American Economic Review 2021, 111(11): 3611–3662
https://doi.org/10.1257/aer.20181853

Venting Out: Exports during a Domestic Slump[†]

By Miguel Almunia, Pol Antràs,
David Lopez-Rodriguez, and Eduardo Morales*

We study the relationship between domestic-demand shocks and exports using data for Spanish manufacturing firms in 2002–2013. Exploiting plausibly exogenous geographical variation caused by the Great Recession, we find that firms whose domestic sales declined by more experienced a larger increase in export flows, controlling for firms' supply determinants. This result illustrates the capacity of export markets to counteract the negative impact of local demand shocks. By structurally estimating a heterogeneous-firm model of exporting with nonconstant marginal costs of production, we conclude that these firm-level responses accounted for half of the spectacular increase in Spanish goods exports over the period 2009–2013. (*JEL* D22, E32, F14, L60)

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 the southern economies of the European Monetary Union did. Spain is a case in point. From its peak in 2008, Spain's real GDP fell by an accumulated 8.9 percent in the following five years, until bottoming out in 2013. During the same period, private final consumption contracted by 14.0 percent. Portugal and Greece also experienced marked contractions between 2008 and 2013, with their GDPs shrinking by 7.9 percent and 26.3 percent, respectively.

*Almunia: CUNEF Universidad and CEPR (email: miguel.almunia@cunef.edu); Antràs: Harvard University, NBER, and CEPR (email: pantras@fas.harvard.edu); Lopez-Rodriguez: Banco de España (email: david.lopezr@bde.es); Morales: Princeton University, NBER, and CEPR (email: ecmorale@princeton.edu). Emi Nakamura was the coeditor for this article. We thank Pablo García-Guzmán, María Jesús González Sanz, and Xiang Zhang for excellent research assistance; Rafael Frutos and Francesco Serti for help with the tax records of firm-level sales within Spain; Carlos Llano for help with the C-Intereg data; and the coeditor, three anonymous referees, Antoine Berthou, Rafael Dix-Carneiro, Fernando Leibovici, Tim Schmidt-Eisenlohr, and Daniel Ramos for their detailed comments and discussions. We are also grateful to Olivier Blanchard, Kirill Borusyak, Olympia Bover, James Fenske, Clément Imbert, Wolfgang Keller, Danilo Leiva-León, Sergio Mayordomo, Enrique Moral, Michael Peters, Matías Pacce, Pedro Portugal, Felix Tintelnot, Alberto Urtasun; and seminar audiences at the EEA meetings in Lisbon, the EIIT conference in Washington, DC, Bank of Spain, Hitotsubashi, Warwick, CUNEF, Georgetown, Austin, IE Business School, University of Michigan, Banco de la República (Bogotá), CEMFI, Vienna, Vanderbilt, the NBER Summer Institute, UIBE (Beijing), UQAM (Montreal), Princeton, Harvard, the ECB, Erasmus University of Rotterdam, LSE, RIETI (Tokyo), Bilkent, Nebrija, Munich, Maryland, Boston College, UAM, UCM-ICAE, and the Philly Fed for useful comments. Finally, we particularly thank Óscar Arce for his continuous support throughout this project. Financial support from the Spanish Ministerio de Ciencia e Innovación (grant PID2019-111616GA-I00) is gratefully acknowledged. All errors are our own. Any views expressed in this paper are only those of the authors and should not be attributed to the Banco de España or the Eurosystem.
[†]Go to https://doi.org/10.1257/aer.20181853 to visit the article page for additional materials and author disclosure statements.

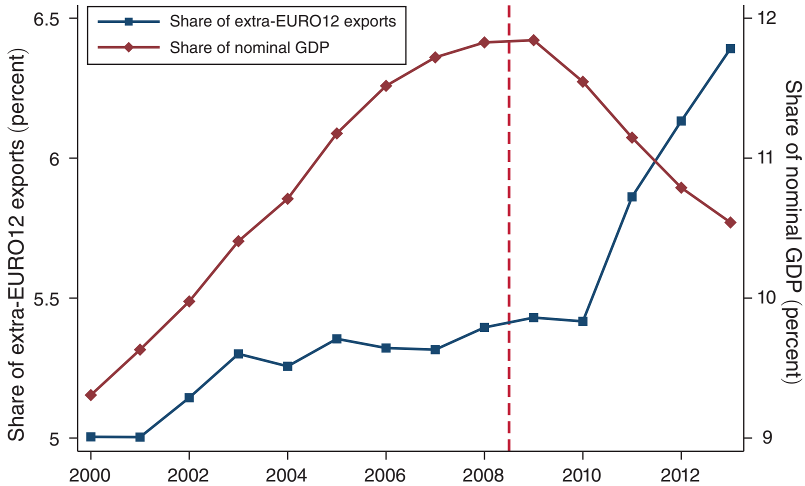

Figure 1. The Spanish Export Miracle

Despite these severe domestic slumps, merchandise exports in these economies demonstrated a remarkable resilience and partly contributed to mitigating the effects of the Great Recession. In the Spanish case, after tumbling by 11.5 percent in real terms during the global trade collapse of 2008–2009, Spanish merchandise exports quickly recovered and grew by 30.7 percent in real terms between 2009 and 2013. Overall, real Spanish merchandise exports grew by an accumulated 15.6 percent during the 2008–2013 period, while real merchandise exports in the rest of the euro area increased by only 6.8 percent during the same years. As a result, and 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 (especially in 2010–2013), despite the contemporaneous decline in the relative weight of Spain's GDP in the euro area's GDP. Very similar patterns are observed for the cases of Portugal and Greece.[1]

At first glance, this remarkable export performance appears to be consistent with the goals of the type of "internal devaluation" processes advocated by international organizations (such as the International Monetary Fund, the European Central Bank, or the European Commission) since the onset of the crisis. According to these organizations, wage moderation coupled with a set of structural reforms (most notably labor market reforms) were expected to lead to a fall in relative unit labor costs, allowing southern European firms to reduce their relative export prices and increase their market shares abroad. Nevertheless, in the Spanish case, the adjustment in labor costs achieved via these policies is estimated to have been modest up to 2013, and this channel is believed to have had a limited contribution to export

[1] In online Appendix D.1, we replicate Figure 1 for Portugal, Greece, Ireland, Italy, Germany, France, and the Netherlands. For Portugal and Greece, and less clearly for Germany and France, we observe a negative relationship between their GDP shares in the euro area and their shares in euro area exports of goods to other countries. See online Appendix D.1 for a description of the data sources underlying these figures.

growth over the period 2010–2013 (see, for instance, International Monetary Fund 2015, 2018; Salas 2018).

What explains then the remarkable export growth in Spain over the period 2010–2013? An often-invoked explanation relates the growth in exports *directly* to the collapse in domestic demand.[2] According to this hypothesis, the unexpected demand-driven reduction in firms' domestic sales, in combination with the irreversibility of certain investments in inputs, freed up capacity that these firms used to serve customers abroad. More precisely, this explanation posits that, as domestic demand fell, 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 exports.[3]

This alternative 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.[4] Despite its intuitive nature and distinguished lineage, 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 economies of scale—assume that firms face constant marginal costs of production, an assumption that implies that demand shocks in one market do not affect a firm's sales in another market.

In this paper, we leverage Spanish firm-level data from 2002 to 2013 to study the empirical relevance of the "vent-for-surplus" mechanism. To do so, and given the contrasting macroeconomic behavior in the pre- and postcrisis period, we first divide our sample into a "boom" period (2002–2008) and a "bust" period (2009–2013), and aim to measure the extent to which, at the firm level, a demand-driven decline in the domestic sales in the bust period relative to the boom period is associated with an increase in export sales over the two periods.

To guide us in the specification of an empirical strategy to estimate the causal impact of demand-driven changes in domestic sales on exports, we first rely on a commonly used model of firms' export behavior à la Melitz (2003). This framework helps us identify several empirical challenges that one encounters when measuring the relevance of the "vent-for-surplus" mechanism.[5] We draw two main conclusions

[2] See "La exportación como escape" in El País, January 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, May 27, 2018 (http://elpais.com/economia/2018/05/25/actualidad/1527242520_600876.html).

[3] 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. This effect is distinct from that of an "internal devaluation," which is associated with a downward *shift* in firms' marginal cost or supply curves caused by a reduction in the price of factors of production.

[4] In *The Wealth of Nations,* Smith (1776, p. 403) 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* (Mill 1848) and popularized by Williams (1929) and Myint (1958).

[5] 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 in Section VI, 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 increasing marginal costs of production.

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 is not informative about the impact of demand-driven changes in the former on the latter. Second, an instrumental variable (IV) approach identifies the causal impact of demand-driven changes in domestic sales on exports as long as the instrument satisfies two conditions: (i) it is a good predictor of the domestic sales of Spanish firms, and (ii) it is not correlated with firms' unobserved marginal cost or export-demand shifters.

With these considerations in mind, we first show via ordinary least squares (OLS) regressions that, at the firm level, a decline in the domestic sales in the bust period relative to the boom period is associated with an increase in export sales over the two periods. 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 account for potential internal devaluation effects. However, it is likely that there are additional marginal cost shifters that we do not observe and, thus, the estimated negative association between boom-to-bust changes in domestic sales and exports likely underestimates the true positive impact on the firm's exports of demand-driven reductions in its domestic sales.

To better estimate the export impact of demand-driven changes in domestic sales, we rely on two IVs that exploit the fact that the Great Recession affected different geographical areas in Spain differentially. Both IVs rely on municipality-level registration data on a major household durable consumption item, vehicles, as a proxy for the extent to which the Great Recession affected the demand for Spanish manufacturing goods in each municipality. The use of vehicle purchases as a proxy for "local demand" is justified by an extensive literature in empirical macroeconomics documenting that consumption of durable goods such as vehicles is strongly procyclical (see, for instance, the survey by Stock and Watson 1999). More recently, Mian, Rao, and Sufi (2013); Hausman, Rhode, and Wieland (2019); and Waugh (2019) have also documented a strong link between wealth (or income) shocks and vehicle consumption. While both of our IVs rely on vehicle registration as a proxy for local demand, they differ in the assumed exposure of each firm to changes in local demand across different Spanish municipalities.

To measure the extent to which firms were differentially exposed to the demand shocks that different Spanish municipalities experienced in the years around the Great Recession, we use firm-to-municipality manufacturing sales data (from tax records within Spain) for the year 2006. A clear feature of these data is that shipments by Spanish manufacturing firms are extremely localized, consistently with the facts documented in Hillberry and Hummels (2008) for the United States and Díaz-Lanchas, Llano, and Zofío (2019) for Spain. Firms' shipments within their municipality of location are on average five times larger than those to any other municipality, after controlling for bilateral distance and population. Consequently, our first IV uses boom-to-bust changes in the stock of vehicles per capita in a municipality as an instrument for the boom-to-bust changes in the Spain-wide sales of firms located in that same municipality.

A second clear pattern in the firm-to-municipality manufacturing sales data is that a large fraction of Spanish firms ship outside of their municipality the bulk of their domestic sales. Consequently, we also use a second, model-based, IV that, for each firm, equals a weighted sum of our proxy for the municipality-specific local demand shocks, using as weights gravity-based estimates of the forces the determine the size of the bilateral trade flows between any two Spanish municipalities. We compute these gravity-based estimates using municipality-to-municipality sales data (aggregated from the firm-to-municipality tax records). These estimates reveal a significant amount of "home bias" *within Spain*, with shipments declining in distance with an elasticity of around -0.4 even after controlling for the discontinuity in sales observed when shipping outside a firm's municipality. To ensure that this alternative instrument exploits distinct variation than the first one, we assign a zero weight to the municipality where the firm is located: this second instrument is thus a weighted-sum of the stock of vehicles per capita in all municipalities *other than* the municipality in which the firm is located.

The first-stage estimates for both of our IVs suggest that both of them are strong (with F-statistics above 30 in both cases). Armed with these first-stage results, we then show that a larger *demand-driven* drop in domestic sales in the bust period relative to the boom period is associated with a significantly larger growth in export sales from boom to bust (conditional on exporting in both periods). Furthermore, the IV estimates based on our two instruments are significantly larger than the OLS ones. This is consistent with the biases predicted by our baseline Melitz-style model in the plausible scenario in which our covariates only imperfectly control for a firm's supply determinants. Specifically, depending on which of the two IVs described above we use, we obtain estimates of the intensive-margin elasticity of exports to domestic sales in the neighborhood of -1.3 or -1.6, while the OLS one is around -0.3. Besides our baseline two-period specification, which exploits changes in exports, domestic sales, and our IVs between the boom and bust periods, we also present panel estimates that exploit the annual frequency of our data, and which allow for the inclusion of municipality-specific time trends as additional controls. Although our annual panel OLS estimates are very similar to those obtained in the boom-to-bust specification, our IV estimates are not, the reason being that our instruments lose their predictive power at the annual frequency. Nevertheless, when considering rolling averages over two or three years, our instruments regain strength and the resulting IV estimates resemble those in the boom-to-bust specifications even when municipality-specific trends are accounted for.

A potential challenge to our identification approach is that the boom-to-bust changes in our instruments 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. This is arguably less problematic for our second instrument, which does not use information on demand changes in the firm's own municipality when constructing the instrument, but even in that case one may still be concerned about spatial correlation in supply shocks posing a threat to identification. With that in mind, we provide in Section V additional pieces of evidence that address some specific sources of endogeneity that could affect the validity of our instruments.

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. Specifically, if a significant share of vehicles is sold in the vicinity of where they are produced, 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 nearby purchases of new vehicles. Our results are robust to this identification threat. Both the relevance of our instrument and the finding of a sizable negative elasticity between domestic sales and exports are robust to excluding firms that could have been impacted by production costs shocks affecting car manufacturers.

Second, although we control for firm-specific average wages in all specifications, compositional changes in firms' workforces may have caused changes in effective labor costs that our average wage measure does not capture correctly. Indeed, an important feature of the Spanish labor market is the division of the workforce into permanent and temporary workers, the latter group being typically less productive than the former (see Dolado, García-Serrano, and Jimeno 2002). We find, however, that the elasticity of exports with respect to domestic sales remains 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 exporters or for proxies of trade credit available to firms does not materially affect our main estimates. In addition, we also explore in Section V the robustness of our results to alternative constructions of our IV that rely on different proxies for the exposure of a firm to demand shocks in municipalities other than its municipality of location, as well as to alternative proxies for a firm's total factor productivity.

Having established a causal link between changes in domestic demand and exports, we generalize our baseline model à la Melitz (2003) to allow for nonconstant 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 firm's marginal cost function is related to the elasticity of output with respect to all flexible factors. Furthermore, we demonstrate how to estimate the curvature of the marginal cost function using a simple variant of our IV estimator. Consistently with our microfoundation, we find that our estimate of this curvature is smaller for firms whose output elasticity with respect to flexible factors is larger, though the statistical significance of this differential effect is sensitive to which factors one classifies as "flexible."

Finally, we employ our model with increasing marginal costs and the corresponding IV estimates to quantitatively evaluate the importance of the "vent-for-surplus" mechanism in explaining the 2009–2013 export miracle in Spain. More specifically, we implement a variance-decomposition exercise to determine the extent to which the domestic slump in Spain was driven by demand versus supply shocks. We then use our model to predict the boom-to-bust growth in Spanish exports that we would have observed if there had been no change in demand between the boom and bust periods. We find that, in this case, the growth in Spanish exports would have been 51.71 percent smaller than what we observe in the data and, thus, we conclude that slightly more than half of the Spanish export miracle of the period 2009–2013 can be attributed to the "vent-for-surplus" mechanism.

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*.[6] 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, Claro, and Horstmann (2013); Soderbery (2014); and Ahn and McQuoid (2017). Relative to this prior literature, our paper exploits plausibly exogenous variation in demand during a particularly salient episode to identify the causal effect of a demand-driven drop in domestic sales on exports. Additionally, it provides an approach to identify and structurally estimate the slope of firms' short-run marginal cost curves. Relatedly, in contemporaneous work, Fan et al. (2020) 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, using French data over the period 1995–2001, Berman, Berthou, and Héricourt (2015) document a *positive* causal effect of changes in firm-level exports on firm-level domestic sales. 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. In online Appendix H.1, we use data on Spanish firms for the period 2002–2007 to perform an analysis analogous to that in Berman, Berthou, and Héricourt (2015), and we find no evidence supporting the positive causal relationship between exports and domestic sales that these authors previously found; on the contrary, for most specifications, we find a *negative* causal effect of (plausibly) exogenous changes in exports on domestic sales, in line with our core finding of substitution between exports and domestic sales.[7]

The rest of the paper is structured as follows. In Section I, we lay out a baseline model of firm behavior in the spirit of Melitz (2003). In Section II, we introduce our firm-level data and, in Section III, we develop our core IV estimation approach. Our main results are presented in Section IV, while we present additional evidence in favor of the "vent-for-surplus" mechanism in Section V. In Section VI, we generalize our baseline model to allow for nonconstant marginal costs. In Section VII, we use this extended framework to quantify the contribution of the "vent-for-surplus" channel in the growth of Spanish exports. We offer some concluding remarks in Section VIII.

[6] A broader search to include top general-interest journals identified Neary and Schweinberger (1986).

[7] Our paper also relates to a prior literature describing the behavior of firm-level exports in Spain around the Great Recession, including Antràs (2011), Myro (2015), Eppinger et al. (2018), and de Lucio et al. (2017a, b).

I. 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, we first consider the implications of a model of exporting with heterogeneous firms along the lines of Melitz (2003), 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 evidence contradictory with this assumption, we will develop in Section VI an extension of this benchmark model that allows for nonconstant marginal costs. Crucially, the lessons we learn in this section about the properties of different estimators will also apply in the more general model.

A. *Benchmark Model: Estimating Equation*

We index manufacturing firms producing in Spain by i, the sectors to which firms belong by s, and the two potential markets in which they may sell by $j = \{d,x\}$, with d denoting the domestic market and x denoting the export market. In principle, both the domestic and export markets are an aggregate of several destinations, but due to data limitations, we focus in the main text on this dichotomous case (we develop a multidestination extension of our model in online Appendix E.2).

In any given period, firm i faces the following isoelastic demand in market j,

$$Q_{ij} = \frac{P_{ij}^{-\sigma}}{P_{sj}^{1-\sigma}} E_{sj} \xi_{ij}^{\sigma-1}, \qquad \sigma > 1, \tag{1}$$

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 sector s price index in j, E_{sj} is the total sectoral expenditure in market j, and ξ_{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

$$c_{ij} Q_{ij} \qquad \text{with} \quad c_{ij} \equiv \tau_{sj} \frac{1}{\varphi_i} \omega_i, \tag{2}$$

where c_{ij} denotes the marginal cost to firm i of selling one unit of output in market j, τ_{sj} denotes an iceberg trade cost, φ_i denotes firm i's productivity, and ω_i is the firm-specific cost of a bundle of inputs. 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

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

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

$$\text{(3)} \qquad \ln R_{ix} = \ln \kappa + (\sigma - 1)\left(\ln \xi_{ix} + \ln \varphi_i - \ln \omega_i\right) - (\sigma - 1)\left(\ln \tau_{sx} - \ln P_{sx}\right) + \ln E_{sx}.$$

The bulk of our empirical analysis will compare firm-level export behavior in a *bust* period, relative to a *boom* period. 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

$$\text{(4)} \qquad \Delta \ln R_{ix} = (\sigma - 1)\left(\Delta \ln \xi_{ix} + \Delta \ln \varphi_i - \Delta \ln \omega_i\right) - (\sigma - 1)\left(\Delta \ln \tau_{sx} - \Delta \ln P_{sx}\right) + \Delta \ln E_{sx}.$$

To transition to an estimating equation, we model the change in firm-specific foreign demand, productivity and input bundle cost as follows:

$$\text{(5)} \qquad \begin{aligned} \Delta \ln \xi_{ix} &= \xi_{sx} + u_{ix}^{\xi}, \\ \Delta \ln \varphi_i &= \varphi_s + \delta_{\varphi} \Delta \ln \varphi_i^* + u_i^{\varphi}, \\ \Delta \ln \omega_i &= \omega_s + \delta_{\omega} \Delta \ln \omega_i^* + u_i^{\omega}. \end{aligned}$$

Note that we are decomposing these terms into (i) a sector fixed effect, (ii) an *observable* part of these terms for the case of productivity (φ_i^*) and for the input bundle cost (ω_i^*), and (iii) a residual term. We can thus rewrite equation (4) as

$$\text{(6)} \qquad \Delta \ln R_{ix} = \gamma_{sx} + (\sigma - 1)\,\delta_{\varphi} \Delta \ln \varphi_i^* - (\sigma - 1)\,\delta_{\omega} \Delta \ln \omega_i^* + \varepsilon_{ix},$$

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

$$\text{(7)} \qquad \varepsilon_{ix} = (\sigma - 1)\left[u_{ix}^{\xi} + u_i^{\varphi} - u_i^{\omega}\right].$$

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

$$\text{(8)} \qquad \Delta \ln R_{id} = \gamma_{sd} + (\sigma - 1)\,\delta_{\varphi} \Delta \ln \varphi_i^* - (\sigma - 1)\,\delta_{\omega} \Delta \ln \omega_i^* + \varepsilon_{id},$$

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

$$\text{(9)} \qquad \varepsilon_{id} = (\sigma - 1)\left[u_{id}^{\xi} + u_i^{\varphi} - u_i^{\omega}\right].$$

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: demand-driven changes in $\ln R_{id}$ have no causal effect on $\ln R_{ix}$. However, many estimators of the impact of log domestic sales on log exports based on observational data will differ from zero and, thus, will be biased, 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):

$$(10)\quad \Delta \ln R_{ix} = \gamma_{sx} + (\sigma - 1)\delta_{\varphi}\Delta \ln \varphi_i^* - (\sigma - 1)\delta_{\omega}\Delta \ln \omega_i^* + \beta \Delta \ln R_{id} + \varepsilon_{ix}.$$

From equations (7), (9), and (10), the probability limit of the OLS estimator of the coefficient on domestic sales can be written as

$$(11)\qquad p\lim\left(\hat{\beta}_{OLS}\right) = \frac{\operatorname{cov}(\Delta \ln \mathcal{R}_{ix}, \Delta \ln \mathcal{R}_{id})}{\operatorname{var}(\Delta \ln \mathcal{R}_{id})}$$

$$= \frac{\operatorname{cov}\left(u_{ix}^{\xi} + u_i^{\varphi} - u_i^{\omega}, u_{id}^{\xi} + u_i^{\varphi} - u_i^{\omega}\right)}{\operatorname{var}\left(u_{id}^{\xi} + u_i^{\varphi} - u_i^{\omega}\right)},$$

where we denote by $\Delta \ln \mathcal{X}$ the residual of a regression of a variable $\Delta \ln X$ on a set of sector fixed effects and the observable covariates $\Delta \ln \varphi_i^*$, and $\Delta \ln \omega_i^*$.

We draw two conclusions from equation (11). First, if changes in productivity and production factor costs are not perfectly observable—and their unobserved component is not fully captured by the sector fixed effects—there will be a 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 will lead $\hat{\beta}_{OLS}$ to be positive and, thus, to be an upward biased estimator of the impact of demand-driven changes in domestic sales on exports. Second, even if one proxies for changes in productivity and factor costs perfectly (i.e., $u_i^{\varphi} = u_i^{\omega} = 0$), in the presence of a nonzero correlation in the change in residual demand faced by firms in domestic and foreign markets (i.e., $\operatorname{cov}(u_{ix}^{\xi}, u_{id}^{\xi}) \neq 0$), the estimator $\hat{\beta}_{OLS}$ will again converge to a nonzero value. As this residual demand does not capture sector- and market-specific aggregate shocks (which are controlled by the sector fixed effects), it seems plausible that u_{ix}^{ξ} and u_{id}^{ξ} will be positively correlated, leading $\hat{\beta}_{OLS}$ again to be biased upward.

Consider next using an IV estimator of the parameter β in equation (10). Specifically, consider instrumenting $\Delta \ln R_{id}$ with an observed covariate Z_{id} such that Z_{id} is either a proxy for $\Delta \ln \xi_{id}$ or has a causal impact on this firm-specific domestic demand shifter. In this case, the probability limit of the IV estimator of β is

$$(12)\qquad p\lim\left(\hat{\beta}_{IV}\right) = \frac{\operatorname{cov}(\Delta \ln \mathcal{R}_{ix}, \mathcal{Z}_{id})}{\operatorname{cov}(\Delta \ln \mathcal{R}_{id}, \mathcal{Z}_{id})} = \frac{\operatorname{cov}\left(u_{ix}^{\xi} + u_i^{\varphi} - u_i^{\omega}, \mathcal{Z}_{id}\right)}{\operatorname{cov}\left(u_{id}^{\xi} + u_i^{\varphi} - u_i^{\omega}, \mathcal{Z}_{id}\right)},$$

where, as above, we use $\mathcal{Z}_{id}$ to denote the residual from projecting Z_{id} on a vector of sector fixed effects and the observable covariates $\Delta \ln \varphi_i^*$, and $\Delta \ln \omega_i^*$. The constant-marginal-cost model predicts that $\hat{\beta}_{IV}$ converges in probability to the true zero causal effect of demand-driven changes in domestic sales on exports as long as the variable $\mathcal{Z}_{id}$ satisfies two conditions: (i) it is correlated with the change in domestic sales of firm i after partialing out sector fixed effects as well as observable determinants of the firm's marginal cost; and (ii) it is mean independent of the change in firm-specific unobserved productivity, u_i^{φ}, factor costs, u_i^{ω}, and export demand u_{ix}^{ξ}. As illustrated by the second equality in equation (12), an instrument can only verify conditions (i) and (ii) if its effect on domestic sales works exclusively through the component of the change in domestic demand that is not accounted for by the sector fixed effects and the observable covariates included in the estimating equation; i.e., if it works exclusively through u_{id}^{ξ}.

Although our discussion above has centered around the role of unobserved supply and export demand factors in biasing estimates of β, Berman, Berthou, and Héricourt (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 firm's total sales, measurement error in firm total sales and exports will lead to a bias in the OLS estimate $\hat{\beta}_{OLS}$ that is likely to be of the opposite sign to that generated by the unobserved supply and export demand shocks accounted for by the residuals defined in equations (7) and (9). Consequently, as we detail in online Appendix E.1 (see also Berman, Berthou, and Héricourt 2015), negative values of $\hat{\beta}_{OLS}$ in large samples may be compatible with firms having constant marginal costs as long as the researcher's measures of either total sales or exports are affected by measurement error. Nevertheless, as we also show in online Appendix E.1, if an instrument satisfies the same conditions (i) and (ii) outlined above and is mean independent of the measurement error in exports, the IV estimator in equation (12) will still converge to zero in the presence of measurement error in total sales and exports.[8]

We have focused our discussion on the intensive margin of exports, namely the impact of domestic demand shocks on the level of exports conditional on exporting. In online Appendix E.3, 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 demand-driven changes in domestic sales on the probability of exporting, even if the true effect were to be zero, one is likely to obtain a positive OLS estimate whenever productivity and production factor costs are not perfectly captured by sector fixed effects and observable controls, or whenever unobserved firm-specific demand shocks are positively correlated across domestic and export markets. An instrument satisfying conditions (i) and (ii) above will continue to effectively remove these biases as long as it satisfies the additional

[8] As mentioned above, we generalize our model in online Appendix E.2 to incorporate multiple domestic and foreign markets. Theoretically, firms' choices over multiple export destinations may render the instruments $\mathcal{Z}_{id}$ invalid, even if they satisfy the conditions outlined in the main text. However, model simulations presented in online Appendix E.2.3 illustrate that the resulting potential bias in the IV estimates is small for most parameter values.

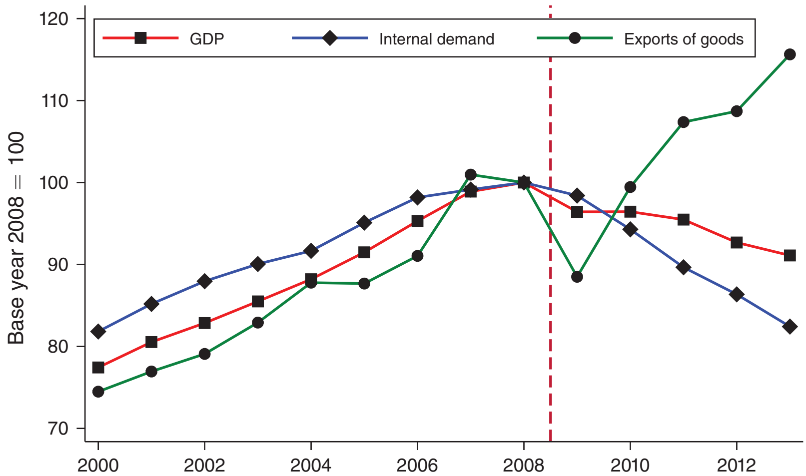

Figure 2. The Great Recession in Spain

condition of being mean independent of the part of the change in the firm's fixed cost of exporting not captured by sector fixed effects and marginal cost proxies.

II. Setting and Data

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

A. *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 20 percent in real terms.[9] In the five subsequent years until 2013, domestic demand decreased to the level of the year 2000, while real GDP fell by an accumulated 8.9 percent.

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 fueled by a real estate bubble. The construction sector accumulated an increasing share of GDP and employment.[10] For instance, in 2006, 658,000 new houses were built in Spain, a

[9] Domestic demand is defined as final consumption expenditure by households and nonprofit institutions serving households plus investment plus acquisitions of public administrations minus imports.

[10] The share of total employment in construction peaked at 13.5 percent in 2007 and then collapsed to 5.4 percent by 2014, with a similar pattern for the contribution of this sector to Spain's GDP (12.4 percent in 2007 and 6.8 percent in 2014).

number corresponding to 80 percent of those built in Germany, Italy, and the United Kingdom *combined* (European Commission, n.d.a). This real estate boom was in turn fostered 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 2017). As a result, the ratio of mortgage credit to GDP went up from 40 percent in 2000 to 100 percent in 2008 (Basco, Lopez-Rodriguez, and Elias 2021). Importantly, the very high loan-to-value ratios associated with residential mortgage credit were partly used by households to finance private consumption, particularly vehicle purchases (Masier and Villanueva 2011).

The unraveling of the subprime mortgage market in the United States 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 (Basco, Lopez-Rodriguez, and Elias 2021). The recession officially started in the fourth quarter of 2008, and intensified during 2009 with a 3.6 percent annual drop in real GDP. The growth in the stock of vehicles in Spain, which had been stable at an average rate of 3.6 percent a year during the boom, suddenly came to a halt in 2008 (see Figure C.1 in online Appendix C). 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, affecting mainly some parts of the Mediterranean coast and medium-sized and large cities. As we shall document in Section III, this in turn translated into substantial geographic variation in the extent to which the Great Recession affected the domestic sales of Spanish firms.

B. *The Spanish Export Miracle*

As Figure 2 illustrates, the evolution of Spain's aggregate merchandise exports during the period 2008–2013 was significantly different from that of aggregate domestic demand. After a significant 11.5 percent drop in real terms during the global trade collapse of 2008–2009, aggregate exports grew during the period 2009–2013 at an even faster rate than during the boom years. Specifically, while exports had grown by a cumulative 34 percent in the eight-year period 2000–2008, they grew by a very similar cumulative 31 percent 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, and the ratio of exports of goods to GDP grew from 15.1 percent in 2009 to 23.3 percent in 2013. In online Appendix D.2, we use the firms in our sample to describe the dynamics of the exports-to-sales ratio by sector.

One might wonder whether a depreciation in the euro could explain the growth in Spanish exports during the period 2009–2013. Figure 1 in the introduction shows that this could not have been the main explanation, as Spanish exports to non-euro area countries clearly outperformed those of other countries in the euro area (while

Spain's GDP dropped faster than the euro area average).[11] It has also been argued that Spain underwent an internal devaluation during this period (through wage moderation starting in 2009, and via a labor market reform in 2012), but there is little evidence that these policies had a significant effect on relative production costs before 2012. For instance, unit labor costs in Spain were only 2.2 percent lower in 2012 than in their peak in 2009 (OECD Statistics). However, as we document in online Appendix D.3, Spanish manufacturing export prices (unit values from product-level export data) fell relative to export prices in other euro area countries from the onset of the crisis, before Spanish unit labor costs had started to fall.

Motivated by these facts, we will hereafter focus on an exploration of the "vent-for-surplus" hypothesis, according to which the domestic slump, by freeing up production capacity, *directly* incentivized Spanish producers to sell their goods in foreign markets. More precisely, we hypothesize that the domestic slump led firms to move down along their short-run marginal-cost schedule, thereby lowering their export prices and leading them to gain market share in export markets.[12]

In principle, the 2009–2013 growth in exports could have materialized along the intensive margin (with continuing exporters increasing their exports) or along the extensive margin (via net entry into the export market). Descriptive evidence suggests that the bulk of the growth was driven by the intensive margin. Using detailed Spanish customs data, de Lucio et al. (2017b) find that net firm entry (i.e., new exporters net of firms quitting exporting) contributed a mere 14 percent to the export growth between 2008 and 2013, while the remaining 86 percent was driven by continuing exporters. Similarly, in our sample, we find that continuers contributed 91 percent of the growth in exports between the boom and the bust periods, and the extensive margin only accounted for 9 percent of export growth.[13]

C. *Data Sources*

Our data cover the period 2000–2013 and come from various confidential administrative data sources. The first is the Commercial Registry (*Registro Mercantil Central*). It contains annual financial statements for around 85 percent of registered firms in the nonfinancial market economy in Spain. Among other variables, it includes information on sector of activity (four-digit NACE Rev. 2 code),[14] five-digit zip code of 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), number of employees (full-time equivalent), and total fixed assets. We provide more details regarding this dataset in Appendix B (see also Almunia, Lopez-Rodriguez, and Moral-Benito 2018).

[11] Figure 1 also shows that most of the *relative* take-off of Spain occurred after 2010. The same is not true when looking at Spain's share in overall goods exports (including exports to euro area countries); in that case, Spain's share increased markedly already in 2009. This suggests that the increase in Spanish exports (relative to euro area countries) immediately following the Great Recession was largely driven by increased exports within the euro area.

[12] This is in contrast with the type of downward shift in marginal costs associated with internal devaluations.

[13] De Lucio et al. (2017b) 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 is 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-country-level.

[14] *Nomenclature statistique des activités économiques dans la Communauté européenne* (NACE).

The second dataset is the foreign transactions registry collected by the Bank of Spain (*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),[15] the country of the foreign client, and the exact date of the operation (no matter when the payment was made). 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, to save on administrative costs, 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 during the crisis, 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 required 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 until the end of the mandatory registry in 2013, information had to be reported for all transactions performed by a firm during a natural year as long as at least one of these transactions exceeded 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 firms that had at least one transaction exceeding 50,000 euros in that year (see Appendix B for more details). The foreign transactions registry collected by the Bank of Spain was discontinued in early 2014, which precludes us from extending our analysis past the year 2013.

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 each firm's fiscal identifier. Using the merged database, we define each firm's domestic sales as the difference between its total sales and its total exports.

To check the accuracy of the information contained in the resulting dataset, we compare its implied annual aggregate output, employment, total wage bill, and goods exports with the official publicly available data. Figure C.3 in online Appendix C shows that our dataset tracks well the evolution over time of these aggregates. Due to the reporting thresholds described above, aggregate exports in our sample fall a bit short of aggregate exports in the customs data, but the gap is similar in the boom and bust periods (the average coverage is 91.8 percent in 2000–2008 and 91.3 percent in 2009–2013).[16]

We complement the firm-level data described above with yearly municipality-level data on the stock of vehicles per capita. The information on the stock of vehicles by

[15] Standard International Trade Classification (SITC).

[16] Most of the gap in coverage is explained by the fact that a nontrivial share of Spanish exports is carried out by legal entities or individuals that are not registered as firms undertaking economic activity in Spain, and are thus exempted from submitting their financial statements to the Commercial Registry. The share of goods exports by nonregistered entities was on average around 8 percent in 2010–2013 (own calculations based on public Customs data).

municipality is provided by the Spanish Registry of Motor Vehicles, compiled by the General Directorate of Traffic (*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: 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 of the stock of vehicles per capita to all firms located in the same municipality, independently of the zip code of location; for firms in zip codes containing multiple municipalities, we associate with them a stock of vehicles per capita constructed as an average of the stocks of vehicles per capita across these municipalities.

We also employ information from two datasets provided to us by the Spanish Tax Agency (*Agencia Estatal de Administración Tributaria*, AEAT): first, aggregate data on municipality-to-municipality flows for *all* firms in the manufacturing sector, excluding sales of entities in the auto industry; and, second, firm-to-municipality sales only for those manufacturing firms in our sample that exported in the boom as well as in the bust.[17] The AEAT was willing to share with us only one year of data, so we work with data for the year 2006 as it is the first year for which a comprehensive digitization of the data are available.

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

III. Identification Approach

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

A. *Geography-Based Proxies of Demand Changes*

As explained in Section IIA, a key characteristic of the Great Recession in Spain is that it affected different regions differently. Panel A in Figure 3 illustrates this fact. The figure plots the standardized percentage change in domestic sales for the average manufacturing 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).[18] 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. Figure 3 thus 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

[17] We thank Francesco Serti for having brought to our attention the existence of these data.

[18] Figure C.2 in online Appendix C.2 shows the yearly average number of firms and exporters by province for the period 2002–2008. Economic activity in Spain is concentrated mostly in the coast (Galicia, Basque Country, Catalonia, Valencian Community, Murcia, and Andalusia) and in the center (Madrid). Exporting firms are concentrated in the center (Madrid) and in the Mediterranean coast (Catalonia and Valencian Community).

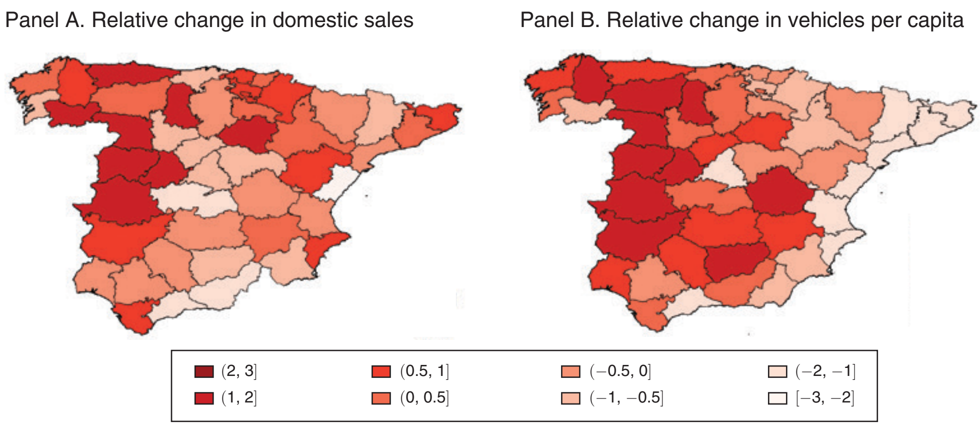

Figure 3. The Great Recession in Spain: Variation across Provinces

Notes: For each province, panel A illustrates the standardized percentage change in average firm-level domestic sales between the period 2002–2008 and the period 2009–2013, where the average is computed across manufacturing firms active in at least one year in both periods. Therefore, if this variable takes value p for a given province, it means that the average firm located in this province 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 the average province. Panel B illustrates the standardized percentage change in vehicles per capita between the period 2002–2008 and the period 2009–2013. The color scale is in standard deviations as in panel A.

regions experienced relatively large domestic sales reductions. Furthermore, deviations from the national average are sizable in many cases.

The heterogeneity in the changes in domestic sales that we document in panel A of Figure 3 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 measuring 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) for which we have highly geographically disaggregated data. Panel B in Figure 3 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.[19] Specifically, 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 coast experienced a relative reduction. As in panel A of Figure 3, the regional deviations from the national averages in panel B are large for many provinces.

By illustrating provincial averages, Figure 3 hides substantial spatial variation at the subprovince level (across municipalities) in the boom-to-bust changes in both average firm-level domestic sales and changes in the stock of vehicles per capita. We

[19] Changes in the stock 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 that our measure of domestic demand for manufacturing firms is contaminated by the effect of the "cash for clunkers" program (*Plan PIVE*) that the Spanish government put in place during the bust period.

illustrate this variation in online Appendix Figure C.4 for the case of the two most populated Spanish provinces: Madrid and Barcelona.

Our core empirical strategy exploits the variation illustrated in online Appendix Figure C.4 to identify the impact of domestic demand shocks on firms' exports operating through its effect on the firms' domestic (Spain-wide) sales. Specifically, we divide our sample into a "boom" period (2002–2008) and a "bust" period (2009–2013), and assess the extent to which a demand-driven decline in a firm's domestic sales in the bust period relative to the boom period is associated with a relative increase in its export sales between these two periods. We choose this "long-differences" approach as our baseline because the macroeconomic evidence in online Appendix Figure C.3 cleanly identifies the year 2009 as the break between two distinct periods. Having said this, we will show that our results are similar when breaking the sample into shorter subperiods, with the exception of panel regressions with yearly data, a frequency at which our instruments cease to have significant predictive power (see Section IVB).

To build a measure of the boom-to-bust change in domestic demand for each Spanish firm, we follow a two-step procedure. First, we use observed boom-to-bust changes in the stock of vehicles per capita at the municipality level as a proxy for the boom-to-bust changes in the demand for manufacturing goods in those municipalities. For this, we rely on a body of work (see Stock and Watson 1999) documenting that durable goods consumption, and vehicle purchases in particular, are strongly procyclical, and are thus a useful proxy for changes in "local demand."[20] Second, with this measure of local demand at hand, we construct instruments capturing the boom-to-bust changes in domestic demand experienced by firms located in different Spanish municipalities. We do this in two distinct and complementary ways.

Our first instrument (or *local* instrument) builds on the work of Hillberry and Hummels (2008) highlighting the extremely localized nature of manufacturing shipments in the United States: we posit and verify that changes in municipality-level demand (as captured by changes in vehicles per capita) are a good predictor for changes in domestic (Spain-wide) sales of Spanish firms producing in the corresponding municipality.

Our second instrument (or *gravity-based* instrument) instead acknowledges that the majority of a firm's sales are actually shipped outside its municipality of location, and constructs a theoretically grounded measure that takes into account the exposure of firms to local demand shocks in municipalities *other* than their municipality of location. To measure the different exposure of each firm to other municipalities' local demand shocks, we rely on information on the location of these firms together with municipality-to-municipality trade flows data (aggregated across all manufacturing firms) for the year 2006. We use these data to estimate municipality-to-municipality gravity regressions, and use the estimated coefficients for log population and log distance to build the relevant weights needed to construct our gravity-based measure of firm-level exposure to local demand shocks, which is a

[20] Relatedly, Mian, Rao, and Sufi (2013) document that variation in the extent to which the US subprime mortgage default crisis of 2007–2010 affected household housing wealth in different areas in the United States translated into geographical variation in vehicle purchases. Hausman, Rhode, and Wieland (2019) and Waugh (2019) provide corroborating evidence in other settings.

weighted sum of the local shocks (i.e., changes in the stock of vehicles per capita) in all locations other than the one in which the firm is based. As a robustness check, we also experiment with alternative *gravity-based* instruments that rely on alternative sets of weights.

To be more precise, our two baseline instruments take the form

$$(13)\quad \Delta \ln Z_{id} = \begin{cases} \Delta \ln V_{\ell(i)}, & \text{for the } local \text{ IV;} \\ \Delta \ln\Big(\sum_{\ell' \neq \ell(i)} (Pop_{\ell'})^{\hat{\beta}_{pop}} (Dist_{\ell'\ell(i)})^{\hat{\beta}_{dist}} V_{\ell'}\Big), & \text{for the } gravity\text{-}based \text{ IV,} \end{cases}$$

where V_ℓ are vehicles per capita in municipality ℓ, $\ell(i)$ is the municipality in which firm i is located, $Pop_{\ell'}$ is population in location ℓ', $Dist_{\ell'\ell(i)}$ is the distance between municipalities ℓ' and $\ell(i)$, and $\hat{\beta}_{pop}$ and $\hat{\beta}_{dist}$ are estimates of the coefficients on (destination) population and on bilateral distance in a gravity equation estimated using Spain's municipality-to-municipality sales data. Our gravity-based instrument is closely related to the so-called Harris market access measure, a widely used measure of demand in economic geography studies (see Redding and Venables 2004). Such a demand measure arises naturally in multimarket versions of the Melitz (2003) model; in fact, as Jacks and Novy (2018) show, this demand term also arises in more general frameworks, including all models that yield a structural gravity equation à la Anderson and van Wincoop (2003).[21]

Estimates of our municipality-to-municipality gravity equation for Spanish manufacturing flows in the year 2006 are presented in columns 1 through 3 of Table 1. These estimates are based on the municipality-to-municipality aggregate trade flows data provided to us by the Spanish Tax Agency (see Section IIC). The first column presents estimates for a specification with municipality of origin fixed effects, log population of the destination municipality, and log distance between origin and destination. The results illustrate the relevance of gravity forces, with shipments increasing in destination population with an elasticity of $\hat{\beta}_{pop} = 0.493$, and declining in distance with an elasticity of $\hat{\beta}_{dist} = -0.429$. The inclusion in column 2 of dummies for own-municipality and own-province flows slightly reduces this distance elasticity, while these two dummies appear to have themselves a positive and significant effect on shipments. This suggests that part of the negative effect of distance on within-Spain municipality-to-municipality shipments is related to a discontinuous fall in shipments at the municipality border and at the province border. The extent of "home bias" at the municipality level is remarkably large: it implies that, ceteris paribus, shipments are $\exp(1.607) \approx 5$ times larger within a municipality than outside. The existence of such strong local home bias is in line with the findings of Hillberry and Hummels (2008) for the United States, although

[21] There are two important differences between our measure of market access and standard uses of market access as a proxy for demand. First, we measure the change in demand in a location not as the change in a destination fixed effect estimated from a gravity equation, but as the change in the stock of vehicles per capita in that location. Second, as the value of our instrument for a municipality excludes the change in vehicles per capita in such municipality, our instrument excludes what Redding and Venables (2004) denote the domestic market access term. The first difference is a consequence of the fact that we observe within-Spain flows only for the year 2006; the second one is due to our aim to make this second instrument distinct from the first one based on local demand (and also more plausibly exogenous). Similar market access terms have been defined in Harris (1954), Hanson (2005), and Donaldson and Hornbeck (2016).

Table 1—Estimates from Gravity Equations at Municipal Level

	ln(Trade Flows)				
	(1)	(2)	(3)	(4)	(5)
ln(Population)	0.493	0.490	0.485	0.300	0.322
	(0.031)	(0.031)	(0.030)	(0.012)	(0.015)
ln(Distance)	−0.429	−0.378		−0.150	−0.145
	(0.011)	(0.019)		(0.021)	(0.019)
Dummy for own-municipality flows		1.607			
		(0.111)			
Dummy for own-province flows		0.131			
		(0.065)			
Dummy for distance 5–10km			−1.159		
			(0.079)		
Dummy for distance 10–50km			−1.924		
			(0.090)		
Dummy for distance 50–100km			−2.465		
			(0.114)		
Dummy for distance 100–200km			−2.718		
			(0.113)		
Dummy for distance 200–500km			−2.962		
			(0.120)		
Dummy for distance 500–1,000km			−3.237		
			(0.120)		
Dummy for distance >1,000km			−3.590		
			(0.141)		
Observations	417,936	417,936	417,936	675,715	675,589
R^2	0.30	0.31	0.30	0.18	0.31
Municipality of origin fixed effects	Yes	Yes	Yes	Yes	No
Sector fixed effects	No	No	No	Yes	No
Firm fixed effects	No	No	No	No	Yes

Notes: Standard errors clustered by province of origin reported in parentheses. The data on municipality-level trade flows for manufacturing firms are for the 2006 fiscal year. ln(Population) denotes the log of the population of the destination municipality in 2006. ln(Distance) denotes the log of the distance, in kilometers, between the two municipalities in each pair. The estimates in columns 1 to 3 use municipality-to-municipality sales data; the estimates in columns 4 and 5 use firm-to-municipality data. The estimation in this and all other tables in the paper uses the Stata commands reghdfe (Correia 2017) and ivreghdfe (Correia 2019).

the magnitude is larger in our setting.[22] This finding buttresses the potential relevance of our first *local* instrument. Column 3 presents estimates for a specification analogous to that in column 1 but employing a set of distance dummies to capture the effect of distance on sales. The results show that, controlling for municipality of origin fixed effects and for the population of the municipality of destination, sales decay monotonically with distance.

In columns 4 and 5 of Table 1, we estimate gravity equations at the firm-destination level exploiting the information on the second of the datasets provided to us by the

[22] Díaz-Lanchas, Llano, and Zofío (2019) also estimate a very sizable "zip code effect" on the basis of a random sample of shipments by road within Spain during the period 2003–2007 (the C-Intereg database). Although we do not have access to this information at the zip code level, we have obtained aggregated province-to-province shipments from that database. In online Appendix D.4, we compare some aggregate statistics on firms' within-Spain sales from our sales data based on tax records and from the C-Intereg data. The extent of provincial home bias is very similar in both datasets.

Spanish Tax Agency, which contains firm-to-destination shipments for the subset of firms that exported both in the boom as well as in the bust (see Section IIC). The specification in column 4 is analogous to that in column 1, while that in column 5 additionally accounts for firm fixed effects. The results are in line with those in column 1, but with somewhat smaller log population and log distance coefficients, as one would expect given that these specifications do not account for extensive margin variation in the set of municipalities firms sell to.[23]

B. *Threats to Validity of the Identification Approach*

The main concern with our identification approach is that our municipality-level measures of demand changes between the boom and the bust may be correlated with changes in marginal cost shifters affecting the firms located either in the corresponding municipalities or in neighboring ones. This exclusion restriction is central to the validity of our strategy, so we next outline how we try to deal with potential threats to the validity of our identification approach.

First, we control in our specifications for sector fixed effects. Thus, 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 experiencing different exposure to local demand changes. By controlling for sector fixed effects, we control for sector-specific foreign demand shocks, sector-specific trade cost shocks, and domestic supply shocks affecting all Spanish firms operating in the same sector, irrespective of their location (see the definition of γ_{sx} in equation (6)). For example, these fixed effects control for shocks such as the expiration of the Multi Fiber Arrangement on January 1, 2005, which eliminated all European Union quotas for textiles imported from China, and which increased the competition that Spanish textile manufacturers faced both in the domestic and foreign markets.[24]

Second, as different firms operating in the same sector may experience different supply shocks, we also control throughout for firm-specific measures of productivity and labor costs. 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 2009–2013 might have been heterogeneous across different Spanish municipalities in a manner that is correlated with our instruments.

A third concern is that, even after controlling for sector fixed effects and proxies for firm-level productivity and wage costs, there may still be unobserved *residual* location-specific marginal cost shifters that might be correlated with our proxy for

[23] We have also estimated gravity equations that use the firm-to-destination data and include the border and distance dummies introduced in columns 2 and 3. Notably, we again find a remarkably large coefficient of 1.308 for the own-municipality dummy variable.

[24] Sector fixed effects may not effectively control for all heterogeneity across firms in their export demand shocks; specifically, firms located in different Spanish regions may be differentially affected by export demand shocks even if they operate in the same sector. A possible source of this heterogeneity in demand shocks is the different exposure of firms located in different Spanish regions to changes in demand in different foreign countries (e.g., firms located in southern regions are more exposed to demand changes in northern African countries than firms located in the north of Spain). In online Appendix E.4, we provide suggestive evidence that this type of heterogeneity in export demand shocks does not, in practice, affect the validity of our instrument.

changes in local demand. For instance, changes in labor payments not properly controlled for by our measure of firm-level wages may impact the purchasing power of consumers living in the corresponding municipalities, and thus affect vehicle purchases. This identification threat is likely to be particularly salient for our "local" instrument. In the construction of our gravity-based instrument, we do not use information on the change in the number of vehicles per capita in the municipality of location of a firm, which helps assuage this concern as long as there is no strong spatial correlation in the unobserved residual marginal cost shifters. Furthermore, to address concerns motivated by this possible spatial correlation in residual supply shocks, we present in Section V estimates from regression specifications in which we control for additional measures of municipality-specific boom-to-bust changes in economic conditions.

A fourth concern relates to the presence of a nontrivial number of car manufacturers in our sample of Spanish manufacturing firms. These firms' supply shocks are especially likely to have impacted the boom-to-bust changes in the stock of vehicles per capita in their own municipality and in geographically close ones. More specifically, if a disproportionate share of cars in Spain was sold in municipalities that are geographically close to where the car was manufactured, supply shocks in these firms may affect the number of vehicles per capita not just by affecting the purchasing power of consumers in certain municipalities, but by affecting directly the supply of cars in those municipalities. Roughly three quarters of all cars purchased in Spain are imported (as indicated by data from the Spanish National Institute of Statistics); thus, supply shocks affecting car manufacturers are likely to have a limited impact on the total amount of cars in Spain. However, to deal with this threat to identification, we exclude all firms operating in the auto industry (NACE Rev. 2 code 29) in all the regressions we present. We also explore in Section V how our 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, (ii) all firms located in a zip code or in the proximity of a zip code with a significant share of manufacturing employment accounted for by the auto industry, and (iii) all firms producing in sectors that are either leading input providers or leading buying industries of the vehicles manufacturing industry.

Finally, it is important to remark that, as illustrated in equation (12), any unobserved factor costs that are negatively correlated with either of the two instruments we use will cause the corresponding IV estimator to be positively biased. For example, if the tightening of the credit supply in a region caused firms' marginal production costs to increase and consumers' demand to fall, the resulting endogeneity of our instrument would bias our IV estimator upwards. Thus, negative IV estimates of the elasticity of firm-level exports with respect to a firm's domestic sales would still reflect patterns in the data that would be inconsistent with the constant marginal cost model described in Section I, and that would be suggestive of the existence of a negative relationship between demand-driven changes in domestic sales and exports.

IV. Baseline Results

In this section, we present our baseline results on the impact of demand-driven changes in domestic sales on firms' behavior in the export market. Specifically, we

Table 2—Intensive Margin: OLS Estimates

	Δln(Exports)					
	(1)	(2)	(3)	(4)	(5)	(6)
Δln(Domestic Sales)	0.063	−0.209	−0.298	−0.292	−0.284	−0.271
	(0.044)	(0.049)	(0.043)	(0.032)	(0.032)	(0.036)
Δln(TFP)		1.142	1.448	1.535	1.522	1.514
		(0.043)	(0.046)	(0.057)	(0.055)	(0.057)
Δln(Average Wages)			−0.744	−0.723	−0.712	−0.706
			(0.062)	(0.072)	(0.070)	(0.067)
Observations	8,009	8,009	8,009	8,009	8,009	7,502
R^2	0.001	0.100	0.126	0.162	0.171	0.278
Sector fixed effects	No	No	No	Yes	Yes	Yes
Province fixed effects	No	No	No	No	Yes	No
Municipality fixed effects	No	No	No	No	No	Yes

Notes: Standard errors clustered by province reported in parentheses. For any X, $\Delta\ln(X)$ is the difference in $\ln(X)$ between its average in the 2009–2013 period and its average in the 2002–2008 period. The estimation sample includes all firms exporting in at least one year in the period 2002–2008 and in the period 2009–2013.

present in Sections IVA and IVB evidence of the impact of the Great Recession on Spanish firms' intensive margin of exports. In Section IVC, we present analogous evidence of its impact on the extensive margin.

A. *Intensive Margin*

Table 2 presents OLS estimates of the elasticity of boom-to-bust changes in firms' exports 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 $8,009$ such firms in our dataset.

As discussed in Section I, unobserved (residual) supply factors tend to make the OLS estimate of a firm's change in domestic sales on its change in foreign sales positive even in a world with constant marginal costs. Conversely, measurement error in both total sales and exports tends to make this OLS estimate negative. As illustrated in column 1 of Table 2, when no controls are included, we estimate an OLS elasticity of export flows with respect to domestic sales that is very close to zero. In column 2, we control for the change in firms' productivity (estimated following the procedure in Gandhi, Navarro, and Rivers 2020, as detailed in online Appendix F), and in column 3 for the change in the firm's average wages. Consistent with the discussion in Section I, controlling for these supply shocks reduces the OLS estimate of the coefficient on domestic sales. In fact, the coefficient turns significantly negative (-0.298), 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 control for additional unobserved determinants of firms' marginal costs that vary between the boom and the bust. To do so, and motivated by the specification in equation (10), we sequentially add sector fixed effects (in column 4), province fixed effects (column 5) and municipality fixed effects (column 6). The resulting estimates continue to be negative and indicate that a 1 percent decrease in a firm's domestic sales, keeping its productivity and average wages constant, is associated with close to a 0.3 percent increase in its overall export flows.

Table 3—Intensive Margin: 2SLS Estimates

	Δln(Domestic Sales)				Δln(Exports)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Local instrument								
Δln(Domestic Sales)					−2.428	−1.272	−1.185	−1.327
					(0.685)	(0.306)	(0.276)	(0.337)
Δln(Vehicles p.c. in municipality)	0.310	0.455	0.474	0.418				
	(0.064)	(0.071)	(0.072)	(0.073)				
Δln(TFP)		0.800	0.992	0.980		1.983	2.320	2.533
		(0.027)	(0.031)	(0.032)		(0.244)	(0.277)	(0.323)
Δln(Average Wages)			−0.606	−0.511			−1.279	−1.240
			(0.035)	(0.043)			(0.184)	(0.178)
Observations	8,009	8,009	8,009	8,009	8,009	8,009	8,009	8,009
Sector fixed effects	No	No	No	Yes	No	No	No	Yes
Province fixed effects	No	No	No	Yes	No	No	No	Yes
F-statistic	23.19	41.64	43.26	33.10				
Panel B. Gravity-based instrument								
Δln(Domestic Sales)					−10.068	−2.081	−1.751	−1.607
					(3.454)	(0.319)	(0.238)	(0.248)
Δln(Dist-Pop-Weighted Vehicles p.c.)	0.339	1.194	1.346	1.312				
	(0.121)	(0.145)	(0.135)	(0.119)				
Δln(TFP)		0.829	1.031	1.023		2.623	2.876	2.810
		(0.028)	(0.029)	(0.028)		(0.241)	(0.222)	(0.213)
Δln(Average Wages)			−0.621	−0.526			−1.620	−1.387
			(0.037)	(0.047)			(0.174)	(0.151)
Observations	8,009	8,009	8,009	8,009	8,009	8,009	8,009	8,009
Sector fixed effects	No	No	No	Yes	No	No	No	Yes
F-statistic	7.85	67.47	99.84	122.44				

Notes: Standard errors clustered by municipality (panel A) or by province (panel B) reported in parentheses. For any X, $\Delta\ln(X)$ is the log difference between the average of X in 2009–2013 and its average in 2002–2008. Vehicles p.c. denotes the stock of vehicles per capita in a municipality. Δln(Dist-Pop-Weighted Vehicles p.c.) is the instrument constructed using data on vehicles per capita at the municipal level and applying the weights from the gravity equation reported in column 1 of Table 1. Columns 1–4 contain first-stage estimates; columns 5–8 contain second-stage estimates. F-statistic denotes the corresponding test statistic for the null hypothesis that the coefficient on the IV equals zero.

In Table 3, we turn to our baseline two-stage least squares (2SLS) 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. As discussed in Section I, when firms' marginal costs are constant, provided that our local and gravity-based instruments are orthogonal to both unobserved supply factors and to measurement error in both total sales and exports, the corresponding 2SLS estimators should converge to zero. The first-stage estimates reported in columns 1 to 4 of Table 3 reveal that firms located in municipalities that experienced a larger drop in either their local (panel A) or gravity-based (panel B) proxy for demand also suffered a larger decline in their domestic (Spain-wide) sales. This relationship is robust to controlling for our measures of firms' changes in productivity and labor costs, for sector fixed effects and, when using the local instrument, for province fixed effects. The statistics of F-tests of the null hypothesis that the coefficient on our instruments is equal to zero in the first-stage regressions is in all specifications above threshold values generally applied to detect weak instrument problems, the only exception being the value of 7.85 in column 1 of panel B.[25]

The second-stage estimates (reported in columns 5 to 8 of Table 3) reveal elasticities of exports with respect to domestic sales that are significantly larger in absolute value than the OLS elasticities reported in Table 2.[26] This suggests that, even after controlling for sector fixed effects and firm proxies of productivity and average labor costs, there still remains substantial unobserved determinants of firms' marginal costs that induce a spurious positive correlation between their sales in the domestic and foreign markets. Our preferred estimates in column 8 indicate an elasticity of exports with respect to domestic sales of around -1.3 for our local instrument and of around -1.6 for our gravity-based instrument. These negative estimates are suggestive of the firm's marginal cost function not being flat.

One might be concerned that, because firms' total sales are a key input in the computation of our proxy for a firm's productivity, 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 the model described in Section I), the correlation is far from perfect, particularly when considering log changes in these variables. More specifically, the correlation between log changes in our proxy for a firm's productivity and log changes in observed total sales is 0.31 at the yearly level, and is 0.56 when looking at boom-to-bust "long differences" in these variables. To further assuage this concern, we explore in Section VC the robustness of our results to using an alternative measure of firms' productivity; this alternative measure uses information on firms' value-added instead of total sales, and has a much lower correlation with this latter variable.

In terms of the quantitative relevance of our results, it is worth emphasizing 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 $100 \times \chi$ percent, a demand-driven drop of €100 in their domestic sales would lead to a €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 percent would able to recoup €53.3 via exports, while a firm with an export share of one-third would be able to recoup €80.[27]

In terms of the statistical significance of our results, it is important to remark that, unless otherwise noted, standard errors are clustered by municipality when using the local instrument and by province when using the gravity-based instrument. It is also worth highlighting that none of our instruments fall in the class of shift-share instruments studied recently by Adão, Kolesár, and Morales (2019); Borusyak, Hull, and Jaravel (2020); and Goldsmith-Pinkham, Sorkin, and Swift (2020); among others. The reason is that our instrument does not use changes in a weighted-average

[25] In online Appendix Figure C.5, we present binned scatter plots of both the first-stage and reduced-form relationships.

[26] The unrealistically high point estimate and the corresponding large standard error reported in column 5 of panel B should be discounted on the basis that, as shown in column 1, our instrument is weak in this regression specification.

[27] The median export share among the 8,009 firms exporting in both boom and bust periods is 16.2 percent.

of vehicles per capita across municipalities but log changes in such a weighted average.[28] Although our choice of functional form prevents us from computing the standard errors according to the formulas introduced in Adão, Kolesár, and Morales (2019), it is conceivable that our standard error estimates suffer from the downward bias that typically affect clustered standard errors when the instrument is of the shift-share type. We revisit this question in Section VB, where we provide suggestive evidence showing that the bias affecting our standard errors, if present, is likely to be very small.

B. *Panel Specifications*

Although the evidence shown in Figures C.1 and C.3 in online Appendix C shows that, from a macroeconomic perspective, there are clearly two distinct periods in our sample (a boom and a bust period), our long-differences approach comparing the boom to the bust has the limitation that it does not allow to control for location-specific trends that could help account for the evolution of location-specific unobserved supply conditions. To address this limitation, in Table 4 we report 2SLS estimates for various specifications that exploit the higher frequency of our data, and that thus allow for the inclusion of municipality-specific time trends in our estimating equation.

In panel A of Table 4, we present estimates from panel specifications in which each observation in the time dimension corresponds to a three-year rolling average, starting with 2002–2004 all the way to 2011–2013. We report results for our local instrument in columns 1 to 3, and for our gravity-based instrument in columns 4 to 6. The OLS, first-stage, and second-stage results are all qualitatively similar to those reported in Tables 2 and 3, although the key second-stage elasticities are slightly larger in this case (-1.44 and -2.16, respectively). In panel B, we present analogous results using two-year rolling averages. The results are again qualitatively similar, but our instruments become significantly weaker in this case, especially the gravity-based one. Finally, we report in panel C results in which each observation in the time dimension corresponds to a year, exploiting thus the full time variation in our annual data. Although the OLS estimates remain stable, our instruments become very weak in this case.

There are several possible reasons for why spatial variation in vehicle sales fails to predict demand conditions at the yearly level. First, vehicle sales are not only procyclical (as other durable goods are) but also tend to be lead indicators for recessions. For instance, in our own aggregate data from Spain, vehicles per capita declined already in 2008 (see online Appendix Figure C.1), while the recession in Spain only unfolded in 2009. Second, the precise extent to which vehicles sales led or lagged the drop in municipality-level demand during the Great Recession varies across municipalities in Spain. Both of these concerns affect the relevance of the instruments when each period corresponds to a year, but they do not do so when each observation in the time dimension corresponds to an average across

[28] The reason for employing log changes to build this instrument is that, according to the multidestination model in online Appendix E.2, log changes in domestic sales are linearly related to log changes in a weighted average of local demand shifters across the different domestic markets.

Table 4—Panel Regressions

	Local Instrument			Gravity-based instrument		
	OLS (1)	1st Stage (2)	2SLS (3)	OLS (4)	1st Stage (5)	2SLS (6)
Panel A. 3-year rolling average						
ln(Domestic Sales)	−0.259		−1.441	−0.259		−2.159
	(0.019)		(0.528)	(0.022)		(0.242)
ln(Vehicles p.c. in municipality)		0.278				
		(0.056)				
ln(Dist-Pop-Weighted Vehicles p.c.)					1.016	
					(0.157)	
ln(TFP)	1.420	0.930	2.517	1.420	0.941	3.184
	(0.045)	(0.027)	(0.486)	(0.052)	(0.021)	(0.226)
ln(Average Wages)	−0.629	−0.416	−1.119	−0.629	−0.422	−1.417
	(0.044)	(0.031)	(0.229)	(0.037)	(0.032)	(0.136)
Observations	66,711	66,710	66,710	66,711	66,711	66,711
F-statistic		24.45			41.73	
Panel B. 2-year rolling average						
ln(Domestic Sales)	−0.259		−1.040	−0.259		−2.380
	(0.019)		(0.528)	(0.017)		(0.603)
ln(Vehicles p.c. in municipality)		0.193				
		(0.051)				
ln(Dist-Pop-Weighted Vehicles p.c.)					0.775	
					(0.279)	
ln(TFP)	1.407	0.920	2.125	1.407	0.923	3.359
	(0.043)	(0.028)	(0.582)	(0.055)	(0.023)	(0.549)
ln(Average Wages)	−0.614	−0.394	−0.921	−0.614	−0.395	−1.449
	(0.040)	(0.030)	(0.256)	(0.034)	(0.030)	(0.258)
Observations	65,709	65,708	65,708	65,709	65,709	65,709
F-statistic		14.35			7.70	
Panel C. Annual data						
ln(Domestic Sales)	−0.273		2.222	−0.274		2.066
	(0.014)		(5.777)	(0.018)		(1.580)
ln(Vehicles p.c. in municipality)		0.023				
		(0.050)				
ln(Dist-Pop-Weighted Vehicles p.c.)					0.677	
					(0.453)	
ln(TFP)	1.385	0.920	−0.911	1.382	0.921	−0.773
	(0.042)	(0.030)	(5.313)	(0.064)	(0.027)	(1.481)
ln(Average Wages)	−0.581	−0.373	0.349	−0.579	−0.373	0.295
	(0.035)	(0.029)	(2.152)	(0.037)	(0.031)	(0.582)
Observations	60,199	60,198	60,198	60,199	60,199	60,199
F-statistic		0.21			2.24	

Notes: Standard errors clustered by municipality (columns 1 to 3) or by province (columns 4 to 6) reported in parentheses. All specifications include firm and sector-year fixed effects, as well as municipality-specific time trends. In columns 1 to 3, they additionally include province fixed effects. The dataset used in panel A is constructed calculating three-year rolling averages of all the variables for each firm, where the periods are 2002–2004, 2003–2005, etc., for a total of ten periods. In panel B, we calculate two-year rolling averages, where the periods are 2002–2003, 2003–2004, etc., for a total of 11 periods. In panel C, we use the original annual data with 12 periods between 2002 and 2013.

years, as in our baseline boom-to-bust regressions and in the three- and two-year rolling window specifications.[29]

C. *Extensive Margin*

To study the impact of demand shocks on the extensive margin of exporting, we again divide the sample period into a boom (2002–2008) and a bust period (2009–2013), and explore how demand-driven changes in domestic sales impact changes in firms' probability of exporting between these two periods. More specifically, we use data on all firms in our sample that are active in the domestic market in both the boom and the bust, and compute 2SLS estimates 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, sector-period, and province-period fixed effects, and the log of the firm's average wages and productivity measures, with log domestic sales in a given period instrumented by either the local or the gravity-based proxy for the firm's demand in that period.[30] We present in Table 5 estimates that use the gravity-based instrument, and in online Appendix G.1 estimates that rely on the local instrument. In addition, for both types of instruments, we also present estimates for specifications in which the dependent variable is the proportion of years in a given period (boom or bust) that a firm exports.

Column 1 in Table 5 reports the first-stage estimates. As in Table 3, the results indicate that domestic sales fell more for firms located in municipalities that, according to our gravity-based instrument, experienced a larger drop in domestic demand. The F-statistic (69.77) is, as in our intensive margin specifications, well above standard threshold values. Columns 2 and 3 present OLS and 2SLS estimates of the link between domestic sales and export status, while columns 4 and 5 report OLS and 2SLS estimates of the link between domestic sales and the proportion of years exported. The results of these two specifications deliver statistically significant estimates of opposite signs but, in both cases, these are quantitatively very small. As shown in Table G.1 of online Appendix G.1, we find similarly weak and mixed extensive margin effects when using the local instrument.

Taken together, these results lead us to conclude that the vent-for-surplus mechanism did not appear to operate strongly via the extensive margin (i.e., via entry and exit from the export market). This result aligns with the fact, discussed in Section IIB, that more than 90 percent of the growth in Spanish exports during the bust period was explained by continuing exporters. There are two potential explanations that make our muted extensive margin results compatible with the sizeable intensive margin effects discussed in Section IVA. The first explanation relates to the fact that we only have data on aggregate exports, and thus changes in the extensive margin in our context refer to entry and exit from export markets *altogether*, which is a decision involving much larger investments than entry and exit from

[29] While the first of these two concerns could be addressed by including lags of the instruments in the first-stage specification, the second one is harder to address. In Table G.4 in online Appendix G, we report results of yearly regressions that also include lags of the instruments in the first-stage specification. The instruments (regardless of whether it is the local or the gravity-based one) continue to be weak in this case.

[30] Our results are similar when we use as left-hand-side variable a dummy that treats a firm as an "exporter" only if it exports for two or more years in a given period.

Table 5—Extensive Margin: 2SLS Estimates

		Export dummy		Proportion of years	
	1st Stage (1)	OLS (2)	2SLS (3)	OLS (4)	2SLS (5)
ln(Domestic Sales)		0.021	−0.157	0.010	0.048
		(0.006)	(0.039)	(0.004)	(0.019)
ln(Dist-Pop-Weighted Vehicles p.c.)	0.944				
	(0.113)				
ln(TFP)	1.177	0.069	0.273	0.063	0.019
	(0.019)	(0.007)	(0.047)	(0.005)	(0.020)
ln(Average Wages)	−0.596	−0.048	−0.150	−0.042	−0.020
	(0.016)	(0.007)	(0.025)	(0.004)	(0.010)
Observations	126,024	126,024	126,024	126,024	126,024
F-statistic	69.77				

Notes: Standard errors clustered by province reported in parentheses. ln(Dist-Pop-Weighted Vehicles p.c.) is the instrument constructed using data on vehicles per capita at the municipal level and applying the weights from the gravity equation reported in column 1 of Table 1. *F*-statistic denotes the corresponding test statistic for the null hypothesis that the coefficient on ln(Dist-Pop-Weighted Vehicles p.c.) equals zero. All specifications include firm and sector-period fixed effects. The estimation sample includes all firms selling in the domestic market in at least one year in the period 2002–2008 and in the period 2009–2013.

specific export markets. Second, a richer model than that described in Section IA could easily account for the muted impact of demand-driven changes in domestic sales on the extensive margin of trade. For instance, a model that incorporates sunk costs of exporting will tend to generate hysteresis in exporting status and, in this context, any shock to export profitability may have a very different impact on the intensive and extensive margins of trade depending on firms' expectations about its persistence (see Dickstein and Morales 2018). In sum, the estimates in Table 5, and the fact that most of the growth in Spanish exports in the years following the Great Recession was due to firms that were already exporting during the boom, strongly suggest that the relationship between demand-driven changes in domestic sales and the extensive margin of exports in Spain was not quantitatively important in this period. Consequently, we focus in the remainder of this paper on variants of the intensive margin results in Table 3.

V. Robustness

In this section, we present additional evidence that further supports the empirical relevance of the "vent-for-surplus" mechanism. Specifically, we present estimates of regression specifications that address some specific sources of endogeneity that could bias the IV estimates presented in Section IV. To save on space, we only report here regressions that use our gravity-based instrument, and present in online Appendix G.1 analogous results that use our local instrument.

A. *Further Purges of the Auto Industry*

While the sample used to compute the estimates in Table 3 excludes firms classified in the manufacturing of motor vehicles sector, one may still be concerned that

Table 6—Intensive Margin: Robustness to Excluding Zip Codes Linked to Auto Industry

	Panel A. Exclude zip codes with high auto employment share		*Panel B. Exclude zip codes with at least one sizable automaker*		*Panel C. Exclude zip codes "neighboring" zip codes in Panel A*		*Panel D. Exclude sectors with input-output links to automakers*	
	1st Stage (1)	2SLS (2)	1st Stage (3)	2SLS (4)	1st Stage (5)	2SLS (6)	1st Stage (7)	2SLS (8)
Δln(Domestic Sales)		−1.693		−1.663		−1.693		−1.864
		(0.277)		(0.371)		(0.317)		(0.343)
Δln(Dist-Pop-Weighted Vehicles p.c.)	1.290		1.372		1.311		1.238	
	(0.119)		(0.166)		(0.132)		(0.130)	
Δln(Avg. Wages)	−0.511	−1.372	−0.505	−1.364	−0.490	−1.325	−0.491	−1.459
	(0.052)	(0.166)	(0.070)	(0.222)	(0.053)	(0.183)	(0.049)	(0.165)
Δln(TFP)	1.022	2.867	1.006	2.801	1.008	2.817	1.010	3.011
	(0.034)	(0.243)	(0.050)	(0.300)	(0.037)	(0.277)	(0.036)	(0.285)
Observations	7,180	7,180	4,595	4,595	6,131	6,131	6,072	6,072
F-statistic	118.30		68.58		98.78		91.09	

Notes: Standard errors clustered by province reported in parentheses. For any X, $\Delta\ln(X)$ is the log difference between the average of X in 2009–2013 and its average in 2002–2008. Δln(Dist-Pop-Weighted Vehicles p.c.) denotes the baseline instrument constructed using data on vehicles per capita at the municipal level and applying the weights from the gravity equation reported in column 1 of Table 1. F-statistic denotes the corresponding statistic for the null hypothesis that the coefficient on the Δln(Dist-Pop-Weighted Vehicles p.c.) covariate is equal to zero. See text for details on the construction of each subsample. All regressions include sector fixed effects.

the salient presence of firms in that industry in a given municipality might lead to a negative association between the boom-to-bust changes in the stock of vehicles per capita and the boom-to-bust changes in residual marginal costs shifters of the firms located in that municipality (even if they operate in other industries). This would be the case if the boom-to-bust drop in the number of vehicles per capita in a municipality was caused by an exogenous increase in marginal costs affecting the firms in the motor vehicles industry, and this negative supply shock was transmitted to other firms within the same municipality, shifting upwards their marginal cost functions.[31] Notice however that this source of endogeneity in our instrument would cause the 2SLS estimates presented in Table 3 to be upward biased, as unobserved shocks that increase firms' marginal costs would have a negative impact on their exports. In order to evaluate the robustness of our estimates to this concern, in Table 6 we report 2SLS estimates for regressions specifications analogous to those in columns 4 and 8 of Table 3, but 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 percent of zip codes by share of manufacturing employment accounted for by motor-vehicles producers (as computed from our microlevel 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 zip codes neighboring a zip code that ranks in the top 25 percent of zip codes by share of manufacturing employment in motor-vehicles

[31] For example, the post-2009 trade collapse may have increased the input costs for firms in the motor vehicles industry, which may have passed these higher costs through to their buyers, both final consumers and other firms located in the same municipality but operating in different industries.

producers.[32] 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. In all panels, we obtain slightly larger (in absolute value) estimated elasticities than in our baseline results, consistently with the hypothesis that these sample restrictions attenuate concerns about our estimates being upward biased.

B. *Alternative Instruments*

As described in detail in Section IIIA, the value of our gravity-based instrument for each municipality is computed as the log change in a weighted average of vehicles per capita in every other municipality, where the weight attached to a municipality depends on its population and the distance to the municipality of origin, and where the elasticities of this weight with respect to these covariates correspond to the estimates reported in column 1 of Table 1. In columns 2 to 4 of Table 7, we test the robustness of our results to instruments constructed similarly to our baseline gravity-based instrument, but with weights that depend on distance and population in different ways. In particular, after reproducing our baseline estimates in column 1, columns 2 to 4 present results corresponding to gravity-based instruments built using weights that depend on distance and population according to the estimates reported in columns 2 to 4 of Table 1. As a reminder, the first of these differs from our baseline in that it includes own-municipality and own-province dummies; the second one uses a more flexible specification to estimate the impact of distance on municipality-to-municipality shipments; and the third one relies on a gravity equation analogous to the one we use to build our baseline instrument, but exploits firm-to-municipality shipment flows instead of municipality-to-municipality ones. The resulting second-stage elasticities of exports to demand-driven changes in domestic sales are in all three cases very similar to the baseline ones.[33]

In column 5 of Table 7, we present results based on instruments computed in an analogous manner as in our baseline specification, but with the difference being that, instead of using weights that only vary bilaterally between municipalities and are predicted by a gravity equation, we use weights that vary across firm-municipality pairs and that correspond to the actual domestic sales share of each firm in each Spanish municipality in 2006. As in our baseline instrument, we assign a zero weight to the municipality of location of each firm. This specification results in a larger (in absolute terms) elasticity of exports to domestic sales, although the difference from our baseline of -1.6 is approximately only one standard deviation.

To address potential concerns about downward bias in our gravity estimates due to the many zeroes in our sample (see Fitzgerald and Haller 2018), we present in column 6 estimates that rely on an instrument that is analogous to our baseline except for relying on weights built as if we had obtained a coefficient of 1 on log population, a coefficient of -1 on log distance, and a coefficient of 1.5 on the province dummy as our gravity equation estimates. These are standard estimates in the

[32] We identify two zip codes as neighboring each other if they share the first four digits of their five-digit code.

[33] The results are also virtually identical when using the estimated distance and population elasticities reported in column 5 (rather than column 4) of Table 1.

Table 7—Alternative Instruments

	Δln(Domestic Sales)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. First stage with alternative instruments							
Δln(Dist-Pop-Weighted Vehicles p.c.)	1.312						
Gravity: mun-mun flows (Baseline)	(0.119)						
Δln(Dist-Pop-Weighted Vehicles p.c.)		1.322					
Baseline incl. own mun. and prov. dummies		(0.117)					
Δln(Dist-Pop-Weighted Vehicles p.c.)			0.954				
Gravity: distance dummies			(0.128)				
Δln(Dist-Pop-Weighted Vehicles p.c.)				1.310			
Gravity: firm-mun flows				(0.106)			
Δln(Weighted Vehicles p.c.)					0.521		
Weights: firm-level mun. shares					(0.111)		
Δln(Dist-Pop-Weighted Vehicles p.c.)						0.406	
Fixed coefficients: $\beta_{pop} = 1, \beta_{dist} = -1$						(0.112)	
Δln(Dist-Pop-Weighted Vehicles p.c.)							0.967
Baseline in levels							(0.115)
Adão, Kolesár, and Morales (2019) SE							(0.208)
Observations	8,009	8,009	8,009	7,906	7,850	8,009	8,009
F-statistic	122.44	128.04	55.90	152.40	21.82	13.14	70.94
							21.57
	Δln(Exports)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B. Second stage with alternative instruments							
Δln(Domestic Sales)	−1.607	−1.608	−1.336	−1.658	−2.162	−1.227	−1.701
	(0.248)	(0.245)	(0.228)	(0.212)	(0.412)	(0.415)	(0.262)
							(0.271)
Observations	8,009	8,009	8,009	7,906	7,850	8,009	8,009

Notes: Standard errors clustered by province reported in parentheses. In column 7, we first report standard errors clustered by province, and below we report standard errors that apply the formula in Adão, Kolesár, and Morales (2019). All specifications include firm-level log differences in TFP and log differences in average wages as additional controls. Additionally, all specifications also include sector fixed effects. For a detailed description of each of the instruments, see text.

intranational gravity literature that uses province-to-province trade flows.[34] The results are again similar to those in our baseline specification.

Finally, in column 7, we present results computed using an instrument that equals the exponential of our baseline instrument and that, consequently, is simply a weighted sum of the change in vehicles per capita across different municipalities. This instrument belongs to the category of shift-share instruments considered by Adão, Kolesár, and Morales (2019), thus allowing us to compute standard errors in the manner recommended in that paper. Given that both the baseline instrument and the shift-share instrument used in column 7 rely on the same identification assumptions, it is reassuring that the second-stage point estimates they yield are very similar

[34] See among, many others, Wolf (2000) for the United States; Garmendia et al. (2012) for Spain; Helliwell (1996) for Canada; Combes, Lafourcade, and Mayer (2005) for France; Poncet (2005) and Xing and Li (2011) for China; Daumal and Zignago (2010) for Brazil; and Volker (2002) and Helble (2007) for Germany. We have run an analogous gravity equation by aggregating our AEAT data at the province level, and have found a coefficient of 1.332 on log population, −1.091 on log distance, and 1.449 on a dummy for own province flows (see Table G.14 in online Appendix G.8).

(-1.607 versus -1.701). To evaluate the possible bias of standard errors clustered by province in our empirical application, in column 7 we compute both standard errors clustered by province (first number in parentheses) and the standard errors suggested in Adão, Kolesár, and Morales (2019) (second number in parentheses). As illustrated in column 7 of Table 7, the downward bias affecting the standard errors based on clustering by province is very small; while the second-stage standard error that clusters by province is 0.262, the standard error computed according to the formula introduced in Adão, Kolesár, and Morales (2019) is 0.271.[35]

In Section VE, we discuss additional alternative instrumentation strategies related to the deep roots of the differential fall in demand across Spanish regions.

C. *Controlling for Additional Confounding Factors*

The controls and various fixed effects included in our baseline specification may still not fully account for the impact on exports of certain marginal cost shifters that could be correlated with our instruments, thus biasing our estimates. We assess here the robustness of our results to the inclusion of additional proxies for firm- and municipality-level cost shifters.

The first set of additional controls aims to avoid possible biases in our IV estimates arising from the dual nature of the Spanish labor market, with large differences in dismissal costs between temporary- and permanent-contract workers. This difference in dismissal costs might have led firms to shed a disproportionate number of temporary workers during the bust. If so, given the average differences in skill and experience between both types of workers, firms shedding temporary workers may have undergone a skill and experience upgrade that changed firms' marginal production costs in a way that is not properly accounted for by our firm-level measures of productivity and average wages. The second set of additional controls aims to proxy for factor costs other than labor costs; more specifically, it aims to control for changes in the financial costs (explicit via interest rates, or implicit via rationing) that firms experienced during the Great Recession years.

After reproducing our baseline estimates in column 1, in column 2 of Table 8 we additionally control for the firm-level boom-to-bust change in the share of temporary workers. The negative and statistically significant point estimate indicates 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 about the differences in productivity between temporary and permanent workers. The IV estimate of the causal effect of demand-driven changes in domestic sales on exports is however only slightly increased (elasticity of -1.639).[36] In columns 3 and 4, we introduce municipality-level controls for local labor market

[35] The similarity in our application between the standard errors that cluster by province and those suggested in Adão, Kolesár, and Morales (2019) is hardly surprising: municipalities located in the same province assign similar weights to every other municipality and, thus, firms located in the same province are, according to our shift-share instrument, similarly exposed to changes in the stock of vehicles per capita across all Spanish municipalities.

[36] We obtain similar results when instead controlling for the (initial) firm-level share of temporary workers during the boom period. Specifically, firms that entered the bust period with a larger share of temporary workers (and thus had a larger potential to affect their skill composition when transitioning to the bust period) experienced higher export growth in the bust relative to the boom, but the estimate of our parameter of interest is largely unaffected.

Table 8—Confounding Factors

	Δln(Exports)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δln(Domestic Sales)	−1.607	−1.639	−1.618	−1.638	−1.678	−1.680	−1.611
	(0.248)	(0.251)	(0.263)	(0.260)	(0.251)	(0.257)	(0.264)
ΔShare of Temp. Workers (firm level)		−0.250					
		(0.089)					
ΔShare of Temp. Workers (muni. level)			−0.019				
			(0.170)				
ΔManufacturing Empl. p.c. (muni. level)				0.257			
				(0.047)			
Δln(Financial Costs) (firm level)					−0.027		
					(0.014)		
Financial Costs in Boom (firm level)						−0.008	
						(0.015)	
Financial Costs in Boom (muni. level)							−0.039
							(0.041)
Observations	8,009	7,640	7,743	7,745	6,879	6,945	7,741
F-statistic	122.44	131.97	138.65	136.78	88.43	89.27	139.26

Notes: Standard errors clustered by province reported in parentheses. In all specifications, Δln(Domestic Sales) is instrumented by Δln(Distance-Population-Weighted Vehicles per capita), defined as in previous tables. All specifications include firm-level log changes in TFP and in log wages as additional controls (coefficients not included to save space), and sector fixed effects.

conditions. Column 3 includes the same change in the ratio of temporary workers over total employment as in column 2, but computed at the municipality level. In column 4, we further control for a municipality-level measure of the change in the manufacturing employment per capita. The inclusion of these two controls has a negligible impact on the main coefficient of interest, and only the second one has a significant effect on exporting.

In columns 5 to 7 of Table 8, we study 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 annually reported by firms in their financial statements). In column 5, we add the log change in this firm-level measure of financial costs as an additional control; the impact of this measure on firms' changes in exports is statistically different from zero only at the 10 percent level, and including this variable only has a marginal effect on the estimate of the elasticity of exports to domestic sales (which becomes -1.678). In columns 6 and 7, 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. Regardless of whether we measure financial costs in the boom using each firm's financial information (column 6) or as the average financial costs of all other firms located in the same municipality (column 7), our results indicate that either credit rationing had little impact on firms' exports or our conjecture that

Table 9—Alternative TFP Measures

Dependent Variable:	Δln(Exports)			
TFP Measure:	TFP Sales		TFP Value Added	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)
Δln(Domestic Sales)	−0.292	−1.607	0.020	−1.057
	(0.032)	(0.248)	(0.038)	(0.197)
Δln(Average Wages)	−0.723	−1.387	−0.753	−1.024
	(0.072)	(0.151)	(0.070)	(0.100)
Δln(TFP Sales): Baseline	1.535	2.810		
	(0.057)	(0.213)		
Δln(TFP Value Added)			1.014	1.338
			(0.076)	(0.096)
Observations	8,009	8,009	8,009	8,009
F-statistic		122.44		66.29

Notes: Standard errors clustered by province reported in parentheses. For any X, $\Delta\ln(X)$ is the difference in $\ln(X)$ between its average in the 2009–2013 period and its average in the 2002–2008 period. All specifications include sector fixed effects.

it may be measured through the firms' financial costs in the boom has little empirical support.[37]

D. *Alternative Productivity Estimates*

We next test the robustness of our results to an alternative approach to measuring firms' productivity. Columns 1 and 2 in Table 9 replicate our baseline OLS and IV estimates presented in column 4 of Table 2 and column 8 of Table 3, while columns 3 and 4 of Table 9 present estimates of specifications that differ exclusively on the productivity measure.

Consistently with the model described in Section I, our baseline and alternative productivity measures exploit the assumption that firms (i) face a constant elasticity of substitution demand function and are monopolistically competitive in both the domestic and the foreign market, and (ii) take all factor prices as given. The two approaches we implement differ however on the assumptions we impose on the shape of the production function and on the underlying output series they employ in the estimation. In both estimation approaches, which we describe in detail in online Appendix F, we invoke optimality conditions for the static inputs (labor and materials) in order to estimate the relevant parameters of the production function and, in this sense, both approaches are specific cases of the general estimation framework in Gandhi, Navarro, and Rivers (2020). But while our baseline approach exploits data

[37] In online Appendix Table G.11, we additionally control for the change in the number of bank offices per capita in the municipality of location of a firm and for the change in firm-level short-term liabilities over total liabilities. We interpret the first of these two variables as an alternative proxy for firms' financial constraints, and the second one as a way of partly capturing the potential role of international trade credit in facilitating the growth of exports in municipalities that were hard-hit by the financial crisis. In online Appendix Table G.11, we also control for the change in the value of land (measured at the municipality level). The estimate of our key parameter is robust to the inclusion of these additional controls.

on the firm's total sales, the alternative approach in columns 3 and 4 uses information on the firm's value added.

As anticipated in Section IVA, a general concern with our productivity estimates is that, if we do not account in their estimation 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 0.56 for our baseline approach and 0.22 for our alternative approach. The higher correlation of our baseline approach is consistent with its partly accounting for the firm's usage of material inputs.

Perhaps reflecting the lower correlation between our alternative productivity proxy and the firm's total sales, the OLS estimator in column 3 reveals a *positive* partial correlation between exports and domestic sales. However, the IV elasticity in column 4 is again negative and, though it is significantly lower in absolute value than in our baseline specification (see column 2), it still implies a sizeable substitution between domestic sales and exports at the firm level.

A second concern with our productivity estimates is that, because we do not observe separately prices and quantities for each firm, they may capture not only the firm's actual productivity but also the firm's demand shifter. Specifically, this would be a concern if our productivity estimates were implicitly already controlling for the impact of our instrument. There is, however, no empirical evidence of this being relevant in our data: the correlation between our gravity-based instrument for location-specific demand shocks and log changes in our productivity measures is actually negative. (It is -0.18 for our baseline productivity estimate and -0.33 for our alternative one.)

E. *Additional Robustness Tests*

We finally succinctly discuss a number of additional robustness tests. To save space, these results are reported in online Appendices G and H, and we focus throughout on the gravity-based instrument. First, in columns 1 to 3 of online Appendix Table G.5, we study how our results are affected when restricting our estimation sample according to different criteria. First, we exclude Spanish subsidiaries of foreign multinationals, since both their exports and domestic sales may react to local demand shocks differently than for other firms in Spain. Second, we restrict our attention either to firms with a single manufacturing establishment or even to firms with a single establishment, since multi-establishment firms might have production plants in locations other than the one where they are incorporated and, consequently, may react to local demand shocks in the headquarters' locations very differently from single-establishment firms. No matter which of these sample restrictions we implement, the results are not significantly affected.[38] In columns 4 and 5 of online

[38] Relatedly, we have used information on the location of all car assembly plants in Spain (many of which are part of multi-plant firms), and have confirmed that our results are robust to excluding all provinces where these plants are located.

Appendix Table G.5, we also verify that our results are not materially affected when defining the bust period as 2010–2013 or 2011–2013, instead of 2009–2013.[39]

In online Appendix Tables G.6, G.8, and G.10, we modify our baseline regression specification so as to (i) include province or province-sector fixed effects, (ii) cluster standard errors at various levels other than province, and (iii) weight the observations according to different criteria (number of years exporting, log average sales during the boom period, log average employment during the boom period, and log average assets during the boom period). The inclusion of province and province-sector fixed effects and the weighting of observations have a very small effect on our estimates. Some forms of clustering (particularly two-way clustering by province and sector) tend to yield larger standard errors, but our key estimates remain significant at the 1 percent level.

Next, in online Appendix Tables G.11 and G.12, we report specifications that control for a variety of additional firm- or municipality-level supply factors; and specifications that control for firm-specific log average sales, log average employment, log average assets, or average export-to-sales ratio during the boom period. When introducing these controls, the estimates of the elasticity of interest are never lower than -1.4 or higher than -1.9 in absolute value. In online Appendix Tables G.18 and G.19, we reproduce our findings when aggregating the firm-level data at the municipality-sector level. When estimating our baseline specification at the municipality-sector level, we obtain lower (in absolute value) elasticities of exports with respect to domestic sales, but the qualitative nature of our findings remains unaffected.

In online Appendix H, we explore alternative strategies to identify the potential substitutability of domestic and export markets. First, in online Appendix H.1, we follow the identification approach implemented by Berman, Berthou, and Héricourt (2015) to estimate the causal impact of demand-driven changes in exports on domestic sales, swapping then the role that these two variables play in our main specification. Due to data restrictions (see Section IIC), we can only carry out such analysis for the period 2002–2007. Consistently with our main findings, and contrary to the results in Berman, Berthou, and Héricourt (2015) using French data, we find a *negative* effect of demand-driven changes in exports on domestic sales. Second, in online Appendix H.2, we re-estimate the parameters of our main regression specification but explore alternative instrumentation strategies that focus on the deep roots of the differential fall in demand across Spanish regions. More specifically, we posit that, relative to the boom years, municipality-level demand shocks were larger (i) in municipalities with lower housing supply elasticities (in which house prices grew disproportionately during the boom years), (ii) in municipalities with a larger precrisis contribution of the construction sector to total labor income, and (iii) in provinces that experienced larger declines in tourism during the bust years.[40] We then weigh the municipality-specific demand shocks in (i) and (ii) following

[39] The motivation for exploring these alternative definitions of the bust period is that the acceleration in the growth rate of Spanish exports starts in 2010 (see Figure 2) but, relative to other countries in the euro area, this acceleration starts in 2011 (see Figure 1).

[40] 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 manufactured goods.

the same procedure as in our baseline gravity-based instrument. The second-stage estimates of our parameter of interest computed using these alternative instruments are all negative and generally a bit lower in absolute value than our baseline one; however, one should interpret these estimates with caution, as these instruments are generally not as strong as our baseline one.[41]

VI. Model with Increasing Marginal Costs

Our empirical results suggesting a negative impact of demand-driven changes in domestic sales on changes in exports are in contradiction with the framework described in Section I. In this section, we show how a simple extension of that framework can rationalize our empirical results.

A. *Model with Increasing Marginal Costs: Estimating Equation*

The theoretical environment we consider here is identical to that in Section I, 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

$$\frac{1}{\tilde{\varphi}_i}\omega_i\frac{1}{\lambda+1}\left(\tau_d Q_{id}+\tau_x Q_{ix}\right)^{\lambda+1}, \qquad \lambda \geq 0, \tag{14}$$

where $\tau_d Q_{id} + \tau_x Q_{ix}$ denotes firm i's total output in the presence of iceberg trade costs in the domestic (τ_d) and foreign (τ_x) markets. Notice that the parameter λ governs how steeply marginal costs increase with output. When $\lambda = 0$, marginal costs are constant and equation (14) reduces to our previous expression in equation (2). We show in Appendix A that the cost function in equation (14) can be derived in a model in which the firm's production function is a Cobb-Douglas aggregator of a fixed or predetermined input and a flexible and static input; without loss of generality, we can refer to these two inputs as capital and labor, respectively. Under this microfoundation, the parameter λ is decreasing in the elasticity of output with respect to the flexible factor, and λ is equal to zero when this elasticity is equal to one. Note also that we denote firm productivity with the new notation $\tilde{\varphi}_i$ (rather than φ_i), as the microfoundation in Appendix A shows that this productivity level $\tilde{\varphi}_i$ depends not only on the TFP level φ_i but also on the stock of fixed factors.

Solving for the optimal level of exports by firm i under the cost function in equation (14), and taking log differences, we obtain

$$\begin{aligned}\Delta \ln R_{ix} = {} & (\sigma-1)\left[\Delta\ln\xi_{ix}+\Delta\ln\tilde{\varphi}_i-\Delta\ln\omega_i\right] \\ & -(\sigma-1)\left(\Delta\ln\tau_{sx}-\Delta\ln P_{sx}\right)+\Delta\ln E_{sx} \\ & -(\sigma-1)\lambda\Delta\ln\left(\tau_d Q_{id}+\tau_x Q_{ix}\right),\end{aligned} \tag{15}$$

[41] When adding each of these instruments one by one to our baseline gravity-based instrument, standard tests of overidentifying restrictions fail to reject at typically used significance levels the null hypothesis that our instruments are jointly valid.

which is analogous to equation (4) except for the last term, which reflects the effect of total output on marginal production costs.[42] Next, note that, due to constant markup pricing, we can write

$$\ln\left(\tau_d Q_{id} + \tau_x Q_{ix}\right) = \ln\left(\frac{\tau_d R_{id}}{P_{id}} + \frac{\tau_x R_{ix}}{P_{ix}}\right) \tag{16}$$

$$= \ln\left(R_{id} + R_{ix}\right) - \ln\left(\frac{\sigma \omega_i \left(\tau_d Q_{id} + \tau_x Q_{ix}\right)^{\lambda}}{(\sigma - 1)\tilde{\varphi}_i}\right).$$

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 that

$$\Delta \ln R_{ix} = \gamma_{sx} + \frac{(\sigma - 1)}{1 + \lambda} \delta_{\varphi} \Delta \ln \tilde{\varphi}_i^* \tag{17}$$

$$- \frac{(\sigma - 1)}{1 + \lambda} \delta_{\omega} \Delta \ln \omega_i^* - \frac{(\sigma - 1)\lambda}{1 + \lambda} \Delta \ln\left(R_{id} + R_{ix}\right) + \varepsilon_{ix},$$

where $\varepsilon_{ix} \equiv u_{ix}^{\xi} + ((\sigma - 1)/(1 + \lambda))(u_i^{\varphi} - u_i^{\omega})$. This equation is analogous to equation (10), except that it features the log difference of *total* sales (instead of domestic sales) on the right-hand side, and that it calls for the inclusion of the firm's stock of fixed factors as an additional control. 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 I, but also because the fact that the log change in total sales naturally depends on the log change in exports implies that any unobserved determinant of exports accounted for by the regression residual will be correlated with our covariate of interest and, thus, will bias the OLS estimate of $(\sigma - 1)\lambda/(1 + \lambda)$. Importantly, because the regression residual in equation (17) depends on the same terms as those in equation (10) (i.e., unobserved productivity, factor costs and export demand shifters), a 2SLS estimator based on our instrument will deliver consistent estimates of this regression coefficient as long as the identification assumptions outlined in Section IA hold. Consequently, the threats to the validity of our instrument discussed in Section IIIB also apply here.

In Table 10, we present OLS and 2SLS estimates of the regression coefficients in equation (17). In columns 1 to 3 of panels A and B, we include in the regression specification the same controls as in Tables 2 and 3. In columns 4 and 5, we additionally control for the change in the stock of capital, as indicated by the microfoundation in Appendix A. As expected, the OLS estimates in column 1 indicate a strong positive correlation between exports and total sales, even when controlling for sector fixed effects and for our measures of firms' average wages and TFP. The first-stage results

[42] All expressions in this section implicitly assume there are only two markets, one domestic market and one foreign market. For an extension of this model to a setting with multiple domestic and foreign markets, see online Appendix E.2.

Table 10—Intensive Margin with Total Sales

	Δln(Exp) OLS (1)	Δln(TotSales) 1st Stage (2)	Δln(Exp) 2nd Stage (3)	Δln(TotSales) 1st Stage (4)	Δln(Exp) 2nd Stage (5)
Panel A. Local instrument					
Δln(Total Sales)	0.734		−2.038		−2.253
	(0.037)		(0.633)		(0.687)
Δln(TFP)	0.494	1.033	3.339	0.987	3.404
	(0.058)	(0.025)	(0.639)	(0.025)	(0.662)
Δln(Average Wages)	−0.205	−0.499	−1.579	−0.483	−1.633
	(0.054)	(0.032)	(0.312)	(0.031)	(0.330)
Δ Ln(Vehicles p.c. in municipality)		0.272		0.255	
		(0.051)		(0.047)	
Δ ln(Stock of Capital)				0.102	0.348
				(0.009)	(0.076)
Observations	8,009	8,009	8,009	8,009	8,009
Sector fixed effects	Yes	Yes	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes	Yes	Yes
F-statistic		28.99		29.27	
Panel B. Gravity-based instrument					
Δln(Total Sales)	0.724		−2.374		−2.590
	(0.050)		(0.526)		(0.606)
Δln(TFP)	0.509	1.063	3.690	1.015	3.739
	(0.055)	(0.026)	(0.482)	(0.027)	(0.539)
Δln(Average Wages)	−0.217	−0.509	−1.750	−0.493	−1.801
	(0.063)	(0.043)	(0.250)	(0.041)	(0.282)
Δln(Dist-Pop-Weighted Vehicles p.c.)		0.888		0.838	
		(0.103)		(0.107)	
Δln(Stock of Capital)				0.101	0.382
				(0.009)	(0.067)
Observations	8,009	8,009	8,009	8,009	8,009
Sector fixed effects	Yes	Yes	Yes	Yes	Yes
F-statistic		75.00		61.30	

Notes: Standard errors clustered by province reported in parentheses. For any X, $\Delta\ln(X)$ is the difference in $\ln(X)$ between its average in the 2009–2013 period and its average in the 2002–2008 period. All specifications include sector fixed effects.

in column 2 indicate that both the local and the gravity-based instruments are strong predictors of a firm's total sales, with an F-statistic of 28.99 and 75.00, respectively. The second-stage elasticities of exports to total sales in column 3 are negative and significant and stand at a value of -2.038 and -2.374, respectively. Adding the boom-to-bust log change in the firm's stock of physical capital does not affect significantly the first-stage nor the second-stage results. Thus, henceforth, we treat the estimates in column 3—specifically the one in panel B—as our baseline estimates.

To understand the magnitude of our estimates, take a firm with an initial export share of 16.2 percent (which corresponds to the median export share during the boom in our sample of 8,009 continuing exporters). Suppose that, due to a drop in demand, this firm experiences a 1 percent drop in its domestic sales. Our estimated elasticity of exports to domestic sales in Table 3 indicates that, other things equal, the firm should see its exports increase by 1.6 percent. This also implies that the firm's total sales will decrease by $83.8\% \times 1\% + 16.2\% \times (-1.6\%) = 0.58\%$. For this change in total sales, our estimated elasticity of exports to total sales in panel B of Table 10 suggests an implied increase in exports of $0.58\% \times 2.374 = 1.4\%$, which is close to the 1.6 percent increase predicted by the estimates in Table 3. This demonstrates that our IV results in Tables 3 and 10 deliver congruent estimates for the response of exports to local demand shocks.[43]

With an estimate of the demand elasticity σ in hand, it is easy to infer an estimated value of λ from the estimates in Table 10. Specifically, given the estimates in column 3 of panel B, we can compute an estimate of λ as $\hat{\lambda} = 2.374/(\sigma - 1 - 2.374)$. For $\sigma = 6$, we obtain $\hat{\lambda} = 0.90$, which indicates a significant departure from constant marginal costs.

To complement these findings, in online Appendix I we conduct additional tests of several implications of the vent-for-surplus mechanism. These tests are based on the idea that one should expect the increase in exports in reaction to a common demand-driven drop in domestic sales to be larger for those firms whose short-run marginal cost function is steeper or, equivalently, for those firms whose elasticity of output with respect to flexible inputs is lower. The results in online Appendix Table I.8 confirm that the elasticity of the change in exports to changes in total sales is higher for firms having lower output elasticities with respect flexible inputs, although few of the results are statistically significant at standard levels.

VII. Quantification

In this final section, we use the extended model with increasing marginal costs to evaluate the quantitative importance of the "vent-for-surplus" channel for explaining the remarkable growth in Spanish exports during the period 2009–2013.

To achieve this goal, we implement a three-step procedure. First, we use the model described in Section VI to trace the impact that arbitrary counterfactual changes in Spain's sectoral domestic aggregate demand shifters—or $B_{sd} \equiv E_{sd}/P_{sd}$, where E_{sd} denotes the total expenditure in Spain in sector s and P_{sd} is the price index of sector s in Spain—would have had on Spanish aggregate exports, domestic and total sales. In doing so, we quantify the impact that these demand changes had on firms' sales exclusively via the "vent-for-surplus" channel (i.e., the movement of firms along their marginal cost curves). Second, we perform a variance decomposition of the observed change in firms' total sales with the aim of informing the precise extent to which B_{sd} fell during the crisis. With this number at hand, in our last step, we compute our model's prediction for the counterfactual change in aggregate exports that would have been observed if there had been no change in aggregate demand shifters between the boom and the bust. If this number is small relative to the observed boom-to-bust growth in exports, we can conclude that the "vent-for-surplus" channel must have been an important determinant of the Spanish export miracle during the period 2009–2013.

[43] In online Appendix I, we present results for specifications analogous to those in Tables 2 to 9, with the only difference being that the boom-to-bust log change in *total* rather than *domestic* sales is included as right-hand-side variable. The conclusions discussed in Section V are generally corroborated by the results reported in online Appendix I.

We next explain these steps in more detail, and also highlight the underlying assumptions behind them.

Step 1 (Computing Counterfactual Changes in Aggregate Domestic Sales and Exports for Given Changes in the Sectoral Demand Shifters): $\{B_{sd}\}_{s=1}^{S}$. Through the lens of our model, we capture the domestic demand changes that affected Spanish firms between the boom and the bust as changes in the sectoral demand shifters $\{B_{sd}\}_{s=1}^{S}$. According to our model, changes in B_{sd} for a sector s determine changes in the residual demand function that each firm in s faces and, thus, from the perspective of each individual firm, are purely demand shifters.[44]

The system of equations that allows us to map relative counterfactual boom-to-bust changes in sectoral demand shifters to changes in aggregate domestic sales and exports (and, thus, total sales) boils down to three sets of equations. The first one maps the boom-to-bust counterfactual change in exports of each firm to its counterfactual change in domestic sales. The second one maps the boom-to-bust counterfactual change in domestic sales of each firm to its counterfactual change in exports and to the counterfactual changes in the aggregate demand shifter and price index in the firm's sector. Finally, the third one maps the counterfactual change in the price index of each sector to the counterfactual change in the aggregate demand shifter of the corresponding sector. We describe these three sets of equations in detail in online Appendix J.1.

Using these equations, we compute the impact that relative counterfactual changes in sectoral demand shifters—$(B'_{sd1}/B_{sd0})(B_{sd1}/B_{sd0})^{-1}$—have on firm-specific counterfactual changes in exports and domestic sales. We then aggregate these firm-specific counterfactual changes across all firms active in the boom and bust periods and thus construct counterfactual changes in aggregate domestic sales and exports. When performing our baseline quantification, we set $\sigma = 5$, which is a central value in the range of estimates used in the international trade literature (see Head and Mayer 2014), and $(\sigma - 1)\lambda/(1 + \lambda) = 2.374$, which corresponds to the estimate reported in column 3 of Table 10.[45]

Among all the relative counterfactual changes in sectoral demand shifters we could consider, we focus on relative changes that are constant across sectors; i.e., $(B'_{sd1}/B_{sd0})(B_{sd1}/B_{sd0})^{-1}$ equals a constant, which we denote as Γ_B. In Figure 4, we plot the changes in aggregate exports, domestic sales, and total sales predicted by our model when Γ_B takes different values between 0.5 and 1.5. For the set of firms that we use in our counterfactual analysis, we observe in our data that, between the boom and the bust, aggregate domestic sales dropped by 15.91 percent, exports grew by 11.99 percent, and total sales dropped by 10.23 percent. These are the values that our counterfactual analysis naturally generates when we set the relative counterfactual change in the aggregate demand shifter of every sector s to equal one;

[44] In models that endogenize these demand shifters, changes in B_{sd} could be due to a variety of factors. For example, in a model in which consumers have Cobb-Douglas preferences over the S manufacturing sectors we consider in our analysis and other "outside" sectors, decreases in the share of spending on manufacturing goods (e.g., a decrease in the Cobb-Douglas parameters associated with manufacturing sectors) would reduce the demand shifters $\{B_{sd}\}_{s=1}^{S}$. In a model in which consumers own houses, demand shifters would respond to wealth effects associated to changes in housing prices, which may themselves reflect changes in expectations of future housing demand and supply shocks.

[45] In online Appendix J.4, we explore how robust our results are to alternative values of $(\sigma - 1)\lambda/(1 + \lambda)$.

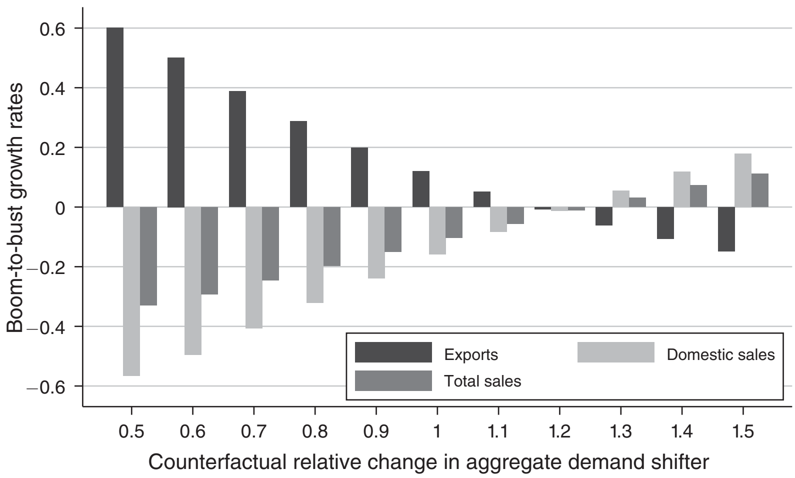

Figure 4. Impact of Aggregate Demand Shocks

Notes: The horizontal axis indicates the value of Γ_B. The export and domestic sales growth rates indicated in the vertical axis correspond to those computed using equations (J.4), (J.5), and (J.11), described in online Appendix J.1. Given these counterfactual growth rates in export and domestic sales, we compute the counterfactual growth rate in total sales as $(R'_{ix1}/R_{ix0})\chi_{i0} + (R'_{id1}/R_{id0})(1 - \chi_{i0})$.

i.e., $\Gamma_B = 1$. If the value of the aggregate demand shifters in the bust had been 50 percent smaller than they actually were (i.e., $\Gamma_B = 0.5$), our model predicts that aggregate domestic sales would have dropped by 56.64 percent and aggregate exports would have increased by 60.1 percent. In this case, aggregate total sales would have dropped by 32.87 percent. Conversely, if it had been 50 percent larger (i.e., $\Gamma_B = 1.5$), aggregate domestic sales would have grown by 17.82 percent and aggregate exports would have dropped by 14.76 percent, and the result would have been an 11.18 percent growth in total sales.

The predictions above rely on several key assumptions. First, when computing the aggregate change in domestic sales and exports that would have been observed for different changes in the demand shifter, we effectively maintain the boom-to-bust changes in the supply parameters $(\varphi_i, \omega_i, \tau_{sx}, \tau_{sd})$ and in the firms' idiosyncratic demand shifters (ξ_{id}, ξ_{ix}) at their realized values, independently of the size of the counterfactual demand change. Admittedly, the simultaneous move of all Spanish firms along their marginal cost curves could in principle have impacted equilibrium wages and, more generally, equilibrium input prices. Although we do not take these effects into account in our baseline quantification, as a robustness check, we present in online Appendix J.5 results in which we illustrate how our main predictions are affected if we allow firms' wages to change as firms move along their marginal cost curves.[46]

Second, our counterfactual calculations rely on the assumption that, in every sector, Spain is a small open economy; thus, counterfactual boom-to-bust changes in Spanish

[46]The movement of each firm along its marginal cost curve will impact its output price and, consequently, the simultaneous move of all firms along their marginal cost curves will impact the domestic sectoral price index P_{sd} for every sector s. This effect *is* taken into account in our quantification.

aggregate demand affect neither the boom-to-bust change in the foreign price index nor the boom-to-bust change in foreign firms' marginal production costs.[47]

Third, consistently with the estimates presented in Table 5, we assume that firms do not change their export status in reaction to changes in the domestic demand shifters. Consequently, we focus on quantifying the impact of counterfactual changes in aggregate demand on the aggregate volume of exports of those firms that export during both the boom and the bust periods.

Step 2 (**Decomposing the Variance of Total Sales**)**:** Our ultimate goal is not to indicate how aggregate exports and domestic and total sales react to arbitrary counterfactual domestic demand changes, but to predict the change in these variables that we would have observed if demand shifters in Spain had remained constant between the boom and the bust. Doing so requires measuring the extent to which the Spanish domestic demand actually fell between these two periods. Given the lack of firm-specific output prices in our dataset, our measure of Spain's boom-to-bust drop in domestic demand shifters uses as key input a decomposition of the variance of firms' boom-to-bust changes in total sales into changes due to demand factors and changes explained by supply factors. This variance decomposition exercise is analogous to that implemented in Autor, Dorn, and Hanson (2013) with the purpose of measuring the impact that productivity growth in China had on the growth in US imports from China during the period 1990–2007. Specifically, we use equation (17) to decompose the variance of $\Delta \ln(R_i)$, with $R_i \equiv R_{id} + R_{ix}$, into a component due to firms' marginal cost and export demand shifters, and a component attributed to factors orthogonal to these shifters (see online Appendix J.2 for details). We find the contribution of the combination of marginal cost and export demand shifters to be 59 percent, and that of factors orthogonal to it to be 41 percent. On the basis of this number, we infer that 41 percent of the 10.23 percent drop in total sales between the boom and the bust periods was due to domestic demand factors.[48]

Step 3 (**Quantification Results**)**:** Given the finding that 41 percent of the 10.23 percent boom-to-bust drop in total sales was due to changes in demand, we use the counterfactual results described in Step 1 to find the value of Γ_B for which our model predicts a drop in total sales of $6.04\% = (1 - 41\%) \times 10.23\%$. Intuitively, this is the drop in total sales that we would have observed if aggregate demand shifters had not changed between the boom and the bust. Our model predicts a drop in total sales of 6.04 percent if $\Gamma_B = 1.09$.[49] For this value of Γ_B, our model predicts that

[47] In a model with increasing marginal costs such as ours, the invariability of the foreign price index to changes in Spanish aggregate demand shifters implies that Spain is a small exporter to the rest of the world, and the invariability of foreign firms' marginal production costs implies that Spain is a small importer from the rest of the world.

[48] Being precise, our decomposition reveals that 41 percent of the variance of the changes in firms' total sales is due to *any* factor orthogonal to firms' marginal cost shifters and export demand shocks. Thus, our conclusion that changes in demand shifters explain 41 percent of the variance of the changes in firms' total sales implicitly assumes that these shifters are the only determinants of firms' total sales whose boom-to-bust changes are orthogonal to changes in firms' marginal cost shifters and export demand shocks.

[49] If it were true that, as we impose in our analysis, the boom-to-bust change in the aggregate demand shifters $\{B_{sd}\}_{s=1}^{S}$ was the same across all manufacturing sectors $s = 1, \ldots, S$, then we can infer from our analysis that the aggregate demand shifters fell between the boom and the bust in $1 - 1/\Gamma_B^* = 1 - 1/1.09 = 1 - 0.9174 = 8.26$ percent.

exports would have grown by 5.79 percent. As the observed growth in exports was 11.99 percent, our analysis indicates that the vent-for-surplus mechanism explains $(11.99\% - 5.79\%)/11.99\% = 51.71\%$ of the total growth in exports.[50]

VIII. Conclusion

In this paper, we provide evidence suggesting that export and domestic sales decisions are interdependent at the firm level. Faced with a severe domestic slump during the Great Recession, Spanish producers appear to have experienced a decline in their short-run marginal production costs, with this gain in competitiveness translating into an increase in their sales in foreign markets. We circumvent the inherent difficulties associated with establishing a causal link between demand-driven changes in domestic sales and exports by exploiting geographic variation in the incidence of the Great Recession in Spain.

Our empirical findings are inconsistent with international trade models featuring constant markups and technologies with constant marginal costs of production. We 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 approximately half of the growth in Spanish exports in the period 2009–2013. Although there are a few singular aspects of the Spanish experience in the years around the Great Recession that may make the "vent-for-surplus" mechanism particularly important in this context (e.g., the large boom in investment preceding the bust, or the sclerotic nature of the Spanish labor market), we think that some of the insights and methodology in our paper can be transported to other countries that experienced severe domestic slumps.

Our paper also offers a new perspective on the literature studying interdependencies in the extensive and intensive margins of trade (e.g., Antràs, Fort, and Tintelnot 2017; or Morales, Sheu, and Zahler 2019). Due to data limitations, we have restricted our analysis to the study of interdependencies between the domestic market and a single (aggregate) export destination, and we have modeled these interdependencies as arising exclusively from an increasing marginal cost function. In such a case, the firm's profit function is submodular in the extensive margin of trade, a feature that would be preserved in a model with multiple export markets, as shown in online Appendix E.2. With access to data on firms' exports by destination market, and borrowing tools from Arkolakis and Eckert (2017), one could estimate the key parameters of a multicountry version of our model, and thus explore interdependencies also in the intensive and extensive margin of trade *across export markets*. Even more ambitiously, with better data on prices, one could potentially expand our analysis to explore the extent to which endogenous markup adjustments (see De Loecker et al. 2016) or price stickiness in both the domestic and export prices (Gopinath and Rigobon 2008; Nakamura and Steinsson 2008, 2013) affect the way in which a

[50] Our analysis accounts for the changes in trade costs that took place between the boom and the bust. In online Appendix J.3, we explore further the role that trade costs may play in inhibiting firms' ability to exploit the vent-for-surplus mechanism. Our results show that trade costs play an important role: if trade costs in the bust had been 10 percent larger than they were, then total sales would have dropped by 13.97 percent (while they dropped by 10.23 percent in the data).

firm's exports react to domestic demand shocks. We leave the study of these questions for future research.

Appendix

A1. *Convexity of the Short-Run Marginal Cost Function*

Suppose a firm's production function depends on fixed or predetermined input K_i and a flexible and static input L_i. Let us refer to the former as capital and the latter as labor. Assuming a Cobb-Douglas technology in capital and labor, the cost minimization problem of a firm with productivity φ_i seeking to produce a total amount of output Q_i can be expressed as

$$\min \omega_i L_i,$$

subject to

$$\varphi_i K_i^{\alpha_K} L_i^{\alpha_L} \ \geq \ Q_i,$$

where ω_i denotes the nominal wage that firm i faces, and α_K and α_L denote then the output elasticities with respect to capital and labor, respectively. The first-order condition of the cost-minimization problem of the firm delivers

$$\omega_i \ = \ \mu \alpha_L \frac{Q_i}{L_i},$$

$$\varphi_i K_i^{\alpha_K} L_i^{\alpha_L} \ = \ Q_i,$$

where μ denotes the Lagrange multiplier on the constraint $\varphi_i K_i^{\alpha_K} L_i^{\alpha_L} \ = \ Q_i$. After solving for L_i in the second of these equalities, we can rewrite the short-run total costs as a function of output, Q_i, as follows:

$$\omega_i L_i \ = \ \omega_i \big(\varphi_i K_i^{\alpha_K}\big)^{-\frac{1}{\alpha_L}} \big(Q_i\big)^{\frac{1}{\alpha_L}}.$$

Note that, unless $\alpha_L \ = \ 1$, this short-run total cost function is not linear in the total output Q_i. More specifically, as long as $0 \ < \ \alpha_L \ < \ 1$, this short-run cost function will be convex in Q_i. Using $\tilde{\varphi}_i$ to denote a shifter of the short-run costs, and using λ to denote the deviation of the output elasticity of the short-run cost function relative to the case in which this function is linear in Q_i, i.e.,

$$\tilde{\varphi}_i \ = \ \alpha_L \big(\varphi_i K_i^{\alpha_K}\big)^{\frac{1}{\alpha_L}},$$

$$\lambda \ = \ \frac{1 \ - \ \alpha_L}{\alpha_L},$$

we can rewrite the short-run total costs as

$$\omega_i L_i \ = \ \frac{1}{\tilde{\varphi}_i} \omega_i \frac{1}{1 \ + \ \lambda} \big(Q_{it}\big)^{1+\lambda}.$$

The elasticity of the short-run total costs with respect to output is thus

$$\frac{\partial \ln(\omega_i L_i)}{\partial \ln(Q_{it})} = 1 + \lambda.$$

Note that, the lower the value of α_L (i.e., the lower the elasticity of output with respect to the flexible input), the larger the elasticity with respect to output of the short-run total cost function. The curvature of the total cost schedule is thus crucially shaped by the parameter determining the elasticity of output with respect to the flexible input.

Data Appendix

B1. *Macroeconomic Data*

Data on merchandise exports and real GDP shares for the countries that belong to the European Monetary Union used in Figure 1 come from the UN Comtrade database (United Nations, n.d.) and the annual macro-economic database of the European Commission's Directorate General for Economic and Financial Affairs, AMECO (European Commission, n.d.b). Data on Spanish unemployment, real GDP, internal demand, private final consumption expenditure and exports of goods used in Figure 2 come from the Spanish National Statistical Office (INE n.d.b, n.d.c, n.d.d). Data on unit labor costs (ULC) in the manufacturing sector for Spain and the European Monetary Union were obtained from the Bank of Spain (Banco de España, n.d.b) and the OECD dataset on productivity and ULC by main economic activity (OECD, n.d.). We use the input-output tables produced by the Spanish National Statistical Office 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 6).

B2. *Construction of the Commercial Registry Dataset*

As described in Section IIC, our main source of firm-level data is the Commercial Registry (*Registro Mercantil Central*), which contains annual financial statements of around 85 percent of registered firms in the nonfinancial economy. We collect data from two separate sources to construct our own firm-level dataset: (i) the *Central de Balances* dataset from the Bank of Spain (Banco de España n.d.a); and (ii) *Sabi*, from Informa, a private company (Informa, n.d.). The Bank of Spain made an effort to expand and treat the information for small firms gathered in the Commercial Registry but the Central de Balances dataset does not cover the universe of private-sector firms. In particular, this dataset excludes, mainly, medium and large firms that submit information after the regular submission deadline or that do not use a digital support. Conversely, the Informa dataset puts special emphasis on compiling information on large and medium-sized firms that submit their statements either late or on paper. We combine the information in these two datasets to take advantage of their complementarities in order to maximize the coverage of the resulting database. 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).

In terms of the sectoral disaggregation of the data, note that NACE (*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 sectoral classification during the period of analysis. In the boom-to-bust regressions reported later, we assign to these firms a fixed zip code and sector using their most frequent value in each case. A firm's zip code corresponds to the location of its headquarters.

B3. *Foreign Transactions Dataset*

As described in Section IIC, until 2014 the Bank of Spain required all financial institutions and a set of large companies to report all foreign transactions, including imports, exports, and other financial transactions. Until 2007, there is information for each transaction on the country of destination (or origin). However, from 2008 to 2013, 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 data corresponds 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 exports or imports by country of destination (or origin) nor by product in a consistent way for periods spanning around 2008. The foreign transactions registry collected by the Bank of Spain was discontinued in early 2014. Since then, the Bank of Spain's monitoring of foreign transactions mainly relies on aggregate data built from transaction-level information that is provided by the Spanish tax administration.

B4. *Minimum Reporting Threshold*

Between 2001 and 2007, all foreign transactions of more than €12,500 had to be reported to the Bank of Spain. In order to reduce the compliance costs for reporting institutions, the minimum reporting threshold was updated in 2008 to €50,000. 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 2002–2013, we apply the post-2008 minimum reporting threshold to the data from 2002 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 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.

B5. *Vehicles per Capita and Tax Records of Firm-Level Sales within Spain*

The information on the stock of vehicles by both municipality and province is provided by the Spanish Registry of Motor Vehicles (DGT, n.d.). According to Spanish Law, vehicles have to be registered in the municipality where the owner has her permanent residence. This residence status should match the one reported in the municipal census (*Padrón*). In the case of legal entities (business or institutions), vehicles must be registered in the municipality where they undertake their main activity, which should match the location reported to the tax authorities. The census of vehicles at the municipal level is maintained by the General Directorate for Traffic (*Dirección General de Tráfico*). Each city council has the capacity to levy a small fee on the registered vehicles in its municipality (*Impuesto sobre Vehículos de Tracción Mecánica*). This fee usually depends on several criteria such as vehicle power, type of vehicle, pollution level, etc. In aggregate terms, these fees collectively raised tax revenue equivalent to around 0.2 percent of GDP in 2016.

The information on population both at the municipality and province levels is provided by the Spanish National Statistical Office (INE, n.d.a).

Regarding our data on firm-level sales within Spain, Spanish Tax Law mandates that all firms (legal entities) and professionals (natural persons) that undertake economic activities must report detailed information on the transactions with their trading partners. In 2006, this information is collected in Form 347, officially called "Annual information return on transactions with third parties." We work with data for the year 2006 because it is the first year for which a precise and consistent comprehensive digitization of the data is available. In particular, each business must report the monetary value of its individual sales to each trading partner. The reported transactions include all domestic sales to businesses, households and the public sector. The law uniquely exempts mandatory reporting of individual transactions when the annual aggregate sales to a trading partner do not exceed €3,005.06. This tax record of sales to third-parties is a fundamental tool of tax enforcement for both the VAT and the corporate income tax given that transactions included in Form 347 must be reported consistently in both tax returns.

The Spanish Tax Agency (*Agencia Estatal de Administración Tributaria*, AEAT) shared with the Bank of Spain aggregate data on municipality-to-municipality flows for firms in the manufacturing sector, excluding sales of businesses in the auto industry, for the year 2006 (AEAT, n.d.). In particular, for each municipality where manufacturing sellers are located, the Spanish Tax Agency computed the total amount of sales to any municipality where purchases are made. We thus have access to a matrix of bilateral flows of manufactured goods between Spanish municipalities. This matrix contains data on 485,565 municipality-to-municipality flows, with 2,305 municipalities of origin and 6,623 municipalities of destination. After restricting the sample of municipalities to those observed in our subsample of continuing exporters, the dataset contains a matrix of 412,500 bilateral municipality flows, with 1,224 municipalities of origin and 6,587 municipalities of destination.

Apart from the matrix of aggregate municipality-to-municipality flows, the Spanish Tax Agency also provided us, for each firm in our sample of 8,009 continuing exporters, the 2006 share of its total domestic sales going to each Spanish municipality.

REFERENCES

Adão, Rodrigo, Michal Kolesár, and Eduardo Morales. 2019. "Shift-Share Designs: Theory and Inference." *Quarterly Journal of Economics* 134 (4): 1949–2010.

AEAT. n.d. "Flujos de Ventas Municipales de las Empresas en el Sector Industrial en el Año 2006." Agencia Estatal de Administración Tributaria, Madrid (accessed September 2019).

Ahn, JaeBin, and Alexander F. McQuoid. 2017. “Capacity Constrained Exporters: Identifying Increasing Marginal Cost.” *Economic Inquiry* 55 (3): 1175–91.

Almunia, Miguel, Pol Antràs, David Lopez-Rodriguez, and Eduardo Morales. 2021. “Replication Data for: Venting Out: Exports during a Domestic Slump.” American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]. https://doi.org/10.3886/E136861V1.

Almunia, Miguel, David Lopez-Rodriguez, and Enrique Moral-Benito. 2018. “Evaluating the Macro-representativeness of a Firm-level Dataset: An Application for the Spanish Economy.” Banco de España Occasional Paper 1802.

Anderson, James E., and Eric van Wincoop. 2003. “Gravity with Gravitas: A Solution to the Border Puzzle.” *American Economic Review* 93 (1): 170–92.

Antràs, Pol. 2011. “El Comportamiento de las Exportaciones Españolas.” In *Apuntes Fedea*, Vol. 2, 1–11.

Antràs, Pol, Teresa C. Fort, and Felix Tintelnot. 2017. “The Margins of Global Sourcing: Theory and Evidence from US Firms.” *American Economic Review* 107 (9): 2514–64.

Arkolakis, Costas, and Fabian Eckert. 2017. “Combinatorial Discrete Choice.” Unpublished.

Autor, David H., David Dorn, and Gordon H. Hanson. 2013. “The China Syndrome: Local Labor Market Effects of Import Competition in the United States.” *American Economic Review* 103 (6): 2121–68.

Banco de España. n.d.a. “Central de Balances Integrada (2000–2013).” Banco de España, Madrid (accessed March 2018).

Banco de España. n.d.b. “Estadística de costes laborales unitarios (2002–2013).” Banco de España, Madrid (accessed March 2018).

Banco de España. n.d.c. “Microdatos de Transacciones con el Exterior, Balanza de Pagos (2000–2013).” Banco de España, Madrid (accessed March 2018).

Basco, Sergi, David Lopez-Rodriguez, and Ferran Elias. 2021. “Credit Supply, Education and Mortgage Debt: The BNP Securitization Shock in Spain.” Unpublished.

Berman, Nicolas, Antoine Berthou, and Jérôme Héricourt. 2015. “Export Dynamics and Sales at Home.” *Journal of International Economics* 96 (2): 298–310.

Blum, Bernardo S., Sebastian Claro, and Ignatius J. Horstmann. 2013. “Occasional and Perennial Exporters.” *Journal of International Economics* 90 (1): 65–74.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel. 2020. “Quasi-Experimental Shift-Share Research Designs.” Unpublished.

Combes, Pierre-Philippe, Miren Lafourcade, and Thierry Mayer. 2005. “The Trade-Creating Effects of Business and Social Networks: Evidence from France.” *Journal of International Economics* 66 (1): 1–29.

Correia, Sergio. 2017. “REGHDFE: Stata Module to Perform Linear or Instrumental-Variable Regression Absorbing Any Number of High-Dimensional Fixed Effects.” Statistical Software Components s457874, Boston College Department of Economics. https://ideas.repec.org/c/boc/bocode/s457874.html.

Correia, Sergio. 2019. “IVREGHDFE: Stata Package That Integrates reghdfe into ivreg2, Through an Absorb() Option, Allowing IV/2SLS Regressions with Multiple Levels of Fixed Effects.” Available at: https://github.com/sergiocorreia/ivreghdfe.

Daumal, Marie, and Soledad Zignago. 2010. “Measure and Determinants of Border Effects of Brazilian States.” *Papers in Regional Science* 89 (4): 735–58.

De Loecker, Jan, Pinelopi K. Goldberg, Amit K. Khandelwal, and Nina Pavcnik. 2016. “Prices, Markups, and Trade Reform.” *Econometrica* 84 (2): 445–510.

de Lucio, Juan, Raúl Mínguez, Asier Minondo, and Francisco Requena. 2017a. “Is Spain Experiencing an Export Miracle?” *Funcas SEFO* 7 (4): 53–66.

de Lucio, Juan, Raúl Mínguez, Asier Minondo, and Francisco Requena. 2017b. “Los márgenes del crecimiento de las exportaciones españolas antes y después de la Gran Recesión.” *Estudios de Economía Aplicada* 35 (1): 43–62.

DGT. n.d. “Parque de Vehículos (2000–2013).” Dirección General de Tráfico, Madrid, https://www.dgt.es/es/seguridad-vial/estadisticas-eindicadores/parque-vehiculos/series-historicas/ (accessed March 2018).

Díaz-Lanchas, Jorge, Carlos Llano, and José Luis Zofío. 2019. “A Trade Hierarchy of Cities Based on Transport Cost Thresholds.” JRC Working Papers on Territorial Modelling and Analysis 02/2019.

Dickstein, Michael J., and Eduardo Morales. 2018. “What Do Exporters Know?” *Quarterly Journal of Economics* 133 (4): 1753–1801.

Dolado, Juan J., Carlos García-Serrano, and Juan F. Jimeno. 2002. "Drawing Lessons from the Boom of Temporary Jobs in Spain." *Economic Journal* 112 (480): F270–95.
Donaldson, Dave, and Richard Hornbeck. 2016. "Railroads and American Economic Growth: A 'Market Access' Approach." *Quarterly Journal of Economics* 131 (2): 799–858.
Eppinger, Peter S., Nicole Meythaler, Marc-Manuel Sindlinger, and Marcel Smolka. 2018. "The Great Trade Collapse and the Spanish Export Miracle: Firm-Level Evidence from the Crisis." *World Economy* 41 (2): 457–93.
European Commission. n.d.a. "EU Buildings Database (2006)." Directorate General for Economic and Financial Affairs (accessed March 2018).
European Commission. n.d.b. "Annual Macro-Economic database (2000– 2013)." European Commission's Directorate General for Economic and Financial Affairs (accessed September 2019).
Fan, Haichao, Yu Liu, Larry D. Qiu, and Xiaoxue Zhao. 2020. "Export to Elude." *Journal of International Economics* 127: Article 103366.
Fisher, Eric O'N., and Vikas Kakkar. 2004. "On the Evolution of Comparative Advantage in Matching Models." *Journal of International Economics* 64 (1): 169–93.
Fitzgerald, Doireann, and Stefanie Haller. 2018. "Exporters and Shocks." *Journal of International Economics* 113: 154–71.
Gandhi, Amit, Salvador Navarro, and David A. Rivers. 2020. "On the Identification of Gross Output Production Functions." *Journal of Political Economy* 128 (8): 2973–3016.
Garmendia, Aitor, Carlos Llano, Asier Minondo, and Francisco Requena. 2012. "Networks and the Disappearance of the Intranational Home Bias." *Economics Letters* 116 (2): 178–82.
Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. "Bartik Instruments: What, When, Why, and How." *American Economic Review* 110 (8): 2586–2624.
Gopinath, Gita, and Roberto Rigobon. 2008. "Sticky Borders." *Quarterly Journal of Economics* 123 (2): 531–75.
Hanson, Gordon H. 2005. "Market Potential, Increasing Returns and Geographic Concentration." *Journal of International Economics* 67 (1): 1–24.
Harris, Chauncy D. 1954. "The Market as a Factor in the Localization of Industry in the United States." *Annals of the Association of American Geographers* 44 (4): 315–48.
Hausman, Joshua K., Paul W. Rhode, and Johannes F. Wieland. 2019. "Recovery from the Great Depression: The Farm Channel in Spring 1933." *American Economic Review* 109 (2): 427–72.
Head, Keith, and Thierry Mayer. 2014. "Gravity Equations: Workhorse, Toolkit, and Cookbook." In *Handbook of International Economics,* Vol. 4, edited by Gita Gopinath, Elhanan Helpman, and Kenneth Rogoff, 131–95. Amsterdam: Elsevier.
Helble, Matthias. 2007. "Border Effect Estimates for France and Germany Combining International Trade and Intranational Transport Flows." *Review of World Economics/Weltwirtschaftliches Archiv* 143 (3): 433–63.
Helliwell, John F. 1996. "Do National Borders Matter for Quebec's Trade?" *Canadian Journal of Economics* 29 (3): 507–22.
Hillberry, Russell, and David Hummels. 2008. "Trade Responses to Geographic Frictions: A Decomposition Using Micro-Data." *European Economic Review* 52 (3): 527–50.
INE. n.d.a. "Cifras oficiales de población de los municipios españoles: Revisión del Padrón Municipal (2002–2013)." Instituto Nacional de Estadística, Madrid (accessed March 2018).
INE. n.d.b. "Contabilidad Nacional Anual de España (2000–2013)." Instituto Nacional de Estadística, Madrid (accessed March 2018).
INE. n.d.c. "Encuesta de Población Activa (2000–2013)." Instituto Nacional de Estadística, Madrid (accessed March 2018).
INE. n.d.d. "Índice de Precios al Consumo (2000–2013)." Instituto Nacional de Estadística, Madrid (accessed March 2018).
International Monetary Fund. 2015. "Crisis Program Review."
International Monetary Fund. 2018. "Spain." IMF Country Report 18/330.
Jacks, David S., and Dennis Novy. 2018. "Market Potential and Global Growth over the Long Twentieth Century." *Journal of International Economics* 114: 221–37.
Masier, Giacomo, and Ernesto Villanueva. 2011. "Consumption and Initial Mortgage Conditions." ECB Working Paper 1297.
Melitz, Marc J. 2003. "The Impact of Trade on Intra-Industry Reallocations and Aggregate Industry Productivity." *Econometrica* 71 (6): 1695–1725.
Mian, Atif, Kamalesh Rao, and Amir Sufi. 2013. "Household Balance Sheets, Consumption, and the Economic Slump." *Quarterly Journal of Economics* 128 (4): 1687–1726.
Mill, John S. 1848. *Principles of Political Economy with Some of Their Applications to Social Philosophy.* London: John W. Parker.

Morales, Eduardo, Gloria Sheu, and Andrés Zahler. 2019. "Extended Gravity." *Review of Economic Studies* 86 (6): 2668–2712.

Myint, Hla. 1958. "The "Classical Theory" of International Trade and the Underdeveloped Countries." *The Economic Journal* 68 (270): 317–37.

Myro, Rafael. 2015. *España en la Economía Global: Claves del Éxito de las Exportaciones Españolas*. Barcelona: RBA Libros.

Nakamura, Emi, and Jón Steinsson. 2008. "Five Facts about Prices: A Reevaluation of Menu Cost Models." *Quarterly Journal of Economics* 123 (4): 1415–64.

Nakamura, Emi, and Jón Steinsson. 2013. "Price Rigidity: Microeconomic Evidence and Macroeconomic Implications." *Annual Review of Economics* 5 (1): 133–63.

Neary, J. Peter, and Albert G. Schweinberger. 1986. "Factor Content Functions and the Theory of International Trade." *Review of Economic Studies* 53 (3): 421–32.

Organisation for Economic Co-operation and Development (OECD). n.d. "Productivity and Unit Labor Costs by Main Economic Activity (ISIC Rev.4)." OECD Productivity Database, OECD Online Databases (accessed March 2018).

Poncet, Sandra. 2005. "A Fragmented China: Measure and Determinants of Chinese Domestic Market Disintegration." *Review of International Economics* 13 (3): 409–30.

Redding, Stephen, and Anthony J. Venables. 2004. "Economic Geography and International Inequality." *Journal of International Economics* 62 (1): 53–82.

Salas, Jorge. 2018. "Drivers of Spain's Export Performance and the Role of Labor Market Reforms." IMF Working Paper 18/283

Santos, Tano. 2017. "Antes del diluvio: The Spanish Banking System in the First Decade of the Euro." In *After the Flood: How the Great Recession Changed Economic Thought*, edited by Edward L. Glaeser, Tano Santos, and E. Glen Weyl, 153–208. Chicago: University of Chicago Press.

Smith, Adam. 1776. *An Inquiry into the Nature and Causes of the Wealth of Nations.* New York: Random House [reprint 2000].

Soderbery, Anson. 2014. "Market Size, Structure, and Access: Trade with Capacity Constraints." *European Economic Review* 70: 276–98.

Stock, James H., and Mark W. Watson. 1999. "Business Cycle Fluctuations in US Macroeconomic Time Series." In *Handbook of Macroeconomics*, Vol. 1, edited by John B. Taylor and Michael Woodford, 3–64. Amsterdam: North-Holland.

United Nations. n.d. "UN Comtrade Database (2000–2013)." United Nations (accessed September 2019).

Vannoorenberghe, G. 2012. "Firm-Level Volatility and Exports." *Journal of International Economics* 86 (1): 57–67.

Volker, Nitsch. 2002. "Border Effects and Border Regions: Lessons from the German Unification." Unpublished.

Waugh, Michael. 2019. "The Consumption Response to Trade Shocks: Evidence from the US-China Trade War." Unpublished.

Williams, John H. 1929. "The Theory of International Trade Reconsidered". *The Economic Journal* 39 (154): 195–209.

Wolf, Holger C. 2000. "Intranational Home Bias in Trade." *Review of Economics and Statistics* 82 (4): 555–63.

Xing, Weibo, and Shantong Li. 2011. "Home Bias, Border Effect and Internal Market Integration in China: Evidence from Inter-provincial Value-Added Tax Statistics." *Review of Development Economics* 15 (3): 491–503.