Journal of International Economics 95 (2015) 145–156

Contents lists available at ScienceDirect

Journal of International Economics

journal homepage: www.elsevier.com/locate/jie

Spatial exporters ☆

Fabrice Defever [a], Benedikt Heid [b], Mario Larch [c]

[a] University of Nottingham, GEP, CEP, CESifo, United Kingdom
[b] University of Bayreuth, Germany
[c] University of Bayreuth, ifo Institute, CESifo, GEP, Germany

ARTICLE INFO

Article history:
Received 10 December 2013
Received in revised form 10 November 2014
Accepted 10 November 2014
Available online 17 November 2014

JEL classification:
F12
F13

Keywords:
Export destination choice
Spatial correlation
Extended gravity
Firm-level customs data
MFA/ATC quota removal

ABSTRACT

In this paper, we provide causal evidence that firms serve new markets which are geographically close to their prior export destinations with a higher probability than standard gravity models predict. We quantify the impact of this spatial pattern using a data set of Chinese firms which had never exported to the EU, the United States, and Canada before 2005. These countries imposed import quotas on textile and apparel products until 2005 and experienced a subsequent increase in imports of previously constrained Chinese firms. Controlling for firm-destination specific effects and accounting for potential true state dependence we show that the probability to export to a country increases by about two percentage points for each prior export destination which shares a common border with this country. We find little evidence for other forms of proximity to previous export destinations like common colonizer, language or income group.

1. Introduction

Firm exports exhibit a geographical pattern. Not only do different firms serve different numbers of countries but also the spatial distribution of those countries differs across firms. Standard gravity models predict that firms are more likely to export to larger countries and to countries that are closer to the country of origin of the firm. These standard gravity forces generate some degree of unconditional spatial concentration of export destinations of firms. Recently, the literature has highlighted that the observed spatial correlation is larger than what the standard gravity model would predict. This fact has been labeled 'extended gravity' (see Morales et al., 2011, and Albornoz et al., 2012) or 'spatial exporters' (see Defever et al., 2011).

In this paper, we provide causal evidence for 'extended gravity' or 'spatial exporters', i.e. time-varying firm-specific heterogeneity in export destination choices shaped by firms' previous export experience in spatially close countries. We take into account unobserved time-invariant heterogeneity at the firm-country level. It may arise because firms can differ in their ability to serve specific markets, e.g. due to differences in language skills of their sales force. We also control for true state dependence at the firm-destination level which captures market-specific sunk costs of exporting (see Das et al., 2007). We show that the probability that a firm exports to a country increases by about two percentage points for each additional prior export destination with a common border with this country.

One reason for observing spatial exporter patterns may be the crucial need for gathering local information from trading partners over time. Different local information which has been acquired through previous export experience may then lead to different trade networks across firms. Recently, Chaney (2014) has developed a model describing trade patterns as an international network. Firms tend to build on their network for finding new trading partners, similar to social interactions

☆ A previous version of this paper has been circulated under the title "Spatial Exporter Dynamics" (mimeo, January 2010). We thank two anonymous referees and the assigned co-editor for their comments which have considerably improved the paper. We also thank James Anderson, Roc Armenter, Kala Krishna, Laura Márquez Ramos, Thierry Mayer, Emanuel Ornelas, Henry Overman, Jennifer Poole, and James Rauch for helpful comments on an earlier version of this paper and participants of the 2012 ESEM conference in Málaga, the WTO-UNCTAD-University of Geneva-Graduate Institute Seminar, Oct. 2011, the 2011 CEPR-ERWIT conference at the University of Nottingham, the 2010 EIIT conference at the University of Chicago, the 2010 EEA conference in Glasgow, the 2010 Econometric Society World Congress in Shanghai, the 2010 ETSG conference, the CAED 2010 conference at Imperial College London, the Copenhagen Asian Dynamics Initiative 2010 Conference, the 2010 IAW workshop, the 2010 CESifo-GEP workshop, the 2010 Korea University workshop, the 2010 GEP-Malysia conference and the 2010 GEP-China conference for their comments. We thank Zheng Wang for providing us with the US industry-HS6 concordance table. Thanks also to Julian Langer for excellent research assistance. Funding from the Leibniz-Gemeinschaft (WGL) under project "Pakt 2009 Globalisierungsnetzwerk" and the DFG under project 592405 is gratefully acknowledged. The usual disclaimer applies.

E-mail addresses: fabrice.defever@nottingham.ac.uk (F. Defever), benedikt.heid@uni-bayreuth.de (B. Heid), mario.larch@uni-bayreuth.de (M. Larch).

http://dx.doi.org/10.1016/j.jinteco.2014.11.006
0022-1996/

between individuals (see Jackson and Rogers, 2007).[1] When demand is uncertain but correlated across markets, firms may enter new destinations gradually to learn about profits in proximate markets from their previous export experience (see Albornoz et al., 2012; Nguyen, 2012). Also, when firms have to adapt products to specific markets, adaptation costs may be reduced if a firm already has entered markets which are relatively similar (see Morales et al., 2011). As a consequence, when trade barriers fall, firms expand their export destinations following a spatial pattern.

These channels highlight that one has to take into account two different aspects of the firm's problem: i) when to enter a new destination, and ii) where to go. When destination choices of a firm are independent, the decision problem is simple: Every market entry decision can be analyzed on its own. Hence, the two problems of when and where to export can be separated.[2] However, if destination choices are not independent, these two decisions become intrinsically related. Empirically, this leads to a dynamic discrete choice problem. As explained by Morales et al. (2011), this problem is formulated in a straight-forward way theoretically but quickly leads to an empirically de facto unsolvable problem. The insolvability arises because one would have to compute the expected profits for every possible combination of time paths of entries into destinations to identify the firm's profit-maximizing choice.[3] Complementary to the structural empirical approach suggested by Morales et al. (2011), we use reduced form regressions exploiting a quasi-natural experiment.

We present evidence for 'spatial exporters' relying on the removal of binding import quotas under the MultiFiber Arrangement/Agreement on Textiles and Clothing (MFA/ATC) regime in 25 EU countries, the United States, and Canada in 2005. This exogenous shock has generated a large entry of firms in a set of potential new destinations (see Khandelwal et al., 2013). Our sample consists of Chinese textile and apparel exporters which never exported to these countries before 2005. We study these firms' subsequent export destination choices in other countries which were not directly affected by the lifting of the MFA quotas. As the timing of the MFA quota removal was exogenous to firms, it helps us to overcome the endogeneity problem due to the dynamic nature of the firm's export destination choice.

Our empirical strategy gauges the relative importance of the time-varying cross-country correlation of a firm's export destination choices. This correlation may be a result of a firm's export history in close markets. A previous export destination is considered as close when it is geographically or culturally close. Cultural closeness is measured by sharing a common language, sharing a common colonizer, or having similar income levels. As we use reduced form regressions we do not rely on a specific channel imposed by an underlying structural model. Rather, we establish the causal impact of a firm's export history on the probability to export to a specific country, irrespective of whether it arises from the demand or supply side.

Our paper provides causal evidence of the spatial correlation of export decisions at the firm level that has been put upfront by recent theoretical developments on export dynamics (see Albornoz et al., 2012; Nguyen, 2012; Morales et al., 2011 and Chaney, 2014). It could also contribute to explain the pattern of zero bilateral trade flows observed empirically (see Evenett and Venables, 2002). Understanding exporting firm behavior is also crucial from a policy perspective. If across-country path dependence in firm destination choices is important, it also has ramifications for trade liberalization policies: if two countries liberalize trade with each other, their level of trade with non-liberalizing nearby countries will be higher than standard gravity would predict. This gives rise to externalities across countries.[4] Therefore, our research highlights another reason for potential efficiency increases from trade liberalization through policy coordination between countries.

The remainder of the paper is organized as follows: Section 2 describes the data set and our identification strategy. Section 3 presents our baseline empirical results. We start with a differences-in-differences (diff-in-diff) approach which investigates the impact of the lifting of the MFA quotas on the probability of exporting to a country which is contiguous to a previously restricted MFA country. We then investigate the impact of previous export experience in close markets on a firm's destination choice. Our regressor of interest in the latter specification is potentially endogenous. We therefore present instrumental variable regressions where we use the lifting of the MFA quotas as an instrument. Finally, we present dynamic panel specifications. These allow us to control for our potentially endogenous regressor of interest as well as the persistence and true state dependence in export destination choices. Section 4 presents evidence at the firm-product-couple level. Section 5 presents robustness checks. The last section concludes.

2. Data and identification

2.1. Sample and dependent variable

We use transaction level customs panel data on the universe of Chinese exporters for the years 2000 to 2006. We only keep products which fall in the Harmonized System (HS) chapters of textile and clothing products, i.e. chapters 50 to 63, as these are the products covered by the MFA regime. We aggregate all transactions of a firm in a country in one year into one observation. The sample is restricted to continuous exporters, i.e. firms that export at least to one country every year.[5] Specifically, we investigate the export destination choice between 150 non-MFA member countries of firms which did not export in any of the MFA restricted countries during the years 2000 to 2004.[6] Hence, our sample includes both firms that enter the MFA member countries after 2004 as well as those which export to other countries between 2000 and 2006. Overall, our sample is composed of 1295 continuous exporters which never entered the MFA restricted countries before 2005.

[1] For instance, an exporting firm may gain access to a new export market via a multinational retailer which already serves a third country. As the network of subsidiaries of wholesalers and of multinational firms expands spatially (see Basker, 2005 and Defever, 2012), this mechanism also implies a spread of exports to contiguous countries. In addition to geography, cultural closeness can generate a similar pattern through networks of ethnically related firms. For instance, networks may reduce search costs as firms may learn about potential suitable suppliers within their ethnic community (see for instance Rauch, 2001).

[2] For instance, Das et al., (2007) estimate the parameters of a firm's dynamic problem of when to start and stop exporting, irrespective of the specific export market choice.

[3] Therefore, Morales et al. (2011) do not solve this dynamic problem explicitly. Instead, they use moment inequality estimators to obtain parameter bounds for their structural empirical model. Their estimates based on firm-level export data for the Chilean chemicals sector show that startup costs of accessing a new country are determined by a firm's previous export destinations. Note that this paper has changed its title and now circulates as Morales et al. (2014). Albornoz et al. (2012) and Nguyen (2012) study the timing of entry only and assume a hierarchy of countries in terms of profitability and a constant correlation of profits across all export destinations. Together, these assumptions elude the question of where to go. Antras et al. (2014) propose another solution to deal with the interdependence of firm's entry decisions. Building on Jia (2008), Antras et al. (2014) rely on complementarities in the global sourcing decisions of firms to study extended gravity effects on the import side. Lawless (2013) shows that entry decisions of firms are correlated with their export status in previous geographically close export destinations. However, she does not control for true state dependence nor firm-specific country fixed effects as we do.

[4] For instance, Defever and Ornelas (2014) show that the end of the MFA turned China into a better export base for previously restricted products, encouraging entry in the industry and increasing exports to all destinations. Borchert (2008) finds that the growth of Mexican exports to Latin America was higher for products with a large reduction in the preferential U.S. tariff under NAFTA. Similarly, Molina (2010) identifies a strong positive effect of RTAs in promoting exports outside the bloc of liberalized countries. While it is difficult to explain these findings with standard trade models, they can easily be rationalized in the presence of firm-specific cross-country correlations in export destination choices.

[5] This allows us to abstract from selection into exporting at the firm-extensive margin. See Das et al. (2007) for a structural model of selection into exporting.

[6] The previously restricted MFA countries are the 25 EU countries as of 2005, the United States, and Canada. A comprehensive list of all non-MFA countries in our sample can be found in the online Appendix in Table A.37.

Our dependent variable is the firm-specific vector of export status $\mathbf{y}_{it} = \left(y_{i1t}, \ldots, y_{ijt}, \ldots, y_{i\mathcal{J}t}\right)$ which indicates whether a firm i exports to a specific destination j in year t. $\mathcal{J}$ is the number of non-MFA countries in our sample. We present descriptive statistics for all variables in the online Appendix in Table A.38.[7] 1.2% of our observed destination choices turn out to be positive. Hence, serving a specific foreign market is a rare event.

2.2. Identification strategy

Under the MultiFiber Arrangement/Agreement on Textiles and Clothing (MFA/ATC) regime, restrictions were upheld on many products even after China acceded to the WTO on December 11th, 2001. On January 1st, 2005 the removal of import quotas led to the entry of a large number of firms in the then 25 EU countries, the United States, and Canada.[8] Fig. 1 shows the average number of exporters into these markets across all restricted HS-6 products. While around 100 to 150 firms had been exporting a restricted MFA product while the import restrictions were still upheld, this number jumped to more than 300 in 2005.

One possible reason behind the large and rapid entry of firms into MFA countries in 2005 can be seen in the fear that safeguard mechanisms could potentially re-introduce quotas. Actually, the EU countries, the United States, and Canada had product-specific safeguard mechanisms which were not phased out until 2008. The possible use of these safeguard measures was likely and it was unclear which products would be affected. This is corroborated by Fig. 1 which shows that the average number of exporters across products did not increase in 2006 so that there is no evidence of a gradual entry of firms into the previously restricted MFA countries, at least on average. This can be explained by the new and transitional license system for textile exports that has been reintroduced in 2005 by the Chinese government. The intention was to limit the growth of Chinese exports of MFA products for the years 2006 to 2008. Looking back, the restrictions imposed in 2005 were by and large ineffective. However, the new restrictions had an impact on the growth of Chinese textile exports for 2006 to 2008.[9] The lifting of the MFA quotas in 2005 exogenously changed the potential profitability of exporting to the previously restricted MFA countries. New entrants could reap part of the quota rents which previously accrued to those firms with an export license, leading to the increase in the number of firms in the EU, the United States, and Canada. If firms are ‘spatial exporters’, this change should have influenced the subsequent export destination choices in non-MFA countries. The same firms which quickly entered the previously restricted MFA countries *for the first time* could then potentially learn about other profitable export opportunities in countries which are geographically or culturally related to the previously restricted MFA countries.

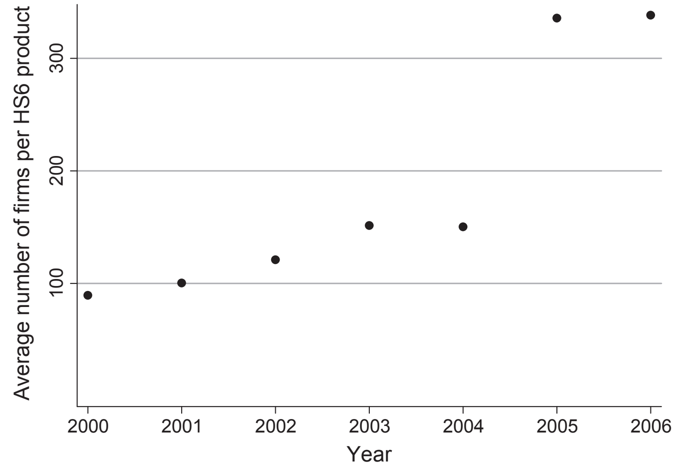

Fig. 1. Average number of exporting firms to one EU country, the United States or Canada per restricted MFA 6-digit product.

3. Specifications

We have now described our identification strategy in general terms. It is compatible with several complementary empirical specifications which rely on different assumptions about the data-generating process. Specifically, we will use a differences-in-differences (diff-in-diff) strategy, (panel) instrumental variable regressions and dynamic panel estimations. This multitude of specifications provides robust evidence for spatial exporters. We will next discuss in turn our specifications and the corresponding results.

3.1. Differences-in-differences

Viewing the removal of the quotas as a quasi-natural experiment, it seems natural to start with a differences-in-differences (diff-in-diff) specification.

MFA restrictions were removed January 1st, 2005. This lifting opened up new potential export markets but was not influenced by the decisions of individual firms and thus exogenous at the firm level. Beginning from this date, firms in our sample were able to enter the previously restricted MFA countries for the first time. There they could potentially acquire information about contiguous export markets. Therefore, firms should export more to destinations which are contiguous to MFA countries after the removal of the MFA restrictions. Hence, our treatment indicator C_j is defined at the country-level.[10] It is a dummy variable indicating whether a country j is contiguous to an MFA-restricted country. This also renders our treatment exogenous to the firm's choices, as the set of MFA-restricted countries is the same for all firms. Similar to Morales et al. (2011), we assume a one year lag to quantify ‘spatial exporters’, reflecting the fact that the learning or product adaptation processes of the firm take time. Hence, we define the year 2006 as our post-treatment period. $y2006_t$ is the corresponding dummy variable for the year 2006. The treatment effect, δ, measures whether firms export more frequently to countries that are contiguous to previously restricted MFA countries in 2006 and is captured by the interaction term of $y2006_t$ and C_j.

Specifically, our first empirical specification is therefore given by

$$y_{ijt} = \delta\left(y2006_t \times C_j\right) + \theta_{ij} + \theta_t + \varepsilon_{ijt}, \tag{1}$$

where y_{ijt} is a dummy variable indicating whether a firm i exported to country $j \in \mathcal{J}$ in year t, where $\mathcal{J}$ is the set of non-MFA countries. We

[7] We use the years 2000 to 2005 to construct our lagged regressors of interest. Our final data set then covers the years 2001 to 2006. As we include two lags in our dynamic panel specifications, we are left with four years for our estimation. For reasons of comparability, we use these four years for all our specifications.

[8] See Harrigan and Barrows (2009), Brambilla et al. (2010), Upward et al. (2011), and Khandelwal et al. (2013).

[9] In June 2005, China and the EU agreed to re-impose quotas on some products. Despite the implementation of a new license system China did not restrict the number of the licenses nor the volume of exports. As a reaction, EU retailers ordered large amounts of Chinese textile products before the quota implementation. Only two months after the signing of this agreement import quotas were exhausted and 75 million items of textile and clothing products were stuck in European ports (see Brambilla et al., 2010; Buckley, 2005 and Wikipedia, 2013). In September 2005, the EU and China settled the issue to end what the UK press called the “Bra Wars” (see e.g. White and Gow, 2005 and Wikipedia, 2013).

[10] We therefore use standard errors clustered at the country-level following the recommendation for differences-in-differences estimates by Bertrand et al. (2004).

also introduce θ_{ij}, a firm-destination fixed effect, and θ_t, a year fixed effect.[11] ε_{ijt} is the error term. Note that this regression is equivalent to a diff-in-diff specification as the year and firm-destination fixed effects control for the treatment period as well as the treatment group dummies. We estimate specification (1) with ordinary least squares which leads to a linear probability model.[12]

The firm-destination fixed effects capture all country-firm characteristics that do not change over the considered time period. This includes time-constant destination-specific variables generally known to influence bilateral trade flows from the gravity literature such as market size, overall remoteness of a country (multilateral resistance terms), and trade costs. Crucially, it also controls for time-constant firm-specific heterogeneity such as productivity, quality, labor costs, and assortative matching of workers. For example, a firm might employ managers with specific language skills which influence the firm's export destination choice.[13] θ_t captures the general time trend in the empirical probability of exporting to a country.

We expect δ to be positive if firms are spatial exporters. δ is identified by firms which start to export to a country in 2006 which is contiguous to an MFA-restricted country. A positive effect can stem from two sources: 1.) The additional expected profit from learning about previously restricted MFA countries. This makes a country j more attractive as a potential export destination if it is contiguous to a previously MFA-restricted country. This is *independent of whether the firm has exported to an MFA-restricted country or not*. 2.) Firms which actually *did* export to an MFA-restricted country in 2005 *for the first time* and gained knowledge about potential business opportunities in contiguous country j. We disentangle these two sources in our alternative empirical specifications presented in Sections 3.2 to 3.4. Note that firms which stop exporting to country j in 2006 decrease the estimate of δ (and may even render the coefficient negative).

Table 1 reports estimates of the diff-in-diff specification as given in Eq. (1). Specifications I to VI give the estimated treatment effects for exporting to a contiguous MFA country one year after the lift of the quota restrictions for different definitions of contiguity. A firm's destination choice can be correlated not only in markets which are geographically proximate to its previous export destinations but also in markets which share some other form of closeness. Specifically, we define contiguity according to whether the countries share a common border, a common language, a common colonizer, a common income group, or whether they are located on the same continent using data provided by CEPII, see Mayer and Zignago (2011). Therefore, our concept of space is general and can refer to geographic as well as cultural cross-country correlation in export destination choices. Section L in the online Appendix gives a detailed description of the construction of our contiguity variables.

In specification I, contiguity is defined according to whether countries share a common border. The coefficient estimate of 0.003 implies an average increase of 0.3 percentage points in the probability of choosing a new export destination that is contiguous to a previously restricted MFA country in 2006. This effect may sound small. We therefore compare this marginal effect to the observed empirical probability of a firm exporting to a particular country in our sample reported. We report these empirical probabilities in Table A.37 in the online Appendix. For example, this implies about a 14% (0.003/0.022) increase in the probability of a firm exporting to Russia in 2006, as Russia shares a common border with Finland, an MFA country.[14]

Specifications II to V run separate regressions where we construct our contiguity measure according to whether countries share the same language (specification II), whether countries have common colonial ties (specification III), whether countries are in the same income group (specification IV), or whether countries are located on the same continent (specification V). Evidently, especially space in the geographic sense (common border and common continent) plays a significant role in firms' export location choice. We do not find evidence for other definitions of contiguity, like common language, common colonizer or common income group, as important determinants for spatial exporters.

In column VI, we include all different contiguity measures at the same time to gauge the relative importance of the different measures. The marginal effects are hardly affected by conditioning on all other contiguity measures. Also significance stays by and large the same.

In the specification given in Eq. (1) we do not condition on whether the firm has exported to a previously restricted MFA country. Hence, we identify a combination of the effects 1.) and 2.) mentioned before. Whereas 1.) increases the profitability of a destination only due to the option value of exporting to an MFA restricted country and therefore for all firms in our sample without any action from the firm,[15] 2.) directly measures actually occurred spatial exporting only for firms that did export to an MFA restricted country first and afterwards to a contiguous one.

While Table 1 provides a first step towards evidence for spatial exporters, we now turn to identify how a firm's export destination choice is influenced by its export history in contiguous markets. Hence we disentangle the additional expected profit from learning about previously restricted MFA countries from actual export experience by focusing on the second effect only.

3.2. Fixed effects regression taking into account firm-level history

Until now, we only focused on those countries which were contiguous to previously restricted MFA countries and neglected the impact of a firm's previous export history. To capture spatial exporting which takes into account firm-level history, we construct our contiguity measure, $N_{ij,t-1}$, which measures the number of countries which are contiguous to country j and to which firm i has exported in $t-1$ for each firm i and destination j. As the set of the previous export destinations is firm-specific, so are the contiguity variables. Specifically, $N_{ij,t-1} = \mathbf{w}_j'\mathbf{y}_{i,t-1}^*$, where $\mathbf{y}_{i,t-1}^*$ is the $(\mathcal{N} \times 1)$ vector of the export indicators for firm i in $t-1$ whose typical element $y_{i\ell,t-1}$ is 1 if firm i exported to country ℓ in year $t-1$, and zero otherwise. To construct our explanatory variable, $N_{ij,t-1}$, we use a set of $\mathcal{N} = 177$ countries, *including* the previously restricted MFA countries. In our regression sample, however, we continue to investigate the choice between $\mathcal{J} = 150$ non-MFA countries as in the previous section. $\mathbf{w}_j$ is the jth row of $\mathbf{W}$, a $(\mathcal{N} \times \mathcal{N})$ contiguity matrix. The typical entry $w_{\ell m}$ of $\mathbf{W}$ is 1 if countries ℓ and m are contiguous, and zero otherwise. Note that for this specification, we do not exploit the quasi-natural experiment of the lifting of the MFA quota restrictions. We will use it again in Section 3.3.[16]

[11] See Section A of the online Appendix for evidence on firm-specific heterogeneity in export destinations.

[12] As we are only interested in average effects and not in predictions for individual firms and given the high number of fixed effects, we stick to the linear probability model, see Winkelmann and Boes (2009). As we also control for lagged endogenous variables in later specifications, we can extend our regression framework by using a linear dynamic panel estimator in a straight-forward way, simplifying the interpretation and comparison of results across our different specifications.

[13] In a strict sense, some gravity variables may change over time (such as market size and the multilateral resistance terms). However, note that we only consider one post-treatment year (2006). Hence, to bias our results gravity variables would have to be considerably different in 2006 and at the same time this change would have to be correlated with our regressor ($y2006_t \times C_j$).

[14] Note that we do not compare our estimates to the unconditional observed frequency of exporting to a country (the mean of our dependent variable, 0.012). Such a comparison would ignore the spatial correlation of exports due to standard gravity forces such as country size and distance between origin and destination countries. Russia is the first country in our list of most frequent export destinations which shares a common border with an MFA country.

[15] Note that this effect is heterogeneous across firms as it depends on a firm's export history.

[16] In principle, one could also think about using $\mathbf{y}_{i,t-1}^{MFA}$ to construct $N_{ij,t-1}$, whose dimension is $(\mathcal{N} \times 1)$ and whose typical element $y_{i\ell,t-1}^{MFA}$ is 1 if firm i exported to country ℓ in $t-1$, and this country is an MFA country, and zero otherwise. By using $\mathbf{y}_{i,t-1}^*$ instead of $\mathbf{y}_{i,t-1}^{MFA}$ to construct $N_{ij,t-1}$, we also count previous export destinations of a firm which are not previously restricted MFA countries. We reran all our specifications using this alternative regressor. Results hardly changed. Note, however, that focusing on $\mathbf{y}_{i,t-1}^{MFA}$ would potentially bias our coefficient estimates as $\mathbf{y}_{i,t-1}^{MFA}$ sets all those elements of $\mathbf{y}_{i,t-1}^*$ equal to 0 which identify positive non-MFA country export flows.

Table 1
Diff-in-diff.

	I	II	III	IV	V	VI
$y2006_t \times C_j$ defined according to						
Common border	0.003***					0.002***
	(0.001)					(0.001)
Common language		0.000				0.000
		(0.000)				(0.000)
Common colonizer			−0.001			−0.000
			(0.000)			(0.000)
Common income group				−0.000		−0.000
				(0.000)		(0.000)
Common continent					0.001***	0.001**
					(0.000)	(0.000)
Observations	777,000	777,000	777,000	777,000	777,000	777,000
# of firms	1295	1295	1295	1295	1295	1295

Notes: The dependent variable is y_{ijt} which is a dummy variable indicating whether a firm i exported to country j in year t. All regressions include firm-destination fixed effects, as well as year dummies (not reported). Standard errors are in parentheses. All regressions use robust standard errors clustered at the country level to take into account that the regressor only varies at the country level following the suggestion for differences-in-differences estimates by Bertrand et al. (2004). *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

As with C_j, we measure $N_{ij,t-1}$ by defining contiguity in terms of the countries sharing a common border, sharing a common language, sharing a common colonizer, being in a common income group, or being located on the same continent. For example, $N_{ij,t-1} = 2$ measured in terms of common border means that for firm i, country j shares a common border with two countries to which firm i has exported in $t-1$.

To take into account whether a firm actually has exported to a contiguous country in the previous year, we run the following regression:

$$y_{ijt} = \delta \mathbb{I}\Big(N_{ij,t-1}>0\Big)_{ijt} + \theta_{ij} + \theta_t + \varepsilon_{ijt}, \tag{2}$$

where $\mathbb{I}$ is the indicator function taking value one if $N_{ij,t-1} > 0$. In this regression, δ now quantifies the effect of actual experience in a previous export destination on future export decisions in contiguous countries. We expect δ to be positive if previous export experience from contiguous countries matters. Note that in contrast to $y2006_t \times C_j, \mathbb{I}\Big(N_{ij,t-1}>0\Big)_{ijt}$ varies at the firm-level. Table 2 gives the result for specification (2) and is organized in the same way as Table 1. Column I shows that the probability of exporting to a country increases by 1.4 percentage points if the firm previously exported to an export destination with a common border. Is this effect large or small? We again compare this marginal effect to the empirical probability of a firm exporting to a particular country in our sample reported in Table A.37 in the online Appendix. Given these empirical probabilities, this implies e.g. a 20% increase in the probability of a firm exporting to Singapore when it has previously exported to Malaysia.[17] This effect is larger than the effect identified in Table 1 because we now focus on source 2.), i.e. the effect of actual export experience in contiguous countries.

Again, the effect of sharing a common border is the largest and most significant effect. Also sharing a common language or colonial ties is significant, albeit with smaller magnitudes. For example, the probability of exporting to Australia increases by about 4% (0.002/0.054) if the firm has previously exported to Great Britain (or some other English-speaking country). Similarly, the probability of exporting to India increases by about 11% (0.002/0.019) if the firm has previously exported to Great Britain with which it shares a common language. Column VI shows quantitatively very similar effects when conditioning on all different dimensions of spatial exporters jointly.

[17] Note that Japan and South Korea, our most frequent export destinations, do not have a common border with any country (the Democratic People's Republic of Korea is not included in our data set). We therefore chose Singapore, the third most frequent export destination. Malaysia shares a common border with Singapore.

Similarly, we can also estimate the impact of an increase in the number of previous contiguous export destinations by omitting the indicator function from Eq. (3), i.e.:

$$y_{ijt} = \delta N_{ij,t-1} + \theta_{ij} + \theta_t + \varepsilon_{ijt}. \tag{3}$$

Table 3 reports the estimates. Results are virtually unchanged, with sharing a common border remaining the regressor with the largest point estimate. The slight change in the specification implies that the probability of exporting to a country that shares a common border with a previous export destination increases by 1.2 percentage points if the firm actually exports to one additional contiguous country in the previous year.

A problem of regressions (2) and (3) is that, contrary to regression (1), now the regressor of interest is potentially endogenous: firms may anticipate that they may learn from previous export destinations and potentially choose their export destinations accordingly. We will therefore present (panel) instrumental variable regressions in the next subsection.

3.3. Instrumental variable regressions

To account for the potential endogeneity of our regressor $\mathbb{I}(N_{ij,t-1}>0)_{ijt}$, we instrument it with the exogenous regressor of interest from regression (1), $y2006_t \times C_j$, which is 1 for countries that are contiguous to previously restricted MFA countries in 2006, and zero otherwise. The exogeneity of our instrument is again justified as it is a country-specific variable and is not influenced by firm decisions. Still, it is relevant as the instrument and the potential endogenous regressor are correlated by construction: C_j indicates countries contiguous to (previously) MFA restricted countries and $N_{ij,t-1}$ is positive if a firm exports to at least one country. As the MFA restricted countries in sum make up a large share of the world market, it is very likely that $N_{ij,t-1} > 0$ if $C_j = 1$. In addition, the diff-in-diff regression results clearly show the relevance of the proposed instrument. For our estimation, we use the two-stage least-squares within panel instrumental variables estimator which includes firm-country fixed effects as in the previous specification.

We present the instrumental variable regressions that allow $\mathbb{I}(N_{ij,t-1}>0)_{ijt}$ to be endogenous in Table 4. Allowing for endogeneity does not lead to a qualitative change in our results (compare with Table 2). However, the size of the effect of contiguity is approximately seven times larger. Again, sharing a common border has the largest effect (coefficient of 0.104) and only geographical contiguity turns out to be statistically significant. Results also remain largely unchanged when including all contiguity measures simultaneously (see column

Table 2
Fixed effects regression taking into account firm-level history—dummy.

	I	II	III	IV	V	VI
$\mathbb{I}(N_{ij,t-1}>0)_{ijt}$ defined according to						
Common border	0.014***					0.014***
	(0.003)					(0.003)
Common language		0.002***				0.002***
		(0.001)				(0.001)
Common colonizer			0.002**			0.001
			(0.001)			(0.001)
Common income group				0.001		0.000
				(0.001)		(0.001)
Common continent					0.001	−0.000
					(0.001)	(0.001)
Observations	777,000	777,000	777,000	777,000	777,000	777,000
# of firms	1295	1295	1295	1295	1295	1295

Notes: The dependent variable is y_{ijt} which is a dummy variable indicating whether a firm i exported to country j in year t. All regressions include firm-destination fixed effects, as well as year dummies (not reported). Standard errors are in parentheses. All regressions use robust standard errors clustered at the firm level to take into account the potential autocorrelation in the export destination choice at the firm level. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

VI in Table 4). The F-statistics for the excluded instruments in the first stage regressions are also larger than 10 (with the exception of column III), indicating that our instruments are relevant. Partial R^2 measures admittedly are very low. However, this is not too surprising given the generally very low R^2 of firm-level export destination choice models, see Albornoz et al. (2012), as within-models remove the explanatory power of the firm-destination fixed effects.[18]

Table 5 reproduces Table 3 but instruments $N_{ij,t-1}$ with $y2006_t \times N_j$, which counts the number of countries that are contiguous to previously restricted MFA countries in 2006, and is zero otherwise.[19] Comparing results shows that the effects of geographical contiguity (common border and common continent) are about seven times larger. Hence, our estimate in specification I implies that the probability of exporting to a country that shares a common border with a previous export destination increases by eight percentage points if the firm actually exports to one additional contiguous country in 2005.[20]

One may wonder about the increase of the IV estimates in comparison with the OLS estimates. If unobserved factors increase the probability that a firm enters a particular country, they may as well increase the probability of exporting to a similar country in the future. Hence, these omitted factors would lead to an upward bias of our OLS estimates. However, endogeneity may also arise due to measurement error in the explanatory variable. As pointed out by Morales et al. (2011) and Nguyen (2012), when deciding about the export decision in t, the firm actually solves a dynamic optimization problem taking into account the spatial correlation of profits across destinations. Our econometric specification proxies this dynamic component by including our regressor of interest, $\mathbb{I}(N_{ij,t-1}>0)_{ijt}$, which tries to control for the firm's state variable. Obviously, this is only a very crude way to introduce dynamics into a static regression framework. As is well known, measurement error leads to attenuation bias, which may very well explain why our OLS estimates underestimate the true effect (see e.g. Cameron and Trivedi, 2005, chapter 26.2).

Even though we rely on panel data for our regressions so far, we have, until now, ignored the persistence and state dependence in the export status of firms. We turn to this issue in the next section.

3.4. Dynamic panel results taking into account state dependence

At least since Roberts and Tybout (1997) and Das et al. (2007) it is well known that whether a firm has exported in the previous period is highly correlated with its current export status. Evidence for this is provided at the firm level, irrespective of the variation of export destinations within a firm across time. Hence, it is based on persistence of the export status at the firm level, not at the firm-destination level. In principle, it is possible that this persistence is also evident at the firm-destination level. And indeed in our data set, the correlation between our dependent variable and its one year lag is 0.75.

One can distinguish between two major sources of this observed persistence. First, there may be some unobserved time-invariant firm-destination component which determines whether a firm enters a specific destination. Second, there can be true state dependence, i.e. the previous export history of a firm in a specific country drives future export destination choices. In other words, export history in export destination choice matters.

Whereas the first persistence is captured in our specification by the firm-destination fixed effect θ_{ij}, we did not properly account for potential true state dependence in our estimations so far. As has been demonstrated by Nickell (1981), fixed effect estimators are biased in the presence of true state dependence. How does this affect our estimates? In our setting, consider a firm which exports to both Singapore and Malaysia in 2005 and 2006. Then, when not including lags of the dependent variable, our regressor of interest explains the firm's exporting behavior in Malaysia by its previous export experience in Singapore and vice versa.[21] To control for this confounding factor, avoid the Nickel bias, and account for the high persistence in our dependent variable, we employ the system-GMM dynamic panel estimator by Blundell and Bond (1998).[22]

Specifically, we estimate

$$y_{ijt} = \phi_1 y_{ij,t-1} + \phi_2 y_{ij,t-2} + \delta\mathbb{I}\left(N_{ij,t-1}>0\right)_{ijt} + \theta_{ij} + \theta_t + \varepsilon_{ijt}. \tag{4}$$

[18] Full results of first stage regressions are available in Section B of the online Appendix.

[19] We use $y2006_t \times N_j$ as this has the same type of country-level variation as our potentially endogenous regressor, $N_{ij,t-1}$. We could also again instrument by $y2006_t \times C_j$, or even use $y2006_t \times N_j$ in our diff-in-diff specification. These choices hardly matter for our results.

[20] We also experimented with the years 2004 and 2005 to construct our instrument, finding similar but larger effects. When defining the treatment period to begin in 2004, the estimate for common border is 0.311 for $\mathbb{I}(N_{ij,t-1}>0)_{ijt}$ and 0.268 for $N_{ij,t-1}$. When we define the treatment to begin in 2005, the estimates are 0.255 and 0.187, respectively. Hence, defining the treatment earlier results in an upward bias as exporting to contiguous countries is confounded by other factors. By using the lifting of the MFA restrictions, we likely minimize these other effects.

[21] Note that for firms which continuously export to both destinations in all years included in the sample, this will be captured by the firm-destination fixed effects. However, firm-destination fixed effects will not cover this persistence for intermittent exporters.

[22] We present results using the difference-GMM dynamic panel estimator from Arellano and Bond (1991) as robustness checks in the online Appendix in Tables A.9 and A.10. Results even more strongly support evidence for spatial exporters, and even the Sargan model specification tests do not reject the validity of the instruments.

Table 3
Fixed effects regression taking into account firm-level history—*N*.

	I	II	III	IV	V	VI
$N_{ij,t-1}$ defined according to						
Common border	0.012***					0.010***
	(0.003)					(0.003)
Common language		0.001**				0.000
		(0.001)				(0.000)
Common colonizer			0.003***			0.002*
			(0.001)			(0.001)
Common income group				0.002**		0.001
				(0.001)		(0.001)
Common continent					0.001*	0.000
					(0.001)	(0.001)
Observations	777,000	777,000	777,000	777,000	777,000	777,000
# of firms	1295	1295	1295	1295	1295	1295

Notes: The dependent variable is y_{ijt} which is a dummy variable indicating whether a firm *i* exported to country *j* in year *t*. All regressions include firm-destination fixed effects, as well as year dummies (not reported). Standard errors are in parentheses. All regressions use robust standard errors clustered at the firm level to take into account the potential autocorrelation in the export destination choice at the firm level. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 4
Instrumental variable regressions—dummy.

	I	II	III	IV	V	VI
$\mathbb{I}(N_{ij,t-1}>0)_{ijt}$ defined according to						
Common border	0.104***					0.089**
	(0.029)					(0.045)
Common language		0.004				0.005
		(0.004)				(0.012)
Common colonizer			−0.128			−0.008
			(0.833)			(0.768)
Common income group				0.043		0.154
				(0.280)		(0.364)
Common continent					0.009***	−0.001
					(0.002)	(0.033)
Observations	777,000	777,000	777,000	777,000	777,000	777,000
# of firms	1295	1295	1295	1295	1295	1295
First stage *F*-statistic	1355	5561	37.753	24.381	22,181	(◇)
First stage partial R^2	0.002	0.009	0.000	0.000	0.037	(◇)

Notes: The dependent variable is y_{ijt} which is a dummy variable indicating whether a firm *i* exported to country *j* in year *t*. All regressions include firm-destination fixed effects, as well as year dummies (not reported). We use the two-stage least-squares within panel instrumental variables estimator where we instrument the endogenous regressor by $y2006_t \times C_j$. Standard errors are in parentheses. All regressions use robust standard errors clustered at the firm level to take into account the potential autocorrelation in the export destination choice at the firm level. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. First stage *F*-statistic denotes the value of the *F*-statistic for the excluded instruments in the first stage regression and first stage partial R^2 reports the explanatory power of the instrument, netting out exogenous regressors from the first stage regression. (◇): The five first stage regressions and statistics for the five endogenous variables for column VI are reported in the online Appendix in Table A.3.

Table 5
Instrumental variable regressions—*N*.

	I	II	III	IV	V	VI
$N_{ij,t-1}$ defined according to						
Common border	0.080***					0.075***
	(0.029)					(0.028)
Common language		−0.000				0.002
		(0.001)				(0.002)
Common colonizer			−0.027			−0.016
			(0.057)			(0.100)
Common income group				−0.003		−0.004*
				(0.002)		(0.002)
Common continent					0.005***	0.003
					(0.001)	(0.003)
Observations	777,000	777,000	777,000	777,000	777,000	777,000
# of firms	1295	1295	1295	1295	1295	1295
First stage *F*-statistic	1617	15,095	192.5	15,703	7770	(◇)
First stage partial R^2	0.003	0.025	0.000	0.026	0.013	(◇)

Notes: The dependent variable is y_{ijt} which is a dummy variable indicating whether a firm *i* exported to country *j* in year *t*. All regressions include firm-destination fixed effects, as well as year dummies (not reported). We use the two-stage least-squares within panel instrumental variables estimator where we instrument the endogenous regressor by $y2006_t \times N_j$. Standard errors are in parentheses. All regressions use robust standard errors clustered at the firm level to take into account the potential autocorrelation in the export destination choice at the firm level. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. First stage *F*-statistic denotes the value of the *F*-statistic for the excluded instruments in the first stage regression and first stage partial R^2 reports the explanatory power of the instrument, netting out exogenous regressors from the first stage regression. (◇): The five first stage regressions and statistics for the five endogenous variables for column VI are reported in the online Appendix in Table A.4.

We include two lags of the dependent variable as Roberts and Tybout (1997) show that typically two lags have a significant and decaying impact on the export decision of a firm.[23]

Note that the dynamic panel estimator allows us to treat our contiguity variable as predetermined. This is consistent with the fact that lagged values of our regressor of interest, $\mathbb{I}(N_{ij,t-1}>0)_{ijt}$, cannot be changed by the firm in the current period but future values may be adjusted by the firm, as stressed by the mechanisms in Morales et al. (2011), Albornoz et al. (2012), and Nguyen (2012). The system-GMM dynamic panel estimator uses moment conditions derived from Eq. (4) in levels and in differences. These moment conditions imply different sets of instruments for the equation in levels and in differences. For the level equation, we use the lagged differences of our dependent variable as well as the differences of our regressor of interest. For the differenced equation, we use the second and third lag of the level of the dependent variable as well as the first and second lag of the level of our regressor of interest (see Baltagi, 2008, chapter 8.5 and Cameron and Trivedi, 2009, chapter 9.4). Note that we restrict the maximum number of lags to two to prevent a proliferation of instruments.[24]

Table 6 presents our dynamic panel estimates for specification (4), i.e. using dummy variables to indicate contiguity between a destination and previous export destinations. The table is organized in the same way as the previous tables. We find true state dependence in all our specifications even at the firm-destination level. Our result that sharing a common border is the largest and most significant contiguity effect is corroborated by the dynamic panel estimates. Sharing a common language, colonial ties or being in the same income group are all significant but have smaller effects than common border.

Column VI presents results when we include all regressors at the same time. Sharing a common border still has a similar impact on the probability of exporting to a country compared to the specification in column I. The same holds for sharing a common language or being in the same income group. Interestingly, sharing a common colonizer has a significant and positive effect in column III. This effect vanishes, however, in column VI. Being on the same continent even turns out to have an albeit small but significantly negative effect. Note, however, that a country which is located on the same continent very likely also shares a common border or a common language with a previous export destination. In other words, there is a high correlation between our different contiguity measures. We again compare our estimated marginal effect to the empirical probability of a firm exporting to a particular country from Table A.37 from the online Appendix. Given the empirical probabilities, this implies e.g. a 33% (0.023/0.070) increase in the probability of a firm exporting to Singapore when it has previously exported to Malaysia.

We use the Sargan test and a test for the first and second order autocorrelation of the residuals to test our specifications. The bottom three lines of Table 6 report their p-values. While we find evidence for first order autocorrelation in the residuals, we do not find evidence for second order autocorrelation, implying that the moment conditions used for the dynamic panel estimator are valid. We also report a Sargan overidentification test even though this test is only valid under homoskedasticity. In most specifications also the Sargan test does not reject our model specification. Only in specification VI the Sargan test rejects our internal instruments. Overall, results suggest a proper model specification.

[23] While most applications of dynamic panel estimators only include one lag, Cameron and Trivedi (2005) show that the dynamic setting can easily be extended to more lags. We also experimented with including only one lag. However, these specifications were clearly rejected by model specification tests such as the autocorrelation tests or Sargan test. Full results are available in the online Appendix in Tables A.5 and A.6.

[24] In the online Appendix in Tables A.7 and A.8, we additionally use our instrument from the instrumental variable regressions from Section 3.3, $y2006_t \times C_j$ and $y2006_t \times N_j$, respectively, as an external instrument. Using the additional instrument increases our point estimates and, as expected, improves the performance of the model specification Sargan test.

We again can use the number of contiguous export destinations as an alternative regressor. Then, the dynamic panel specification is given by

$$y_{ijt} = \phi_1 y_{ij,t-1} + \phi_2 y_{ij,t-2} + \delta N_{ij,t-1} + \theta_{ij} + \theta_t + \varepsilon_{ijt}. \tag{5}$$

Table 7 shows that results are hardly affected. Again, we find strong evidence for true state dependence, and again sharing a common border has the largest impact on the destination choice. Specification tests for first and second order autocorrelation again do not invalidate our regressions. However, the Sargan test does reject the validity of our internal instruments in specifications IV–VI. Note, however, that the test is only valid under homoskedasticity which is violated in trade data (see Santos Silva and Tenreyro, 2006).

4. Multi-product firms

Until now our analysis considered an export destination as contiguous if the firm previously exported any product to a contiguous market. It is well known that a substantial fraction of firms produce and export multiple products, and that multi-product firms make up for the majority of sales in a given industry, see Arkolakis and Muendler (2010) and Bernard et al. (2010). In our sample, 56% of firms export in more than one HS-6 product category. If there exists within-firm correlation of export destination choices between products, then a firm may enter a new export market with a product when it has previously sold a different product in a contiguous market.

Both supply and demand side reasons may explain these economies of scope. When costs for product adaptation are lower for other products within a firm once they have been incurred for a specific market and product, the additional cost of adapting the product for a similar market may be lower. When a firm sells its products under a single brand to benefit from brand loyalty of consumers, successful exports of one product provide information about likely profitable exports across the whole product mix of a firm's brand.

We modify our dynamic panel specification given in Eq. (4) as follows:

$$\begin{aligned} y_{ijt} = {} & \phi_1 y_{ij,t-1} + \phi_2 y_{ij,t-2} + \delta_1 \mathbb{I}\left(N_{ij,t-1}^{sameproduct} > 0\right)_{ijt} \\ & + \delta_2 \mathbb{I}\left(N_{ij,t-1}^{otherproducts} > 0\right)_{ijt} + \theta_{ij} + \theta_t + \varepsilon_{ijt}, \end{aligned} \tag{6}$$

where i now denotes a firm-product couple at the HS-6-digit product category and no longer a single firm, and where $N_{ij,t-1}^{sameproduct}$ is the number of contiguous destinations where the firm has exported the same product before and $N_{ij,t-1}^{otherproducts}$ is the number of contiguous destinations where the firm has previously exported products from other HS-6 categories. θ_{ij} now captures unobserved time-invariant firm-product-destination characteristics.

As we now focus on firm-product couples, we use all firm-product couples which have never entered the previously restricted countries before 2005. In our sample, there are 6573 firm-product couples of 1965 firms, implying that a firm exports about 3.3 products on average.[25] In our previous regressions, we kept only those firms that never exported any product into the previously restricted MFA countries. As firms may have entered into the previously restricted MFA countries only with a subset of their products, we now keep all other firm-product couples where we do not observe exports into the previously restricted MFA countries before 2005. Hence, there are more firms in our multi-product sample than in the previous regressions.[26]

[25] Descriptive statistics can be found in Table A.39 in the online Appendix.

[26] Imagine a firm which has exported panties to an MFA country in 2004 but not bras. In our firm level regressions, this firm is dropped from the sample. However, in our multi-product regressions we will keep the bra observations.

Table 6
Dynamic panel estimates—dummy.

	I	II	III	IV	V	VI
$\mathbb{I}(N_{ij,t-1}>0)_{ijt}$ defined according to						
Common border	0.024***					0.023***
	(0.004)					(0.004)
Common language		0.003***				0.003***
		(0.001)				(0.001)
Common colonizer			0.003***			0.001
			(0.001)			(0.001)
Common income group				0.003***		0.002***
				(0.001)		(0.001)
Common continent					−0.001	−0.005***
					(0.001)	(0.001)
$y_{ij,t-1}$	0.344***	0.344***	0.343***	0.343***	0.344***	0.347***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
$y_{ij,t-2}$	0.077***	0.078***	0.076***	0.078***	0.076***	0.074***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Observations	777,000	777,000	777,000	777,000	777,000	777,000
# of firms	1295	1295	1295	1295	1295	1295
AR(1)	0	0	0	0	0	0
AR(2)	.821	.822	.889	.803	.929	.950
Sargan	.383	.604	.170	.406	.061	.008

Notes: The dependent variable is y_{ijt} which is a dummy variable indicating whether a firm i exported to country j in year t. All regressions include firm-destination fixed effects, as well as year dummies (not reported). Standard errors are in parentheses. All regressions use robust standard errors and treat the lags of the dependent variable as well as the regressors of interest as predetermined. We use the two-step system GMM estimator from Blundell and Bond (1998) and, due to the two-step estimation, we use the Windmeijer (2005) finite sample correction for the standard errors. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. The values reported for AR(1) and AR(2) are the p-values for first and second order autocorrelated disturbances in the first differences equations. The row for the Sargan reports the p-values for the null hypothesis of validity of the overidentifying restrictions assuming homoskedasticity.

We present results in Table 8. Overall we find hardly any evidence that exporting to a country is more likely after a previous entry into contiguous export destinations across products, the only exception being the common border coefficient in specifications I and VI. We find that the probability of choosing a country increases by 1.8 percentage points when a firm previously has exported the same HS-6 product to a contiguous country, and by 0.2 percentage points if it has exported other HS-6 products. For the other contiguity measures, our results indicate no (economically) significant effect of across product learning. Interestingly, we find small significant negative effects for common colonizer and common continent. This may hint at a potential for diversification in a firm's export portfolio by selling different products to different contiguous countries when they share a colonial past or are located on the same continent. Note that our results for the same HS-6 product are in line with the effects found at the firm-level in Section 3.4. The tests for autocorrelation in the disturbances in first differences indicate a well-specified model. However, contrary to the firm-level regressions, the Sargan test now rejects the validity of the overidentifying restrictions. With nearly four million observations based on 6573 firm-product couples, the amount of heteroskedasticity is higher by construction as compared to the firm-level regressions. This may explain the rejection of the overidentifying restrictions by the Sargan test based on the assumption of homoskedasticity.

Table 7
Dynamic panel estimates—*N*.

	I	II	III	IV	V	VI
$N_{ij,t-1}$ defined according to						
Common border	0.023***					0.013***
	(0.004)					(0.004)
Common language		0.002***				−0.001**
		(0.000)				(0.000)
Common colonizer			0.004***			0.000
			(0.001)			(0.001)
Common income group				0.006***		0.005***
				(0.000)		(0.001)
Common continent					0.004***	0.002***
					(0.000)	(0.000)
$y_{ij,t-1}$	0.344***	0.348***	0.343***	0.338***	0.342***	0.356***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
$y_{ij,t-2}$	0.077***	0.081***	0.079***	0.081***	0.084***	0.098***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Observations	777,000	777,000	777,000	777,000	777,000	777,000
# of firms	1295	1295	1295	1295	1295	1295
AR(1)	0	0	0	0	0	0
AR(2)	.825	.673	.738	.582	.535	.212
Sargan	.346	.057	.081	.010	.003	0

Notes: The dependent variable is y_{ijt} which is a dummy variable indicating whether a firm i exported to country j in year t. All regressions include firm-destination fixed effects, as well as year dummies (not reported). Standard errors are in parentheses. All regressions use robust standard errors and treat the lags of the dependent variable as well as the regressors of interest as predetermined. We use the two-step system GMM estimator from Blundell and Bond (1998) and, due to the two-step estimation, we use the Windmeijer (2005) finite sample correction for the standard errors. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. The values reported for AR(1) and AR(2) are the p-values for first and second order autocorrelated disturbances in the first differences equations. The row for the Sargan reports the p-values for the null hypothesis of validity of the overidentifying restrictions assuming homoskedasticity.

Table 8
Multi-product firms: dynamic panel estimates—dummy.

		I	II	III	IV	V	VI
$\mathbb{I}(N_{ij,t-1}>0)_{ijt}$ defined according to							
Common border	$\mathbb{I}\left(N^{sameproduct}_{ij,t-1}>0\right)_{ijt}$	0.018***					0.018***
		(0.002)					(0.002)
	$\mathbb{I}\left(N^{otherproducts}_{ij,t-1}>0\right)_{ijt}$	0.002***					0.004***
		(0.001)					(0.001)
Common language	$\mathbb{I}\left(N^{sameproduct}_{ij,t-1}>0\right)_{ijt}$		0.002***				0.001***
			(0.000)				(0.000)
	$\mathbb{I}\left(N^{otherproducts}_{ij,t-1}>0\right)_{ijt}$		−0.000**				−0.000
			(0.000)				(0.000)
Common colonizer	$\mathbb{I}\left(N^{sameproduct}_{ij,t-1}>0\right)_{ijt}$			0.002***			0.001**
				(0.001)			(0.001)
	$\mathbb{I}\left(N^{otherproducts}_{ij,t-1}>0\right)_{ijt}$			−0.001**			−0.001
				(0.000)			(0.000)
Common income group	$\mathbb{I}\left(N^{sameproduct}_{ij,t-1}>0\right)_{ijt}$				0.001***		0.001*
					(0.000)		(0.000)
	$\mathbb{I}\left(N^{otherproducts}_{ij,t-1}>0\right)_{ijt}$				−0.000		−0.000
					(0.000)		(0.000)
Common continent	$\mathbb{I}\left(N^{sameproduct}_{ij,t-1}>0\right)_{ijt}$					−0.000	−0.003***
						(0.000)	(0.000)
	$\mathbb{I}\left(N^{otherproducts}_{ij,t-1}>0\right)_{ijt}$					−0.003***	−0.003***
						(0.000)	(0.000)
$y_{ij,t-1}$		0.309***	0.315***	0.311***	0.310***	0.325***	0.338***
		(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
$y_{ij,t-2}$		0.076***	0.081***	0.076***	0.077***	0.093***	0.106***
		(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations		3,943,800	3,943,800	3,943,800	3,943,800	3,943,800	3,943,800
# of firm-product couples		6573	6573	6573	6573	6573	6573
# of firms		1965	1965	1965	1965	1965	1965
AR(1)		0	0	0	0	0	0
AR(2)		.693	.964	.657	.705	.219	.016
Sargan		0	0	0	0	0	0

Notes: The dependent variable is y_{ijt} which is a dummy variable indicating whether a firm-product couple i exported to country j in year t. All regressions include firm-product-destination fixed effects, as well as year dummies (not reported). Standard errors are in parentheses. All regressions use robust standard errors and treat the lags of the dependent variable as well as the regressors of interest as predetermined. We use the two-step system GMM estimator from Blundell and Bond (1998) and, due to the two-step estimation, we use the Windmeijer (2005) finite sample correction for the standard errors. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. The values reported for AR(1) and AR(2) are the p-values for first and second order autocorrelated disturbances in the first differences equations. The row for the Sargan reports the p-values for the null hypothesis of validity of the overidentifying restrictions assuming homoskedasticity.

In the online Appendix, we also present results using the number of contiguous countries as regressor in Table A.11. Results remain similar. To sum up, we hardly find evidence for across product learning of exporters. This probably hints at only small economies of scope for multi-product firms when entering new export markets with several products, at least across markets.

5. Robustness checks

We now discuss several effects that could influence our results and which are unrelated to the cross-country correlation in export destination choices of firms. Detailed regression results pertaining to these robustness checks can be found in the online Appendix. Unless otherwise noted, all robustness checks use specification VI from Table 7 as a starting point.

5.1. Lagged export values

In addition to learning from its previous export experience, a firm may also exhibit increasing returns to scale via a learning by doing mechanism in textile and apparel production. Since the MFA quotas represent an artificial quantity restriction, removing it should result in a large increase in the volume of export sales. As our regressor of interest is correlated with a firm's export volume by construction and this might bias our results, we include the lagged export value as an additional control variable. Contiguity between export destinations still has a significant positive impact on a firm's exporting decision even when controlling for the lagged export value.

5.2. Competitors' success

Krautheim (2012) theoretically investigates the importance of spillover effects from competing firms on exporting fixed costs. The number of exporting firms of the same product or the number of export markets already entered by close competitors may influence a firm's ability to export to a specific destination. Wen (2004) shows that Chinese firms producing in the same industry tend to cluster geographically across Chinese regions. We therefore use the sum of the number of previously entered contiguous export destinations over all competitors in the same Chinese prefecture, $N_{-ij,p,t-1}$, to control for these spillover effects. We can construct this control variable using again all of our different contiguity measures. Controlling for spillover effects from close competitors hardly affects our results.

5.3. Trading agents

The raw data contains a number of trading agents (“intermediary firms”) which mediate trade for other firms but do not directly engage in production. Including these firms could cause problems as their behavior is probably different from that of manufacturing firms. To exclude the possibility that our results are driven by these trading agent business networks, we exclude trading firms which are identified by certain keywords in their names. Ahn et al. (2011) use the Chinese characters for "importer", "exporter", and "trading" to identify "intermediary firms". By contrast, we follow Upward et al. (2011) and use a more comprehensive list of keywords which are typically used by various kinds of trading agents in China. These trading companies represent about 4% of our observations. Dropping trading agents does not change our conclusions.

5.4. State-owned firms

Khandelwal et al. (2013) argue that state-owned firms seem to have been more likely to obtain a license before the MFA quota restrictions were lifted. This makes them potentially different from privately-owned firms. We therefore exclude state-owned firms. Again, our results hold up.

5.5. Foreign-owned firms

We exclude all foreign-owned firms and processing trade as the destination choice of firms could be influenced by the foreign headquarters' location or by the location of other foreign direct investments realized by the parent company. While qualitative results are similar, our results lose some of their significance. This may well be due to the large drop in the number of firms and observations to about a tenth of the full sample.

5.6. Processing trade

Our data allow us to distinguish between processing and ordinary exports. The former refers to exports that are assembled in an export processing zone and use a high share of imported intermediate inputs. Note that foreign owned firms often engage in processing exports but not necessarily so. Processing exports may be special with respect to the export location choice because they could be influenced by a third foreign party. In addition, processing trade firms may have less liberty in their export destination choice. Excluding processing trade export transactions leads again to a substantial drop in the number of observations to around a fifth of the original sample. Our results are again qualitatively similar but lose some of their significance.

5.7. Excluding Russia

As Russia shares a common border with both China and MFA-restricted countries, our identification for the diff-in-diff could mainly stem from Russia. Indeed, our coefficient on common border reduces in size but remains positive and significant.

5.8. Country-specific time trends

We include country-specific time trends in all the regressions presented in Sections 3 and 4. The diff-in-diff and instrumental variable specifications are not robust to country-specific time trends and hint at high multicollinearity between the country-specific time trends and our regressors of interest. The fixed effects and dynamic panel estimates, however, retain their significance and are similar in magnitude compared to the regressions without time trends. When using continent-specific time trends, also the diff-in-diff and instrumental variable specifications lead to a significant and similar effect of sharing a common border.

5.9. Different effect of previous entry in contiguous MFA-restricted versus non-MFA-restricted countries

We follow up on footnote 16 and introduce $N^{MFA}_{ij,t-1}$, the number of contiguous previous export destinations which are MFA countries, and $N^{nonMFA}_{ij,t-1}$, the number of contiguous previous export destinations which are not MFA countries, instead of our default regressor $N_{ij,t-1}$, the total number of contiguous previous export destinations, in our regressions from Section 3.2. Note that $N_{ij,t-1} = N^{MFA}_{ij,t-1} + N^{nonMFA}_{ij,t-1}$. We find that evidence for spatial exporters in our sample comes predominantly from entering in previously restricted MFA countries if we define our regressor as sharing a common border, consistent with our identification and sample selection strategy.

5.10. Full sample

As in principle the dynamic panel regressions take account of the previous export experience of a firm by the lagged dependent variable, we re-estimate our model by including also those firms which entered in MFA-restricted countries between 2000 and 2004, i.e. those which did have an export license. Estimated coefficients remain similar. However, the model specification tests clearly reject all regressions. This hints at the endogeneity bias introduced by not restricting the sample to firms who have never exported to MFA-restricted countries between 2000 and 2004.

6. Conclusion

How do firms choose new export destinations? While there are many factors that are important for this decision, one empirical regularity stands out: Firms tend to choose new export markets that are geographically close to their prior export destinations more often than standard gravity models would predict.

We quantify this spatial pattern using Chinese customs data and the quasi-natural experiment of the end of the import quota restrictions on Chinese textile exports which creates an exogenous set of potential new destinations (25 EU countries, the US, and Canada). We use the sample of firms which had never exported to the 27 previously restricted MFA countries before 2005. These firms allow us to identify the effect of previous export history in contiguous countries on the probability of exporting to one of the 150 countries which were not covered by the MFA import restrictions. This enables us to quantify the importance of 'extended gravity' or 'spatial exporters', i.e. the time-varying firm-specific heterogeneity in export destination choices shaped by firms' previous export experience in spatially close countries. Our regressions control for unobserved time-invariant heterogeneity at the firm-country level as well as true state dependence.

Our baseline results show that the probability to export to a country increases by about two percentage points for each prior export destination with a common border with this country. For example, this implies a 33% increase in the probability of a firm exporting to Singapore, one of the top export destinations in our data set of non-MFA countries, when it has previously exported to Malaysia, a country which shares a border with Singapore.

Appendix A. Online Appendix

The online Appendix to this article can be found at http://dx.doi.org/10.1016/j.jinteco.2014.11.006.

References

Ahn, J., Khandelwal, A.K., Wei, S.-J., 2011. The role of intermediaries in facilitating trade. J. Int. Econ. 84 (1), 73–85.

Albornoz, F., Calvo Pardo, H.F., Corcos, G., Ornelas, E., 2012. Sequential exporting. J. Int. Econ. 88 (1), 17–31.

Antràs, P., Fort, T. C., and Tintelnot, F. (2014). The margins of global sourcing: theory and evidence from U.S. firms. *unpublished working paper*.

Arellano, M., Bond, S.R., 1991. Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. Rev. Econ. Stud. 58 (2), 277–297.

Arkolakis, C., Muendler, M.-A., 2010. The extensive margin of exporting products: a firm-level analysis. NBER Working Paper 16641.

Baltagi, B.H., 2008. Econometric Analysis of Panel Data, 4th ed. Wiley, Chichester.

Basker, E., 2005. Job creation or destruction? Labor market effects of Wal-Mart expansion. Rev. Econ. Stat. 87 (1), 174–183.

Bernard, A.B., Redding, S.J., Schott, P.K., 2010. Multiple-product firms and product switching. Am. Econ. Rev. 100 (1), 70–97.

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust differences-in-differences estimates? Q. J. Econ. 119 (1), 249–275.

Blundell, R., Bond, S.R., 1998. Initial conditions and moment restrictions in dynamic panel data models. J. Econ. 87 (1), 115–143.

Borchert, I., 2008. Essays on Market Access and Export Product Variety. (PhD thesis) University of St. Gallen.

Brambilla, I., Khandelwal, A.K., Schott, P.K., 2010. China's experience under the Multi-Fiber Arrangement (MFA) and the Agreement on Textiles and Clothing (ATC). In: Feenstra, R.C., Wei, S.-J. (Eds.), China's Growing Role in World Trade. The University of Chicago Press, Chicago and London, pp. 345–387.

Buckley, C., 2005. Europe and China in accord over end to a textile dispute. New York Times September 6. Available from: http://www.nytimes.com [accessed 11/09/2013].

Cameron, A.C., Trivedi, P.K., 2005. Microeconometrics: Methods and Applications. Cambridge University Press, Cambridge.

Cameron, A.C., Trivedi, P.K., 2009. Microeconometrics Using Stata. Stata Press, College Station.

Chaney, T., 2014. The network structure of international trade. Am. Econ. Rev. 104 (10), 3600–3634.

Das, S., Roberts, M.J., Tybout, J.R., 2007. Market entry costs, producer heterogeneity, and export dynamics. Econometrica 75 (3), 837–873.

Defever, F., 2012. The spatial organization of multinational firms. Can. J. Econ. 45 (2), 672–697.

Defever, F. and Ornelas, E. (2014). Trade liberalization and third-market effects. *unpublished working paper*.

Defever, F., Heid, B., Larch, M., 2011. Spatial exporters. CESifo Working Paper 3672.

Evenett, S. J. and Venables, A. J. (2002). Export growth by developing countries: market entry and bilateral trade. *unpublished working paper*.

Harrigan, J., Barrows, G., 2009. Testing the theory of trade policy: evidence from the abrupt end of the multifiber arrangement. Rev. Econ. Stat. 91 (2), 282–294.

Jackson, M.O., Rogers, B.W., 2007. Meeting strangers and friends of friends: how random are social networks? Am. Econ. Rev. 97 (3), 890–915.

Jia, P., 2008. What happens when Wal-Mart comes to town: an empirical analysis of the discount retailing industry. Econometrica 76 (6), 1263–1316.

Khandelwal, A.K., Schott, P.K., Wei, S.-J., 2013. Trade liberalization and embedded institutional reform: evidence from Chinese exporters. Am. Econ. Rev. 103 (6), 2169–2195.

Krautheim, S., 2012. Heterogeneous firms, exporter networks and the effect of distance on international trade. J. Int. Econ. 87 (1), 27–35.

Lawless, M., 2013. Marginal distance: does export experience reduce firm trade costs? Open Econ. Rev. 24 (5), 819–841.

Mayer, T., Zignago, S., 2011. Notes on CEPII's Distances Measures: The GeoDist Database. CEPII Working Paper 2011-25 .

Molina, A.C., 2010. Are preferential agreements stepping stones to other markets? IHEID Working Paper.

Morales, E., Sheu, G., Zahler, A., 2011. Gravity and extended gravity: estimating a structural model of export entry. MPRA Paper No. 30311.

Morales, E., Sheu, G., and Zahler, A. (2014). Extended gravity. *unpublished working paper*.

Nguyen, D.X., 2012. Demand uncertainty: exporting delays and exporting failures. J. Int. Econ. 86 (2), 336–344.

Nickell, S.J., 1981. Biases in dynamic models with fixed effects. Econometrica 49 (6), 1417–1426.

Rauch, J.E., 2001. Business and social networks in international trade. J. Econ. Lit. 39 (4), 1177–1203.

Roberts, M.J., Tybout, J.R., 1997. The decision to export in Colombia: an empirical model of entry with sunk costs. Am. Econ. Rev. 87 (4), 545–564.

Santos Silva, J.M.C., Tenreyro, S., 2006. The log of gravity. Rev. Econ. Stat. 88 (4), 641–658.

Upward, R., Bernhofen, D. M., and Wang, Z. (2011). Quota as a Sorting Machine: Quantity restrictions and price adjustments of chinese textile exports to the U.S. *unpublished working paper*.

Wen, M., 2004. Relocation and agglomeration of Chinese industry. J. Dev. Econ. 73 (1), 329–347.

White, M., Gow, D., 2005. EU and China in 'Bra Wars' Deal. The Guardian (September 6. Available from http://www.theguardian.com [accessed 11/09/2013]).

Wikipedia, 2013. Multi fibre arrangement (Available from http://en.wikipedia.org [accessed 11/09/2013]).

Windmeijer, F., 2005. A finite sample correction for the variance of linear efficient two-step GMM estimators. J. Econ. 126 (1), 25–51.

Winkelmann, R., Boes, S., 2009. Analysis of Microdata, 2nd ed. Springer, Berlin.