \ln \xi_{ix} + \Delta \ln \varphi_i - \Delta \ln \omega_i = d_s + d_\ell + \delta_\varphi \Delta \ln(\varphi_i^*) + \delta_\omega \Delta \ln(\omega_i^*) + u_i,\) with \(u_i\) incorporating the unobserved components of export demand, productivity and factor costs, and \(E[u_i] = E[u_i|\{d\}_s, \{d\}_e, \Delta \ln(\varphi_i^*), \Delta \ln(\omega_i^*)] = 0,\) where \(\{d\}_s\) denotes a complete set of sector-specific dummy variables, and \(\{d\}_e\) is a complete set of location-specific dummy variables.
domestic sales can be written as

$$\text{plim}(\hat{\beta}_{OLS}) = \frac{\text{cov}(\Delta \ln R_{ix}, \Delta \ln R_{id})}{\text{var}(\Delta \ln R_{id})} = \frac{\text{cov}(u_{ix}^{\xi} + u_{i}^{\varphi} - u_{ix}^{\omega}, u_{id}^{\xi} + u_{i}^{\varphi} - u_{id}^{\omega})}{\text{var}(u_{id}^{\xi} + u_{i}^{\varphi} - u_{id}^{\omega})},$$

(11)

where we denote by $\hat{X}$ the residual of a regression of a variable $X$ on $\{d\}_s$, $\{d\}_f$, $\Delta \ln \phi_i$, and $\Delta \ln \omega_i$. We draw two main conclusions from equation (11). First, as long as changes in productivity and production factor costs are not perfectly observable – and their unobserved component is not fully captured by the sector and location fixed effects – there will be a spurious positive correlation between changes in exports and changes in domestic sales. Intuitively, unobserved productivity or factor cost changes will affect sales in the same direction in all markets in which a firm sells. In large samples, this spurious positive correlation will lead $\hat{\beta}_{OLS}$ to be biased upwards. Second, even when one proxies for changes in productivity and factor costs perfectly (i.e., $u_{ix}^{\xi} = u_{i}^{\varphi} = 0$), in the presence of a non-zero correlation in the change in residual demand faced by firms in domestic and foreign markets (i.e. $\text{cov}(u_{ix}^{\xi}, u_{id}^{\xi}) \neq 0$), the OLS estimator of $\beta$ will also converge to a non-zero value. Because this residual demand does not capture market-specific aggregate shocks (which are controlled by the sectoral fixed effects), it seems plausible that $u_{ix}^{\xi}$ and $u_{id}^{\xi}$ will be positively correlated in the data, leading $\hat{\beta}_{OLS}$ again to be biased upwards. Notice also that, if we had not controlled for sectoral and location fixed effects, the probability limit of the OLS estimator of $\beta$ would likely be even larger.\(^{18}\)

Consider next using an IV estimator of the parameters in equation (11). Specifically, consider instrumenting $\Delta \ln R_{id}$ with an observed covariate $Z_{id}$ such that $Z_{id}$ is either a proxy of $\Delta \ln \xi_{id}$ or has a causal impact on this firm-specific domestic demand shifter. In this case, the probability limit of the IV estimator of $\beta$ is

$$\text{plim}(\hat{\beta}_{IV}) = \frac{\text{cov}(\Delta \ln R_{ix}, Z_{id})}{\text{cov}(\Delta \ln R_{id})} = \frac{\text{cov}(u_{ix}^{\xi} + u_{i}^{\varphi} - u_{ix}^{\omega}, Z_{id})}{\text{cov}(u_{id}^{\xi} + u_{i}^{\varphi} - u_{id}^{\omega}, Z_{id})},$$

(13)

The constant-marginal-cost model predicts that $\hat{\beta}_{IV}$ converges in probability to its true value of zero as long as the instrument $Z_{id}$ verifies two conditions: (a) it is correlated with the change in domestic sales of firm $i$ after controlling for (or partialing out) sector and location fixed effects as well as observable determinants of the firm’s marginal cost; and (b) it is mean independent of the change in firm-specific unobserved productivity, $u_{i}^{\varphi}$, factor costs, $u_{i}^{\omega}$, and export demand $u_{ix}^{\xi}$. As illustrated by the second equality in equation (13), an instrument can only (generically) verify conditions (a) and (b) if its effect on domestic sales works exclusively through the change in

\(^{18}\)To give an example, the probability limit of $\hat{\beta}_{OLS}$ in the absence of production location fixed effects is:

$$\text{plim}(\hat{\beta}_{OLS}) = \frac{\text{cov}(u_{ix}^{\xi} + \xi_{ix} + \phi_i - \omega_i + u_{i}^{\varphi} - u_{ix}^{\omega}, u_{id}^{\xi} + \xi_{id} + \phi_i - \omega_i + u_{i}^{\varphi} - u_{id}^{\omega})}{\text{var}(u_{id}^{\xi} + \xi_{id} + \phi_i - \omega_i + u_{i}^{\varphi} - u_{id}^{\omega})},$$

(12)

which is likely larger than the expression in equation (11) due to: the presence of $\phi_i - \omega_i$ in both terms of the covariance in the numerator of the expression in equation (12); and, the likely positive correlation between $\xi_{ix}$ and $\xi_{id}$.
domestic demand not accounted for by the fixed effects and observable covariates included in the estimating equation, i.e., $u_{id}$.

Although our discussion above has centered around the role of unobserved supply and demand factors in biasing estimates of $\beta$, Berman et al. (2015) emphasize that measurement error in both domestic sales and exports constitutes an additional source of possible bias when estimating the effect of exports on domestic sales (or vice versa). Because in many empirical settings – ours included – domestic sales are computed by subtracting exports from the total sales of firms, it is important to stress that measurement error in this setting does not just lead to attenuation bias as in the classical error-in-variables model. More specifically, and as we detail in Appendix A.1 (see also Berman et al., 2015), under plausible conditions, measurement error in firm total sales and exports will lead to a negative bias in the OLS estimate $\hat{\beta}_{OLS}$. As we show in the Appendix, however, if an instrument satisfies the same conditions (a) and (b) outlined above, and is also mean independent of the measurement error in exports, the IV estimator in equation (13) will still converge to zero in the presence of measurement error in total sales and exports.

We have focused our discussion so far on the intensive margin of exports, namely the impact of domestic demand shocks on the level of exports conditional on exporting. In Appendix A.2, we show that an analysis of the extensive margin of exports modeled as a linear probability model delivers very similar insights. More specifically, when estimating the effect of changes in domestic sales on the probability of exporting, even if the true effect were to be zero, one is likely to estimate a spurious positive elasticity whenever productivity and production factor costs are not perfectly captured by sector and location fixed effects and observable controls, or whenever unobserved residual demand shocks are positively correlated across markets. An instrument satisfying conditions (a) and (b) above will continue to effectively remove these biases as long as it satisfies the additional condition of being mean independent of the part of the change in the firm’s fixed cost of exporting not captured by the various fixed effects and marginal cost proxies. Consequently, if the instrument affects domestic sales exclusively through the demand shock $u_{id}$, it will continue to be valid in those extensive margin specifications (see Appendix A.2 for more details).

3 Setting and Data

To construct a plausibly exogenous measure of the domestic demand faced by firms, we exploit geographical variation in the severity of the impact of the Great Recession of the late 2000s and early 2010s in Spain. In this section, we will describe the setting and data, and in section 4 we will provide a more detailed account of our identification strategy.

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19 The implications of measurement error in total sales and exports in this extensive margin specification are analogous to those in the intensive margin specification (see Appendix A.2).
3.1 The Great Recession in Spain: Description

The macroeconomic history of Spain during the period 2000-2013 is a tale of a boom followed by a bust. As shown in Figure 2, between the year 2000 and the peak of the cycle in 2008, Spain’s GDP and domestic demand grew by approximately 25% in real terms. In the five subsequent years until 2013, domestic demand decreased to the level of the year 2000, while real GDP fell by an accumulated 9.2%. In that same period, the unemployment rate shot up from 9% to 26%.

Figure 2: The Great Recession in Spain

The particularly severe impact of the Great Recession in Spain is largely explained by the fact that the economic boom of the early 2000s was primarily driven by a real estate bubble. The construction sector accumulated an increasing share of GDP and employment. For instance, in 2006, 735,000 new houses were built in Spain, a number comparable to that in Germany, Italy and the UK combined. This real estate boom was in turn fueled by the increased availability of cheap credit to households, firms and real estate developers, which resulted from capital inflows related to the adoption of the euro in 2002 and the global savings glut (Santos, 2014). The ratio of mortgage credit to GDP went up from 40% in 2000 to 100% in 2008 (Basco and Lopez-Rodriguez, 2018). Importantly, the very high loan-to-value (LTV) ratios associated with mortgage credit were partly used to finance private consumption, particularly vehicle purchases (Masier and Villanueva, 2011).

The unraveling of the subprime mortgage market in the U.S. in the summer of 2007 had an immediate effect on the supply of credit in Spain. However, the effects were fully transmitted to the real economy only about one year later, coinciding with the fall of Lehman Brothers in September 2008, and the sudden stop in capital inflows. The recession officially started in the fourth quarter of 2008, and intensified during 2009 with a 4% drop in GDP. The growth in the stock of vehicles in

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20The share of total employment in the construction sector peaked at 13.5% in the summer of 2007 and then collapsed, reaching 5.4% by early 2014, with a similar pattern for the contribution of this sector to Spain’s GDP (12.4% in 2007 and 6.8% in 2014).

Spain, which had been stable at an average rate of 3.6% a year during the boom, suddenly came to a halt in 2008. In fact, in 2013, the national stock of vehicles in Spain was lower than in 2008 by around 52,000 units.

Importantly for the identification strategy we describe in the next section, the real estate boom and subsequent bust featured significant geographic variation, concentrating mainly in some parts of the Mediterranean coast and in medium-sized and large cities. As we shall document in the next section, this in turn translated into substantial geographic variation in the extent to which the slump affected domestic demand and the domestic sales of Spanish firms.

### 3.2 The Spanish Export Miracle

As Figure 2 illustrates, the evolution of Spain’s aggregate exports during the period 2000-2013 was significantly different from that of aggregate domestic demand. After a significant 19.2% drop during the global trade collapse of late 2008 and early 2009, aggregate exports grew during the period 2010-2013 at an even faster rate than during the boom years. Specifically, while exports had grown by an accumulated 35% in the eight-year period 2000-2008, they grew by a very similar 33% in just the four years between 2009 and 2013. This acceleration in export growth occurred at a time during which all indicators of domestic economic activity were showing a significant decline. As a consequence, the fall in real GDP was significantly smaller than the fall in domestic demand.

One might wonder whether changes in international relative prices could explain the growth in Spanish exports during the period 2009-2013. It is however easy to rule out exchange rate movements as a key operating mechanism since, as shown in Figure 1 in the Introduction, Spanish exports clearly outperformed those of other countries in the euro area (even though Spain’s GDP dropped faster than it did on average in the euro area). It has also been argued that Spain underwent an internal devaluation (through wage moderation starting in 2009, and via a labor reform in 2012), but there is little evidence that export prices in Spanish manufacturing fell relative to export prices in other Euro area countries in the period 2009-2013.

Motivated by these facts, we will hereafter focus on an exploration of the “vent-for-surplus” mechanism, according to which the domestic slump, by freeing up production capacity, might have directly incentivized Spanish producers to step up their efforts to market their goods in foreign markets. In principle, the associated growth in exports could have materialized along the intensive margin (with continuing exporters increasing the share of exports in their sales) or along the extensive margin (via net entry into foreign markets). Later in the paper, we will explore both margins, but descriptive evidence suggests that the bulk of the growth was driven by the intensive margin. Using detailed Spanish Customs data, De Lucio et al. (2017) find that net firm entry (i.e., new exporters net of firms quitting exporting) contributed a mere 14% to the export growth between 2008 and 2013, while the remaining 86% of export growth was driven by continuing exporters. Similarly, in our sample of manufacturing firms, we find that continuers contributed 91% of the growth in exports between the boom and the bust periods, and the extensive margin only
accounted for 9% of export growth.\textsuperscript{22}

### 3.3 Data Sources

Our data cover the period 2000-2013 and come from two separate confidential administrative data sources. The first is the Commercial Registry (\textit{Registro Mercantil Central}). It contains the annual financial statements of around 85% of registered firms in the non-financial market economy in Spain.\textsuperscript{23} Among other variables, it includes annual information for each firm on the following: sector of activity (4-digit NACE Rev. 2 code), 5-digit zip code location, net operating revenue, material expenditures (cost of all raw materials and services purchased by the firm in the production process), labor expenditures (total wage bill, including social security contributions), and total fixed assets.\textsuperscript{24}

The second dataset is the foreign transactions registry collected by the Bank of Spain (\textit{Banco de España}). For both exports and imports, it contains transaction-level information on the fiscal identifier of the Spanish firm involved in the transaction, the amount transacted, the product code (SITC Rev. 4), the country of the foreign client, and the exact date of the operation (no matter when the payment was performed). Starting in 2008, however, the dataset’s information on the product code and on the destination country became unreliable. The reason for this is that the entities reporting to the Bank of Spain were given the option of bundling a set of transactions together. In those cases, each entry reflects only the country of destination and product code of the largest transaction in that bundle (see Appendix B for more details). This feature of the dataset precludes us from studying exports at the firm-product-destination-year level, but we can still reliably aggregate this transaction-level data to obtain information on total export volume by firm and year.

This international trade database has an administrative nature because Banco de España legally requires financial institutions and external (large) operators to report this information for foreign transactions above a fixed monetary threshold. Until 2007, the minimum reporting threshold was fixed at 12,500 euros per transaction. Since 2008, information must be reported for all transactions performed by a firm during a natural year as long as at least one of these transactions exceeds 50,000 euros. In order to homogenize the sample, for the period 2000 to 2007, we only record a positive export flow in a given year for legal entities that have at least one transaction exceeding

\textsuperscript{22}De Lucio et al. (2017) also show that a third of the contribution of continuing exporters is due to entry into new destination countries and products, while the other two thirds are due to growth in existing product-country combinations. Unfortunately, the nature of the export data available to us does not allow us to explore the firm-level extensive margin at the product or destination-market level. See Section 3.3 for a description of our data limitations.

\textsuperscript{23}We obtain information on the Commercial Registry from two different sources: (i) the \textit{Central de Balances} dataset, compiled by the Bank of Spain and (ii) the \textit{Sabi} dataset, compiled by Informa (a private company). For details on how we combine these two datasets, see Almunia, Lopez-Rodriguez and Moral-Benito (2018).

\textsuperscript{24}NACE (\textit{Nomenclature générale des activités économiques dans les Communautés Européennes}) is the European statistical classification of economic activities. It classifies manufacturing firms into 24 different sectors. Some firms move to a different zip code or change their sector classification during the period of analysis. We assign to these firms a fixed zip code and 2-digit sector, using their most frequent value in each case. A firm’s zipcode corresponds to the location of the firm’s headquarters.
50,000 euros in that year (for more details see Appendix B).

In both datasets, a firm is defined as a business constituted in the form of a Corporation (**Sociedad Anónima**), a Limited Liability Company (**Sociedad Limitada**), or a Cooperative (**Cooperativa**). We merge both datasets using the fiscal identifier of each firm. Using the merged database, we define each firm’s domestic sales as the difference between its total annual net operating revenue and its total export volume, which motivated our discussion of measurement error in Section 2.

![Figure 3: Output, Employment, Wage Bill and Export Dynamics](image)

Panel (a): Output  
Panel (b): Employment  
Panel (c): Wage Bill  
Panel (d): Exports

To confirm the validity of the information contained in these two data sources, we compare the coverage of our resulting dataset with the official publicly available aggregate data on output, employment and total wage bill (from National Accounts) and on goods exports (from Customs). Figure 3 shows that our dataset tracks nearly perfectly the aggregate evolution of output, employment, total payments to labor, and exports over time. Due to the reporting thresholds described above, aggregate exports in our sample naturally fall a bit short of aggregate exports in the Customs data, but note that the gap is very similar in the boom and bust periods (the average coverage is 91.8% in 2000-08 and 91.3% in 2009-13).²⁵

²⁵Most of the gap in coverage is explained by the fact that a nontrivial share of Spanish exports recorded by Customs is carried out by legal entities or individuals that are not registered as firms undertaking economic activity
We complement the firm-level data described above with yearly municipality-level data on the stock of vehicles and on total population. The information on the stock of vehicles by municipality is provided by the Spanish Registry of Motor Vehicles (Dirección General de Tráfico), while the information on the population by municipality is provided by the Spanish National Statistical Office (Instituto Nacional de Estadística). When matching this municipality-level data with our firm-level data, we need to deal with the fact that the information on the location of firms is provided at the zip code level, and that the mapping between municipalities and zip codes is not one-to-one. More precisely, larger municipalities are often assigned multiple zip codes, and in a very small number of cases, a single zip code is assigned to more than one municipality. In the former case, we associate the same value for the stock of vehicles and population to all firms located in the same municipality, independently of the zip code of location; for firms in zip codes containing multiple municipalities, we construct a zip code-level instrument by averaging the stock of vehicles per capita across these municipalities.

When exploring the robustness of our results, we use information on additional variables. The underlying sources for these variables are discussed in Appendix B.

4 Identification Approach

In this section, we first discuss in detail our identification approach, and later highlight various potential threats affecting this approach and how we seek to address them.

4.1 Identification Approach

As explained in Section 3.1, a key characteristic of the Great Recession in Spain is that it affected different regions differently. Panel (a) in Figure 4 illustrates this fact. The figure plots the standardized percentage change in domestic sales for the average firm located in each of the 47 Spanish peninsular provinces and operating in at least one year of the boom period (2002-2008) and at least one year of the bust period (2009-2013). The provinces where the average firm experienced a reduction in domestic sales smaller than the national average are in darker color, while those where the average firm experienced a larger reduction in domestic sales are in lighter color. Specifically, Figure 4 illustrates that firms located in the northern and western regions saw changes in domestic sales larger (less negative) than the average, while firms located in the center of the country and in southern and eastern regions experienced relatively large domestic sales reductions.

The heterogeneity in the changes in domestic sales that we document in panel (a) of Figure 4 could have been caused by heterogeneity in supply factors or by heterogeneity in factors affecting
local demand for manufacturing goods. We next propose an approach to attempting to isolate variation in local demand for manufacturing goods.

Our approach consists in proxying changes in local demand for manufacturing goods using observed changes in demand per capita for one particular type of manufacturing products: vehicles. Panel (b) in Figure 4 shows that there is substantial variation in the degree to which the number of vehicles per capita changed across provinces between the boom and the bust years. Specifically, the provinces in the Northwest and in the Southwest experienced a relative increase in the number of vehicles per capita, while the region around Madrid and the provinces in the Northeast and along the Mediterranean cost experienced a relative reduction.

By illustrating provincial averages, the maps in Figure 4 hide substantial spatial variation at the sub-province level (across 5-digit zip codes) in both the boom-to-bust changes in average firm-level domestic sales and in the boom-to-bust changes in the number of vehicles per capita. We illustrate this variation in Figure 5 for the case of the two most populated provinces in Spain: Madrid and Barcelona. To facilitate a comparison of the within-province across-zip codes variation illustrated in Figure 5 with the across-province variation illustrated in Figure 4, the average zip code changes illustrated in Figure 5 have been standardized using the Spain-wide mean and standard deviation of the corresponding variable used to standardize the corresponding variables in Figure 4.

Changes in the number of vehicles per capita between the boom and the bust years could have been due either to purchases of new vehicles or to scrapping of old ones. We measure the change in the stock, rather than just new purchases, to avoid contamination from the “cash for clunkers” program (Plan PIVE) that the Spanish government put in place during the bust period.
Panels (a) and (b) reveal a large heterogeneity in the change in both firms’ average domestic sales and vehicles per capita across zip codes located in the region of Madrid: while the center area of the region that contains a large number of tightly packed zip codes (this area corresponds to the city of Madrid) experienced small reductions in firm average domestic sales (relative to the Spain-wide average), surrounding zip codes experienced changes in domestic sales that were more than two standard deviations above the national average. Similarly, while the zip codes belonging to the city of Madrid experienced a large reduction in the number of vehicles per capita (more than two standard deviations smaller than the Spain-wide average), other zip codes to the east, north and west of the city of Madrid saw increases in vehicles per capita significantly above the national average. Panels (c) and (d) provide analogous information for the region of Barcelona. Although the heterogeneity across zip codes located in the province in Barcelona is smaller than that observed within the Madrid region, panel (c) still shows how certain zip codes experienced growth rates smaller than the national average while others experienced changes in firm average domestic sales more than a standard deviation above that average.

In the next section, we exploit the variation illustrated in Figures 4 and 5 to identify the impact of a local demand shock on firms’ exports operating through its effect on the firms’ domestic (Spain-wide) sales. Specifically, we divide our sample into a “boom” period (2002-08) and a “bust” period (2009-13), and assess the extent to which a demand-driven decline in the domestic sales in the bust period relative to the boom period is associated with a relative increase in export sales in the bust period. With this aim, we will use observed “boom-to-bust” changes in the stock of vehicles per capita at the zip code level as a proxy for the changes in the aggregate demand for manufacturing goods that the corresponding geographical area experienced in the bust relative to the boom period. Equipped with this proxy for local goods demand, we will use it to instrument for firm-level changes in domestic sales for firms located in that zip code.

Our identification strategy is based on three main pillars. First, it builds on the fact that durable goods consumption, and vehicle purchases in particular, are strongly procyclical and thus are a useful proxy for changes in ‘local demand’, i.e., the overall propensity of an area’s inhabitants to consume (see Stock and Watson, 1999). Consistent with this notion, Mian and Sufi (2013) have documented how variation in the extent to which the U.S. subprime mortgage default crisis of 2007-10 affected household housing wealth in different areas in the United States translated into geographical variation in vehicle purchases. It would be interesting to tie the geographical variation in the change in the stock of vehicles per capita in Spain to the housing slump, but idiosyncratic features of the Spanish housing market complicate such an analysis. In Section 6, we will revisit this issue and explore the robustness of our results to an alternative shifter of firms’ domestic sales that uses a determinant of the housing supply elasticity in a given zip code as a proxy for the magnitude of the negative impact of the Great Recession on household housing wealth and consumption.

The second building block of our identification strategy is that changes in zip code-level demand are a good predictor for changes in domestic sales of Spanish firms producing in the corresponding zip code. This would naturally be the case if domestic sales of firms were disproportionately lo-
Figure 5: The Great Recession in Madrid and Barcelona: Variation Across Zip Codes

Notes: Panel (a) illustrates the standardized percentage change in average firm-level domestic sales between the period 2002-2008 and the period 2009-2013. Therefore, if this variable takes any given value $p$ for a given zip code, it means that the average firm located in this zip code experienced a relative change in average yearly domestic sales between 2002-2008 and 2009-2013 that was $p$ standard deviations above the change experienced by a firm located in the (Spain-wide) mean zip code. Panel (b) illustrates the standardized percentage change in cars per capita between the period 2002-2008 and the period 2009-2013. Therefore, if this variable takes any given value $p$ for a given zip code, it means that this zip code experienced a relative change in vehicles per capita between 2002-2008 and 2009-2013 that was $p$ standard deviations above the change experienced by the (Spain-wide) mean zip code. Zip codes that do not host any of the firms in our dataset appear in white, with the label “No data”.

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calized in the zip code in which production takes place. Indeed, as mentioned in the Introduction, Hillberry and Hummels (2008) document the existence of such a ‘zip code home bias’ in U.S. manufacturers’ shipments. We cannot replicate their results with our Spanish data, but we document in Appendix C.3 the existence of significant home bias in manufacturing shipments with data at the province level.28

The third and final pillar of our identification approach is ensuring that changes in local vehicle purchases per capita are not correlated with supply shocks that might have an independent effect on the exporting decisions of Spanish firms. This exclusion restriction is absolutely central to the validity of our strategy, so we next outline how it might be violated and how we will deal with potential threats to identification.

4.2 Threats to Identification

The main concern with our approach is that the geographical variation in our demand measure might be correlated with geographical variation in unobserved supply shocks. While we cannot test our exclusion restriction formally, we address this endogeneity concern in two different ways.

First, we control in our specifications for sector and location (province) fixed effects and for firm-specific measures of productivity and labor costs. By controlling for sector fixed effects, we base our identification on observing how domestic sales and exports changed between the boom and the bust for different firms operating in the same sector but located in regions that experienced different changes in the stock of vehicles per capita. For example, these sector fixed effects control for shocks such as the expiration of the Multi Fiber Arrangement (MFA) on January 1, 2005, which eliminated all European Union quotas for textiles imported from China and which had a large impact on both the domestic sales and exports of Spanish textile manufacturers.29 By controlling for changes in wages and productivity at the firm level, we aim to identify the effect that changes in local demand had on firms’ exports through channels other than the internal devaluation channel. More specifically, these controls help address the concern that the reduction in unit labor costs observed in Spain during the period 2009-13 might have been heterogeneous across different Spanish regions in a manner that is correlated with our demand measure. This concern also motivates the inclusion of province fixed effects, with which we seek to control for unobserved variation in factor costs and productivity that is not picked up by our proxies for these variables.30

Our second approach to assuage endogeneity concerns is motivated by the fact that our various fixed effects and proxies for firm-level productivity and wage costs might not perfectly capture

28 More specifically, we find that own-province sale shares range from a low 18% in Transport Equipment (an industry we exclude from our analysis, as explained in the next section) to a high of 43% for Nonmetallic Minerals. The overall provincial home-bias in manufacturing is 28% (see Figure C.3 in Appendix C.3). The data on province-to-province shipments comes from the C-Intereg database (for details on this database, see Llano et al., 2010.)

29 If sector fixed effects had not been included in our specifications and textile firms were to be on average located in Spanish regions that suffered larger negative local demand shocks, our estimates would confound the impact of the MFA expiration and the negative local shocks.

30 As our instrument only varies at the municipality level and we use information only in one long-difference, it is not feasible to introduce municipality fixed effects.
supply-side factors, and that unobserved, residual supply shocks might be correlated with our proxy for changes in local demand. For instance, if a disproportionate share of cars in Spain was sold in the municipalities in which car producing plants are located, then negative residual supply shocks affecting those car plants and their workers could well generate a correlation at the municipality level between car purchases and domestic sales. The Spanish motor vehicles sector represented on average around 7% of manufacturing employment during this period, so this is not an unreasonable concern, though we should stress that roughly 75% of cars purchased in Spain are imported (as indicated by data from the Spanish National Institute of Statistics). To deal with this threat to identification, in all regressions presented in the next section we exclude all firms operating in the auto industry (NACE Rev. 2 code 29). To further assuage this, in Section 6 we also explore how our baseline results are impacted when excluding from our sample: (i) all firms located in a zip code that hosts at least one firm in the auto industry employing more than 20 workers; (b) all firms located in a zip code or in the neighborhood of a zip code with a significant share of manufacturing employment accounted for by the auto industry; and (c) all firms producing in sectors that are either leading input providers or leading buying industries of the vehicles manufacturing industry.

In Section 6, we also perform several additional robustness tests of the vent-for-surplus hypothesis. Specifically, (i) we explore whether various heterogeneous effects are in line with what one would expect if the vent-for-surplus mechanism was operating, (ii) we present IV estimates using alternative instruments for firms’ domestic sales that exploit different variation in the data, (iii) we present regressions that control for several additional confounding factors, and finally, (iv) we perform falsification tests.

5 Baseline Results

5.1 Intensive Margin

Table 1 presents OLS estimates of the elasticity of “boom-to-bust” changes in firms’ export flows with respect to “boom-to-bust” changes in domestic sales for continuing exporters – i.e., firms that exported both in the boom as well as in the bust. There are 8,018 such firms in our dataset.

As discussed in Section 2, when no firm-specific controls are included in the regression, we expect to observe a positive relationship between a firm’s changes in domestic sales and the changes in its volume of exports. This positive relationship is indeed observed in column 1 of Table 1, in which we estimate an elasticity of export flows with respect to domestic sales of 0.131. In the remaining columns of Table 1, we control for various sources of marginal cost heterogeneity across firms, with the aim of controlling for sources of correlation between firms’ exports and domestic sales other than those captured by the vent-for-surplus mechanism. In column 2 and 3, we control for log changes in firms’ observed marginal costs. Specifically, in column 2 we control for the change in firms’ productivity (estimated following the procedure in Gandhi et al., 2016, as detailed in Appendix B.5), and in column 3 we control for a measure of the change in firms’ average wages (reported by the firm in its financial statement). Consistent with the discussion in Section 2, controlling for these
Table 1: Intensive Margin: Ordinary Least Squares Estimates

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<th>Dependent Variable:</th>
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<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>ΔLn(Domestic Sales)</td>
<td>0.131***</td>
<td>-0.147***</td>
<td>-0.228***</td>
<td>-0.217***</td>
<td>-0.204***</td>
<td>-0.186***</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.028)</td>
<td>(0.027)</td>
<td>(0.027)</td>
<td>(0.027)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>ΔLn(TFP)</td>
<td>1.057***</td>
<td>1.298***</td>
<td>1.375***</td>
<td>1.357***</td>
<td>1.336***</td>
<td>1.338***</td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td>(0.052)</td>
<td>(0.051)</td>
<td>(0.051)</td>
<td>(0.051)</td>
<td>(0.053)</td>
</tr>
<tr>
<td>ΔLn(Avg. Wages)</td>
<td>-0.590***</td>
<td>-0.540***</td>
<td>-0.525***</td>
<td>-0.525***</td>
<td>-0.525***</td>
<td>-0.482***</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.051)</td>
<td>(0.051)</td>
<td>(0.051)</td>
<td>(0.051)</td>
<td>(0.054)</td>
</tr>
<tr>
<td>Observations</td>
<td>8,018</td>
<td>8,018</td>
<td>8,018</td>
<td>8,018</td>
<td>8,018</td>
<td>7,507</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.005</td>
<td>0.088</td>
<td>0.106</td>
<td>0.146</td>
<td>0.158</td>
<td>0.265</td>
</tr>
<tr>
<td>Sector FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Province FE</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Municipality FE</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Note: Standard errors clustered at the municipality level in parenthesis. For any X, ΔLn(X) is the difference in Ln(X) between its average in the 2002-2008 period and its average in the 2009-2013 period. The estimation sample includes all firms selling in at least one year in the period 2002-2008 and in the period 2009-2013. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

supply shocks reduces the OLS estimate of the coefficient on domestic sales in the regression in equation (10). In fact, the coefficient turns negative (−0.228), indicating that, once we control for the observable part of firms’ supply shocks, domestic sales and exports are negatively correlated. Columns 4, 5 and 6 aim to additionally control for unobserved determinants of firms’ marginal costs that are time varying. To do so, and motivated by the specification in equation (10), we sequentially add sector fixed effects (in column 4) and location fixed effects (in columns 5 and 6). In the latter case, we first include province fixed effects and, in column 6, we instead include municipality fixed effects. The resulting estimates continue to be negative and indicate that a 10% decrease in a firm’s domestic sales, keeping its productivity and average wages constant, implies around a 2% increase in its aggregate export flows.\(^{31}\)

One might be concerned that because total sales are a key input in the computation of our firm-level measure of TFP, our empirical results are just unveiling a mechanical negative correlation between exports and domestic sales once one holds total sales revenue constant (by controlling for it). Although log TFP and log total sales are obviously positively correlated (as one would expect in light of our model), the correlation is far from perfect, particularly when considering log changes in these variables. More specifically, the correlation between log changes in TFP and log total sales in our yearly data is 0.31, while it is 0.54 when looking at “long differences” in these variables. To

\(^{31}\)In Appendix E, we present OLS regressions using the full firm-year data for the period 2002-2013. Our results are quite similar to those in Table 1. Without controlling for supply factors, changes in domestic sales are positively associated with changes in exports. However, once we control for observable determinants of firms’ marginal costs and for various fixed effects, we estimate a negative elasticity of exports to domestic sales. This elasticity is around −0.3 and thus somewhat larger (in absolute value) than the one obtained in our “long differences” specification. In Appendix E, we also explore variation in the elasticity of exports with respect to domestic sales across sectors, across firms of different sizes and across firms with different pre-determined propensities to export.
further assuage this concern, in Section 6.4 we explore the robustness of our results to alternative measures of log firm TFP that feature an even lower correlation with log firm sales.

In Table 2, we turn to our two-stage least squares estimates of the elasticity of the firm’s “boom-to-bust” change in exports with respect to its “boom-to-bust” demand-driven change in domestic sales. The first-stage estimates (reported in columns 1 to 4) reveal that firms located in municipalities that experienced a larger drop in the stock of vehicles per capita also suffered a larger decline in their domestic (Spain-wide) sales. This relationship is robust to controlling for sector and province fixed effects and for our measures of firms’ changes in productivity and labor costs: the statistic of an $F$-test for the null hypothesis that changes in the stock of vehicles per capita in a region have no impact on the domestic sales of the firms located in that zip code is comfortably above widely accepted critical values in all specifications.

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>$\Delta \ln(\text{Domestic Sales})$</th>
<th>$\Delta \ln(\text{Exports})$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>$\Delta \ln(\text{Domestic Sales})$</td>
<td>-2.185***</td>
<td>-1.346***</td>
</tr>
<tr>
<td>$\Delta \ln(\text{Vehicles p.c. in municipality})$</td>
<td>0.336***</td>
<td>0.447***</td>
</tr>
<tr>
<td>$\Delta \ln(\text{TFP})$</td>
<td>0.785***</td>
<td>0.948***</td>
</tr>
<tr>
<td>$\Delta \ln(\text{Avg. Wages})$</td>
<td>-0.544***</td>
<td>-0.447***</td>
</tr>
<tr>
<td>F-statistic</td>
<td>31.17</td>
<td>46.39</td>
</tr>
<tr>
<td>Observations</td>
<td>8,018</td>
<td>8,018</td>
</tr>
<tr>
<td>Sector FE</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Province FE</td>
<td>No</td>
<td>No</td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by municipality. For any $X$, $\Delta \ln(X)$ is the log difference between the average of $X$ in 2002-2008 and its average in 2009-2013. Vehicles p.c denotes the stock of vehicles per capita. Columns 1-4 contain first-stage estimates; columns 5-8 contain second-stage estimates. F-statistic denotes the corresponding statistic for the Vehicles p.c covariates. Significance levels: ***$p<0.01$, **$p<0.05$, *$p<0.1$.

The second stage estimates (reported in columns 5 to 8) reveal elasticities of exports with respect to domestic sales that are significantly larger (in absolute value) than the OLS elasticities reported in Table 1. This is true regardless of whether or not one controls for sector and province fixed effects as well as for changes in our measures of the firm’s productivity and labor costs. Our preferred estimate in column 8 indicates an elasticity of exports with respect to domestic sales of around $-1.6$ ($-1.602$). These significantly more negative IV elasticities are consistent with the hypothesis, formalized in equation (11), that, even after controlling for sector and location fixed effects and for firm proxies of productivity and average labor costs, there still remain unobserved determinants of firms’ marginal costs that induce a spurious positive correlation between their sales in the domestic and foreign markets.
It is important to stress that an elasticity of $-1.6$ does not necessarily imply a more-than-complete substitution of exports for domestic sales. For a firm with an initial export share of $\chi\%$, a demand-driven drop of €100 in their domestic sales would lead to a $\frac{160 \times \chi}{1 - \chi}$ increase in exports. For example, for every €100 of lost domestic sales, a firm with an export share of 25% would able to recoup €53.3 via exports, while a firm with an export share of one-third would be able to recoup €80.

5.2 Extensive Margin

We next turn to studying the causal impact of demand shocks on the extensive margin of exporting. As in our intensive margin regressions, we divide the sample period into a boom (2002-08) and a bust period (2009-13), and explore how (plausibly) exogenous demand-driven changes in domestic sales affect firms’ probability of exporting in each of these two periods. More specifically, we implement a two-stage least squares estimator of a linear probability model in which a firm’s dummy capturing positive exports in a given period (boom or bust) is regressed on firm and sector-period fixed effects, province-period fixed effects, the log of firm-level average TFP in that period, the log of average wages in that period, and the log of average domestic sales in that period, with log domestic sales instrumented with the average stock of cars per capita in the firm’s municipality of location during that period.\footnote{We have also confirmed the robustness of our results to an alternative specification in which the left-hand-side variable is a dummy variable that treats a a firm as an ‘exporter’ only if it exports for two or more years in a given period.} Besides this linear probability model, we also estimate analogous specifications in which we substitute the export dummy used as dependent variable by a variable capturing the proportion of years in a given period (boom or bust) for which a firm exports.

The results of these specifications are presented in Table 3. Column 1 reports the first stage for our full sample of 62,904 firms. As in Table 2, and given the inclusion of firm fixed effects, the results indicate that domestic sales fell more for firms located in municipalities with a larger decline in the stock of vehicles per capita. The F-stat (14.49) is smaller than in our intensive margin sample but is still comfortably above standard critical values. Columns 2 and 3 then present OLS and IV estimates of the link between domestic sales and export status, while columns 4 and 5 report OLS and IV estimates of the link between domestic sales and the proportion of years exported. The results of these two specifications deliver virtually identical results. First, and despite of the inclusion of firm fixed effects, the OLS result in columns 2 and 4 still suggest a positive relationship between domestic sales and the propensity to export. When isolating plausibly exogenous variation in domestic sales, however, the coefficient in column 3 turns negative and suggests that a 10% drop in domestic sales leads to a 1.07% increase in the probability of exporting, with an implied elasticity equal to $-0.584$. This effect is, however, very imprecisely estimated and it is thus not possible to reject the null hypothesis that demand shocks have no impact on the extensive margin of exporting. The same conclusion (with similar magnitudes) applies to column 5 which presents an estimate of the causal effect of demand shocks on the proportion of years exported.
Table 3: Extensive Margin: Two-Stage Least Squares Estimates

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>1st Stage OLS</th>
<th>2nd Stage OLS</th>
<th>Proportion of Years OLS</th>
<th>2nd Stage OLS</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Ln(Domestic Sales)</td>
<td>0.040***</td>
<td>-0.107</td>
<td>0.021***</td>
<td>-0.071</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.181)</td>
<td>(0.002)</td>
<td>(0.094)</td>
</tr>
<tr>
<td>Ln(Vehicles p.c. in Municipality)</td>
<td>0.089***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ln(TFP)</td>
<td>1.075***</td>
<td>0.038***</td>
<td>0.196</td>
<td>0.050***</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.005)</td>
<td>(0.195)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Ln(Average Wages)</td>
<td>-0.408***</td>
<td>-0.024***</td>
<td>-0.084</td>
<td>-0.031***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.004)</td>
<td>(0.074)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Observations</td>
<td>125,808</td>
<td>125,808</td>
<td>125,808</td>
<td>125,808</td>
</tr>
<tr>
<td>Firm FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Sector-Period FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Province-Period FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>F-statistic on IV</td>
<td>14.49</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean of Dep. Var.</td>
<td>0.183</td>
<td>0.183</td>
<td>0.113</td>
<td>0.113</td>
</tr>
<tr>
<td>Ext-Margin Elasticity</td>
<td>0.221</td>
<td>-0.584</td>
<td>0.181</td>
<td>-0.622</td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by zip code. For any $X$, $\Delta \ln(X)$ is the log difference between the average of $X$ in 2002-2008 and its average in 2009-2013. Vehicles p.c denotes the stock of vehicles per capita. F-statistic denotes the corresponding statistic for the Vehicles p.c covariates. Significance levels: ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Taken together, the results in Table 3 lead us to conclude that the vent-for-surplus mechanism did not appear to operate via the extensive margin. This result is perhaps not entirely surprising in light of the fact, discussed in Section 3.2, that more than 90% of the growth in Spanish exports during the bust period was explained by the intensive margin.

6 Robustness

In this section, we complement our baseline results with a series of robustness tests that further demonstrate the empirical relevance of the “vent-for-surplus” mechanism and that address some specific sources of endogeneity that could affect the validity of our identification strategy. Given the insignificant results obtained in Table 3 regarding the extensive margin of exports, we focus throughout this section on exploring the robustness of the intensive margin results in Table 2.

6.1 Further Purges of the Auto Industry

As explained in Section 4.2, while the sample used to compute the estimates presented in Table 2 excludes firms classified in the manufacturing of motor vehicles sector, one might still be concerned that the salient presence of firms in that industry in a given zip code might lead to a positive
Table 4: Intensive Margin: Robustness to Excluding Zip Codes Linked to Auto Industry

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Panel A: Exclude zipcodes w/ high auto employment share</th>
<th>Panel B: Exclude zipcodes with at least one sizeable auto maker</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>∆Ln(Domestic Sales)</td>
<td>-0.218***</td>
<td>-2.382***</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.535)</td>
</tr>
<tr>
<td>∆Ln(Vehicles p.c. in municipality)</td>
<td>0.328***</td>
<td>0.936***</td>
</tr>
<tr>
<td></td>
<td>(0.075)</td>
<td>(0.075)</td>
</tr>
<tr>
<td>∆Ln(TFP)</td>
<td>1.349***</td>
<td>0.936***</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.032)</td>
</tr>
<tr>
<td>∆Ln(Avg. Wages)</td>
<td>-0.487***</td>
<td>-0.436***</td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>F-statistic</td>
<td>-</td>
<td>19.04</td>
</tr>
<tr>
<td>Observations</td>
<td>7,178</td>
<td>7,178</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Panel C: Exclude zipcodes ‘neighboring’ zipcodes in Panel A</th>
<th>Panel D: Exclude sectors w/ I-O links to automakers</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>∆Ln(Domestic Sales)</td>
<td>-0.204***</td>
<td>-2.487***</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.632)</td>
</tr>
<tr>
<td>∆Ln(Vehicles p.c. in municipality)</td>
<td>0.332***</td>
<td>0.928***</td>
</tr>
<tr>
<td></td>
<td>(0.085)</td>
<td>(0.085)</td>
</tr>
<tr>
<td>∆Ln(TFP)</td>
<td>1.324***</td>
<td>0.928***</td>
</tr>
<tr>
<td></td>
<td>(0.062)</td>
<td>(0.032)</td>
</tr>
<tr>
<td>∆Ln(Avg. Wages)</td>
<td>-0.471***</td>
<td>-0.401***</td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>F-statistic</td>
<td>-</td>
<td>15.42</td>
</tr>
<tr>
<td>Observations</td>
<td>6,137</td>
<td>6,137</td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by municipality. All specifications include sector and province fixed effects. For any $X$, $\Delta \ln(X)$ is the log difference between the average of $X$ in 2002-2008 and its average in 2009-2013. Vehicles p.c denotes the stock of vehicles per capita. F-statistic denotes the corresponding statistic for the Vehicles p.c covariates. Significance levels: ***$p<0.01$, **$p<0.05$, *$p<0.1$.

correlation between unobserved residual supply shocks and our instrument. This would be the case if the boom-to-bust drop in the number of vehicles per capita was caused not by demand changes but by a negative supply shock to firms in the motor vehicles industry. Notice however that this source of endogeneity in our instrument would cause our two-stage least squares estimates to be downward biased, as unobserved residual supply shocks will also be positively correlated with firms’ exports. In order to evaluate the robustness of our estimates to this concern, in Table 4 we report the results of our two-stage least squares estimation for four alternative samples. In Panel A, we
exclude from our sample all firms located in a zip code that ranks in the top 25% of zip codes by share of manufacturing employment accounted for by motor-vehicles producers (as computed from our micro-level data). In Panel B, we further restrict the sample relative to Panel A by excluding all firms located in a zip code in which at least one motor-vehicles producer with more than 20 workers operates. In Panel C, we exclude all firms from ‘neighboring’ zip codes sharing the first four digits with a zip code that ranks in the top 25% of zip codes by share of manufacturing employment in motor-vehicles producers. Finally, in Panel D, we exclude all firms producing in sectors that are either one of the two top leading input providers or two top leading buying industries of the vehicles manufacturing industry. The results in all panels point at slightly larger estimated elasticities (in absolute value), consistently with the notion that these sample restrictions attenuate concerns about our estimates being up upward biased due to a positive correlation between our instrument and unobserved supply factors. The estimated elasticity in Panel B (−2.787) is particularly large, but notice that the associated standard error is such that we cannot reject the null hypothesis that this elasticity is equal to −1.6.

6.2 Heterogeneous Effects

Our first-stage and second-stage intensive margin specifications in Table 2 do not allow for plausible sources of heterogeneity in how firms are affected by local demand shocks and how they might respond to them. For instance, it seems reasonable to expect relatively small firms to see their domestic sales being more impacted by municipality-specific shocks than larger firms. This indicates that one might expect our first stage to perform better for the former set of firms than for the latter. Similarly, the possibility of the “vent-for-surplus” mechanism operating at the intranational level suggests that firms featuring a higher proclivity to sell only in Spain (and thus a lower propensity to export) might have a larger incentive to diversify their sales across municipalities and provinces within Spain. This would lead us to expect the first stage to perform worse for these inward-oriented firms. Furthermore, conditional on a common shock to a firm’s Spain-wide sales, it is natural to expect the percentage change in exports following a given percentage change in domestic sales to be larger for firms with initially lower export shares.\textsuperscript{33}

To formally explore these hypotheses, in Table 5, we replicate our baseline results for different subsamples of firms. More specifically, we first divide our sample into very small (less than 25 employees), small (25-50 employees) and large (50 or more employees) firms using information on their average number of employees during the years they are active, and we later divide firms according to their export propensity based on whether the export share for their sector and province is below or above the median export share in 2001 (prior to the beginning of our sample). To save space, in Table 5 we only report the key coefficients and regression diagnostics, but all specifications include sector and province fixed effects and our controls for firm-level TFP and wages.\textsuperscript{33}

\textsuperscript{33}To better understand this, consider a specific example: if a firm is attempting to recoup \(\varepsilon100,000\) in lost domestic sales, which constitute a 10% drop in domestic sales, the required percentage increase in exports will be larger if the firm initially exported \(\varepsilon111,000\) worth of goods (i.e., an initial trade share of 10%) than if it initially exported \(\varepsilon250,000\) (i.e., an initial trade share of 20%).
Table 5: Heterogeneous Effects

<table>
<thead>
<tr>
<th>Sample</th>
<th>Very Small Firms</th>
<th>Small Firms</th>
<th>Large Firms</th>
<th>Low prov/sec</th>
<th>X share</th>
<th>Low prov/sec</th>
<th>X share</th>
<th>Low capacity constraints</th>
<th>High capacity constraints</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>( L \leq 25 )</td>
<td>( (25, 50) )</td>
<td>( L &gt; 50 )</td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>OLS Elasticity</td>
<td>-0.201***</td>
<td>-0.282***</td>
<td>-0.175***</td>
<td>-0.239***</td>
<td>-0.162***</td>
<td>-0.230***</td>
<td>-0.190***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.054)</td>
<td>(0.045)</td>
<td>(0.040)</td>
<td>(0.037)</td>
<td>(0.042)</td>
<td>(0.039)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV Elasticity</td>
<td>-1.221***</td>
<td>-2.271**</td>
<td>-1.247**</td>
<td>-3.034**</td>
<td>-0.839*</td>
<td>-1.299*</td>
<td>-1.698***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.449)</td>
<td>(1.037)</td>
<td>(0.625)</td>
<td>(1.177)</td>
<td>(0.460)</td>
<td>(0.672)</td>
<td>(0.639)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1st Stage Coef.</td>
<td>0.520***</td>
<td>0.294***</td>
<td>0.331***</td>
<td>0.235***</td>
<td>0.477***</td>
<td>0.315***</td>
<td>0.393***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.117)</td>
<td>(0.113)</td>
<td>(0.095)</td>
<td>(0.073)</td>
<td>(0.095)</td>
<td>(0.094)</td>
<td>(0.087)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1st Stage F-Stat.</td>
<td>19.86</td>
<td>6.71</td>
<td>12.19</td>
<td>10.23</td>
<td>24.98</td>
<td>11.33</td>
<td>20.31</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>2,657</td>
<td>2,183</td>
<td>3,190</td>
<td>4,005</td>
<td>4,009</td>
<td>3,912</td>
<td>3,913</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by municipality. All specifications include sector and province fixed effects. Vehicles p.c. denotes the stock of vehicles per capita. First stage coefficient and F-statistic denote the corresponding statistic for the Vehicles p.c. covariate. Significance levels: ***\( p < 0.01 \), **\( p < 0.05 \), *\( p < 0.1 \).

Our results in Table 5 are broadly consistent with the heterogeneous effects we had hypothesized above. More specifically, our instrument is a stronger predictor of Spanish-wide sales for very small firms than for larger firms (coefficient of 0.520 versus 0.294 and 0.331), while it is weaker for “inward-oriented” firms (which might have had a more diversified portfolio of domestic destinations). Furthermore, the IV elasticity of changes in exports to demand-driven changes in domestic sales is larger for firms with a lower (pre-sample) propensity to export (elasticity of -3.034 versus -0.839 for firms with high export propensity).

In columns 6 and 7 of Table 5 we explore one additional source of heterogeneous effects. This test is motivated by the micro-foundation of our model with non-constant marginal costs developed in section 7.1 below (see, in particular, Appendix A.3). More specifically, this micro-foundation suggests that the elasticity of exports to changes in domestic sales should be larger, the more constrained by fixed factors is a firm’s production capacity. It is far from straightforward to distinguish between fixed and variable factors of production in the data, but a crude way to do so is to use information on the relative importance of capital versus labor in a firm’s production process. With that in mind, and building on our production function estimates underlying our proxy of firm-level productivity, we divide the sample into sectors with an above-median or below-median ratio of the output elasticity of capital relative to the output elasticity of labor (as dictated by the micro-foundation in Appendix A.3). Our results in columns 7 and 8 of Table 5 then confirm that the elasticity of the change in exports to changes in domestic sales is indeed higher for firms.

---

34 We have also re-run columns 4 and 5 while measuring a firm’s propensity to export directly via its pre-sample (2001) export share. The second-stage elasticity continues to be larger for firms with lower export shares but unlike in columns 4 and 5, in the first stage, we find that the instrument is a stronger predictor for firms with low export shares. It is plausible, however, that such a firm-level export share encapsulates many firm-level unobserved characteristics that might make the firm less prone to sell a large share of their output in their municipality. It is for this reason that, in Table 5, we measure inwardness at the sector and province-level, rather than at the firm level.
facing higher capacity constraints (or lower flexibility to adjust their capacity). The difference in the estimates is however not sizeable (-1.7 vs. -1.3) and not statistically significant given the limited precision of the estimates.\footnote{We obtain similar results when splitting the sample according to the reciprocal of the output elasticity of the flexible factor (labor), as dictated by the micro-foundation in Appendix A.3 in the presence of non-constant returns to scale.}

\section*{6.3 Alternative Instruments}

We next revert back to our baseline specifications with our full sample of firms but explore the robustness of our results to alternative instruments. Motivated again by the possibility that the vent-for-surplus mechanism might partly operate at the intranational level – with firms redirecting sales to less affected zip codes – we first construct two alternative instruments that account for changes in demand not only in the municipality of location of a firm but also in surrounding towns. The first alternative instrument is our benchmark one, namely the change in the stock of vehicles per capita, but computed at the province level rather than at the municipality level. The second alternative instrument is a distance- and population-weighted sum of the change in the stock of vehicles per capita in all zip codes surrounding the zip code in which the firm is located.\footnote{To construct our baseline measure, we weight each zip code by its population divided by the logarithm of its distance to the zip code where the corresponding firm is located. We have experimented with alternative weights, such as dividing population by power functions of distances, and have found qualitatively very similar results.}

We next construct a third alternative instrument that attempts to better capture the deep roots of the Great Recession in Spain. More precisely, we construct ratios of available ‘buildable’ urban land to urban land with already built structures in the year 1996 (a year sufficiently removed from the housing boom). We conjecture that this ratio is a proxy for the housing supply elasticity in a given municipality and that municipalities with lower housing supply elasticities should have experienced larger housing price increases during the boom years and, as a result, larger reductions in household wealth.\footnote{Indeed, in Appendix C we show that there is a negative cross-sectional correlation between these housing supply elasticities and housing price growth during the boom years 2004-07. Despite the fact that we view this instrument as an alternative proxy for the change in local demand, in Appendix C we show that there is in fact a negative correlation between these housing supply elasticities and our benchmark instrument for local demand, that is, the log change the average stock of cars per capita in a municipality between the boom and the bust. This negative correlation however turns mildly positive when weighting municipalities by the number of firms in our intensive margin regression.} A potential threat to the validity of this instrument is the fact that housing supply elasticities could also impact firm’s marginal costs by affecting the cost of non-residential structures (i.e., factories).\footnote{More specifically, municipalities with a lower housing supply elasticity might have experienced larger boom-to-bust reductions in the cost of land, which might have contributed to a larger relative export growth for firms located in those municipalities.}

Relatedly, we also experiment with a fourth alternative instrument directly related to the construction sector. As discussed in Section 3.1, the reduction in the supply of credit that caused the domestic slump affected directly the construction sector. As mentioned in footnote 20, the share of total employment in the construction sector peaked at 13.5\% in the summer of 2007 and then collapsed, reaching 5.4\% by early 2014. A larger share of the workers employed in the construction sector...
sector during the boom ended up unemployed during the bust period. These workers saw their consumption capacity severely reduced in the bust period relative to the boom. Consequently, one may conjecture that the boom-to-bust drop in demand for manufacturing products was larger in those zip codes for which the construction sector was a particularly important source of income during the boom years. Accordingly, we use the 2002 construction wage bill share in a zip code, interacted with the log change in national construction wage bill between the boom and the bust, as a determinant of the boom-to-bust changes in demand in the corresponding zip code.39

Our fifth and last alternative instrument is quite distinct in nature and is motivated by the importance of tourism revenue for the Spanish economy. As in the case of many other economic indicators, the number of foreign tourists visiting Spain peaked in 2007 at 58.66 millions visitors, before falling by more than 10% to 52.18 million and 52.68 million visitors in 2009 and 2010, respectively. Because tourism revenue accounts for roughly 10% of Spanish GDP, and because the decline in foreign visitors affected different regions in Spain differently, this generates an alternative source of geographical variation in local (province-level) demand. As in the case of the construction wage bill share, we use a 2002 province-specific measure of exposure to tourism shocks, interacted with the log change in tourists at the national level between the boom and the bust, as an instrument for the boom-to-bust changes in demand in the corresponding province. Our measure of exposure is in this case the number of foreign tourists that visited a province in 2002 divided by the population of the province in the same year.

In Table 6, we report the results obtained under these different alternative instruments. As column 1 demonstrates, the change in the stock of vehicles per capita in a given province continues to be a strong predictor for the change in the domestic sales of firms based in that province (with F-stats above standard critical values). Furthermore, the first-stage elasticity of domestic sales with respect to the province-level change in demand is more than twice as large as the one obtained when the instrument is constructed at the municipality level. This is consistent with the vent-for-surplus mechanism operating partly across municipalities within a province, but it is also highly suggestive that our instrument is capturing demand-driven changes in domestic sales rather than unobserved supply-driven changes, as explained in the Introduction. Despite these differences in the province-level first stage, our second-stage province-level results generate a response of exports to a fall in domestic (Spain-wide) sales very similar to that in our baseline (-1.425 vs. -1.602). Consequently, a test of overidentifying restrictions clearly fails to reject the null hypothesis that our instruments are valid.

The results we obtain under the distance- and population-weighted vehicles per capita instrument in columns 2 and 3 are equally reassuring. In particular, a firm’s domestic sales react significantly to this weighted instrument, even after controlling for the changes in the stock of vehicles in the firm’s municipality (which remains a strong predictor). Furthermore, the associated tests of

39The relevance and validity of our instrument does not depend on the fact that we multiply the zip code-specific 2002 construction wage bill share with the boom-to-bust log change in national construction wage bill, which is common to all observations in our regression. We introduce this shifter in the instrument for the sake of facilitating the interpretation of the point estimate of the coefficient on this instrumental variable in the first-stage regression.
Table 6: Alternative Instruments and Overidentification Tests

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ΔLn(Vehicles p.c. in Province)</td>
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<tr>
<td>(0.223)</td>
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<tr>
<td>ΔLn(Distance-Population weighted vehicles p.c. in other zip codes)</td>
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<td>0.184***</td>
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<tr>
<td>(0.028)</td>
<td>(0.040)</td>
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<tr>
<td>ΔLn(Vehicles p.c. in municipality)</td>
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</tr>
<tr>
<td>Ln(Urban Land Supply Ratio in 1996)</td>
<td>0.029**</td>
<td></td>
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<td></td>
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<td></td>
</tr>
<tr>
<td>(0.012)</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>ΔLn(construction wage bill) × 2002 wage bill share in municipality</td>
<td>0.331***</td>
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<td>(0.054)</td>
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<tr>
<td>ΔLn(foreign tourists) × 2002 foreign tourists p.c. in province</td>
<td>0.280***</td>
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<td>(0.098)</td>
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<tr>
<td>F-statistic</td>
<td>14.61</td>
<td>86.02</td>
<td>43.02</td>
<td>6.36</td>
<td>38.33</td>
<td>8.18</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>ΔLn(Exports)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ΔLn(Domestic Sales)</td>
<td>-1.425***</td>
<td>-1.628***</td>
<td>-1.336***</td>
<td>-1.595*</td>
<td>-1.568***</td>
<td>-1.179***</td>
</tr>
<tr>
<td>(0.400)</td>
<td>(0.527)</td>
<td>(0.395)</td>
<td>(0.927)</td>
<td>(0.535)</td>
<td>(0.257)</td>
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</tr>
<tr>
<td>p-value for Sargan Overid test</td>
<td>0.46</td>
<td>0.34</td>
<td>0.34</td>
<td>0.51</td>
<td>0.99</td>
<td>0.97</td>
</tr>
<tr>
<td>Observations</td>
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<td>7,949</td>
<td>7,949</td>
<td>6,940</td>
<td>7,928</td>
<td>8,018</td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by province except for columns 4 and 5, where they are clustered by municipality. All specifications include firm-level log TFP and log wages as additional controls (coefficients not included to save space). All specifications include sector fixed effects, and column 5 also includes province fixed effects. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

the overidentification restrictions do not reject the validity of our various instruments.

One might argue that the results in columns 1, 2 and 3 of Table 6 are not too surprising since these alternative instruments use a source of variation that is quite similar to that used in our baseline specification. In that respect, the results we obtain when using as instruments our observed housing supply determinant, the 2002 zip code-level construction wage bill or the 2002 tourism share are perhaps more revealing. Despite the fact that the first-stage F-test statistics associated with two of these instruments are below the critical value of 10, the second-stage elasticities of exports to domestic sales are significant and quite similar in value (−1.595, −1.568, and −1.179, respectively) to those obtained with our benchmark instrumentation strategy in Table 2. Furthermore, the p-values associated with a Sargan test of the overidentification restrictions are very large (0.51, 0.99 and 0.97, respectively). In sum, these results enhance our confidence in the existence of a causal relationship between domestic sales shocks and exports with an elasticity roughly equal to −1.6.
6.4 Confounding Factors

In spite of the controls included in our baseline specifications, one may still be concerned that these specifications might not be accounting for the effect of confounding supply factors that could be correlated with our instrument, thus biasing our estimates.

Conceptually, it is useful to distinguish between two types of confounding factors. First, one might be concerned with the fact that our average wages and TFP proxies are too crude to fully capture changes in firm-level supply conditions even when additionally controlling for sector and province fixed effects. For instance, the dual nature of the Spanish labor market, with large differences in pay and job security between temporary- and permanent-contract workers, might have led certain firms to shed a disproportionate number of temporary, lower-paid workers during the bust. If so, changes in our measure of average wages could significantly underestimate the export potential of firms undergoing such skill-upgrading (or at least, experience-upgrading). Similarly, our baseline specifications do not include any proxies for factor costs other than labor costs, yet it is likely that financial costs faced by firms (explicit via interest rates or implicit via rationing) were also significantly impacted by the Great Recession. Additionally, and as already mentioned in Section 5.1, our baseline measure of firm-level TFP features a sizable positive correlation with firm-level sales, which potentially complicates the interpretation of our results (though we should emphasize that this is a correlation that one should expect in light of our model).

With this background in mind, Table 7 presents variants of our baseline specification that include controls for various alternative confounding factors related to the labor costs. We include these additional controls in both the first and second stage, but to save space we only report the second-stage results and the F-statistic associated with the first stage. We begin in column 1 by replicating our baseline second-stage results in column 8 of Table 2. In column 2, we control for the change in the ratio of temporary workers over total employment at the firm level. The results suggest that firms that shed a disproportionate number of temporary workers during the bust period experienced a larger increase in exports, which is in line with our hypothesis above. Notice, however, that the IV estimate of the causal effect of demand shocks on exporting is only slightly lowered (elasticity of −1.443).\textsuperscript{40} In columns 3, 4 and 5, we introduce municipality-level controls for local labor market conditions. Column 3 includes the same change in the ratio of temporary workers over total employment in column 2, but computed with aggregate data at the municipality level, while in columns 4 and 5 we further experiment with municipality-level measures of average wages and of the change in the manufacturing employment per capita. The inclusion of these three controls has a negligible impact on the main coefficients of interest, and only the last of these variables has a significant effect on exporting.\textsuperscript{41}

\textsuperscript{40}We obtain very similar results when repeating this exercise but instead controlling for the (initial) share of temporary workers during the boom period. More specifically, firms that entered the bust with a larger share of temporary workers (and thus a larger potential to affect their skill composition when transitioning to the bust period) experienced higher export growth in the boom relative to the bust, but the causal impact of domestic shocks on exports is largely unaffected.

\textsuperscript{41}More specifically, firms located in municipalities with larger declines in manufacturing per capita experienced higher export growth, presumably due to reductions in labor costs not captured well by our firm-level measures of
In Table 8, we turn to studying potential confounding effects related to financial costs. We construct a measure of the financial costs that each firm faces in each period as the within-period average ratio of financial expenditures over total outstanding debt with financial institutions (both measures are yearly reported by firms in their financial statements). As the results in column 2 of Table 8 illustrate, the impact of this measure of financial costs on firms’ changes in exports is not statistically different from zero. Including this variable has a slight effect on the estimate of the elasticity of exports to domestic sales (which drops to $-1.457$). In column 3, we explore the possibility that the relevant increase in the financial costs faced by firms in the bust relative to the boom happened through credit rationing, instead of via explicit interest rates. Although we do not have measures of firms’ credit applications and whether these were denied, one may conjecture that firms whose financial costs were larger in the boom were more likely to suffer credit rationing in the bust. This hypothesis finds little support in the results presented in column 3 of Table 8: firms with higher financial costs in the boom did not experience lower export growth in the bust relative to the boom. Columns 4 and 5 present analogous measures at the municipality level, to control for potential local financial market effects. The results again reveal no statistically significant effect of these measures of financial costs, and the effect of controlling for them on the estimated elasticity of the boom-to-bust changes in exports with respect to changes in domestic sales is minimal and not statistically significant.

We finally test the robustness of our results to alternative approaches to measuring firms’ productivity. Specifically, columns 1 and 2 in Table 9 contain our baseline OLS and IV estimates, and labor costs.

Table 7: Confounding Factors: Labor Markets

<table>
<thead>
<tr>
<th>Dependent Variable: ΔLn(Exports)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ΔLn(Domestic Sales)</td>
<td>-1.602***</td>
<td>-1.443***</td>
<td>-1.655***</td>
<td>-1.657***</td>
<td>-1.677***</td>
</tr>
<tr>
<td></td>
<td>(0.437)</td>
<td>(0.434)</td>
<td>(0.480)</td>
<td>(0.484)</td>
<td>(0.478)</td>
</tr>
<tr>
<td>ΔShare of Temporary Workers (firm-level)</td>
<td>-0.302**</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.118)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ΔShare of Temporary Workers (municipality-level)</td>
<td>-0.068</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.194)</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>ΔManufacturing Average Wages (municipality-level)</td>
<td>0.083</td>
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<tr>
<td></td>
<td>(0.140)</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>ΔManufacturing Employment p.c. (municipality-level)</td>
<td></td>
<td>-0.266***</td>
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</tr>
<tr>
<td></td>
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<td>(0.057)</td>
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<tr>
<td>F-Statistic</td>
<td>28.32</td>
<td>25.83</td>
<td>25.34</td>
<td>25.16</td>
<td>24.82</td>
</tr>
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<td>Observations</td>
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<td>7,649</td>
<td>7,746</td>
<td>7,748</td>
<td>7,748</td>
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</tbody>
</table>

Note: Standard errors clustered by municipality. All specifications include firm-level log TFP and log wages as additional controls. These coefficients are not included to save space. All specifications also include sector and province fixed effects. Significance levels: ***p<0.01, **p<0.05, *p<0.1.
Table 8: Confounding Factors: Financial Costs

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>ΔLn(Exports)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>ΔLn(Domestic Sales)</td>
<td>-1.602***</td>
</tr>
<tr>
<td></td>
<td>(0.437)</td>
</tr>
<tr>
<td>ΔLn(Financial Costs) at firm level</td>
<td>-0.024</td>
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<tr>
<td></td>
<td>(0.015)</td>
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<tr>
<td>Financial Costs in Boom at firm level</td>
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<td>(0.015)</td>
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<td>ΔLn(Financial Costs) at municipality level</td>
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<td>(0.032)</td>
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<td>Financial Costs in Boom at municipality level</td>
<td>0.023</td>
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<td>(0.035)</td>
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</table>

F-Statistic 28.32 23.22 23.90 24.70 25.26
Observations 8,018 6,924 6,994 7,742 7,743

Note: Standard errors clustered by municipality. All specifications include firm-level log TFP and log wages as additional controls. These coefficients are not included to save space. All specifications also include sector and province fixed effects. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Columns 3 and 4 present analogous estimates using an alternative productivity measure. Consistently with the model described in Section 2, both estimation approaches exploit the assumptions that firms: (a) face a CES demand function; (b) are monopolistically competitive in all output markets and take all factor prices as given; (c) determine their optimal labor usage in every period by maximizing their static profits conditional on a pre-determined stock of capital.

The two approaches we implement differ on the assumptions imposed on the shape of the production function. In our baseline specification, we assume that the firm’s production function is a Leontief aggregator of materials and a translog function of labor and capital (as in Ackerberg et al., 2015). Given these assumptions, we describe our estimation procedure in detail in Appendix B.5. A possible concern with this estimation approach is that, if it were to be the case that materials are not perfect complements with the (translog) output of labor and capital, our measure of the firm’s productivity would automatically incorporate a measure of the firm’s materials’ usage. To address this possible concern, the second approach assumes instead that the production function is a Cobb-Douglas aggregator of materials and the same translog function of labor and capital employed in our baseline approach (as in Bilir and Morales, 2018). Thus, while our baseline approach imposes that material inputs have a zero elasticity of substitution with the output of labor and capital, the second approach imposes instead a unit elasticity of substitution.

In both estimation approaches, we invoke optimality conditions for the static inputs (i.e. labor and materials) in order to estimate the relevant parameters of the production function and, in this sense, we implement an estimation approach à la Gandhi et al. (2016). Both estimation approaches do however use different outcome measures; while the first estimation approach exploits data on the firm’s sales revenue, the second one relies on information on the firm’s value added.
A general concern with our productivity estimates is that, they do not correctly account for the impact of different factors of production on the firm’s total sales, they may just become an imperfect proxy of these total sales, which would cause our estimate of the elasticity of exports with respect to demand-driven changes in domestic sales to be biased downwards. We should however point out that our measures of productivity are far from being perfectly correlated with the firm’s total sales; specifically, this correlation is 0.55 for our baseline approach and 0.23 for our alternative approach. The higher correlation of our baseline approach is consistent with it partly accounting for the firm’s usage of materials inputs.

A second concern with our productivity estimates is that, as we do not observe separately prices and quantities for each firm, they may capture no only the firm’s actual productivity but also the firm’s demand shifter. Specifically, this would be a concern if these productivity estimates were implicitly already controlling for the impact of our instrument. We should however point out that the correlation between the boom-to-bust change in the number of vehicles per capita in the municipality of location of the firm and our measures of productivity is very close to zero; specifically, this correlation is -0.04 for our baseline approach and -0.11 for our alternative approach.

Perhaps reflecting the lower correlation between our productivity proxy and firm-level sales associated with the alternative measure of productivity, the OLS estimator in column 3 indicates a positive partial correlation between exports and domestic sales. However, the IV elasticity in column 4 is again negative, and though it suggests a slightly lower elasticity of exports to reductions in domestic sales, we cannot reject the hypothesis that this elasticity is equal to the baseline estimate of $-1.602$.
6.5 Placebo Tests of First Stage

In order to further solidify the confidence in our results, we next present results of two related falsification tests. These tests are based on the idea that our identification strategy should only work when the change in the stock of vehicles per capita truly reflects a change in local demand. In particular, one could be concerned that our instrument reflects underlying local trends in economic conditions that impact the firms’ domestic sales differentially across different zip codes in Spain. To evaluate the plausibility of this concern, we present results in Table 10 that evaluate the presence of these pre-trends. In column 1, we break the boom period into two subperiods, 2002-04 and 2005-07, and evaluate whether our instrument (changes in demand between the boom and the bust periods) predicts the changes in domestic sales across these two subperiods. In column 3, we perform a similar exercise but with a dependent variable that measures changes in domestic sales between two subperiods included in the bust period, 2009-11 and 2012-13. If the correlation between changes in domestic sales and changes in the stock of vehicles per capita documented in the first four columns of Table 2 was exclusively due to underlying trends, the lack of synchronization between the time frames at which the endogenous variable and the instrument are measured should not affect the capacity of the latter to predict the former. However, as the results in columns 1 and 3 of Table 10 illustrate, the effect of our instrument in these placebo exercises is not statistically different from zero (the statistic of the F-test is below 0.3 in both cases).

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>ΔLn(Domestic Sales)</th>
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<tbody>
<tr>
<td>Sample:</td>
<td>Boom firms</td>
</tr>
<tr>
<td></td>
<td>Within Boom</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td></td>
<td>Boom vs. Bust</td>
</tr>
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</tr>
<tr>
<td>ΔLn(Vehicles p.c. in municipality)</td>
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<td>(0.080)</td>
<td>(0.074)</td>
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<tr>
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<tr>
<td>F-statistic</td>
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</tbody>
</table>

Note: Standard errors clustered by municipality. All specifications include firm-level log TFP and log wages as additional controls. These coefficients are not included to save space. All specifications also include sector and province fixed effects. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

A possible concern with the interpretation of these placebo results is that they might be driven by the fact that the sample of firms in each of these placebo tests is smaller than in our baseline regressions. With that in mind, in columns 2 and 4 of Table 10 we repeat our baseline specifications comparing the boom (2002-08) to the bust (2009-13) for both the change in domestic sales and in the instrument, but for the same sample of firms used to compute the estimates reported in columns 1 and 3, respectively. In both cases, we continue to find a positive and statistically significant effect of the instrument on domestic sales, but the effect is admittedly much stronger in column 4 (with
an $F$-stat of 15.63) than in column 2 (with an $F$-stat of 6.19).

### 6.6 Additional Robustness Tests

We finally briefly comment on a number of additional robustness tests. To save space we relegate the regression tables to Appendix D. First, in Table D.1 we show that our results are not materially affected when excluding multinational firms from our sample, when weighting observations by the number of years a firm is active in export markets, or when defining the bust period as 2010-2013 or 2011-13 instead of 2009-13. Second, in Table D.2 we experiment with additional variants of our instruments in Tables 2 and 6. More specifically, holding municipality-level population constant at its 2002 level when computing vehicles per capita has a negligible effect on our estimates. We also show that vehicles per capita both at the municipality and at the province-level remain significant when including them simultaneously in the first stage, and that our construction-sector instrument delivers similar results when it is based on employment shares or turnover shares, rather than on wage bill shares.

### 7 Structural Interpretation and Quantification

There is an obvious tension between our empirical results suggesting an interdependence between domestic sales and exports, and the theoretical framework we developed in Section 2 to motivate and organize our empirical analysis. In this section, we demonstrate how a simple extension of that framework incorporating non-constant marginal costs delivers insights consistent with our empirical results, and in addition permits a structural interpretation of these results. Armed with this structural interpretation, we close this section by providing a quantitative assessment of the importance of the domestic slump for the observed export miracle in Spain during the period 2009-13.

#### 7.1 Structural Interpretation

The theoretical environment we consider is identical to that in Section 2, except that the cost structure in equation (2) is now replaced with a total variable cost of producing $Q_{id}$ units of output for the domestic market and $Q_{ix}$ units of output for the foreign market given by

$$\frac{1}{\bar{\varphi}_i} \omega_i \frac{1}{\lambda + 1} (\tau_d Q_{id} + \tau_x Q_{ix})^{\lambda+1}, \quad \lambda \geq 0. \quad (14)$$

Notice that the parameter $\lambda$ governs how quickly marginal costs increase with output. When $\lambda = 0$, marginal costs are constant and equation (14) reduces to our previous expression in equation (2). In Appendix A.3, we develop a micro-foundation for the cost function in (14) in a model in which a firm’s production capacity is limited by a fixed factor in the short run. Under a Cobb-Douglas production function in fixed and variable factors featuring constant returns to scale, the parameter $\lambda$ turns out to simply be the ratio of the output elasticity of the fixed factor variable to
the output elasticity of the variable factors.

Solving for the optimal level of exports by firm $i$, and taking log differences, now delivers

$$\Delta \ln R_{ix} = (\sigma - 1) [\Delta \ln \xi_{ix} + \Delta \ln \varphi_i - \Delta \ln \omega_i] - (\sigma - 1) (\Delta \ln \tau_x - \Delta \ln P_{sx}) + \Delta \ln E_{sx}$$

$$- (\sigma - 1) \lambda \Delta \ln (\tau_d Q_{id} + \tau_x Q_{ix})$$, \hspace{1cm} (15)

which is analogous to equation (4) except for the last term reflecting the positive effect of output on the marginal cost of production. Next note that due to constant mark-up pricing, we can write

$$\ln (\tau_d Q_{id} + \tau_x Q_{ix}) = \ln (\tau_d R_{id} P_{id} + \tau_x R_{ix} P_{ix}) = \ln (\tau_d R_{id} + \tau_x R_{ix}) - \ln (\sigma \omega_i (\tau_d Q_{id} + \tau_x Q_{ix})^{\lambda}) \left(\frac{\omega_i}{(\sigma - 1) \varphi_i}\right).$$ \hspace{1cm} (16)

Solving for $\ln (\tau_d Q_{id} + \tau_x Q_{ix})$, plugging this expression into equation (15), and imposing the same decomposition as in equation (5), we then find:

$$\Delta \ln R_{ix} = \gamma_{sx} + \gamma_{tx} + \frac{(\sigma - 1)}{1 + \lambda} \delta \varphi \Delta \ln (\varphi_i) + \frac{(\sigma - 1)}{1 + \lambda} \delta \omega \Delta \ln (\omega_i) - \frac{(\sigma - 1) \lambda}{1 + \lambda} \Delta \ln (R_{id} + R_{ix}) = \varepsilon_{ix}. \hspace{1cm} (17)$$

Note that this equation is analogous to the estimating equation (10) suggested in Section 2, except that it features the log difference of total sales (and not just domestic sales) on the right hand side, and that the true value of the coefficient in front of this variable is not 0, but rather a negative value (as long as $\lambda > 0$). The intuition for the need to include the change in total sales rather than in domestic sales as an explanatory variable is straightforward: marginal costs of production are increasing in total output, not just output destined for the domestic market.

Estimating equation (17) via OLS is problematic not just for the reasons identified in section 2 but also because the inclusion of the log change in exports as part of one of the explanatory variables generates an additional mechanical upward bias when estimating $(\sigma - 1) \lambda / (1 + \lambda)$. Note, however, that our instrumental variable approach will continue to deliver consistent estimates of this coefficient provided that the only way that the instrument affects exporting is by affecting a firm’s change in domestic sales and not by affecting exporting directly. In other words, as long as the instrument satisfies the exclusion restriction, estimating equation (17) via our instrumental variable approach should deliver consistent estimates of $(\sigma - 1) \lambda / (1 + \lambda)$.

In Table 11, we present OLS and two-stage least squares estimates of equation (17). As expected, the OLS estimates in column 1 are severely biased upwards and indicate a strong positive correlation between exports and total sales, even when including all the controls and various fixed effects. The first stage results in column 2 indicate that our baseline instrument is a strong predictor of a firm’s total sales (not just its domestic sales), with an F-stat of 23.42. Finally, the second-stage elasticity of exports to total sales in column 3 is negative and significant and stands at a value of $-2.624$. To understand the magnitude of this elasticity, take a firm with an initial export share of 16.5% (which corresponds to median export share during the boom in our sample of
8,018 continuing exporters). Suppose this firm experiences a 10% drop in its sales. Our estimated elasticity of exports to domestic sales in Table 2 indicates that, other things equal, the firm should see its exports increase by 16%. Now this also implies that the firm’s total sales will decrease by $83.5\% \times 10\% + 16.5\% \times (-16\%) = 5.71\%$. Our estimated elasticity of exports to total sales in Table 11 then suggests an implied increase in exports of $5.71\% \times 2.624 = 14.98\%$, which is quite close to 16%. This demonstrates that our IV results in Tables 2 and 11 deliver congruent estimates for the response of exports to local demand shocks.\footnote{In Table D.3 of Appendix D, we show that we obtain similar results when running equation (17) with our alternative instruments in Table 6. The second-stage elasticity of exports to total sales ranges from $-1.971$ to $-3.120$. We have also re-estimated equation (17) for the sub-samples in Panels A through D in Table 4, and consistently with the results in that table, the purging of observations potentially polluted by supply shocks leads to an increase in the absolute value of the estimated elasticity (details available upon request).}

Notice also that with an estimate $\sigma$ of the demand elasticity in hand, it is easy to infer an estimated value of $\lambda$ from our estimates, as $\hat{\lambda} = 2.624 / (\sigma - 2.624)$. For $\sigma = 6$ and $\sigma = 5$, we obtain $\hat{\lambda} = 0.77$, and $\hat{\lambda} = 1.10$, respectively, which in both cases indicates a significant departure from constant marginal costs.

### 7.2 Quantification

In this final section, we attempt to evaluate the quantitative importance of the “vent-for-surplus” channel for explaining the remarkable growth in Spanish exports during the period 2009-13. Ideally, we would first seek to isolate the contribution of demand factors in explaining the contraction in domestic sales experienced by Spanish firms around the Great Recession, and we would then use our estimates in Table 11 to trace the implications of these demand-driven changes in domestic sales.
for export flows. A key challenge we face, however, is that separately identifying the demand and supply shocks that jointly shaped the evolution of firms’ domestic sales during the Great Recession would require observing firm-specific prices, which are absent in our data. For this reason, we take a more agnostic approach and focus on illustrating the quantitative importance of the vent-for-surplus mechanism for various possible contributions of demand factors to the domestic slump.

Specifically, we explore the impact of boom-to-bust counterfactual changes in the firm-specific total demand shifter $\Xi_{id} \equiv \xi_{id}^{-1}Q_d$, where $Q_d \equiv E_{sd}/P_{sd}$ is an aggregate demand shifter common to all firms (in a sector), and where $\xi_{id}$ is an idiosyncratic demand shifter. In doing so, we maintain the boom-to-bust changes in all other exogenous demand and supply shocks – the changes in foreign total demand $\Xi_{ix}$, and the changes in all supply parameters ($\varphi_i, \omega_i, \tau_{sx}, \tau_{sd}$) – to their actual values. With this exercise, we aim to quantify the impact that domestic demand shocks had on export flows through the increasing marginal cost function that firms faced. Given our results regarding the extensive margin of trade in Table 3, we assume that firms do not change their export status in reaction to our counterfactual change in the domestic demand shifter $\Xi_{id}$, and thus focus on the response on continuing exporters.

In order to perform our counterfactuals, the two key equations from our model are:

\[
\Delta \ln R_{ix} = \Delta \ln \Xi_{ix} + \frac{(\sigma - 1)}{1 + \lambda} [\Delta \ln \varphi_i - \Delta \ln \omega_i] - (\sigma - 1) \Delta \ln \tau_{sx} + \sigma \Delta \ln P_{sx} \\
- \frac{(\sigma - 1) \lambda}{1 + \lambda} \Delta \ln (R_i)
\]

\[
\Delta \ln R_{id} = \Delta \ln \Xi_{id} + \frac{(\sigma - 1)}{1 + \lambda} [\Delta \ln \varphi_i - \Delta \ln \omega_i] - (\sigma - 1) \Delta \ln \tau_{sd} + \sigma \Delta \ln P_{sd} \\
- \frac{(\sigma - 1) \lambda}{1 + \lambda} \Delta \ln (R_i),
\]

where remember that $\Delta \ln (R_i) = \Delta \ln (R_{ix} + R_{id})$. Equation (18a) is implied by equations (15) and (16), and the definition of $\Xi_{id}$. Equation (18b) is analogous.

For any variable $x$, we define as $\Delta \ln x'$ the counterfactual log change in $x$ that would have taken place between the boom and the bust if the change in the demand shifters $\{\Xi_{idt}\}_t$ between these two periods were equal to $\{\Delta \ln \Xi_{id}'\}_t$ and all other demand and supply shocks had changed as they actually did. Therefore, analogously to equations (18a) and (18b), we can define the following two equations

\[
\Delta \ln R_{ix}' = \Delta \ln \Xi_{ix}' + \frac{(\sigma - 1)}{1 + \lambda} [\Delta \ln \varphi_i - \Delta \ln \omega_i] - (\sigma - 1) \Delta \ln \tau_{sx} + \sigma \Delta \ln P_{sx} \\
- \frac{(\sigma - 1) \lambda}{1 + \lambda} \Delta \ln (R_i')
\]

\[
\Delta \ln R_{id}' = \Delta \ln \Xi_{id}' + \frac{(\sigma - 1)}{1 + \lambda} [\Delta \ln \varphi_i - \Delta \ln \omega_i] - (\sigma - 1) \Delta \ln \tau_{sd} + \sigma \Delta \ln P_{sd}' \\
- \frac{(\sigma - 1) \lambda}{1 + \lambda} \Delta \ln (R_i').
\]
Note that equations (19a) and (19b) allow us to compute the impact of counterfactual demand shocks \( \{ \Delta \ln \Xi_{id}' \} \), on firm’s domestic sales and exports while holding the changes in the foreign price index, \( P_x \), and in the equilibrium wages that each firm \( i \) faces, \( \omega_i \), unaltered by the counterfactual. In the case of non-exporting firms, only equation (19b) applies for all these firms.

The assumption that the foreign price index is not affected by aggregate demand shocks in Spain is consistent with Spain being a small country relative to the foreign one. Domestic equilibrium wages would generally be affected by counterfactual changes in the aggregate demand term \( E_{sd} \). Holding them at their observed path is however consistent with our aim of identifying the effect of aggregate demand shocks on exports working exclusively through the vent-for-surplus channel. In other words, by omitting from our analysis the impact that changes in aggregate demand have on equilibrium wages, we focus on the impact that demand has on firms’ marginal production costs whenever these firms move along their marginal cost curve, but without accounting for any shift in this curve.

From equation (19b), it is easy to see that the counterfactual change in firm \( i \)’s domestic sales, \( \Delta \ln R_{id}' \), is a function of the actual changes in its own supply shocks, \( (\Delta \ln \varphi_i, \Delta \ln \omega_i, \Delta \ln \tau_{sx}, \Delta \ln \tau_{sd}) \), and, through the counterfactual change in the domestic price index, \( \Delta \ln P_{sd}' \), of the actual changes in all other firms supply shocks and their counterfactual demand changes. However, it also easy to see that, for any possible realization of the supply shocks of every firm, choosing a vector of counterfactual domestic demand changes \( \{ \Delta \ln \Xi_{id}' \} \) is equivalent to choosing a vector of counterfactual domestic revenues changes \( \{ \Delta \ln R_{id}' \} \). Consequently, we will focus on computing the impact on the vector of counterfactual export sales \( \{ \Delta \ln R_{ix}' \} \) of a counterfactual change in the vector of domestic revenues \( \{ \Delta \ln R_{id}' \} \), which we interpret as generated by the combination of a counterfactual change in the vector of domestic demand changes \( \{ \Delta \ln \Xi_{id}' \} \) and actual changes in the supply shocks \( (\Delta \ln \varphi_i, \Delta \ln \omega_i, \Delta \ln \tau_{sx}, \Delta \ln \tau_{sd}) \).

Using equation (18a), we can simplify equation (19a) to

\[
\Delta \ln R_{ix}' = \Delta \ln R_{ix} - \frac{(\sigma - 1)\lambda}{1 + \lambda} \left( \Delta \ln (R_i') - \Delta \ln (R_i) \right). \tag{20}
\]

Denoting the average value of a variable in the boom and in the bust with a subscript 0 and 1, respectively, we can further rewrite equation (20) as

\[
\ln \left( \frac{R_{ix1}}{R_{ix0}} \right) = \ln \left( \frac{R_{ix1}}{R_{ix0}} \right) - \frac{(\sigma - 1)\lambda}{1 + \lambda} \left( \Delta \ln \left( \chi_{i0} \frac{R_{ix1}}{R_{ix0}} + (1 - \chi_{i0}) \frac{R_{id1}}{R_{id0}} \right) - \Delta \ln \left( \frac{R_{i1}}{R_{i0}} \right) \right), \tag{21}
\]

where \( \chi_{i0} \) is firm \( i \)'s export share in the boom period. Given a point estimate of \( ((\sigma - 1)\lambda)/(1 + \lambda) \) and observed data on \( \chi_{i0}, R_{ix1}/R_{ix0} \) and \( R_{i1}/R_{i0} \), equation (21) allows us us to compute the counterfactual change in exports of firm \( i \), \( R_{ix1}'/R_{ix0}' \), generated by a counterfactual change in the domestic sales of this firm \( R_{id1}'/R_{id0}' \). Furthermore, as shown in column 3 of Table 11, the number 2.624 is a consistent estimate of the term \( ((\sigma - 1)\lambda)/(1 + \lambda) \).

Because we cannot isolate a precise contribution of demand factors to the observed fall in domes-
tic sales of firms, we implement a range of counterfactuals for which \( R'_{id1} = R_{id0} + (1 - \Gamma) (R_{id1} - R_{id0}) \) or, equivalently, \( R'_{id1}/R_{id0} = 1 + (1 - \Gamma) (R_{id1}/R_{id0} - 1) \), for several different values of the scalar \( \Gamma \in [0, 1] \). Note that the counterfactual in which \( \Gamma = 1 \) corresponds to the case in which we interpret the domestic slump as being purely demand-driven. A counterfactual that restores demand factors to their boom period level would thus fully eliminate the domestic slump. More precisely, when \( \Gamma = 1 \), the change in the demand shifters of every firm, \( \{ \Delta \ln \Xi_{id} \}_i \) is assumed to be such that every firm’s domestic sales in the bust are equal to their domestic sales in the boom; i.e., for every firm \( i \), \( R'_{id1}/R_{id0} = 1 \). Conversely, the counterfactual in which \( \Gamma = 0 \) corresponds to the case in which demand factors played no role in the domestic slump, and thus this counterfactual leaves the change in domestic sales at the exact same value as that observed in the data.

![Figure 6: Impact of “Vent-for-Surplus” on Representative Firm](image)

We first present results of these various counterfactuals for a representative firm \( i \) whose domestic sales and exports in both the boom and bust correspond to the Spanish average of these variables in each time period. The results appear in Figure 6. For this average firm, its domestic sales dropped 13% between the boom and the bust periods and exports grew 11.6%. These are the values corresponding to \( \Gamma = 0 \) in Figure 6 because if the domestic slump were to have been entirely supply driven, then our vent-for-surplus mechanism would not have been operative. A more reasonable scenario, however, is one in which demand and supply factors explained an equal share of the domestic slump. This corresponds to \( \Gamma = 0.5 \) and leads to a counterfactual scenario in which the change in demand of this representative firm is such that its boom-to-bust drop in domestic sales is half of the actual observed drop (i.e., a fall of 6.5% instead of 13%), while all supply factors and export demand follow the same path as observed in the data. In such a case, we find that this firm’s export growth would have shrunk to 3.15% instead of the 11.6% observed.
export growth in the data. This implies that our vent-for-surplus mechanism explains about 73% of the observed export growth during this period. Obviously, for larger values of $\Gamma$, the impact on the growth in exports is larger and counterfactual export growth actually becomes negative for sufficiently large values of $\Gamma$.

We can also use equation (21) to explore the contribution of the vent-for-surplus for explaining export growth for different types of firms. In Table 12, we divide all our continuing exporters into four groups depending on their size and, for each group, we construct a representative firm whose domestic sales and exports in both the boom and bust are equal to the average of the corresponding variable in each time period for the corresponding group of firms. Specifically, each group corresponds to a quartile of the distribution of yearly average domestic sales over the whole sample period. We denote these four representative firms as small, small-medium, medium-large, and large. Table 12 shows that domestic sales dropped for all four representative firms, while exports grew both for the smallest and for the largest, but dropped for the two groups in which we split the medium-sized firms. Our counterfactual results show that, were domestic sales to have dropped by 50% of their actual drop due to a different realization of demand shocks (i.e., $\Gamma = 0.5$), the percentage change in exports of small, small-medium, medium-large and large firms would have been 2.1, 6.9, 5.4 and 7.1 percentage points smaller, respectively. Consequently, the vent-for-surplus mechanism has a much smaller effect for the very small exporters, but a comparable impact for all remaining exporting firms. The same result applies for alternative values of $\Gamma$. The reason for the smaller contribution of our key mechanism for the very small exporters is twofold. On the one hand, this set of firms experienced a lower fall in domestic sales than larger firms. On the other hand, their export share is significantly lower than that of larger firms, and again this attenuates the impact of a reduction in domestic sales on these firms’ marginal costs.

Table 12: Counterfactual Results by Firm Size

<table>
<thead>
<tr>
<th>Actual Growth in Dom. Sales</th>
<th>Export Share in Boom</th>
<th>Counterfactual Export Growth if $\Gamma$ is:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>10%</td>
</tr>
<tr>
<td>Small</td>
<td>−6.9%</td>
<td>71.9%</td>
</tr>
<tr>
<td>Small-Medium</td>
<td>−14.7%</td>
<td>−3.0%</td>
</tr>
<tr>
<td>Medium-Large</td>
<td>−12.4%</td>
<td>−17.4%</td>
</tr>
<tr>
<td>Large</td>
<td>−13.2%</td>
<td>13.8%</td>
</tr>
</tbody>
</table>

8 Conclusion

In this paper, we provide evidence suggesting that export and domestic sale decisions are interdependent at the firm level. Faced with a severe domestic slump during the Great Recession, Spanish producers appear to have used their freed capacity as a motivation to seek new sources of demand
in foreign markets. We circumvent the inherent difficulties associated with establishing a causal link between exports and domestic sales by exploiting geographic variation in the incidence of the Great Recession in Spain.

Our empirical findings are inconsistent with international trade models featuring technologies with constant marginal costs of production. We however rationalize and interpret our results through the lens of a model with increasing marginal costs, and show that the “vent-for-surplus” mechanism is powerful enough to explain the growth in Spanish exports in the period 2009-13.

Due to data limitations, we have restricted our analysis to the optimal allocation of productive capacity to the domestic market and to a single (aggregate) export destination. In future work, we plan to expand the analysis to a multi-country environment featuring a rich set of extensive margin decisions. The interdependencies studied in this paper will naturally carry over to that environment, thereby complicating the estimation of some key parameters of multi-country export models, such as country-specific fixed costs of exporting. Borrowing tools from the work of Antrás et al. (2017) and Arkolakis and Eckert (2017), we hope to surmount these complications and demonstrate the role of increasing marginal costs in shaping the response of firms to shocks to the world economy.
References


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A Theoretical Appendix

A.1 Biases Due to Measurement Error

We discuss here the implications of measurement error in both total sales and exports whenever domestic sales are computed by subtracting exports from the total sales of firms (see also, Berman et al., 2015). Suppose then that one does not observe $R_{id}$ directly, but instead infers it from $R_{iT} - R_{ix}$, where $R_{iT}$ is used to denote the total sales of a firm. Assume furthermore that both $\Delta \ln R_{iT}$ and $\Delta \ln R_{ix}$ are measured with error, so that

$$\Delta \ln R_{iT} = \Delta \ln \hat{R}_{iT} + \omega_{iT},$$
$$\Delta \ln R_{ix} = \Delta \ln \hat{R}_{ix} + \omega_{ix},$$

where $\hat{R}_{iT}$ and $\hat{R}_{ix}$ denote the true values of total sales and exports. Note then that

$$\Delta \ln R_{id} = \Delta \ln R_{iT} - \Delta \ln R_{ix} = \Delta \ln \hat{R}_{iT} - \Delta \ln \hat{R}_{ix} + \omega_{iT} - \omega_{ix},$$

and, thus, the measurement error in exports and domestic sales will not be mean independent. Following the same steps as in the main text, we can reach an estimating equation analogous to equation (10)

$$\Delta \ln R_{ix} = d_s + d_\ell + (\sigma - 1) \delta \varphi \Delta \ln (\varphi_i^*) - (\sigma - 1) \delta \omega \Delta \ln (\omega_i^*) + \beta \Delta \ln R_{id} + \varepsilon_{ix},$$

but we now have

$$\varepsilon_{ix} = (\sigma - 1) [u_{ix}^\xi + u_i^\varphi - u_i^\omega] + \omega_{ix}.$$

Similarly, the error term in the expression for the change in domestic sales is given by

$$\varepsilon_{id} = (\sigma - 1) [u_{id}^\xi + u_i^\varphi - u_i^\omega] + \omega_{iT} - \omega_{ix}.$$

It then follows that the probability limit of the OLS estimator of the coefficient on domestic sales can we written as

$$\text{plim}(\hat{\beta}_{OLS}) = \frac{\text{cov}(u_{ix}^\xi + u_i^\varphi - u_i^\omega + \frac{1}{\sigma - 1} \omega_{ix}, u_{id}^\xi + u_i^\varphi - u_i^\omega + \frac{1}{\sigma - 1} (\omega_{iT} - \omega_{ix}))}{\text{var}(u_{id}^\xi + u_i^\varphi - u_i^\omega + \frac{1}{\sigma - 1} (\omega_{iT} - \omega_{ix}))}.$$

This expression is analogous to that in equation (11) but it highlights the potential for additional sources of bias related to the measurement error terms $\omega_{ix}$ and $\omega_{iT} - \omega_{ix}$. The sign of this bias depends on the correlation between the measurement errors in total sales and in exports. If these variables are constructed from different sources (e.g., total sales are obtained from census data, while exports are drawn from customs data) it seems plausible that these measurement errors will be orthogonal to each other, and the impact of measurement error on the resulting bias in the OLS estimate $\hat{\beta}_{OLS}$ will necessarily be negative. Nevertheless, if errors in measurement of total sales and exports are highly correlated, it is possible for the bias resulting from these errors in measurement to be positive, and particularly so when the variance of the measurement error in total sales is larger than that of the measurement error in exports.

Consider next an IV estimator of $\beta$, where $\Delta \ln R_{id}$ is instrumented with a variable $Z_{id}$. The
probability limit of this IV estimator is
\[
\text{plim}(\hat{\beta}_{IV}) = \frac{\text{cov}(u_i^\xi + u_i^\phi - u_i^\omega + \frac{1}{\sigma-1} \omega_i, \hat{Z}_{id})}{\text{cov}(u_i^\xi + u_i^\phi - u_i^\omega + \frac{1}{\sigma-1} (\omega_i - \omega_i), \hat{Z}_{id})}.
\]

This expression illustrates that \(\text{plim}(\hat{\beta}_{IV}) = 0\) as long as the instrument \(Z_{id}\) verifies three conditions: (a) it is correlated with the change in domestic sales of firm \(i\) after controlling for (or partialling out) sector and location fixed effects and observable determinants of the firm’s marginal cost; (b) it is mean independent of the change in firm-specific unobserved productivity, \(u_i^\phi\), factor costs, \(u_i^\omega\), and export demand \(u_i^\xi\); and (c) it is mean independent of the measurement error in exports \(\omega_i\).

A.2 Biases in the Extensive Margin of Exports

We extend here the analysis in Section 2 to the study of the effect of domestic demand shocks on the extensive margin of exports.

Given the CES demand function in equation (1) and the assumption that firms are monopolistically competitive in every market, firm \(i\) will find it profitable to export at time \(t\) only if export revenue \(R_{ixt}\) exceed a multiple \(\sigma\) of the fixed cost of exporting \(F_{ixt}\). Omitting the subindex \(t\) for simplicity from the notation, we can thus express the probability of the firm exporting as
\[
\Pr(\ln R_{ix} > \sigma \ln F_{ix}) = \mathbb{E}[1 \{\ln R_{ix} > \sigma \ln F_{ix}\}],
\]
where \(1\{A\}\) denotes an indicator function that takes value one if and only if the statement \(A\) is true.

Focusing on a linear probability model, we further rewrite the probability of the firm exporting as
\[
\Pr(\ln R_{ix} > \sigma \ln F_{ix}) = \mathbb{E}[\ln R_{ix} - \sigma \ln F_{ix}],
\]
and, therefore, we can write the change in the probability of exporting as a function of the changes in the log export revenues and log fixed export costs
\[
\Delta \Pr(\Delta \ln R_{ix} > \sigma \Delta \ln F_{ix}) = \mathbb{E}[\Delta \ln R_{ix} - \sigma \Delta \ln F_{ix}]
\]
where, from equation (6),
\[
\Delta \ln R_{ix} = \gamma_{ix} + \gamma_{tx} + (\sigma - 1) \delta_{\phi} \ln(\phi_i^*) - (\sigma - 1) \delta_{\omega} \ln(\omega_i^*) + \varepsilon_{ix},
\]
with the different terms in this expression defined as in Section 2. We analogously decompose the log change in fixed costs of exporting as
\[
\Delta \ln F_{ixt} = \phi_{ix} + \phi_{tx} + \phi_{\phi} \Delta \ln(\phi_i^*) + \phi_{\omega} \Delta \ln(\omega_i^*) + u_i^F,
\]
similarly to how we decomposed the demand shifter, productivity and cost levels in Section 2. Notice that we are being quite flexible, letting firm-level fixed export costs depend on firm-level productivity and factor costs, and on both sector and location fixed effects.

With these expressions at hand, we can write the change in the probability of exporting, ex-
panded to include log domestic sales as an additional covariate, as

$$\Delta \Pr (\Delta \ln R_{ix} > \sigma \Delta \ln F_{ix}) =$$

$$d_s + d_t + [(\sigma - 1) \gamma \varphi - \sigma \phi \varphi] \Delta \ln (\varphi_i^*) - [(\sigma - 1) \delta \omega - \sigma \phi \omega] \Delta \ln (\omega_i^*) + \beta \Delta \ln R_{id} + \varepsilon_{ix} - \sigma u_{ix}^F,$$

where, as in equation (9), $\varepsilon_{ix} = (\sigma - 1) [u_{ix}^\xi + u_i^{\varphi} - u_i^{\omega}]$. Following the same steps as in Section 2, the following asymptotic properties of $\hat{\beta}_{OLS}$ can be derived:

$$\text{plim}(\hat{\beta}_{OLS}) = \frac{\text{cov}(u_{ix}^\xi + u_i^{\varphi} - u_i^{\omega} - \frac{\sigma}{\sigma - 1} u_i^F, u_{id}^{\xi} + u_i^{\varphi} - u_i^{\omega})}{\text{var}(u_{id}^{\xi} + u_i^{\varphi} - u_i^{\omega})}.$$ 

The only difference relative to equation (11) is the addition of the term $-u_i^F$ in the first element of the covariance in the numerator. It is clear that, as in the intensive margin regressions, this covariance is likely to be positive, thus generating a positive value of $\text{plim}(\hat{\beta}_{OLS})$.

The probability limit of the IV estimator of $\beta$ is given by

$$\text{plim}(\hat{\beta}_{IV}) = \frac{\text{cov}(u_{ix}^\xi + u_i^{\varphi} - u_i^{\omega} - \frac{\sigma}{\sigma - 1} u_i^F, \tilde{Z}_{id})}{\text{cov}(u_{id}^{\xi} + u_i^{\varphi} - u_i^{\omega}, \tilde{Z}_{id})}.$$ 

This expression will equal zero as long as the instrument $Z_{id}$ verifies the following two conditions: (a) it is correlated with the domestic sales of firm $i$ in period $t$, after controlling for (or partialling out) firm and year fixed effects and observable determinants of the firm’s marginal cost; and (b) it is mean independent of the firm-year specific unobserved productivity, $u_i^\varphi$, factor costs, $u_i^\omega$, export demand shocks, $u_{ix}^\xi$, and export fixed-cost shocks $u_i^F$ (this latter being the only additional condition relative to our results for the intensive margin regressions). As in our discussion in Section 2, an instrument can only (generically) verify conditions (a) and (b) if its effect on domestic sales works exclusively through the domestic demand shock $u_{ix}^\xi$.

It is straightforward to extend the above analysis to the case in which total sales and exports are measured with error and domestic sales are imputed by subtracting exports from total sales. Following the same steps as in Appendix A.1, we obtain

$$\text{plim}(\hat{\beta}_{IV}) = \frac{\text{cov}(u_{ix}^\xi + u_i^{\varphi} - u_i^{\omega} - \frac{\sigma}{\sigma - 1} u_i^F + \frac{1}{\sigma - 1} \omega_{ix}, \tilde{Z}_{id})}{\text{cov}(u_{id}^{\xi} + u_i^{\varphi} - u_i^{\omega} + \frac{1}{\sigma - 1} (\omega_{id} - \omega_{ix}), \tilde{Z}_{id})},$$ 

and, thus, the only additional requirement on the instrument is that it is mean independent of the measurement error in exports $\omega_{ix}$.

A.3 Microfoundation for Model in Section 7.1

Suppose a firm has a fixed capital/production capacity $K_i$ and decides how much of a variable factor $L_i$ (let us call it labor) to hire to produce a given amount of output. Assuming a Cobb-Douglas technology in fixed capital and labor, the cost minimization problem of a firm with productivity $\varphi$ seeking to produce a total amount of output $Q_i = \tau_{dt} Q_{idt} + \tau_{xt} Q_{ixt}$ can be expressed as:

$$\min \omega_i L_i$$
s.t. \( \varphi K_i^\alpha L_i^{1-\alpha} \geq Q_i \).

The first-order condition of this problem delivers

\[
\omega_i = \mu (1 - \alpha) \frac{Q_i}{L_i} \quad \varphi K_i^\alpha L_i^{1-\alpha} = Q_i.
\]

From these we obtain a total cost of producing \( Q_i \) units of output equal to

\[
\omega_i L_i = \omega_i (\varphi K_i^\alpha)^{-1/(1-\alpha)} (Q_i)^{1/(1-\alpha)}.
\]

Letting

\[
\tilde{\varphi}_i = (1 - \alpha) (\varphi K_i^\alpha)^{1/(1-\alpha)} \quad \lambda = \frac{\alpha}{1 - \alpha},
\]

this can in turn be written as

\[
\omega_i L_i = \frac{1}{\tilde{\varphi}_i} \omega_i \frac{1}{1 + \lambda} (\tau dt Q_{i dt} + \tau xt Q_{i xt})^{1+\lambda},
\]

which is analogous to equation (14) in the main text, but with an endogenous value of \( \lambda = \alpha/(1-\alpha) \) that is tightly related to the ratio of the output elasticity of the fixed factor to the output elasticity of the variable factor. This motivates our heterogeneous effects specifications in columns 7 and 8 of Table 5.

It is important to stress, however, that the result \( \lambda = \alpha/(1-\alpha) \) relies strongly on our assumption of constant returns to scale. If we instead specify output as \( Q_i = \varphi K_i^\alpha K_i^{1-\alpha} \), it is straightforward to verify that one obtains \( \lambda = (1 - \alpha L)/\alpha L \), which suggests that the curvature of the marginal cost schedule is crucially shaped by the reciprocal of the output elasticity of the flexible factor.

B Data Appendix

B.1 Macroeconomic Data

Data on Spanish unemployment, real GDP, internal demand, private final consumption expenditure and exports of goods come from the Spanish National Statistical Office (Instituto Nacional de Estadística). Data on merchandise exports and real GDP shares for the countries that belong to the European Monetary Union and Spain come from AMECO Dataset (i.e., annual macroeconomic database of the European Commission’s Directorate General for Economic and Financial Affairs). Data on unit labor costs in the manufacturing sector for Spain and the European Monetary Union were obtained from the Bank of Spain (Banco de España) and the Eurosystem. We use the input-output tables produced by the Spanish National Statistical Office (Instituto Nacional de Estadística) for the year 2005 to identify the interlinkages across industries (e.g., the two top leading input providers or two top leading buying industries of the vehicles manufacturing industry discarded in the robustness analysis described in Table 4).
B.2 Construction of the Commercial Registry Dataset

As described in Section 3.3, our main source of firm-level data is the Commercial Registry (Registro Comercial Central), which contains annual financial statements of around 85% of registered firms in the non-financial economy. We collate data from two separate sources to construct our own firm-level dataset: (i) the Central de Balances dataset from Banco de España and (ii) SABI, from Informa, a private company. Despite being based on the same original source, these two datasets are complementary: the first includes the largest number of firms and has the best coverage of small and medium enterprises, while the second has the most precise coverage of large firms. A detailed description of how we combine the two sources to construct our firm-level dataset can be found in Almunia, Lopez-Rodriguez and Moral-Benito (2018).

B.3 Foreign Transactions Dataset

As described in Section 3.3, the Bank of Spain requires all financial institutions and a set of large companies to report all foreign transactions, including imports, exports and other financial transactions. Until 2007, for each transaction there is information about the country of destination (or origin). However, from 2008 onwards, the Bank of Spain relaxed this requirement and allowed reporting institutions to group multiple transactions into a single reported transaction. In those cases, the country of destination (or origin) reflected in the entry correspond to the country of the largest transaction in that group. Similarly, the product code reported corresponds to the largest transaction as well. This implies that one cannot analyze changes in the countries of destination (or origin) and product codes in a consistent way for periods spanning around year 2008.

B.3.1 Minimum Reporting Threshold

Between 2001 and 2007, all foreign transactions for more than €12,500 had to be reported to the Bank of Spain, either by financial institutions or by the set of large corporations described above. The minimum reporting threshold was updated in 2008 to €50,000, in order to reduce the compliance costs for reporting institutions. From that year onwards, a firm appears in the dataset if it has at least one transaction larger than €50,000 in that year.

In order to create a homogeneous sample for the period 2000-2013, we apply the post-2008 minimum reporting threshold to the data from 2000 to 2007, meaning that we only record a positive export flow in a given year for firms that have at least one transaction exceeding 50,000 euros in that year. This adjustment reduces substantially the number of exporting firms that appear in the data, but the impact on the aggregate amount exported is small. Even though this is clearly an obstacle to the estimation of extensive-margin effects because we do not observe many small exporters that enter (or exit) the market, the patterns broadly suggest that the extensive margin is not very important on aggregate terms. This is confirmed by the analysis of De Lucio et al. (2017) using data from Customs that does not have a minimum reporting threshold.

B.4 Instruments

The empirical strategy implemented in the paper requires complementing the firm-level dataset of manufacturers with geographic-based data. In particular, we construct several instruments using information available at both zip code and province level. The information on the stock of vehicles by both municipality and province is provided by the Spanish Registry of Motor Vehicles (Dirección...
General de Tráfico). The data on the number of foreign tourists and foreign overnight stays at province level come from the Spanish National Statistical Office (Instituto Nacional de Estadística). The information on the population by both municipality and province is provided by the Spanish National Statistical Office (Instituto Nacional de Estadística). The data to construct the proxy for the housing supply elasticity in a given municipality come from the Spanish Cadastre (Dirección General del Catastro). In particular, we use the measure in Basco and Lopez-Rodriguez (2017), which is a ratio of available “buildable” urban land to urban land with already built structures for Spanish municipalities. Within potential urban land, the measure considers undevelopable total land after excluding protected non-urban areas (e.g., rivers or natural parks), plots classified as being restricted for rural use, and public goods land (e.g., municipality surface occupied by transport and utilities infrastructure). The ratio is calculated in a year sufficiently removed from the housing boom (1996) to avoid feedback effects of booming prices on the availability of undevelopable urban land during the Spanish housing bubble in the 2000s. The information on the residential house prices at municipality level used in Appendix C are obtained from the census of real-estate transactions owned by the Spanish Ownership Registry (Registro de la Propiedad). We calculate the market value price per square meter for each residential housing transaction and then aggregate those prices for all transactions made in a municipality during a natural year to create yearly average prices per square meter. The price indexes for residential housing are calculated from 2004 to 2012 for municipalities with more than 1000 inhabitants and more than 30 transactions per year. These indexes are deflated using the Consumer Price Index provided by the Spanish National Statistical Office (Instituto Nacional de Estadística).

B.5 Estimation of Revenue Productivity

We describe here the procedure we follow to estimate a proxy for firm- and year-specific performance or revenue productivity.

Production function. We assume a production function that is a Leontief function of materials and a translog aggregator of labor and capital:

\[ Q_{it} = \min\{H(K_{it}, L_{it}; \alpha), M_{it}\}\varphi_{it}, \]  
\[ H(K_{it}, L_{it}; \alpha) = \exp(h(k_{it}, l_{it}; \alpha)), \]  
\[ h(k_{it}, l_{it}; \alpha) \equiv \alpha_l l_{it} + \alpha_k k_{it} + \alpha_{ll} l_{it}^2 + \alpha_{kk} k_{it}^2 + \alpha_{lk} l_{it} k_{it}, \]

with \( \alpha = (\alpha_l, \alpha_k, \alpha_{ll}, \alpha_{kk}, \alpha_{lk}) \). In equation (22a), \( K_{it} \) is effective units of capital, \( L_{it} \) is the number of production workers, \( M_{it} \) is a quantity index of materials use, and \( \varphi_{it} \) denotes the Hicks-neutral physical productivity. We use lower-case Latin letters denote the logarithm of the upper-case variable, e.g. \( l_{it} = \ln(L_{it}) \). The production function in equation (22) nests that introduced in Appendix A.3, which implicitly assumes that \( \alpha_{ll} = \alpha_{kk} = \alpha_{lk} = 0 \).

Consistently with the definition of \( \varphi_{it} \) as physical productivity, we assume that

\[ \mathbb{E}[\varphi_{it} | J_{it}] = \varphi_{it}, \]  

where \( J_{it} \) denotes the information set of firm \( i \) at the time at which the period \( t \) pricing and input decisions are taken. Therefore, the firm knows the value of its productivity \( \varphi_{it} \) when making the period \( t \) pricing and input decisions.
We assume that both materials and labor are fully flexible inputs, and that capital is dynamic and determined one period ahead. Consequently, both $M_{it}$ and $L_{it}$ are a function of $J_{it}$, while $K_{ijt}$ is a function of $J_{it-1}$.

**Demand function.** We assume that firms face a constant elasticity of substitution demand function as described in equation (1), and impose the assumption that the demand shock $\xi_{it}$ is known to firms when determining their input and output decisions; i.e.

$$\mathbb{E}[\xi_{it}|J_{it}] = \xi_{it}. \quad (24)$$

**Market structure.** As described in Section 2, we assume that firms are monopolistically competitive in the output markets and that they take the prices of labor, materials and capital as given.

**Revenue function.** Given the assumption that materials is a flexible input, equation (22a) implies that optimal materials usage satisfies

$$M_{it} = H(K_{it}, L_{it}; \alpha).$$

Therefore, we can rewrite the production function in equation (22a) as

$$Q_{it} = H(K_{it}, L_{it}; \alpha)\varphi_{it}, \quad (25)$$

where $H(K_{it}, L_{it}; \alpha)$ is defined as in equations (22b) and (22c). Given this expression and the demand function in equation (1), we can write the revenue function of a firm $i$ at period $t$ as

$$R_{it} = P_{it}Q_{it} = P_{st}^{\sigma+1}\bar{E}_{st}^{\frac{1}{\sigma}}\xi_{st}^{\frac{1}{\sigma}}Q_{st}^{\frac{1}{\sigma}} = \mu_{st}H(K_{it}, L_{it}; \beta)\psi_{it}, \quad (26)$$

where

$$\kappa \equiv \frac{(\sigma - 1)}{\sigma}, \quad (27a)$$

$$\beta \equiv \kappa\alpha, \quad (27b)$$

$$\psi_{it} \equiv (\xi_{it}\varphi_{it})^{\kappa}, \quad (27c)$$

$$\mu_{st} \equiv P_{st}^{\kappa}\bar{E}_{st}^{1-\kappa}. \quad (27d)$$

The parameter $\kappa$ measures the inverse of the firm’s markup. While the parameter vector $\alpha$ includes the production function parameters, the vector $\beta$ includes the revenue function parameters. The variable $\psi_{it}$ captures the revenue productivity of the firm: the residual determinant of a firm’s revenue after controlling for sector- and year-specific fixed effects and for the effect of capital and labor on the firm’s revenue. As illustrated in equation (27c), revenue productivity equals in our model the product of the Hicks-neutral productivity $\varphi_{it}$ and the demand shifter $\xi_{it}$ to the power of the reciprocal of the firm’s markup. The sector-year fixed effects accounts for the price index and total expenditure in the corresponding sector-year pair.
Stochastic process for revenue productivity. We assume that revenue productivity follows a first-order autoregressive process, AR(1), with a state- and year-specific shifter:

\[ \psi_{it} = \gamma_{st} + \rho \psi_{it} + \eta_{it} \quad \text{with} \quad \mathbb{E}[\eta_{it}|J_{it}] = 0. \] (28)

This stochastic process for revenue productivity may arise under different stochastic process for physical productivity \( \varphi_{it} \) and the demand shifter \( \xi_{it} \); e.g. both variables follow AR(1) process with identical persistence parameters equal to \( \rho \); or, one of them follows an AR(1) process with persistence \( \rho \) and the other one is independent over time.

Estimation of demand elasticity. In order to estimate the demand elasticity of substitution \( \sigma \), we follow the approach implemented, among others, in Das, Roberts and Tybout (2007) and Antràs, Fort and Tintelnot (2017). Given the assumption that all firms are monopolistically competitive in their output markets, it will be true that

\[ R_{it} - C_{it}^v = \frac{1}{\sigma} R_{it}, \]

where \( C_{it}^v \) denotes the total variable costs that firm \( i \) incurred at period \( t \) to obtain the sales revenue \( R_{it} \). This expression indicates that the firm’s total profits (gross of fixed costs) is equal to the reciprocal of the demand elasticity of substitution \( \sigma \) multiplied by the firm’s total revenues. Given that the only variable inputs are materials \( M_{it} \) and labor \( L_{it} \), we can rewrite this relationship as

\[ R_{it} - P_{it}^m M_{it} - \omega_{it} L_{it} = \frac{1}{\sigma} R_{it}, \]

where \( P_{it}^m \) denotes the equilibrium materials price faced by firm \( i \) at period \( t \), \( \omega_{it} \) denotes the equilibrium salary and, thus, \( P_{it}^m M_{it} \) denotes total payments for materials and \( \omega_{it} L_{it} \) denotes total payments to labor. Rearranging terms, we obtain the following equality

\[ \left( \frac{\sigma - 1}{\sigma} \right) R_{it} = P_{it}^m M_{it} + \omega_{it} L_{it}, \]

and, allowing for measurement error in sales revenue, \( R_{it}^{obs} \equiv R_{it} \exp(\varepsilon_{it}) \), we obtain

\[ \ln \left( \frac{\sigma - 1}{\sigma} \right) + r_{it}^{obs} - \varepsilon_{it} = \ln (P_{it}^m M_{it} + \omega_{it} L_{it}), \]

where, as indicated above, lower-case Latin letters denote the logarithm of the corresponding upper case variable and, thus, \( r_{it}^{obs} \equiv \ln(R_{it}^{obs}) \). Imposing the assumption that \( \mathbb{E}[\varepsilon_{it}] = 0 \), we identify \( \sigma \) through the following moment condition

\[ \mathbb{E} \left[ \ln \left( \frac{\sigma - 1}{\sigma} \right) + r_{it}^{obs} - \ln (P_{it}^m M_{it} + \omega_{it} L_{it}) \right] = 0. \] (29)
Estimation of labor elasticity parameters. Given equation (26), we can write the profit function of firm $i$ in period $t$ as

$$\Pi_{it} = \mu_{st} H(K_{it}, L_{it}; \beta) \psi_{it} - \omega_{it} L_{it} - P_{it}^m M_{it} - P_{it}^k I_{it},$$

where $\omega_{it}$ denotes the wage that firm $i$ faces at period $t$ and, analogously, $P_{it}^m$ and $P_{it}^k$ denote the materials and capital prices. Assuming that labor is a fully flexible input and that firms are both monopolistically competitive in output markets and take the price of all inputs as given, the first order condition of the profit function with respect to labor implies that

$$\frac{\partial \Pi_{it}}{\partial L_{it}} = (\beta_l + 2\beta_{ll} l_{it} + \beta_{lk} k_{it}) R_{it} - \omega_{it} L_{it} = 0.$$

Reordering terms and taking logs on both sides of the equality, we obtain

$$\ln(\beta_l + 2\beta_{ll} l_{it} + \beta_{lk} k_{it}) = \ln(\omega_{it} L_{it}) - r_{it},$$

and, taking into account that revenues are measured with error, we can further rewrite

$$\ln(\beta_l + 2\beta_{ll} l_{it} + \beta_{lk} k_{it}) = \ln(\omega_{it} L_{it}) - r_{it}^{obs} + \varepsilon_{it}.$$

Assuming that the measurement error in revenue is not only mean zero (as imposed to derive the moment condition in equation (29)) but mean independent of the firm’s labor and capital usage,

$$E[\varepsilon_{it}|l_{it}, k_{it}] = 0,$$

we can derive the following conditional moment:

$$E[r_{it}^{obs} - \ln(\omega_{it} L_{it}) + \ln(\beta_l + 2\beta_{ll} l_{it} + \beta_{lk} k_{it})|l_{it}, k_{it}] = 0.$$

We derive unconditional moments from this equation and use a method of moments estimator to estimate $(\hat{\beta}_l, \hat{\beta}_{ll}, \hat{\beta}_{lk})$. With the estimates $(\hat{\beta}_l, \hat{\beta}_{ll}, \hat{\beta}_{lk})$ in hand, we recover an estimate of the measurement error $\varepsilon_{it}$ for each firm $i$, affiliate $j$, and period $t$:

$$\hat{\varepsilon}_{it} = r_{it}^{obs} - \ln(\omega_{it} L_{it}) + \log(\hat{\beta}_l + 2\hat{\beta}_{ll} l_{it} + \hat{\beta}_{lk} k_{it}).$$

Combining the estimates of the parameters entering the elasticity of the firm’s revenues with respect to labor, $(\hat{\beta}_l, \hat{\beta}_{ll}, \hat{\beta}_{lk})$, and the estimate of the demand elasticity of substitution, we compute estimates of the parameters $(\alpha_l, \alpha_{ll}, \alpha_{lk})$; i.e.

$$(\hat{\alpha}_l, \hat{\alpha}_{ll}, \hat{\alpha}_{lk}) = \frac{\hat{\sigma}}{\hat{\sigma} - 1}(\hat{\beta}_l, \hat{\beta}_{ll}, \hat{\beta}_{lk}).$$

Estimation of capital elasticity parameters. Using the estimates $(\hat{\alpha}_l, \hat{\alpha}_{ll}, \hat{\alpha}_{lk})$ and $\hat{\varepsilon}_{it}$ we can
construct a corrected measure of revenues

\[ \hat{r}_{it} \equiv r_{it} - \hat{\beta}_l l_{it} - \hat{\beta}_ll^2_{it} - \hat{\beta}_k k_{it} - \hat{\beta}_kk^2_{it} - \hat{\epsilon}_{it}, \]

and, given the expression for sales revenues in equation (26), it holds that

\[ \hat{r}_{it} = \beta_k k_{it} + \beta_{kk} k^2_{it} + \psi_{it}. \]

Given this expression and the stochastic process for the evolution of productivity in equation (28), it will be true that

\[ \hat{r}_{it} = \beta_k k_{it} + \beta_{kk} k^2_{it} + \mu \psi_{ijt-1} - \beta_k k_{ijt-1} - \beta_{kk} k^2_{ijt-1}) + \zeta_{st} + \eta_{it}, \quad (30) \]

where \( \zeta_{st} \) is an unobserved sector and time-specific effect that accounts for the revenue shifter \( \mu_{st} \) and the productivity shifter \( \gamma_{st} \). Given that both \( L_{it} \) and \( K_{it} \) are a function of the information set \( J_{it} \), the definition of \( \eta_{it} \) in equation (28) implies that

\[ \mathbb{E}[\eta_{it}|k_{it}, \hat{r}_{ijt-1}, \{d_{st}\}_{s,t}] = 0, \]

where \( \{d_{st}\}_{s,t} \) denotes a full set of sector and period-specific dummy variables. Therefore, we can derive the following conditional moment equality

\[ \mathbb{E}[\hat{r}_{it} - \beta_k k_{it} - \beta_{kk} k^2_{it} - \rho(\hat{r}_{ijt-1} - \beta_k k_{ijt-1} - \beta_{kk} k^2_{ijt-1}) - \zeta_{st}|k_{it}, \hat{r}_{ijt-1}, \{d_{st}\}_{s,t}] = 0 \]

We derive unconditional moments from this equation and use a method of moments estimator to estimate \((\beta_k, \beta_{kk}, \rho)\). When estimating these parameters, we use the Frisch-Waugh-Lovell theorem to control for the full set of sector- and year-specific fixed effects \( \{\zeta_{st}\}_{s,t} \). Combining the estimates of the parameters \((\beta_k, \beta_{kk})\), and the estimate of the demand elasticity of substitution, \( \sigma \), we compute estimates of the parameters \((\alpha_k, \alpha_{kk})\); i.e.

\[ (\hat{\alpha}_k, \hat{\alpha}_{kk}) = \frac{\hat{\sigma}}{\hat{\sigma} - 1}(\hat{\beta}_k, \hat{\beta}_{kk}). \]

**Estimation of productivity.** We can also use the estimates of the parameters \((\beta_k, \beta_{kk})\) and the constructed random variable \( \hat{r}_{it} \) to build an estimate of the revenue productivity \( \psi_{it} \) for every firm and time period

\[ \hat{\psi}_{it} = \hat{r}_{it} - \hat{\beta}_k k_{it} - \hat{\beta}_{kk} k^2_{it}. \]
C Appendix Figures

C.1 Share of Exports and GDP Within the European Union

Figure C.1 plots the share of exports to non-EU countries and GDP for Greece, Portugal, Spain and Germany (see Appendix B.1 for information on the sources of data).

Figure C.1: Share of Exports to non-EU Countries and GDP

Panel (a): Greece

Panel (b): Portugal

Panel (c): Spain

Panel (d): Germany

C.2 Spatial Distribution of Economic Activity in Spain

Figure C.2 plots the 2002-2008 annual average number of firms and number of exporting firms for each of the 47 Spanish peninsular provinces (see Appendix sections B.2 and B.3 for information on the sources of data).
C.3 Province-Level Home Bias in Spanish Manufacturing

Data are for the year 2007 from C-interreg. We are grateful to Carlos Llano for providing them to us.

C.4 Housing Supply Elasticities, Price Growth and Consumption

In this Appendix, we show that there is a negative cross-sectional correlation between the 1996 ratio of available ‘buildable’ urban land to urban land with already built structures (a proxy for the housing supply elasticity) and housing price growth during the boom years 2004-07. We also show that there is a negative correlation between this housing supply elasticity and our benchmark instrument for local demand, that is, the log change the average stock of cars per capita in a zipcode between the boom and the bust. We note, however, that the latter correlation turns positive (though not statistically significant) when weighing observations by the number of firms in that zipcode in our sample of continuing exporters.
Figure C.3: Province-Level Home Bias in Spanish Manufacturing

Figure C.4: Housing Supply Elasticities and Housing Price Growth during 2004-07

The associated regression coefficients and standard errors are -0.0177 and 0.0072 for the first regression and -0.0350 and 0.0023 for the second one.
Figure C.5: Housing Supply Elasticities and Changes in Log Vehicles per Capita in Bust relative to Boom

D Appendix Tables

Table D.1: Additional Robustness Tests

<table>
<thead>
<tr>
<th>Sample:</th>
<th>Excluding multinationals</th>
<th>Weight by # of years exporting</th>
<th>Bust as 2010-2013</th>
<th>Bust as 2011-2013</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
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<tr>
<td>OLS Elasticity</td>
<td>-0.194***</td>
<td>-0.185***</td>
<td>-0.207***</td>
<td>-0.241***</td>
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<td></td>
<td>(0.030)</td>
<td>(0.025)</td>
<td>(0.029)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>IV Elasticity</td>
<td>-1.518***</td>
<td>-1.324***</td>
<td>-1.648***</td>
<td>-1.540***</td>
</tr>
<tr>
<td></td>
<td>(0.469)</td>
<td>(0.460)</td>
<td>(0.497)</td>
<td>(0.477)</td>
</tr>
<tr>
<td>1st Stage Coefficient</td>
<td>0.336***</td>
<td>0.313***</td>
<td>0.327***</td>
<td>0.327***</td>
</tr>
<tr>
<td></td>
<td>(0.066)</td>
<td>(0.066)</td>
<td>(0.069)</td>
<td>(0.068)</td>
</tr>
<tr>
<td>1st Stage F-Stat.</td>
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<td>22.54</td>
<td>22.32</td>
<td>22.89</td>
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<tr>
<td>Observations</td>
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<td>6,722</td>
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<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Province FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by municipality. All specifications include sector and province fixed effects. Vehicles p.c. denotes the stock of vehicles per capita. First stage coefficient and F-statistic denote the corresponding statistic for the Vehicles p.c. covariate. Significance levels: ***p<0.01, **p<0.05, *p<0.1.
Table D.2: Additional Alternative Instruments and Overidentification Tests

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>∆Ln(Domestic Sales)</th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>∆ Ln(Vehicles p.c. in municipality, 2002 pop.)</td>
<td>0.349***</td>
<td>(0.047)</td>
<td></td>
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<tr>
<td>∆ Ln(Vehicles p.c. in province)</td>
<td>0.561**</td>
<td>(0.271)</td>
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</tr>
<tr>
<td>∆ Ln(Vehicles p.c. in municipality)</td>
<td>0.246***</td>
<td>(0.081)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>∆ Ln(Construction employment) × 2002 employment share in municipality</td>
<td>0.379***</td>
<td>(0.071)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>∆ Ln(Construction turnover) × 2002 turnover share in municipality</td>
<td>0.147***</td>
<td>(0.023)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>F-statistic</td>
<td>54.60</td>
<td>19.70</td>
<td>28.31</td>
<td>41.54</td>
<td></td>
</tr>
<tr>
<td>∆Ln(Exports)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>∆ Ln(Domestic Sales)</td>
<td>-1.677***</td>
<td>-1.285***</td>
<td>-1.349**</td>
<td>-1.929***</td>
<td>(0.336)</td>
</tr>
<tr>
<td>p-value for Sargan Overid test</td>
<td>0.80</td>
<td>0.46</td>
<td>0.80</td>
<td>0.68</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>8,018</td>
<td>8,018</td>
<td>7,928</td>
<td>7,928</td>
<td></td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by municipality, except for column 2, where they are clustered by province. All specifications include firm-level log TFP and log wages as additional controls (coefficients not included to save space). All specifications include province and sector fixed effects, except column 2 which only includes sector effects. Significance levels: ***p<0.01, **p<0.05, *p<0.1.
Table D.3: Alternative Instruments and Overidentification Tests for Total Sales

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>$\Delta \text{Ln}(\text{Total Sales})$</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\Delta \text{Ln}(\text{Vehicles p.c. in Province})$</td>
<td>0.617***</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\Delta \text{Ln}(\text{Distance-Population weighted vehicles p.c. in other zip codes})$</td>
<td>0.136***</td>
<td>0.083***</td>
<td>(0.027)</td>
<td>(0.021)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\Delta \text{Ln}(\text{Vehicles p.c. in municipality})$</td>
<td>0.204***</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\ln(\text{Urban Land Supply Ratio in 1996})$</td>
<td>0.018**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\Delta \text{Ln}(\text{construction wage bill}) \times 2002 \text{ wage bill share in municipality}$</td>
<td>0.286***</td>
<td>(0.050)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\Delta \text{Ln}(\text{foreign tourists}) \times 2002 \text{ foreign tourists p.c. in province}$</td>
<td>0.167**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>F-statistic</td>
<td>22.80</td>
<td>25.48</td>
<td>16.97</td>
<td>4.13</td>
<td>32.82</td>
<td>5.50</td>
<td></td>
</tr>
<tr>
<td>$\Delta \text{Ln}(\text{Exports})$</td>
<td>-1.972**</td>
<td>-3.120**</td>
<td>-2.100**</td>
<td>-2.605</td>
<td>-1.819**</td>
<td>-1.971***</td>
<td></td>
</tr>
<tr>
<td>(0.785)</td>
<td>(1.537)</td>
<td>(0.877)</td>
<td>(2.047)</td>
<td>(0.745)</td>
<td>(0.560)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>p-value for Sargan Overid test</td>
<td>0.76</td>
<td>0.23</td>
<td>0.23</td>
<td>0.60</td>
<td>0.56</td>
<td>0.84</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>8,018</td>
<td>7,949</td>
<td>7,949</td>
<td>6,940</td>
<td>7,928</td>
<td>8,018</td>
<td></td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by province except for columns 4 and 5, where they are clustered by municipality. All specifications include firm-level log TFP and log wages as additional controls (coefficients not included to save space). All specifications include sector fixed effects, and column 5 also includes province fixed effects. Significance levels: ***p<0.01, **p<0.05, *p<0.1.
Exports and Domestic Sales: Year-to-Year Variation

Motivated by our identification approach, in the main text we have focused on empirical specifications that compare export behavior in a 
_bust_ period relative to a _boom_ period. In this Appendix, we

study OLS specifications that exploit year-to-year variation in domestic sales and exports. Before doing so, however, we extend our benchmark constant-marginal cost model to a setup with multiple periods.

### E.1 Theoretical Model

Let us index all firms producing in Spain in a given manufacturing sector by \( i = 1, \ldots, I \), the two potential markets in which they may sell by \( j = \{d, x\} \) (resulting in domestic sales and exports), and time periods by \( t = 1, \ldots, T \). To keep the notation tidy, we do not index sectors nor locations, though our specifications below will include both sector-year and municipality-year fixed effects.

Each firm \( i \) faces the following isoelastic demand in \( j \) at period \( t \),

\[
Q_{ijt} = \frac{P_{ijt}^{\sigma}}{P_{jt}^{1-\sigma}} E_{jt}^{\sigma-1} \xi_{ijt}^{\sigma-1}, \quad \sigma > 1,
\]

where \( Q_{ijt} \) denotes the number of units of output of firm \( i \) demanded in market \( j \) at period \( t \) if it sets a price \( P_{ijt} \), \( P_{jt} \) is the sectoral price index in \( j \), \( E_{jt} \) is the total expenditure in market \( j \) expressed in units of the numeraire; and \( \xi_{ijt} \) is a firm-market-year specific demand shifter satisfying \( \mathbb{E} \left[ \xi_{ijt}^{\sigma-1} \right] = 1 \).

Firm \( i \)'s total variable cost of producing \( Q_{ijt} \) units for each market \( j = \{d, x\} \) is

\[
c_{ijt} Q_{ijt} \quad \text{with} \quad c_{ijt} \equiv \tau_{jt} \frac{1}{\varphi_{it}} \omega_{it},
\]

where \( c_{ijt} \) denotes the marginal cost to firm \( i \) of selling one unit of output in market \( j \), \( \tau_{jt} \) denotes an iceberg trade cost, \( \varphi_{it} \) is a measure of firm-specific productivity, and \( \omega_{it} \) is the cost of a bundle of inputs. Additionally, we assume that firm \( i \) needs to pay an exogenous fixed cost \( F_{ijt} \) to sell a positive amount in market \( j \) at \( t \).

Solving the problem of the firm as in the main text, we now derive the following expression for sales by firm \( i \) to market \( j \) at period \( t \):

\[
R_{ijt} = P_{ijt} Q_{ijt} = \left( \frac{\sigma - 1}{\sigma} \frac{\xi_{ijt} \varphi_{it}}{\tau_{jt} \omega_{it}} \right)^{\sigma-1} E_{jt} \frac{P_{jt}^{1-\sigma}}{}.
\]
For the case of exports \((j = x)\), and taking logs, we can rewrite this expression as:

\[
\ln R_{ixt} = \kappa + (\sigma - 1) [\ln \xi_{ixt} + \ln \varphi_{it} - \ln \omega_{it}] - (\sigma - 1) (\ln \tau_{xt} - \ln P_{xt}) + \ln E_{xt},
\]

(E.1)

where \(\kappa\) is a constant. In order to transition into an estimating equation, we model the demand, productivity and cost levels as follows:

\[
\begin{align*}
\ln(\xi_{ixt}) &= \xi_{ix} + \xi_{xt} + \tilde{\xi}_{ix} \times t + \gamma_{ix} + \gamma_{xt} \times t + u_{i1xt}^\xi, \\
\ln(\varphi_{it}) &= \varphi_i + \varphi_t + \tilde{\varphi}_i \times t + \delta_{it} \ln(\varphi^*_{it}) + \gamma_{it} \times t + \delta_{it} \ln(\varphi^*_{it}) + u_{it}^\varphi, \\
\ln(\omega_{it}) &= \omega_i + \omega_t + \tilde{\omega}_i \times t + \delta_{it} \ln(\omega^*_{it}) + u_{it}^\omega.
\end{align*}
\]

(E.2)

Note that we are decomposing these terms into (i) a time-invariant firm fixed effect, (ii) a firm-invariant year fixed effect (which in the regressions will be expanded to include a whole set of municipality-year and sector-year fixed effects), (iii) a firm-specific linear trend, (iv) any observable part of these terms for the case of productivity \((\varphi^*_{it})\) and input bundle costs \((\omega^*_{it})\), and (iv) a conditional mean-zero error term.\(^1\) The latter error terms thus reflect any unobservable part of demand, productivity or input costs not captured by the various fixed effects. Given these decompositions, we can re-write equation (E.1) as:

\[
\begin{align*}
\gamma_{ix} &\equiv (\sigma - 1) [\xi_{ix} + \varphi_i - \omega_i], \\
\gamma_{xt} &\equiv (\sigma - 1) [\xi_{xt} + \varphi_t - \omega_t] - (\sigma - 1) (\ln \tau_{xt} - \ln P_{xt}) + \ln E_{xt}, \\
\psi_{ix} &\equiv (\sigma - 1) [\tilde{\xi}_{ix} + \tilde{\varphi}_i - \tilde{\omega}_i], \text{ and the error term is given by} \\
\varepsilon_{ixt} &= (\sigma - 1) [u_{i1xt}^\xi + u_{it}^\varphi - u_{it}^\omega].
\end{align*}
\]

(E.4)

Now consider the expression for revenues in the local market. Following the exact same steps as above, we can derive

\[
\begin{align*}
\gamma_{id} &\equiv (\sigma - 1) [\xi_{id} + \varphi_i - \omega_i], \\
\gamma_{dt} &\equiv (\sigma - 1) [\xi_{dt} + \varphi_t - \omega_t] - (\sigma - 1) (\ln \tau_{dt} - \ln P_{dt}) + \ln E_{dt}, \\
\psi_{id} &\equiv (\sigma - 1) [\tilde{\xi}_{id} + \tilde{\varphi}_i - \tilde{\omega}_i], \text{ and} \\
\varepsilon_{idt} &= (\sigma - 1) [u_{i1dt}^\xi + u_{it}^\varphi - u_{it}^\omega].
\end{align*}
\]

(E.6)

Consider now using OLS to estimate the parameters of the following regression, which includes log domestic sales as an additional covariate in equation (E.3), with the fixed effects denoted with \(d\)'s:

\[
\begin{align*}
\gamma_{id} &\equiv (\sigma - 1) [\xi_{id} + \varphi_i - \omega_i], \\
\gamma_{dt} &\equiv (\sigma - 1) [\xi_{dt} + \varphi_t - \omega_t] - (\sigma - 1) (\ln \tau_{dt} - \ln P_{dt}) + \ln E_{dt}, \\
\psi_{id} &\equiv (\sigma - 1) [\tilde{\xi}_{id} + \tilde{\varphi}_i - \tilde{\omega}_i], \text{ and} \\
\varepsilon_{idt} &= (\sigma - 1) [u_{i1dt}^\xi + u_{it}^\varphi - u_{it}^\omega].
\end{align*}
\]

(E.7)

From equations (E.6) and (E.7), the probability limit of the ordinary least-squares (OLS) esti-

\(^1\) Formally, \(E \left[ u_{i1xt}^\xi, \xi_{ix}, \xi_{xt}, \tilde{\xi}_{ix} \times t \right] = 0, E \left[ u_{i1t}^\varphi, \varphi_i, \tilde{\varphi}_i \times t, \ln(\varphi^*_{it}) \right] = 0\), and \(E \left[ u_{it}^\omega | \omega_i, \omega_t, \tilde{\omega}_i \times t, \ln(\omega^*_{it}) \right] = 0\).
mator of the coefficient on domestic sales can we written as

\[
\text{plim}(\hat{\beta}_{OLS}) = \frac{\text{cov}(\tilde{r}_{ixt}, \tilde{r}_{idt})}{\text{var}(\tilde{r}_{idt})} = \frac{\text{cov}(u_{ixt}^\xi + u_{it}^\varphi - u_{idt}^\omega, u_{idt}^\xi + u_{it}^\varphi - u_{idt}^\omega)}{\text{var}(u_{idt}^\xi + u_{it}^\varphi - u_{idt}^\omega)},
\]

(E.8)

where we denote by \( \tilde{X} \) the residual of a regression of a variable \( X \) on \( d_i, d_t, \tilde{d}_i \times t, \ln \varphi_{it}^\star, \) and \( \ln \omega_{it}^\star \).

As in the main text, it should be clear that:

1. As long as productivity and production factor costs are not perfectly observable or captured by the various fixed effects or the firm-specific trends, there will be a spurious positive correlation between exports and domestic sales that, in large samples, would lead one to estimate a positive value of \( \hat{\beta}_{OLS} \) even when export and domestic decisions are really independent due to constant marginal costs.

2. In the presence of a non-zero correlation in the residual demand (partialling out fixed effects) faced by firms in domestic and foreign markets, the OLS estimator of \( \beta \) will also converge to a non-zero value. Because this residual variation in demand does not capture market-specific macro shocks (which are controlled for via municipality-year and sector-year fixed effects), it seems particularly plausible that \( u_{ixt}^\xi \) and \( u_{idt}^\xi \) will be positively correlated, leading one again to estimate a positive value of \( \hat{\beta}_{OLS} \).

We have focused so far on the intensive margin, i.e., the impact of domestic demand shocks on the level of exports conditional on exporting. As in Appendix A.2, an analysis of the extensive margin of exports delivers very similar insights. More specifically, even if the true elasticity of domestic sales to the probability of exporting were to be 0, one is likely to estimate a spurious positive elasticity whenever productivity and production factor costs are not perfectly captured by the various fixed effects, firm-specific trends and observable proxies, or whenever unobserved residual demand shocks are positively correlated across markets.

**E.2 Empirical Results: Intensive Margin**

Table E.1 presents OLS estimates from different specifications in which the logarithm of exports of a firm in a given year is regressed on the logarithm of its domestic sales in the corresponding year and different sets of controls. When no firm-specific controls are included in the regression, we expect to observe a positive relationship between a firm’s domestic sales in a given year and its volume of exports. This positive relationship is indeed observed in column 1 of Table E.1, in which we estimate an elasticity of export flows with respect to domestic sales of 0.645. The only controls in that regression are sector-year and municipality-year fixed effects.

In the remaining columns of Table E.1, we control for various sources of marginal cost and demand heterogeneity across firms, with the aim of attenuating the biases identified in equation (E.8). In column 2, we introduce firm fixed effects, thus controlling for differences in firm characteristics that are constant over time and that may impact their productivity, factor prices and demand shifters. The resulting estimated elasticity is very close to zero, \(-0.074\), consistent with the predictions of the constant marginal cost model. Columns 3 and 4 additionally control for observed time-varying determinants of firms’ marginal costs. Specifically, we control in column 3 for a measure of the firm’s productivity (estimated, as in the main text, following the procedure in Gandhi et al., 2016), and we additionally control in column 4 for a measure of the firm’s average
wages (reported by the firm in its financial statement). Consistent with the results in the boom to bust regressions in the main text, controlling for these supply shocks reduces the OLS estimate of the coefficient on domestic sales in regression (E.7), which goes down to \(-0.263\). This indicates that, once we control for the firm’s supply shocks, domestic sales and exports are negatively correlated. Columns 5 and 6 aim to additionally control for unobserved determinants of firms’ marginal costs that are time varying. To do so, we additionally include firm-specific time trends as controls. The resulting estimates are again lower and indicate that a 10% decrease in a firm’s domestic sales, keeping its productivity and average wages constant, implies a 3.19% increase in its aggregate export flows.

Table E.1: Intensive Margin

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Ln(Exports)</th>
<th>ΔLn(Exports)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) (2) (3) (4) (5) (6) (7) (8)</td>
<td></td>
</tr>
<tr>
<td>Ln(Domestic Sales)</td>
<td>0.645*** -0.074*** -0.231*** -0.263*** -0.228*** -0.319***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.013) (0.017) (0.016) (0.015) (0.016) (0.016)</td>
<td></td>
</tr>
<tr>
<td>Ln(TFP)</td>
<td>1.031*** 1.344*** 1.061***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.042) (0.048) (0.063)</td>
<td></td>
</tr>
<tr>
<td>Ln(Average Wages)</td>
<td>-0.627*** -0.463***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.041) (0.047)</td>
<td></td>
</tr>
<tr>
<td>ΔLn(Domestic Sales)</td>
<td>-0.228*** -0.320***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.012) (0.013)</td>
<td></td>
</tr>
<tr>
<td>ΔLn(TFP)</td>
<td>0.917***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td></td>
</tr>
<tr>
<td>ΔLn(Average Wages)</td>
<td>-0.408***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td></td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by firm in parentheses. Exports, domestic sales and average wages are in constant 2011 euros. For any variable \(X\), \(Δ\text{Ln}(X)\) is the difference in \(\text{Ln}(X)\) between two consecutive years. Significance levels: *** \(p<0.01\), ** \(p<0.05\), * \(p<0.1\).

The last two columns in Table E.1 re-estimate the regression models in columns 2 and 6 using a specification in first-differences (instead of in levels). The differences between the coefficients on the domestic sales covariates in columns 2 and 7 (higher in the specification in levels than in that in first differences) reflect the fact that, while some of the missing covariates in these two specifications (i.e., firms’ time-varying productivity and average wages) are strongly serially correlated and share common underlying trends with the corresponding firm’s domestic sales, their year-to-year variation is less correlated with the yearly changes in domestic sales. Consistently with this interpretation, once we control for these serially correlated determinants of firms’ marginal costs, the coefficient on domestic sales in the levels specification (column 6) becomes very similar to that in the first-differences specification (column 8). Given that the specifications in columns 6 and 8 yield very
similar estimates, but the latter is computationally easier to estimate, we focus on the specification in first differences in the remaining tables presented in this Appendix.

One might be concerned that because total sales is a key input in the computation of our firm-level measure of TFP, our empirical results are just unveiling a mechanical negative correlation between exports and domestic sales once one holds total sale revenue constant (by controlling for it). Although log TFP and log total sales are obviously positively correlated (as one would expect in light of our model), the correlation is far from perfect, particularly when considering log changes in these variables, as would be relevant in columns (7) and (8) and for the rest of specifications explored in the paper. More specifically, the correlation between log changes in TFP and log total sales is 0.31. Notice also that columns (5) and (7) in Table E.1 unveil a negative correlation between log changes in exports and log changes in domestic sales even when not controlling for log changes in TFP.

Table E.2: Intensive Margin - Heterogeneity Effects

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>∆Ln(Exports)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>∆Ln(Domestic Sales)</td>
<td>-0.320***</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
</tr>
<tr>
<td>∆Ln(TFP)</td>
<td>0.917***</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
</tr>
<tr>
<td>∆Ln(Average Wages)</td>
<td>-0.408***</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
</tr>
<tr>
<td>∆Ln(Domestic Sales) × Large-Size Dummy</td>
<td>-0.042*</td>
</tr>
<tr>
<td>∆Ln(Domestic Sales × High 2001 Prov-Sec Export Share</td>
<td>0.071***</td>
</tr>
<tr>
<td>∆Ln(Domestic Sales × Bust Dummy</td>
<td>-0.052**</td>
</tr>
<tr>
<td>Observations</td>
<td>54,276</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.368</td>
</tr>
<tr>
<td>Firm FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Sector-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Municipality-Year FE</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Note: Standard errors clustered by firm in parentheses. Domestic sales and average wages are in constant 2011 euros. For any variable X, ∆Ln(X) is the difference in Ln(X) between two consecutive years. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

In Table E.2, we explore a few sources of heterogeneity in the OLS estimates in Table E.1. Column 2 explores the extent to which the negative elasticity between the growth in domestic sales and the growth in export flows reported in column 8 of Table E.1 (and replicated in column 1 of Table E.2) is heterogeneous across firms depending on their size. Specifically, we estimate different elasticities for firms with less than 50 employees (small), and firms with 50 or more employees (large). We classify firms using their average number of employees during the years they are active. The results in column 2 indicate that large firms’ exports react more on average to changes in their domestic sales than small firms’ exports do, though the difference is modest and only statistically
significant at the 10% level. In column 3, we study how the effect of changes in domestic sales interacts with a pre-determined measure of the propensity of a firm to export, namely, whether the export share in their province and sector in 2001 (prior to beginning of our sample) was above or below the median sector-province-level export share in that year. Our results indicate that the substitution of exports for domestic sales is significantly larger for firms that were less likely to be exporting at the beginning of the sample.\(^2\) Intuitively, and as discussed in the main text, to recoup the sale revenue associated with a given percentage drop in domestic sales would require a larger percentage increase in exports for firms with smaller initial export shares.\(^3\) Finally, in column 4 we show that the elasticity of exports to changes in domestic sales is larger (in absolute value) in the “bust” years (2002-2008) than in the “boom” years (2009-2013). Needless to say, the heterogeneous effects displayed in Table E.2 should be interpreted with caution given that identification concerns may be aggravated by the endogenous nature of the interaction variables.

Figure E.1: Intensive Margin - Heterogeneity by Sector

Note: The dotted vertical line reflects the average estimate reported in column 8 of Table E.1. The black dots reflect the sector-specific point estimates; the red lines reflect the 95% confidence interval for each of the sectoral estimates.

Figure E.1 complements this analysis by illustrating the estimates of sector-specific elasticities of exports with respect to domestic sales (see Table E.3 in Appendix D for the associated regression tables).\(^4\) The main conclusion is that the negative elasticity between domestic sales and exports documented in Tables E.1 and E.2 is pervasive across nearly all manufacturing sectors, the only exception being the “Pharmaceutical Products” sector, whose 95% confidence interval is such that we cannot reject the null hypothesis that, after controlling for firm-specific fixed effects and time trends and observed measures of productivity and labor costs, domestic sales and exports are

---

\(^2\)We find similar results when measuring a firm’s propensity to export directly via its pre-sample (2001) export share.

\(^3\)When adding the interactions of the change in log domestic sales with the size dummies, the coefficients in columns 3 and 4 are not materially affected.

\(^4\)We exclude the tobacco and petroleum refining sectors (two-digit industry codes 12 and 19, respectively) due to the extremely low number of firms in those sectors.
Table E.3: Intensive Margin - Heterogeneity by Sector

### Table E.3: Intensive Margin - Heterogeneity by Sector

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Food</th>
<th>Beverages</th>
<th>Textiles</th>
<th>Clothing</th>
<th>Leather</th>
<th>Wood</th>
<th>Paper</th>
<th>Printing</th>
</tr>
</thead>
<tbody>
<tr>
<td>ΔLn(Domestic Sales)</td>
<td>-0.244***</td>
<td>-0.325***</td>
<td>-0.264***</td>
<td>-0.447***</td>
<td>-0.156***</td>
<td>-0.234***</td>
<td>-0.565***</td>
<td>-0.531***</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.082)</td>
<td>(0.048)</td>
<td>(0.105)</td>
<td>(0.029)</td>
<td>(0.056)</td>
<td>(0.117)</td>
<td>(0.097)</td>
</tr>
<tr>
<td>ΔLn(TFP)</td>
<td>0.750***</td>
<td>0.243</td>
<td>0.578***</td>
<td>0.827***</td>
<td>0.449***</td>
<td>0.512***</td>
<td>1.093***</td>
<td>1.082***</td>
</tr>
<tr>
<td></td>
<td>(0.101)</td>
<td>(0.201)</td>
<td>(0.143)</td>
<td>(0.217)</td>
<td>(0.138)</td>
<td>(0.196)</td>
<td>(0.192)</td>
<td>(0.320)</td>
</tr>
<tr>
<td>ΔLn(Average Wages)</td>
<td>-0.350***</td>
<td>0.091</td>
<td>-0.464***</td>
<td>-0.179</td>
<td>-0.225***</td>
<td>-0.003</td>
<td>-0.585***</td>
<td>-0.620***</td>
</tr>
<tr>
<td></td>
<td>(0.065)</td>
<td>(0.146)</td>
<td>(0.102)</td>
<td>(0.168)</td>
<td>(0.078)</td>
<td>(0.125)</td>
<td>(0.211)</td>
<td>(0.224)</td>
</tr>
<tr>
<td>Observations</td>
<td>7,578</td>
<td>1,729</td>
<td>2,489</td>
<td>968</td>
<td>2,620</td>
<td>1,725</td>
<td>1,434</td>
<td>1,258</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.302</td>
<td>0.361</td>
<td>0.379</td>
<td>0.382</td>
<td>0.348</td>
<td>0.402</td>
<td>0.341</td>
<td>0.343</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
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<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>ΔLn(Domestic Sales)</td>
<td>-0.357***</td>
<td>-0.378*</td>
<td>-0.456***</td>
<td>-0.426***</td>
<td>-0.241***</td>
<td>-0.409***</td>
<td>-0.321***</td>
</tr>
<tr>
<td></td>
<td>(0.048)</td>
<td>(0.214)</td>
<td>(0.065)</td>
<td>(0.054)</td>
<td>(0.066)</td>
<td>(0.040)</td>
<td>(0.082)</td>
</tr>
<tr>
<td>ΔLn(TFP)</td>
<td>1.125***</td>
<td>0.973***</td>
<td>1.238***</td>
<td>0.910***</td>
<td>0.870***</td>
<td>1.424***</td>
<td>1.023***</td>
</tr>
<tr>
<td></td>
<td>(0.203)</td>
<td>(0.305)</td>
<td>(0.129)</td>
<td>(0.162)</td>
<td>(0.140)</td>
<td>(0.129)</td>
<td>(0.249)</td>
</tr>
<tr>
<td>ΔLn(Average Wages)</td>
<td>-0.490***</td>
<td>-0.448*</td>
<td>-0.514***</td>
<td>-0.271**</td>
<td>-0.338**</td>
<td>-0.493***</td>
<td>-0.360**</td>
</tr>
<tr>
<td></td>
<td>(0.112)</td>
<td>(0.250)</td>
<td>(0.104)</td>
<td>(0.118)</td>
<td>(0.133)</td>
<td>(0.090)</td>
<td>(0.146)</td>
</tr>
<tr>
<td>Observations</td>
<td>4,854</td>
<td>1,166</td>
<td>3,979</td>
<td>3,190</td>
<td>3,190</td>
<td>7,226</td>
<td>1,490</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.286</td>
<td>0.305</td>
<td>0.345</td>
<td>0.388</td>
<td>0.325</td>
<td>0.283</td>
<td>0.321</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Sector</th>
<th>Electronics</th>
<th>Machine</th>
<th>Vehicles</th>
<th>Oth. Transp.</th>
<th>Furniture</th>
<th>Repair</th>
<th>Other</th>
</tr>
</thead>
<tbody>
<tr>
<td>ΔLn(Domestic Sales)</td>
<td>-0.414***</td>
<td>-0.280***</td>
<td>-0.367***</td>
<td>-0.332***</td>
<td>-0.452***</td>
<td>-0.351***</td>
<td>-0.369***</td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td>(0.024)</td>
<td>(0.048)</td>
<td>(0.102)</td>
<td>(0.092)</td>
<td>(0.075)</td>
<td>(0.062)</td>
</tr>
<tr>
<td>ΔLn(TFP)</td>
<td>0.911***</td>
<td>1.114***</td>
<td>0.992***</td>
<td>1.469***</td>
<td>1.227***</td>
<td>1.262***</td>
<td>1.562***</td>
</tr>
<tr>
<td></td>
<td>(0.259)</td>
<td>(0.146)</td>
<td>(0.202)</td>
<td>(0.322)</td>
<td>(0.231)</td>
<td>(0.309)</td>
<td>(0.399)</td>
</tr>
<tr>
<td>ΔLn(Average Wages)</td>
<td>-0.430*</td>
<td>-0.604***</td>
<td>-0.545***</td>
<td>-1.611***</td>
<td>-0.546***</td>
<td>-0.557***</td>
<td>-0.653**</td>
</tr>
<tr>
<td></td>
<td>(0.225)</td>
<td>(0.120)</td>
<td>(0.153)</td>
<td>(0.411)</td>
<td>(0.159)</td>
<td>(0.181)</td>
<td>(0.300)</td>
</tr>
<tr>
<td>Observations</td>
<td>2,307</td>
<td>7,502</td>
<td>2,833</td>
<td>600</td>
<td>1,407</td>
<td>1,287</td>
<td>1,211</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.336</td>
<td>0.241</td>
<td>0.309</td>
<td>0.444</td>
<td>0.354</td>
<td>0.349</td>
<td>0.388</td>
</tr>
</tbody>
</table>

Note: All specifications contain firm fixed effects and firm-year fixed effects. Standard errors clustered by firm in parentheses. Exports, domestic sales and average wages are in constant 2011 euros. For any variable \( X \), \( \Delta \text{Ln}(X) \) is the difference in \( \text{Ln}(X) \) between two consecutive years. Significance levels: *** \( p<0.01 \), ** \( p<0.05 \), * \( p<0.1 \).

Independent from each other. For all remaining sectors, the estimated elasticity of interest oscillates between \(-0.156\) (manufacture of leather and related products) and \(-0.565\) (manufacture of paper and paper products).

### E.3 Extensive Margin

Local demand shocks may not only generate an intensive margin change in the export volume of those firms participating in export markets but may also lead firms to either start exporting or to stop participating in foreign markets, thus affecting the extensive margin of trade. We next explore
Table E.4: Extensive Margin

<table>
<thead>
<tr>
<th>Model:</th>
<th>Conditional Logit</th>
<th>Linear</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ln(Domestic Sales)</td>
<td>1.184*** 0.141***</td>
<td>-0.038***</td>
</tr>
<tr>
<td></td>
<td>(0.008) (0.018)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Log TFP</td>
<td>1.530*** 2.134***</td>
<td>0.064***</td>
</tr>
<tr>
<td></td>
<td>(0.050) (0.063)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Log Average Wages</td>
<td>-0.982*** -0.033***</td>
<td>-0.033***</td>
</tr>
<tr>
<td></td>
<td>(0.050) (0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Lagged Participation</td>
<td>0.161***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td></td>
</tr>
</tbody>
</table>

Observations: 747,519 129,712 129,712 129,712 129,712 747,519 495,151

Firm FE: No Yes Yes Yes Yes Yes Yes
Sector-Year FE: Yes No Yes Yes Yes Yes Yes
Firm-specific trends: No No No No No Yes No
Elasticities: 1.035 0.016 0.013 -0.150 -0.312 -0.234 -0.284

Note: Columns 1 to 5 present Maximum Likelihood estimators of the corresponding conditional logit model. Column 2 to 5 specifically present estimates computed following the procedure in Chamberlain (1980). Column 6 presents OLS estimates of the corresponding linear probability model. Column 7 presents estimates computed following the procedure in Arellano and Bond (1991). The number of observations in columns 1, 6 and 7 correspond to the number of firm-years that we observe in our sample when taking into account all firms. The number of observations in columns 2 to 5 correspond to the number of firm-years that we observe in our sample when taking into account only those firms that change their export status at least once during the sample period. Standard errors in parentheses. Exports, domestic sales and average wages are in constant 2011 euros. For any variable \( X \), \( \Delta \text{Ln}(X) \) is the difference in Ln(X) between two consecutive years. Significance levels: *** \( p < 0.01 \), ** \( p < 0.05 \), * \( p < 0.1 \).

The results regarding the impact of domestic sales on the extensive margin of exports are similar to those described above for its impact on the intensive margin of exports. When we do not include any control for firm-specific marginal costs, we observe a positive correlation between a firm’s domestic sales and its probability of exporting. As columns 2 and 3 in Table E.4 illustrate, controlling either for firm fixed effects only or for firm and sector-year fixed effects reduces the coefficient on domestic sales in absolute value but preserves its positive sign. Nevertheless, when we include controls for observable time-varying determinants of a firm’s marginal cost, the elasticity of export participation with respect to domestic sales becomes negative. The most general conditional logit specification that we run accounts for firm fixed effects, sector-year fixed effects, and firm-year specific measures of productivity and average wages (column 5); the resulting elasticity of the export probability with respect to domestic sales is \( -0.312 \). \(^5\)

\(^5\)As indicated in the notes to Table E.4, the parameters in columns 2 to 5 have been estimated following the procedure in Chamberlain (1980). This estimation procedure maximizes a conditional likelihood function that does not depend on the firm fixed effects and, consequently, does not yield estimates of these unobserved effects. However, given the nonlinear nature of the model, the elasticity of the export probability with respect to domestic sales does depend on these unobserved effects. For the exclusive purposes of computing the elasticities reported in the last row of Table E.4, we have set all these unobserved effects equal to zero.
Contrary to the specifications discussed in Section E.2, those discussed in columns 1 to 5 of Table E.4 do not account for firm-specific linear time trends. Accounting for both firm fixed effects and firm-specific time trends in a conditional logit model would be problematic for two reasons. First, it would be computationally very challenging. Second, it would give rise to an incidental parameters problem (Chamberlain, 1980), resulting in inconsistent estimates of the elasticity of export participation with respect to domestic sales.\(^6\) Consequently, to test the robustness of our estimates to accounting for firm-specific time trends, we resort to the linear probability model specification. The estimates in column 6 of Table E.4 predict an elasticity of export participation with respect to domestic sales of \(-0.234\), very similar to that predicted by the conditional logit model in column 5.

As shown in Das et al. (2007) and Morales at al. (2017), the export decision of firms is dynamic, depending both on their prior export status as well as on their expectations of future potential profits that a firm may earn by entering export markets. While correctly accounting for firms’ expectations of future export profits is beyond the scope of this paper (see Dickstein and Morales, 2017), accounting for the prior export status of each firm only requires additionally controlling for a dummy that captures each firm’s one-year lagged export participation (see Roberts and Tybout, 1997). We introduce this control in column 7 of Table E.4: the resulting estimate of the export participation elasticity with respect to domestic sales is \(-0.284\), very similar to those obtained in columns 5 and 6.

We have also explored whether the elasticities estimated in Table E.4 are heterogeneous across firms in a manner analogous to our analysis in Table E.2. The results are reported in Table E.5. We find, quite intuitively, that the extensive margin of trade is more responsive to changes in domestic demand for small relative to large firms. Conversely, we observe tiny differences in responsiveness by firms with high vs. low outward orientation (as measured by sector-province-level export shares). Similarly, the elasticity is also slightly larger in the bust period relative to the boom period, but the difference is very small.

In sum, our OLS results in Tables E.1, E.2 and E.4, and Figure E.1 demonstrate the existence of a strong, within-firm negative relationship between demand-driven changes in domestic sales and changes in the intensive and extensive margin of exports. As discussed in section 2, however, it is reasonable to expect that these OLS estimates underestimate the extent to which reductions in domestic demand generate expansions in export markets.

\(^6\)Charbonneau (2017) introduces a new estimator that allows to consistently estimate binary logit models in the presence of an individual-specific fixed effect and a choice-specific fixed effect. This estimator does not apply to our context, in which both sets of unobserved effects are firm-specific.
Table E.5: Extensive Margin - Heterogeneous Effects

<table>
<thead>
<tr>
<th></th>
<th>Exporting Status</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Ln(Domestic Sales)</td>
<td>-0.033***</td>
<td>-0.036***</td>
<td>-0.029***</td>
<td>-0.027***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Ln(TFP)</td>
<td>0.057***</td>
<td>0.057***</td>
<td>0.057***</td>
<td>0.060***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Ln(Average Wages)</td>
<td>-0.029***</td>
<td>-0.029***</td>
<td>-0.028***</td>
<td>-0.030***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Ln(Domestic Sales) ×</td>
<td>0.028***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Large-Size Dummy</td>
<td></td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ln(Domestic Sales) ×</td>
<td></td>
<td></td>
<td>-0.009***</td>
<td></td>
</tr>
<tr>
<td>High 2001 Prov-Sec Export Share</td>
<td></td>
<td>(0.002)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ln(Domestic Sales) ×</td>
<td></td>
<td></td>
<td></td>
<td>-0.010***</td>
</tr>
<tr>
<td>Bust Dummy</td>
<td></td>
<td></td>
<td></td>
<td>(0.001)</td>
</tr>
<tr>
<td>Lagged Exporting Status</td>
<td>0.161***</td>
<td>0.160***</td>
<td>0.161***</td>
<td>0.153***</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
</tbody>
</table>

Observations: 624,058 624,058 624,058 624,058
Number of firms: 103,179 103,179 103,179 103,179
Firm FE: Yes Yes Yes Yes
Sector-Year FE: Yes Yes Yes Yes
Province FE: Yes Yes Yes Yes
Elasticity: -0.331 -0.354 -0.287 -0.274

Note: All specifications contain firm fixed effects and year fixed effects. Standard errors clustered by firm in parentheses. Exports, domestic sales and average wages are in constant 2011 euros. For any variable \( X \), \( \Delta \text{Ln}(X) \) is the difference in \( \text{Ln}(X) \) between two consecutive years. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.