Success and Failure of African Exporters*

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November 17, 2012

Abstract

Using a new dataset with transaction-level export data from four African countries (Malawi, Mali, Senegal and Tanzania), this paper explores the determinants of success upon entry into export markets, defined as survival beyond the first year at the firm-product-destination level. We find that the probability of success *rises* with the number of same-country firms exporting the same product to the same destination, suggesting the existence of cross-firm externalities. We explore several conjectures on the determinants of these externalities and provide evidence suggestive of information spillovers, possibly mediated through the banking system.

Keywords: Africa, Export survival, Spillovers, Firms, Information.
JEL classification: F10, F14, O55.

*Support from the BNPP Trust Fund on Improving the Survival of African Exports, from the World Bank Research Support Budget, and from Switzerland's NCCR is gratefully acknowledged. We are also grateful to the Malawi Revenue Authority, the Direction Générale des Douanes du Mali, the Direction Générale des Douanes du Sénégal, and the Tanzania Revenue Authority for their cooperation and willingness to share their data. We thank Frances Aidoo, William Baah-Boateng, Sidiki Guindo, Anthony Mveyange and Nelson Nsiku for their assistance in the collection of the customs data used in this paper. Special thanks to the editor, two referees, Daniel Lederman, Beata Javorcik, Aaditya Mattoo, Mongoljin Batsaikhan, Jose Guilherme Reis and participants at the World Bank Trade Seminar, the World Bank Economists Forum, NEUDC 2011 in Yale, the CSAE Annual Conference, the Second International Workshop on Firms, the Trade and Development Conference in Aix en Provence, and ETSG 2010 for very useful comments and suggestions. We remain solely responsible for any remaining errors.

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1 Introduction

In spite of great strides since the late 1990s, African exports are still marginal in world trade and suffer from various vulnerabilities, ranging from low unit values to volatility, concentration and low survival (see World Bank, forthcoming). Low survival does not necessarily entail welfare losses if it reflects strong experimentation at the extensive margin. However, it can be inefficient if sunk costs of entry and exit are substantial, as suggested by the work of Das, Robert and Tybout (2007). High failure rates, the flip side of low survival, may also help explain the weak contribution of the extensive margin to overall export growth (Besedes and Prusa 2007). Thus, identifying the determinants of the 'sustainability margin' of exports is important for our understanding of the constraints to low-income country export growth. This is what the present paper sets up to do, relying on a new transaction-level export dataset obtained from the Customs authorities of four African countries.

The key contributions of this paper are twofold. First, we document, for a sample of four low-income African countries, a set of stylized facts on export survival that is broadly consistent with the emerging firm-level literature, so far confined to OECD or middle-income countries. For instance, we find evidence of strong experimentation at the extensive margin and low first-year survival rates, followed by a rising conditional survival probability. Like Eaton et al. (2008), we also find that export spells that survive tend to grow. For instance, a Senegalese firm that launched a new product on a given market in 2001 would have seen sales of that product on that market rise four-fold by 2008 if it had survived till then. Larger initial export scale and scope at the firm level, by which we mean respectively destinations per product and products per destination, also contribute to higher chances of success.

Second, we provide novel evidence on export success, defined as survival beyond the first year after entry. Surprisingly, the success probability of a new entrant on a product-destination market *rises* with the number of firms exporting the same product from the *same country* on that market. This cross-firm 'synergy effect' is suggestive of external economies in export success, a potential driver of exports growth that has been hitherto overlooked in the literature. For a Senegalese exporter, for instance, the probability of success past the first year would rise by nearly a fifth (from a baseline of 22 percent) if the number of national firms selling the same product (at HS 6 digits) on the same destination market were to double from the baseline 22 to 44. This evidence, which is robust to a number of 'placebo' and robustness checks, suggests that the presence of firms selling the same good from the same country on a given product-destination market provides new entrants information on the market's profitability. It may also contribute to success by revealing information on consumer preferences and reliable buyers, which is consistent with our results that the externality effect is stronger in sectors characterized by higher quality heterogeneity.

Information about the profitability of export markets may also become available to banks in the country of origin and may induce them to relax credit constraints by reducing perceived risk, a mechanism consistent with the model of Besedes, Kim and Lugovskyy (2011). Eased financial constraints may in turn enable exporters to respond when buyers ask them to ramp up production after small-scale entry, alleviating a frequent cause of failure in trade relationships. Indeed, we find that cross-exporter externalities are stronger in sectors that depend more on external finance.

Our results may help explain a finding highlighted in Easterly, Resheff and Schwenkenberg (2009), that national export success often takes the form of 'big hits', with one narrow export item suddenly growing rapidly. If a sufficient number of exporters target one market simultaneously, our results imply that their chances of success increase, possibly triggering a virtuous cycle of entry, survival and growth.

From a policy perspective, our findings can be thought of as providing some support to the need for export promotion. Unless fully internalized by exporters, the externalities that we identify lead to a market failure. However, this prescription should be taken cautiously, as the record of publicly-financed export promotion is patchy, especially in developing countries.[1]

The paper is organized as follows. Section 2 discusses the recent literature related to this paper. Section 3 presents a brief description of the data. Section 4 discusses the estimation strategy and result, and Section 5 concludes.

2 Export survival: What do we know?

At the product level, the determinants of export survival have been explored by a small but growing literature. Besedes and Prusa (2006) used two panels of U.S. imports, one spanning 1972-88 with tariff-schedule data, the

[1] Recent evidence is more positive. Lederman, Olarreaga and Payton (2010) find strong effects; Volpe's (2011) survey also shows positive and significant effects for a number of Latin American export-promotion agencies; Cadot, Fernandes, Gourdon and Mattoo (2012) find that Tunisia's export-promotion program had persistent effects only at the extensive margin.

other spanning the 1989-2001 period with 10-digit data (the Feenstra-NBER dataset). In both cases, they found that half of all trade relationships lasted only one year and three quarters lasted three years or less. Once censoring was taken into account, median duration was two years. Most strikingly, this pattern of short duration was robust to aggregation at HS6, even though one would expect interruptions to be smoothed out by aggregation. They also found negative duration dependence, meaning that the hazard rate fell as export spells grew older. This finding, however, has been recently contested by Brenton, Saborowsky and von Uexhull (2010).[2] In terms of survival determinants, Besedes and Prusa (2006) found that industrial-country exports lasted longer, and so did exports of machinery, a finding confirmed by the analysis of Asian trade flows by Obashi (2010).

Besedes and Prusa (2006) further explored the determinants of export survival by testing the implications of a search model proposed by Rauch and Watson (2003) in which importers search for low-cost suppliers and exporters invest optimally in production capacity in the face of moral hazard (risk of non-payment). The model implies that, in general, smaller initial transactions have a lower life expectancy; however, differentiated goods, where information problems and moral hazard are highest, are characterized by both smaller initial transactions *and* longer life expectancy. The model's predictions are upheld by Cox regressions on U.S. import data using Rauch's (1999) index of product differentiation as a regressor. That is, the hazard rate is 23% higher for homogenous products than for differentiated ones, although initial transactions are 40% to 350% larger. In related work, Besedes (2008) also finds supports for the Rauch-Watson hypotheses on a restricted sample of Rauch-differentiated products where he proxies search costs by the number of potential suppliers and reliability by income levels.

More recent evidence largely confirmed the early findings. The determinants of export duration were explored by Nitsch (2009) using Cox regressions on a ten-year panel of German imports at the HS8 level. He found that gravity variables (distance, exporter GDP, common language, common border, etc.) influenced the duration of trade flows pretty much the same way they influenced trade volumes. Interestingly, he found that the short duration of trade flows held even when flows below 10'000 euros were ex-

[2]Brenton et al. argue that the assumption of proportional hazards, which is needed for Cox regressions to be valid, typically does not hold in export-duration samples (this can be verified using a Schőnfeld test). Using the alternative Prentice-Gloeckler (1978) estimator, they find no duration dependence. For a full discussion of the drawbacks of Cox regressions in the context of trade data and alternative estimators, see Hess and Persson (2010).

cluded. Fugazza and Molina (2009) extended the exploration to a nine-year panel of HS6 bilateral trade flows between 96 countries using, as regressors, gravity variables and time required for export procedures (based on the World Bank's *Doing Business* surveys) as proxies for fixed costs. Besides usual findings on the effect of gravity variables and income levels, they also found that fixed costs reduced survival.[3] A similar exercise was carried out on Asian trade flows by Obashi (2010) with largely convergent results. In particular, the 2-to-3 year median survival seems to hold across all samples studied. She also found that vertical trade relationships (involving the sale of semi-finished products) had hazard rates one-third lower than those involving the sale of final goods, and that they were less sensitive to trade costs (e.g. distance or exchange-rate fluctuations). Besedes (2012) showed that NAFTA's effects on the duration of trade spells was ambiguous, *raising* the hazard rate of Mexican and U.S. intra-NAFTA exports, in particular for increasing-returns to scale industries in the case of Mexico, but having different or insignificant effects in other cases.

Additional stylized facts about African export survival have been highlighted in a recent World Bank study (World Bank, 2012). Once income levels and other country-level covariates are controlled, sub-Saharan Africa's performance in terms of export survival is comparable to that of Latin America and better than that of the Middle East. That is, the short survival of African exports is largely explained by observable characteristics of the local business environment and trade costs. Low survival is also mitigated by the presence of large diasporas in destination countries, highlighting the importance of networks in overcoming informational barriers.

Recent research has shed new light on the influence of financial markets on the duration of trade, an issue that is related to this paper. Jaud, Kukenova and Strieborny (2009) show that export products whose factor intensities lie far away from the exporting country's factor endowment tend to survive less than others, and that financial development reinforces that effect, suggesting that financial development induces banks to direct resources more efficiently. Besedes, Kim and Lugovskyy (2011) develop a model where banks lend at an actuarily-fair rate given a risk of export failure that jumps down after the first period. In their model, credit constraints lead exporters to start smaller than if they faced no constraint and to grow faster subsequently, as their lenders' information improves. They show that the model's

[3] This is unintuitive: in microeconomics, the shut-down point depends on average *variable* costs, not on fixed costs. However the fixed export costs they consider are incurred for each transaction, although they do not depend on transaction size. They are therefore not really fixed when looking at flows aggregated to the annual level.

implications are consistent with the behavior of spell duration and growth on highly disaggregated trade data.

A smaller number of recent papers have made use of the growing availability of firm-level datasets to shed new light on the determinants of export success. For instance, Gőrg, Kneller and Murakősy (2012) test the implications of the heterogeneous-firm model of Bernard et al. (2006) on a rich panel of 2,043 Hungarian firms spanning the transition from centrally-planned to market economy (1992-2003). Their data contained firm characteristics and exports at the firm-HS6 level. They find large product turnover during the period as firms constantly rearrange their product portfolios. They also find longer survival for products located close to the firm's core competencies and to the country's comparative advantage. These results are consistent with those of Iacovone and Javorcik (2010) who show the importance of churning at the firm level in response to exogenous opportunities provided by increased globalization. Alvarez and López (2008) used Tobit regressions to study the determinants of industry-level rates of entry and exit into exporting using a 10-year panel of 5'000 Chilean plants. They found that within-industry heterogeneity, measured (inter alia) by the dispersion of firm-level productivity levels, played an important role in explaining firm turnover in and out of exporting. By contrast, trade costs, factor intensities, and exchange-rate fluctuations were found to have only marginal impacts. Carballo and Volpe (2008) used a six-year panel of firm-level Peruvian exports at the HS10 level to explore how diversification strategies (in terms of products and markets) affected the survival of firm-level exporting activity. They found that both geographical and product-wise diversification raised survival, but geographical diversification more so—presumably because it proxies for product quality.

3 Data

3.1 Customs data

Our study focuses on four low-income Sub-Saharan African countries for which we have customs data: Malawi, Mali, Senegal and Tanzania. Malawi, Mali, and Senegal have all been relatively open over the last decade, with ratios of merchandise trade to GDP hovering roughly between 50% and 60%, a level comparable to the rest of the continent, although openness ratios must be interpreted carefully in sub-Saharan Africa where they are largely driven by natural-resource sectors. Tanzania is in a category apart as it has been a relatively closed economy for a long time and opened up for good only over the last decade, with a trade/GDP ratio climbing rapidly from 22% in 2000 to 50% in 2010. Over 2000-2010, dollar exports have grown at a compound annual growth rate of 10.9% for Malawi, 13.8% for Mali, 8.9% for Senegal and 19.3% for Tanzania, against averages of 13.0% for low-income countries and 13.5% for sub-Saharan Africa (South Africa excluded).[4] The share of manufactured products in our countries' merchandise exports ranges from 16.4% for Malawi to 25.6% for Senegal.[5]

Our export dataset is generated from raw data files collected by customs authorities containing export flows at the transaction level. The files were provided by the customs authorities of Malawi, Mali, Senegal and Tanzania. Each of them contains information on products exported at the highest level of disaggregation of the HS code used by these administrations: 10-digit for Mali and Senegal and 8-digit for Malawi and Tanzania. In addition to product information, each file contains information on destination market, FOB shipment value, net weight, port used and date of transaction. Original names and tax IDs identifying the individual firm were replaced by 'dummy' digital IDs so as to preserve confidentiality. We aggregated transactions up to annual totals at the 6-digit level, the standard level used in cross-country comparisons. Finally, for consistency, we filtered out years with different port coverage.[6] Sample periods are 2005-2008 for Malawi and Mali, 2000-2008 for Senegal, and 2003-2008 for Tanzania. While not strictly identical to product-level export data published in UN-COMTRADE, our data is highly correlated with it, with correlation coefficients between the two sources at the HS6/destination level equal to 0.93 for Mali, 0.93 for Malawi, 0.98 for Senegal and 0.998 for Tanzania.

Table 1 presents basic descriptive statistics. Tanzania has the largest number of exporters (1,359), followed by Malawi (856), Senegal (715), and Mali (280); however, they are less diversified than those of other countries in our sample in terms of markets. Senegal's exporters are, on average, the most diversified in terms of products.

Our variables of interests are indexed as follows. Let f be a firm, d a destination, p a product (at HS6), t a year, and v^c_{fpdt} the dollar value of exports of product p to destination d in calendar year t by firm f from country c. Because there are no multi-country firms in our sample, indexing observations by firm eliminates the need to index them by origin country.

[4] World Development indicators.

[5] UN-COMTRADE data.

[6] For instance, for Malawi we have information from 2004 onward; however, as fewer ports were covered in 2004 than in other years, we excluded 2004 from our sample for that country.

We aggregate transactions to annual (f, p, d, t) quartets, our primary unit of observation.

Table 1: Descriptive statistics

	Firms	Products	Dest.	Prod. per firm		Dest. per firm		Firms per prod.		Firms per dest.		Init. value (USD)	
				Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Mali	280	575	99	2.54	2	3.89	2	1.89	1	7.18	2	219,694	5,373
Malawi	856	932	102	1.57	1	4.10	2	3.76	1	13.19	3	106,475	571
Senegal	715	1,653	100	3.10	1	6.76	2	2.92	2	22.17	5	47,111	3,446
Tanzania	1,359	1,689	137	2.49	1	3.62	1	2.91	1	24.69	7	83,078	2,858

This table shows, for each source country and for the year 2006, the number of firms, products, destinations, products per firm, destinations per firm, firms per product, firms per destination, and initial export value for firms entering a product-destination cell.

Before turning to the empirical analysis of determinants of success and failure upon entry (next section), a few observations are in point in order to compare existing evidence on OECD and middle-income countries with ours.

Following the literature on the intensive and extensive margins (e.g. Evenett and Venables 2003 or Brenton and Newfarmer 2007), we group our primary units of observation into (i) new firms, (ii) new products, (iii) new destinations, and continuing firm-product-destinations.[7] Formally, let $v_{f,t-1}$ designate f's exports of any product to any destination in year $t-1$, $v_{fp,t-1}$ its exports of product p to any destination, $v_{fd,t-1}$ its exports of any product to destination d, and $v_{fpd,t-1}$ its exports of product p to destination d. Our four categories are

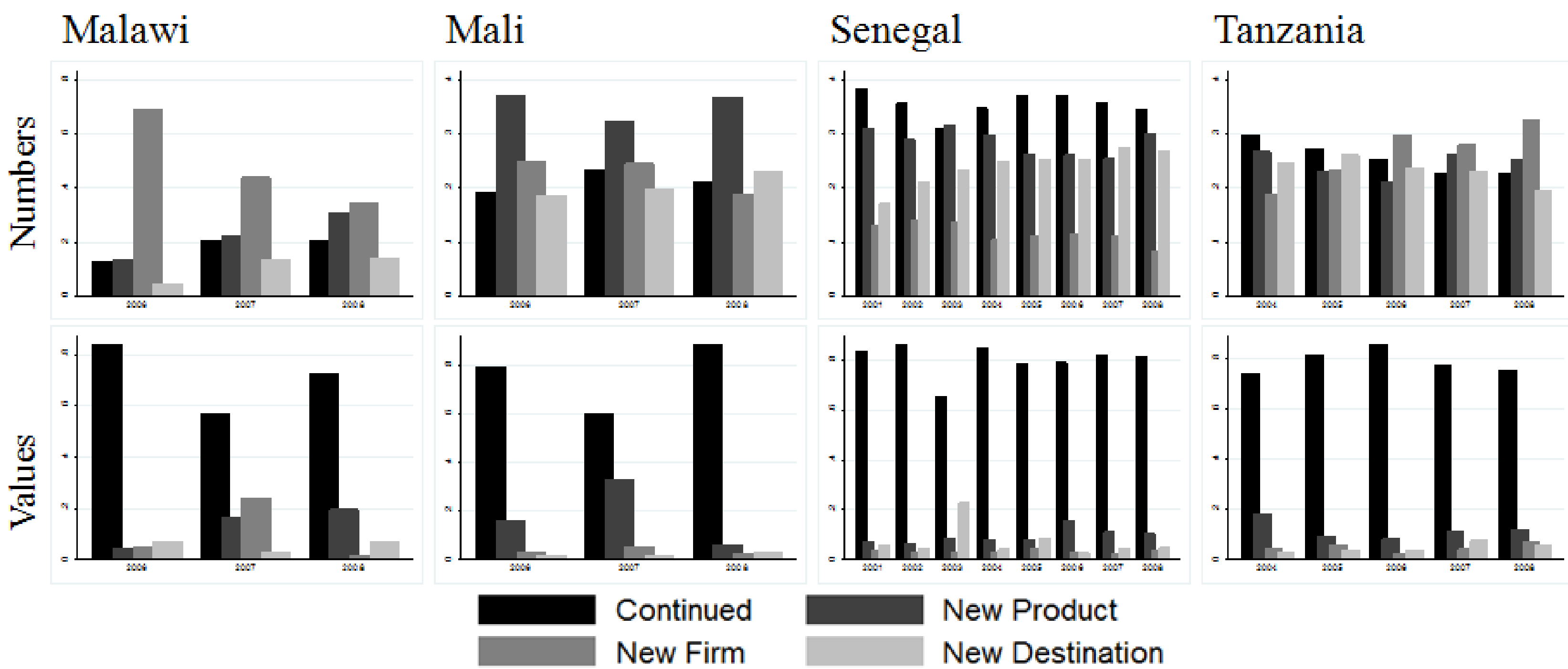
$$\begin{aligned}
\text{NF} &= \{(f,p,d,t) \text{ s.t. } v_{fpdt} > 0 \text{ and } v_{f,t-1} = 0\}, \\
\text{NP} &= \{(f,p,d,t) \text{ s.t. } v_{fpdt} > 0, v_{f,t-1} > 0 \text{ and } v_{fp,t-1} = 0\}, \\
\text{ND} &= \{(f,p,d,t) \text{ s.t. } v_{fpdt} > 0, v_{f,t-1} > 0 \text{ and } v_{fd,t-1} = 0\}, \\
\text{CPD} &= \{(f,p,d,t) \text{ s.t. } v_{fpdt} > 0 \text{ and } v_{fpd,t-1} > 0\}.
\end{aligned} \tag{1}$$

The dollar value of export sales in the first three categories can only go from zero in year $t-1$ to some positive value in year t; these variations add up to the extensive margin. Changes in the dollar value of exports in the last category form the intensive margin.

Figure 1 decomposes the exports flows into these four categories both in terms of their number, i.e. count of trade flows, and value.

[7] Observations in the sample period's initial year are considered left-censored and dropped.

Figure 1: Decomposition of exports flows



As for export values, continuing firm-product-destinations make up a relatively small number of export transactions, but a large share of export values. For instance, in Tanzania, continued firm-product-destinations accounted for 90% of export value in 2006 but only for 25% of the observation count. This suggests that our countries experiment substantially, which is consistent with the findings of Cadot, Carriere and Strauss-Kahn (2010) for low-income countries, Freund and Pierola (2010) for Peru and Iacovone and Javorcik (2010) for Mexico.

Another interesting stylized fact, consistent with the existing firm-level literature modeling exporters dynamics (Rauch and Watson 2003), is that when a firm's product manages to survive in a given destination market beyond its first year, it grows significantly over time. Conditional on survival, Senegalese firm-product-destinations that appeared in 2001 (we don't know the initial year of those appearing in 2000, the sample's initial year, because they are censored) grew by a factor of over four between 2001 and 2008. Similarly, Tanzanian firm-product-destinations that appeared in 2005 grew by a factor of over three by 2008.

Following Brooks (2006), Table 2 shows the number of firms, firm-products, and firm-product-destinations by year of entry and tracks the survival of this cohort over time for each origin country. Naturally, the numbers decrease because of exit. What is remarkable, however, is how large the attrition is in the first year and how quickly it slows down over time. For instance, in Senegal, of the 206 firms that started exporting in 2001, only 84 made it to 2002 (a death rate of 59%); however, of the 24 still around in 2007, only 3 had failed by 2008 (a death rate of "just" 13%). This finding is consistent with Besedes and Prusa's decreasing-hazard rate finding (annual death rates are discrete-time approximations to instantaneous hazard rates) although, as noted, this result must be taken cautiously. Comparing the upper panel (firms) with middle and lower ones (products and product-destinations respectively), there is less stability at more disaggregate levels.

Additionally, the fourth column (labelled "Exit") shows cumulative death rates relative to the first year. In all cases these rates are high, and above 80% in 2008 in most cases (with the only exception of Mali at the firm level). In all four countries, the very high death rates after the first year suggest that a binary coding of survival based on second-year outcomes is a good summary measure of survival.

Overall, the results presented in Figure 1 and Table 2 suggest that African firms do not shy away from trying and experimenting with products and destinations. Thus, Hausman and Rodrik's 'self-discovery' process

Table 2: Survival cohorts

	Senegal Entry:2001			Tanzania Entry:2004			Mali Entry:2005			Malawi Entry:2005		
	Nr	Y-Exit	Exit	Nr	Y-Exit	Exit	Nr	Y-Exit	Exit	Nr	Y-Exit	Exit
Firm												
2001	206											
2002	84	0.59	0.59									
2003	57	0.32	0.72									
2004	40	0.30	0.81	420								
2005	35	0.13	0.83	194	0.54	0.54	273			670		
2006	29	0.17	0.86	118	0.39	0.72	159	0.42	0.42	217	0.68	0.68
2007	24	0.17	0.88	85	0.28	0.80	123	0.23	0.55	154	0.29	0.77
2008	21	0.13	0.90	75	0.12	0.82	103	0.16	0.62	126	0.18	0.81
Product												
2001	2055											
2002	449	0.78	0.78									
2003	192	0.57	0.91									
2004	117	0.39	0.94	2656								
2005	94	0.20	0.95	497	0.81	0.81	1047			3322		
2006	78	0.17	0.96	200	0.60	0.92	305	0.71	0.71	325	0.90	0.90
2007	61	0.22	0.97	106	0.47	0.96	166	0.46	0.84	174	0.46	0.95
2008	54	0.11	0.97	71	0.33	0.97	123	0.26	0.88	127	0.27	0.96
Product destinations												
2001	3326											
2002	718	0.78	0.78									
2003	356	0.50	0.89									
2004	245	0.31	0.93	4908								
2005	167	0.32	0.95	837	0.83	0.83	1391			3828		
2006	129	0.23	0.96	295	0.65	0.94	286	0.79	0.79	509	0.87	0.87
2007	101	0.22	0.97	167	0.43	0.97	122	0.57	0.91	316	0.38	0.92
2008	84	0.17	0.97	113	0.32	0.98	82	0.33	0.94	224	0.29	0.94

Note: In the columns labelled *Nr* we document for each origin country the number of firms products and destinations in the first available year, and follow this cohort of units over time. Column *Y-Exit* shows the exit rate (ie. the share of units that left) with respect to the previous year, and Column *Exit* the exit rate with respect to the entry year.

(Hausman and Rodrik 2003) seems to hold not only at the national level, but also—quite naturally—at the firm level. This pattern is also consistent with the notion that firms face, ex-ante, uncertainty about export costs, demand parameters, and their own capacity to "match" these parameters, a notion that is central to the heterogeneous-firms literature.

4 Estimation strategy and results

4.1 Estimation strategy

After aggregating transactions to cumulated annual totals, the sample remains a panel, as each firm-product-destination (f, p, d) triplet is observed repeatedly over several years. As we are interested in success past the first year, a binary event, the data needs to undertake a second transformation. We define a *new* (f, p, d, t) spell as one that appears for the first time in the database, and say that this spell 'succeeds' if it lasts more than one year. The spell is then associated to a success dummy (our dependent variable) equal to one. If otherwise it lasts only one year, the success dummy is zero. If it has already appeared in the sample or is left-censored (i.e. already active the first year of the sample), we drop it.[8] Thus, we reduce our panel to a set of cross-sections, even though each spell has an initial-year tag allowing us to control for calendar time. Doing so allows us to bypass the issue of how long a spell break should be to be considered a 'failure', an issue that has been discussed at length in the survival literature and that has no clear-cut answer. Two additional reasons make this binary definition of survival attractive. First, our panels are too short to carry out a full-fledged survival analysis. Second, as the descriptive analysis above showed, once a firm has survived the first year, its success probability dramatically increases; so understanding success beyond the first year is especially important.[9]

After this second transformation, our sample is made of new firm-product-destination spells, which can be of two types: Those that succeed and those that do not. As already noted, firm and country indices are redundant, so we use either a country superscript c or a firm subscript f, but not both, and run our regressions on a pooled cross-country sample.[10]

[8] Multiple spells account for only a very small number of observations, since our sample periods are only a few years except for Senegal.

[9] This choice comes with both a cost and a benefit. On one hand, we lose information, as a two-year spell is treated as equivalent to a 3- or 4-year one; on the other hand, we gain robustness, as the probability of wrongly treating a two-or-more year spell as a one-year one is fairly low.

[10] We also ran, for robustness, separate regressions by origin country. The results of

Our dependent variable is

$$s_{fpdt} = \begin{cases} 1 & \text{if } v_{fpdt} > 0, v_{fpd,t-\ell} = 0 \,\forall\, \ell > 0, \text{and } v_{fpd,t+1} > 0 \\ 0 & \text{if } v_{fpdt} > 0, v_{fpd,t-\ell} = 0 \,\forall\, \ell > 0, \text{and } v_{fpd,t+1} = 0 \end{cases} \quad (2)$$

and the estimating equation is

$$\Pr(s_{fpdt} = 1) = \phi\left(\mathbf{x}_{fpdt}\beta + \delta_i + \delta_{cd} + \delta_t + u_{fpdt}\right) \quad (3)$$

where ϕ is the probit function, u_{fpdt} is an error term and δ_i, δ_{cd} and δ_t are respectively industry (HS2), origin-destination, and spell start-year fixed effects. Industry fixed effects control for heterogeneity in survival probabilities due to product/industry characteristics such as durability (for instance, low-frequency purchases of durable goods may be more prone to trade interruptions than high-frequency purchases of foodstuffs). Dyadic origin-destination fixed effects are sometimes used in gravity equations and control for bilateral characteristics such as distance which have been shown by Molina and Fugazza (2009) to affect spell duration. Calendar year fixed effects control for aggregate macroeconomic shocks.

Ideally, (3) should be estimated within-firm (i.e. with firm fixed effects) in order to control for unobserved heterogeneity. However, this would run into an incidental-parameter problem. Therefore, as a compromise, we run a number of alternative specifications relying on different estimators.

First, we run probits including the fixed effects included on the RHS of (3). Second, we run a linear probability model (LPM) including the same set of dummies and controls appearing on the RHS of (3) in order to verify that coefficients are similar to probit marginal effects; then we run a different version of the LPM including *firm* fixed effects, product fixed effects at a higher level of disaggregation (HS6), and product-origin effects to control for supply shocks. Finally, in order to go beyond first-period survival, we run Cox and random-effect probit regressions on the Senegal sub-sample, for which we have the longest time series.[11] In all our specifications, we always use robust standard errors clustered at the product-destination level.[12]

these regressions are available upon request. They are qualitatively similar to those of cross-country (pooled) regressions reported here.

[11]We are grateful to both referees and the editor for helping us to clarify estimation issues.

[12]Clustering at the product-destination level takes care of correlated demand shocks affecting all firms in a product-destination cell. Clustering at the product-origin level (to take care of supply shocks) gives similar results.

The vector of regressors $\mathbf{x}_{fpdt}$ includes measures of the firm's scale and scope as well as proxies for agglomeration and market attractiveness. They are (i) n^c_{pdt}, the number of firms from origin country c exporting product p to destination d; (ii) n_{fpt}, the number of destinations to which firm f exports product p; (iii) n_{fdt}, the number of products that firm f exports to destination d; (iv) n^c_{dt}, the number of (product $\times$ firm) combinations active in the bilateral trade between origin c and destination d; they also include (v) v_{fpdt}, the initial value of firm f's export spell (product p to destination d); and (vi) z_{fp}, the share of product p in firm f's overall export sales. That is, the notation convention is to omit the index of the dimension over which the count is summed. All counts are in logs.[13]

In customs data, E.U. countries are entered as separate destinations rather than as a whole. We have kept this convention, so a destination should be taken, as far as the E.U. is concerned, as a member state. This creates a size asymmetry between the U.S., which is taken as a whole, and the E.U., which is broken down, but as African exports tend to be heavily concentrated on E.U. markets, the alternative assumption (bundling all E.U. destinations together) would have drastically reduced the number of destinations and potentially obfuscated some geographical diversification issues, as marketing channels are, in spite of the Single Market, still somewhat separate across E.U. member states. We verify that our main coefficients of interest remain the same once we pool E.U. Member states into a single destination.[14]

We estimate equation (3) by probit, reporting marginal effects evaluated at the mean. Typically, probit marginal effects can be interpreted like coefficients in a linear model; in the present case, for all results reported below, we verified that linear probability estimates were close to probit marginal effects at the mean. For expositional reasons, we differ the presentation of LPM estimates to Table 4.

4.2 Baseline results

Baseline regression results are shown in Table 3. Before turning to their detailed interpretation, it is important to stress that the effects to be discussed are simultaneously present in each regression and so are conditional on each other. Also, it is important to note that, by construction, these must be interpreted as conditional on starting to export, since our sample is made

[13] These counts include the observation they are attached to and are hence never zero, so we lose no observation by taking logs.

[14] The only coefficient that changes in a statistically significant way is the coefficient on n^c_{dt} which becomes positive, with a magnitude of 0.017.

of new spells. We discuss selection issues in Section 4.3.

The first column presents baseline results. The second differs from the first in that all right-hand side (RHS) variables are lagged by one year. The third includes the share of product p in firm f's export portfolio as an additional control. The fourth adds the origin country c's revealed comparative advantage (RCA) in product p, and the fifth runs a counterfactual experiment discussed below.

Table 3: Determinants of success past the first year

Estimator: Probit Dep. var.: First-year success Regressors (log)		(1)	(2)	(3)	(4)	(5)	(6)
n^{c}_{pdt}	Firm count	0.0566***	0.0431***	0.0544***	0.0563***		0.0661***
		(0.00283)	(0.00306)	(0.00282)	(0.00285)		(0.00685)
n^{-c}_{pdt}	Firm count					0.00449	
						(0.00727)	
n_{fpt}	Dest. count	0.125***	0.0820***	0.125***	0.125***	0.116***	0.125***
		(0.00270)	(0.00296)	(0.00269)	(0.0027)	(0.00397)	(0.00271)
n_{fdt}	Prod. count	0.0375***	0.0224***	0.0478***	0.0375***	0.0301***	376***
		(0.00163)	(0.00152)	(0.00184)	(0.00163)	(0.00218)	(0.00163)
v_{fpdt}	Init. value	0.0304***	0.0332***	0.0277***	0.0304***	0.0335***	0.0303***
		(0.000898)	(0.000889)	(0.000921)	(0.000898)	(0.00125)	(0.000898)
n^{c}_{dt}	Prod. × firm	-0.00477	-0.0213***	-0.00723	-0.00472	-0.00131	-0.00489
		(0.00594)	(0.00397)	(0.00595)	(0.00594)	(0.0084)	(0.00593)
z_{fp}	Prod. share			0.0771***			
				(0.00640)			
RCA_{cp}					<0.0001	<0.0001	
					(<0.0001)	(<0.0001)	
$\left(n^{c}_{pdt}\right)^2$							-0.00404
							(0.00293)
RHS vars lagged		No	Yes	No	No	No	No
HS2 FE		Yes	Yes	Yes	Yes	Yes	Yes
Origin-dest. FE		Yes	Yes	Yes	Yes	Yes	Yes
Time effects		Yes	Yes	Yes	Yes	Yes	Yes
Obs.		57,063	57,063	57,063	57,063	25,294	57,063

Note: Probit marginal effects reported. Robust standard errors clustered by product-destination.

Consider first the results in Column (1). The first regressor of interest is

$\ln n^c_{pdt}$, the log of the number of other firms selling the same product (p) in the same destination (d). That is, if observation (f, p, d, t) is mens' t-shirts sold in France by a Senegalese firm in 2006, $\ln n^c_{pdt}$ is the log of one plus the number of other Senegalese firms exporting mens' t-shirts in France in 2006. The effect is positive and significant at the 1% level in all specifications. That is, more companies from the same country selling the same product in the same destination together raise each other's survival probability. This is a striking network effect, to which we will come back at some length later on. How large is the effect? Let us write the probability of survival as $\pi_{fpdt} = \Pr(s_{fpdt} = 1)$. Recalling that the coefficients reported in Table 3 are marginal effects, using the point estimate of of 0.0566 in the first cell of Column (1), and the average number of Senegalese firms selling to each destination ($n^{SEN}_{pdt} = 22$) we can write

$$\begin{aligned} \Delta\pi_{fpdt} &= 0.0566\,[\ln(n^c_{pdt} + 1) - \ln(n^c_{pdt})] \\ &= 0.0566\,[\ln(23) - \ln(22)] \\ &= 0.0025. \end{aligned} \tag{4}$$

Using the illustrative success rates in Table 2, a Senegalese firm entering in 2001 has a probability of succeeding at the product-destination level of 0.22 (22 percent). We take this number as our baseline survival probability, π_{fpdt}. While a single additional national firm in a given product-destination cell would raise it by a negligible amount, doubling the number of firms, from the baseline of 22 to 44, would raise the first-year success probability by 3.9 percentage points, from 0.22 to 0.26 (a proportional increase of 18%, nearly a fifth). Interestingly, while the LPM (OLS) estimate (0.0567) is very close to the probit marginal effect evaluated at the mean, evaluating probit marginal effects at the median rather than at the mean returns substantially larger effects; the marginal effect of n^c_{pdt} at the median is 0.39 instead of 0.056, which means that doubling the number of Senegalese sellers on a product-destination cell now raises the first-year survival probability by 27 percentage points, from 0.22 to 0.49.[15]

The second regressor in Column (1) is $\ln n_{fpt}$, the count of destinations to which product p is exported by firm f, a proxy for the scope of geographical diversification and likely correlated with the firm's production capabilities. The geographical scope significantly raises the probability of success in all specifications. This may reflect either more robust production lines (say, a

[15]We are grateful to referees for attracting our attention to this issue.

larger number of machines, meaning that failure of one of them is more easily made up by others), better information about the cross-country drivers of a product's demand, or, alternatively, higher product quality. How large is the effect? Using a calculation similar to that in (4), an additional destination[16] raises the probability of success by

$$\Delta\pi_{fpdt} = 0.125\,[\ln(2.55) - \ln(1.55)] = 0.062.$$

That is, the baseline first-year probability of success goes up by 6.2 percentage points, from 0.22 to 0.28, when the mean Senegalese exporter adds one destination to his portfolio at the product level. Were he to double the number of destinations for that product, the first-year success probability would rise by 8.7 percentage points, from 0.22 to 0.31 (a proportional rise of nearly two fifths).

The next regressor, $\ln n_{fdt}$, is the log of the number of products that firm f exports to destination d, a proxy for its 'product scope' in that destination. The effect is, again, positive and significant. As for its magnitude, if our Senegalese firm adds one product to its average destination d, from a baseline of 3.48 products,[17] the usual calculation gives a rise of just one percentage point in the success probability. With a doubling of the number of products, the success probability rises by 2.5 percentage points, from 0.22 to 0.245, a proportional rise of 11%.

Thus, adding one product to a given destination has a smaller effect on exports success (one percentage point) than adding an additional destination for that product (1.7 percentage point). This is somewhat natural, as our analysis is at a disaggregated level in terms of products (5,000 potential products at HS6, although our African countries export far fewer), so the additional product sold on destination d can be very close to the original; by contrast, destination countries are much fewer, so adding one more shipping destination for product p is a substantial move (although it may involve adding one E.U. member state which would mean expanding within the Single Market). An alternative explanation goes as follows. Increasing either scope or size raises the firm's visibility and therefore has a positive demand effect. However, there may be supply effects running at cross-purposes. When a firm adds one export destination to a given line of

[16] The average number of destinations per product, for a Senegalese firm, is 1.55. This is lower than the number appearing in Table 1 which is the total number of destinations per firm, not per firm-product.

[17] Again, this number differs from the one appearing in Table 1, which is the total number of products per firm, not per firm-destination.

products, it expands production, potentially making the value chain more robust to accidental fluctuations. By contrast, when it adds one product to a destination, the firm spreads managerial attention and quality-control capabilities over a wider range of activities, potentially resulting in more incidents. In that case, the supply effect runs against the demand effect, resulting in a lower net change in chances of success.

The next regressor is a control for the export spell's initial value, v_{fpdt}, which has been shown in the literature to correlate with survival at the product-country level. This is confirmed at the firm level, although the effect is, again, small. Using the coefficient in Table 3 (0.0304), a doubling of the initial value of the Senegalese firm's average export trial ($47'111 from Table 1) would raise the probability of success by 0.021, or 2 percentage points, from 0.22 to 0.241.

The last regressor, $\ln n^c_{dt}$, is a count of the total firm-product pairs from country c active on destination d. If c is Senegal and one Senegalese firm sells two HS6 products in Italy and another one sells three, $n^c_{dt} = 5$ for all five observations with c = Senegal and d = Italy. It is a proxy for the size of the bilateral trade relationship. This variable is never significant except in Column (2).

Column (2) of Table 3 is very similar to Column (1) except that all the explanatory variables are lagged by one period. Results are essentially unchanged, except for n^c_{dt} whose, still negative, becomes significant. What that means is that more firm-product combinations from a given origin to a given destination (as opposed to more firms for a given product, origin and destination, our synergy effect) are associated with a lower probability of survival next year . Without making too much of this result, one can interpret it as follows. Given that we include origin-destination fixed effects, n^c_{dt} picks up only the time-variant component of bilateral shocks, like booms in the destination market (again, we are considering here the *aggregate* destination market, whereas our externalities effect is at the product-destination level). The negative coefficient suggests that a growth expansion (a boom) in $t-1$ triggers crowding in followed by retrenchment.[18]

Column (3) introduces an additional regressor. The literature on mul-

[18]Confirming this interpretation, when we exclude the destination fixed effects, the coefficient on this variable becomes positive, suggesting that *permanently* more attractive markets are associated with longer survival, which is consistent with our interpretation. This "crowding-in" result is also consistent with Bussolo, Iacovone, and Molina (2010) who found, using firm-level data from the Dominican Republic, that the reduction of tariffs following the signature of CAFTA led to some over-crowding of Dominican exports, followed by retrenchment.

tiproduct firms suggests that within firms there is product heterogeneity, and firms have stronger capacities at exporting 'core' products (see for instance Eckel and Neary 2010 for a theoretical model and Iacovone, Rauch and Winters 2010 or Eckel, Iacovone, Javorcik and Neary 2010 for empirical tests of this hypothesis). For each multiproduct firm f and product p, we proxy how close is that product from the firm's 'core' by $\ln z_{fp}$, the log of its share in the firm's total export sales. Results suggest that it correlates positively with first-year success probability even after controling for dollar initial value; that is, the probability of success for 'core' products is substantially higher than for others. For instance, a product representing 80% of the firm's export sales (all destinations together) would have a success probability on a given destination higher by 10 percentage points than a product representing 20% of the firm's export sales.

In Column (4) of Table 3, we control for a potential omitted variable that could bias our results if country c had a comparative advantage in product p, explaining both that it had more exporters of that product (in destination d or elsewhere) *and* that product p had a better survival outlook. As a control for this, we use the initial (sample-start) value of Balassa's revealed comparative advantage (RCA) index defined, for product p, as

$$RCA_p^c = \frac{v_p^c / \sum_p v_p^c}{v_p^w / \sum_p v_p^w} \tag{5}$$

where v_{pc} stands for country c's exports of product p and x_{pw} for world exports of that good. We compute it at HS6 from mean exports for 1999, 2000 and 2001. Results are robust to the inclusion of this control.

Column (5) provides a key test of whether our externality effect is spurrious or not by replacing it with a 'placebo'. We replace $\ln n_{pdt}^c$, the number of firms exporting the same product to the same destination from the same country, by $\ln n_{pdt}^{-c}$, the number of firms exporting the same product to the same destination from *other* countries in the sample. For instance, consider an export spell of boys' swimwear (HS611239) to Germany by a Senegalese firm. On the right-hand side of the equation, instead of the number of other Senegalese firms exporting HS611239 to Germany, we will now use the number of firms exporting HS611239 to Germany from Tanzania, Malawi and Mali. This variable may be positive or zero. It may also be missing, as our national samples have some non-overlapping years, so the sample size is substantially lower. It should also be kept in mind that the placebo we are using is neither random nor "matched", being dictated by data availability. Whereas the externality effect comes out very strongly in all specifications, whether pooled across countries (as reported in Table 3) or run separately by country, the placebo effect is never significant.

Column (6) includes the square of n^c_{pdt} to account for a possible non-linearity. One could argue that when n^c_{pdt} is small, an additional exporter from the origin country in that product-destination cell would be beneficial to new exporters due to positive information spillovers. However, as n^c_{pdt} gets large, the competition effect might dominate, and the marginal effect on the survival probability may reverse. A square term would help to detect such a non-linearity. As is clear from the table, the square term is not significant and barely changes the magnitudes and significance levels of other terms.

4.3 Robustness

Two types of selection problems can affect our results: (i) selection of firms into exporting, and (ii) selection of exporting firms into particular product-destination cells. As customs data covers only exporters, we cannot address the first one in our regressions. Thus, our results on the determinants of survival on export markets are valid for currently active exporters and should not be expected to carry over to current non-exporters should they start exporting.

As for the second problem, intuitively one could think of it the following way. Suppose that a sustainable market opportunity suddenly appears in a product-destination cell, say mangoes in Italy. Exporters crowd in, and at the same time success probability improves (because the opportunity is assumed sustainable). Thus, more firms and higher success probability on that particular cell are biased by a selection problem driven by an omitted variable, the demand shock in the product-destination cell. We control for this using a Heckman selection model at the (product $\times$ destination $\times$ year) level. That is, whereas in (2) we restrict the sample to (p, d, t) cells in which firm f starts exporting at t, here we extend it to other cells as well and use an auxiliary equation to predict the probability that cell (p, d, t) is served by a firm (any firm) in our sample, of the form

$$\Pr(v_{fpdt} > 0 | \, v_{fpd,t-1} = 0) = \psi \left(\mathbf{x}_{fpdt}\beta + \delta_i + \delta_{cd} + \delta_t + \gamma v^{row}_{pdt} + u_{fpdt} \right) \tag{6}$$

where v^{row}_{pdt} is the log of rest-of-the-world exports (i.e. world minus Malawi, Mali, Senegal and Tanzania) of product p to destination d a time t, using COMTRADE data. As is well known, the probit's nonlinearity is sufficient for identification in a Heckman two-stage estimator, but it is improved with an excluded variable. The idea here is that if there is a market opportunity on a given product-destination cell, it is likely to have been observed by exporters from other countries with better information than those in our sample of African exporters, and will therefore be reflected in a rise in aggregate trade flows into that particular cell.[19] The inverse Mills ratio retrieved from that equation is then included as an additional regressor in the second-stage equation to correct for selection bias.

Table 4 shows our main regression in linear probability (column 1) and its robustness using the Heckman correction (columns 2-4). As discussed earlier, OLS (LPM) estimates in the first column are very close to probit estimates at the mean (column 1 of Table 3). They provide a benchmark for second-stage Heckman estimates which are also obtained by OLS. Column 2 shows that the selection correction, while significant (the inverse Mills ratio is significant in columns 2-4), does not alter the sign or level of significance of our baseline results. Although this may no doubt not be the last word on it, results in Table 4 do not suggest that selection into specific export cells is likely to be the main driving force behind our results.

In Table 5, we test the robustness of the first three columns of Table 3 to alternative specifications. In Columns (1)-(3) we reproduce the first three columns of Table 3, but drop observations where any of the four count variables n^c_{pdt}, n^c_{fpt}, n^c_{fdt} or n^c_{dt} is equal to zero, ie. when the observed firm-product-destination is the only one from its country of origin. This specification addresses the concern that the variation we find on any of these counts lives from the difference between one and more firms only. These columns show that the results are unchanged when we only consider variation in the group of counts larger than one.

In Columns (4)-(6) we reproduce the first three columns from Table 3, but replace our original variable capturing the externality effect $\ln n^c_{pdt}$ by n^c_{pdt}, thus using the count rather than its log transformation. The results are robust to this check too.

[19] One might object that an alternative scenario is that, say, Senegalese producers of mangoes are suddenly "discovered" by a buyer in a destination market who offers multi-year contracts to several of them simultaneously, thus raising both numbers and chances of success. This would not necessarily concern other suppliers and would therefore not be picked up by COMTRADE export flows. This scenario, however, is not terribly plausible for several reasons. First, supermarket buyers initially test suppliers and rarely–if ever–offer multi-year contracts. Second, as the supermarket buyer brings more suppliers into the supply chain, in thin markets like our African ones, he is bound to go down the quality ladder, with adverse reputation effects on the whole cluster of suppliers. One would therefore expect *lower* survival to be associated with larger numbers of suppliers from the same country. Incidentally, this quality-ladder issue also plays against our results, suggesting that our estimate of the externality effect is a lower bound; that is, it could be higher if all suppliers were of the same quality.

Table 4: Selection

Estimator		OLS	Heckman	Heckman	Heckman
Dep. var.: First-year survival					
		(1)	(2)	(3)	(4)
n^c_{pdt}	Firm count	0.0567***	0.0633***	0.0699***	
		(0.0031)	(0.002)	(0.00199)	
n^c_{fpt}	Dest. count	0.142***	0.120***		
		(0.003)	(0.00178)		
n^c_{fdt}	Prod. count	0.0361***	0.0205***		0.0206***
		(0.002)	(0.00107)		(0.00104)
n^c_{dt}	Firm-prod. count	-0.004	0.0202***		
		(0.006)	(0.000917)		
v_{fpdt}	Initial value	0.0269***	0.0217***	0.0259***	0.0304***
		(0.001)	(0.000487)	(0.00047)	(0.000488)
λ	Inverse Mills ratio		-2.294***	-2.294***	-2.294***
			(0.00357)	(0.00357)	(0.00357)
Obs.		57'063	2,581,215	2,581,215	2,581,215

Finally, in Columns (7)-(9) we replace HS2 fixed effects by HS-6 ones. Because of the large number of dummies, for computational reasons we use a linear probability model rather than a probit, taking advantage of the similarity between the two already noted. Coefficients remain similar in sign and significance.

Table 6 provides a large number of additional robustness checks addressing various concerns. In Column (1), we add firm fixed effects to the baseline specification to control for unobserved firm heterogeneity at the firm level (productivity or managerial ability). In Column (2), we add origin-product fixed effects to control for comparative advantage in a different way than just by the inclusion of the RCA variable. Given computational problems, both specifications are run in linear probability instead of probit. Significance, signs and magnitudes of coefficients remain similar to the baseline specification, i.e. Column (1) of Table 3.

In Column (3), we add as an additional regressor the count of firms exporting product p to any destination, n^c_{pt}. This variable controls for product-related spillovers or unobservables independent of destination market.

Table 5: Robustness of main results

Estimator	Probit	Probit	Probit	Probit	Probit	Probit	OLS	OLS	OLS
Dep. var.	Survival	Survival	Survival	Survival	Survival	Survival	Survival	Survival	Survival
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
n^c_{pdt}	0.0805***	0.0382***	0.0751***	0.00564***	0.0431***	0.00522***	0.0476***	0.0313***	0.0467***
	(0.0145)	(0.0112)	(0.0144)	(0.00107)	(0.00306)	(0.00106)	(0.00350)	(0.00389)	(0.00349)
n^c_{fpt}	0.160***	0.0326***	0.151***	0.127***	0.0820***	0.126***	0.129***	0.0799***	0.129***
	(0.0134)	(0.00992)	(0.0135)	(0.00271)	(0.00296)	(0.00271)	(0.00343)	(0.00390)	(0.00342)
n^c_{fdt}	0.0622***	0.0295***	0.0814***	0.0341***	0.0223***	0.0448***	0.0387***	0.0226***	0.0498***
	(0.00830)	(0.00676)	(0.00864)	(0.00163)	(0.00152)	(0.00184)	(0.00172)	(0.00172)	(0.00192)
n^c_{dt}	-0.0633	-0.0371	-0.0699*	0.00193	-0.0212***	-0.000650	-0.00228	-0.0169***	-0.00501
	(0.0409)	(0.0235)	(0.0407)	(0.00599)	(0.00397)	(0.00598)	(0.00617)	(0.00401)	(0.00616)
v_{fpdt}	0.0521***	0.0559***	0.0431***	0.0313***	0.0332***	0.0285***	0.0304***	0.0320***	0.0277***
	(0.00387)	(0.00373)	(0.00414)	(0.000913)	(0.000889)	(0.000933)	(0.000882)	(0.000882)	(0.000903)
z_{fp}			0.207***			0.0794***			0.0815***
			(0.0306)			(0.00641)			(0.00640)
RHS lag	No	Yes	No	No	Yes	No	No	Yes	No
HS2 FE	Yes	Yes	Yes	Yes	Yes	Yes			
HS6 FE							Yes	Yes	Yes
Orig-dest FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	6,031	6,031	6,031	57,063	57,063	57,063	58,382	58,382	58,382

Note: Marginal effects for probit. Robust standard errors are clustered by product-destination.

Table 6: Additional robustness results

Estimator	OLS	OLS	OLS	OLS	Probit	Probit	Probit	Cox	RE probit
Dep. var.	Survival	Survival	Survival	Survival	Survival	Survival	Survival	Hazard rate	Death
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
n^c_{pdt}	0.0772***	0.0816***	0.0637***	0.0131***	0.0524***			-0.034***	-0.0727***
	(0.00334)	(0.0102)	(0.00393)	(0.00114)	(0.00408)			(0.0107)	(0.008)
n^c_{fpt}	0.129***	-0.171***	0.146***	0.0463***	0.131***	0.0991***	0.128***	-0.160***	-0.211***
	(0.00387)	(0.0167)	(0.00328)	(0.00146)	(0.00352)	(0.0059)	(0.00273)	(0.009)	(0.007)
n^c_{fdt}	0.0399***	0.0936***	0.0357***	0.0182***	0.0344***	0.0281***	0.027***	-0.039***	-0.089***
	(0.00275)	(0.00755)	(0.00160)	(0.000811)	(0.00207)	(0.00346)	(0.00167)	(0.006)	(0.0045)
v_{fpdt}	0.0284***	0.0397***	0.0269***	-0.0327***	0.0303***	0.00303***	0.0313***	-0.0356***	-0.0615***
	(0.000988)	(0.00325)	(0.000813)	(0.00193)	(0.00117)	(0.0018)	(0.0009)	(0.0031)	(0.0025)
n^c_{dt}	-0.00728	-0.0227	-0.00502	0.00311	-0.0186*	0.0106		-0.0376*	-0.039***
	(0.00687)	(0.0166)	(0.00590)	(0.00267)	(0.00747)	(0.0149)		(0.02)	(0.004)
n^c_{pt}			-0.00678***						
			(0.00197)						
$n^{-c\ neighbor}_{pdt}$						0.0181**			
						(0.0072)			
$n^{-c\ non\ neighbor}_{pdt}$						-0.0093			
						(0.0066)			
$n^{c\ restricted}_{dt}$							-0.00243		
							(0.00545)		
$n^{c\ restricted}_{pdt}$							0.0558***		
							(0.00403)		
Observations	58,382	58,382	58,382	58,382	31,762	10,196	57,063	28,666	65,656
HS2 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Orig-dest FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	No	No	No	No	No	No	No	No
Origin-prod FE	No	Yes	No	No	No	No	No	No	No
Measure				Values					
Subsample					Manufacturing			Senegal	Senegal

Note: Marginal effects for probit. Robust standard errors are clustered by product-destination.

In Column (4), we replace all count variables on the right hand side by aggregate export *values*. For instance, instead of counting the number of firms exporting product p to destination d from country c in year t, we aggregate trade values at the (p, c, d, t) level . Point estimates cannot be compared between counts and values, but we expect signs and significance to be similar. They are, with one exception: The coefficient on initial export value switches sign. As initial value enters the computation of all other variables in this specification, its sign is now harder to interpret. Results remain very similar when we omit this variable.

In Column (5), we restrict estimation to manufacturing industries.[20] All coefficients remain very similar in terms of sign, magnitude and statistical significance when we reduce the sample to these industries.

In Column (6), we split the cross-country spillover effect (what we earlier called the "placebo") into two separate variables. One counts firms exporting the same product to the same destination from a *neighboring* country (Mali for Senegal and vice versa, Malawi for Tanzania and vice versa). The other counts firms exporting the same product to the same destination from a *non-neighbor* country (say, Malawi or Tanzania for Senegal). This allows us to assess whether spillovers have a geographical dimension. Indeed, we find limited evidence (significant at 10%) of cross-country spillovers between neighboring countries, and no evidence for spillovers between non-neighbor ones.[21]

In Column (7), we run a specification where count variables relating to cross-firm information flows at t are restricted to firms that were active in the relevant domain at $t-1$. Specifically, we recompute the spillover effect n_{pcdt} counting only firms that already exported the same product to the same destination at $t-1$, and the count of firm-product combinations on a destination market n_{cdt} using only firms that already exported something to that destination at $t-1$. This conditions informational spillovers on survival beyond the first year, i.e. imposes the assumption that the survival of firm f on cell (p, d) at time t is enhanced only by the presence of other firms that successfully passed the 'first-year hurdle'. Coefficients, statistical significance and signs of coefficients do not change.

In Column (8), we go beyond first-year survival by estimating a Cox proportional-hazard model using only Senegal, for which we have the longest time series. The hazard rate is decreasing in the spillover effect, which is

[20] We use a coarse definition of manufactured products at the chapter (HS2) level that includes chapters 16, 19 to 23, and 28 onward with the exception of 41 and 50-53.

[21] The inclusion of variable n^{c}_{pdt} does not alter the significance levels and magnitudes of the reported coefficients in a meaningful way.

consistent with an increasing probability of first-year survival in the probit. Given the recent literature's reservations on the use of Cox regressions in a discrete-time trade context, in Column (9) we estimate a panel random-effects probit model (also for Senegal). As in Column (8), the coefficients' sign is the opposite of the baseline, as the event is now "death at t" rather than 'survival in the first year'. Results are qualitatively the same.

4.4 Interpreting the synergy effect

4.4.1 Extended networks and "institutional production capabilities"

We now turn to possible interpretations of our externality effect. In the light of recent work by Hidalgo et al. (2007), we explore whether "institutional capabilities" at the country-sector level could be an omitted variable driving our results. The inclusion of a revealed-comparative index in Table 3 was already meant to control for this kind of effects, but the issue deserves a bit more scrutiny. Extended networks may pick up the effect of comparative advantage, infrastructure, and intermediation channels in a more robust way than Balassa indices calculated at the HS6 level.

Here, we explore if the externality effect carries over to extended networks of exporters of 'similar' products, and if yes how narrow (HS6) and broad (HS4) networks interact in their effect on exports success.

We define a new variable, EN (for 'extended network'), equal to the number of firms from country c exporting to destination d products other than p but in the same HS4 heading. Table 7 reports results with EN as an additional regressor. The new variable has a positive and significant effect on exports success, but it does not affect the significance or magnitude of our externality effect. In Column (2), we interact this variable with n^c_{pdt}, the one capturing the externality effect. Again, the externality effect itself remains positive and significant, but the coefficient on the interaction term is negative. What this means is that with more similar products (same HS4 but different HS6) sold on destination d, firm f is less sensitive to the narrow network of firms selling the exact same product (at HS6). That is, networks of identical and similar products are somewhat substitutes.

As an alternative, in Column (3) we define a new variable, WEN (for 'weighted extended network'), equal to the weighted sum of the number of firms exporting to destination d products $p^\star$ other than p, each of them weighted by the distance between p and $p^\star$ in the sense of Hausmann and

Klinger (2006).[22] Introduced linearly in Column (3), this alternative regressor has no significant effect on the probability of success and leaves the externality effect unchanged. However, when introduced both linearly and in interaction with the externality effect in Column (4), it turns significant, with a positive effect on the success probability and a negative interaction effect, suggesting again substitutability between narrow and extended networks—however what matters most for us is that the sign and size of the externality effect are unaffected.

4.4.2 Information and access to finance

We now turn to an exploration of the mechanisms that could explain our results, focusing on information spillovers. First, we explore if this effect could indicate the presence of information externalities directly from existing exporters. For instance, when technical regulations or buyer policies change in the destination market, exporters may share information about upcoming changes, improving their ability to anticipate and adapt to these changes. Alternatively, buyers may take suppliers from a given country more seriously (and therefore share more information with them or show more flexibility in the face of glitches) when there is a critical mass of them. The existence of exporters may also produce information for new exporters about the specific demands requirements that consumers have in that market. If this conjecture is correct, we should expect a stronger externality effect for products characterized by higher quality heterogeneity, for which information asymmetries between buyers and producers are potentially more important. We proxy product p's quality heterogeneity by ρ_p, the coefficient of variation of its FOB unit value across exporters in 2000 (the initial value in our sample) using COMTRADE data, with a higher ρ_p indicating more heterogeneous quality. Results are presented in Column (1) of Table 8. The coefficient on the interaction term $\rho_p \times \ln n^c_{pdt}$ is positive, although significant only at the 10% level, confirming that the importance of existing exporters supplying the same market for the success of new exporter is indeed stronger for products characterized by higher quality heterogeneity, where information is more important.

[22] Hausmann and Klinger's measure of proximity is essentially a measure of the probability that two goods are exported simultaneously by a country. We compute this measure in a sample that includes total exports from each country in the year 2000. We count products as being exported if their revealed comparative advantage measure is not smaller than one. For two products p_{i1} and p_{i2} we compute the probability that p_{i1} is exported given that p_{i2} is exported, and the probability that p_{i2} is exported given that p_{i1} is exported. The minimum of these two probabilities is the measure we employ.

Table 7: Extended networks

Estimator: Probit Dep. var.: First-year survival	(1)	(2)	(3)	(4)
n^c_{pdt}	0.0551***	0.0699***	0.0607***	0.0709***
	(0.00364)	(0.00624)	(0.00300)	(0.00362)
n_{fpt}	0.151***	0.151***	0.132***	0.132***
	(0.00374)	(0.00374)	(0.00289)	(0.00289)
n_{fdt}	0.0322***	0.0321***	0.0305***	0.0304***
	(0.00112)	(0.00112)	(0.000916)	(0.000915)
v_{fpdt}	0.0414***	0.0413***	0.0375***	0.0376***
	(0.00199)	(0.00198)	(0.00166)	(0.00165)
n^c_{dt}	-0.0170**	-0.0178**	0.00101	0.00175
	(0.00860)	(0.00858)	(0.00585)	(0.00585)
EN	0.0113***	0.0170***		
	(0.00285)	(0.00330)		
EN $\times n_{pdt}$		-0.00805***		
		(0.00284)		
WEN			0.00466	0.0179***
			(0.00419)	(0.00483)
WEN $\times n_{pdt}$				-0.0241***
				(0.00553)
Origin-dest. FE	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes
HS2 FE	Yes	Yes	Yes	Yes
Obs.	38451	38451	52212	52212

Note: Marginal effects reported. Robust standard errors clustered by product-destination.
EN: Extended network. WEN: Weighted extended network.

Table 8: Interpreting the externality effect

Estimator: Probit Dep. var.: First-year survival	(1)	(2)	(3)	(4)	(5)
n^c_{pdt}	0.0552***	0.0512***	0.0932***	0.0455***	0.0816***
	(0.00437)	(0.00509)	(0.0116)	(0.00591)	(0.0124)
n_{fpt}	0.132***	0.132***	0.138***	0.132***	0.137***
	(0.00290)	(0.00304)	(0.00356)	(0.00305)	(0.00358)
n_{fdt}	0.0375***	0.0377***	0.0369***	0.0378***	0.0370***
	(0.00166)	(0.00178)	(0.00198)	(0.00179)	(0.00199)
v_{fpdt}	0.0305***	0.0307***	0.0290***	0.0307***	0.0291***
	(0.000916)	(0.000949)	(0.00108)	(0.000948)	(0.00108)
n^c_{dt}	0.00110	0.00131	-0.00275	0.00142	-0.00234
	(0.00585)	(0.00608)	(0.00662)	(0.00608)	(0.00662)
ρ_p (unit-value dispersion)	-0.00610			-0.00641	-0.0194***
	(0.00500)			(0.00511)	(0.00578)
$\rho_p \times n^c_{pdt}$	0.00954*			0.00971	0.0139**
	(0.00561)			(0.00594)	(0.00669)
r_p (finance dependence)		0.0114*		0.0115*	
		(0.00665)		(0.00665)	
$r_p \times n^c_{pdt}$		0.0140**		0.0141**	
		(0.00601)		(0.00599)	
κ (asset tangibility)			0.168***		0.174***
			(0.0466)		(0.0466)
$\kappa_p \times n^c_{pdt}$			-0.115***		-0.107***
			(0.0354)		(0.0353)
Origin-dest. FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
HS2 FE	Yes	Yes	Yes	Yes	Yes
Obs.	52,212	52,212	37,838	52,212	37,838

Note: Probit marginal effects reported. Robust standard errors are clustered by product-destination.

Given the importance of finance, the externality effect could be operating through the the banking system. Consider the following scenario. A Senegalese firm is approached by a US buyer to provide a small trial order of t-shirts. Upon successful delivery and sale, the buyer is happy and contacts again the Senegalese firm for a larger order. Now the Senegalese firm has to ramp up capacity and, for that, it needs credit. But banks in Africa are conservative, and often discount letters of credit from even the most reputable buyers as collateral. Anecdotal evidence suggests that, among reasons for this conservatism—including lack of competition and limited capabilities—banks expect all sorts of problems in terms of quality, timely delivery etc. that could be used by buyers to deny full payment. If the bank says 'no', the trade relationship with the US buyer will terminate before it had a chance to bear fruit. However, if several Senegalese firms already sell t-shirts on the US market, some of them possibly clients of the same bank, it will be better placed to evaluate profitability and more easily convinced about chances of success, triggering a virtuous circle of support and success at the level of the firm. Under this scenario, based on anecdotal evidence, the externality effect should be stronger for products that depend more on external finance. In order to test this conjecture, we interact our externality variable with the measure of external-finance dependence proposed by Rajan and Zingales (1998).[23] We construct our r_p variable at the product level by using concordance tables between ISIC3 and HS6 classification, and assigning to each HS product the Rajan-Zingales index of the ISIC code to which that product belong. Column (2) of Table 8 shows that the interaction term $r_p \times \ln n_{pdt}$ is positive and significant. Thus, as expected, the externality effect is stronger in finance-dependent sectors.

As an alternative to measure the degree of dependence from finance, we use a proxy for 'asset tangibility' proposed by Braun (2003).[24] The idea is that firms with more tangible assets present lower risks because tangible assets provide real collateral. As before, we construct the asset-tangibility

[23] Rajan and Zingales' measure of financial dependence is an industry-level variable calculated for 27 3-digits ISIC industries and nine 4-digits ones using compustat data for the US. Let k be capital expenditure and x operational cash flow at the firm level. Rajan and Zingales' index for industry j, r_j, is the median value of $(k-x)/k$ across all compustat firms in industry j. Index values, given in Table 1 of Rajan-Zingales (1998), range from -0.45 for tobacco (ISIC 314) to 1.49 for drugs (ISIC 3522).

[24] Braun proxies asset tangibility by the ratio of net property, plant and equipment to market value at the firm level, using US compustat data. The industry-level variable is constructed, like in Rajan-Zingales, by taking the industry median at the ISIC 3-digit level. Index values, given in Table 1 of Braun (2003), range from 0.09 (leather products) to 0.67 (petroleum refineries).

variable at the product level, κ_p, by assigning to that product the corresponding ISIC3 value of Braun's index. In Column (3) of Table 8, we find that the interaction of asset tangibility (r_p) and the synergy effect has a negative and significant coefficient, implying that firms belonging to industries with high asset tangibility (essentially capital-intensive industries) are *less* sensitive to the synergy effect. In columns (4) and (5), we combine information and financial-constraint effects and find the effects largely stay the same when introduced together.[25]

Our results are broadly in line with a 2009 survey focusing of export entry and survival decisions conducted by the World Bank in Malawi, Mali, Senegal and Tanzania. Two thirds of the respondents identified their first client through relatives, friends, intermediaries and suppliers. More formal or technology-related channels (e.g. trade fairs or online research) came only second, and only a tenth of the initial contacts were made through export promotion agencies or exporters' associations. This highlights the importance of informal networks and suggest that the "thickness" of a certain network where there are many firms exporting similar products to similar destination may help to expand the chance of identifying appropriate buyers and obtain valuable information about consumers requirement through contacts with relatives, friends, intermediaries and suppliers.[26]

Product experience, whether through domestic or foreign sales, appears as a strong driver of geographical export expansion. A majority of respondents reported that their initial export product was one they were already selling domestically, as opposed to starting a new line tailored to the foreign customer's needs. When asked how the opportunity to export a new product came about in the first place, the majority of regular exporters answered that they were approached by an existing buyer asking for a new product, suggesting that *export* experience matters beyond domestic experience in terms of establishing a "network" of buyers to identify market opportunities.

Finally, in an open question about constraints on export (or export expansion in the case of the current exporters), a large proportion of respon-

[25] As a further robustness check, we replicated interaction results using a linear probability model (see Ai and Norton 2003 for a discussion of the issues involved in the estimation of probit models with interaction terms). Results, which are available from the authors, are largely unchanged.

[26] The influence of networks on trade patterns is a theme largely developed in the writings of Rauch (1999). World Bank (2012) shows that a higher number of migrants from country i in country j raises the survival of exports from i to j, pointing to migration/trade complementarities driven by network effects.

dents (31%) identified access to finance as the main factor limiting their operations. Moreover, the percentage was higher (42%) among past (failed) exporters, suggesting that credit constraints are not just a perception, but a reality effectively hurting the chances of success of exporters.

5 Concluding remarks

In spite of growing interest for them, firm-level datasets are still rare for low-income countries, and virtually inexistent for African countries. Our exploration of African customs data on firm-level exports reveals for the first time patterns that have been observed on product-level trade statistics, but it also reveals the existence of an externality effect that could be identified only using firm-level data. Exporters in our set of African countries experiment a lot on export markets, at a low scale and with low survival rates, particularly in the first year. That is, they operate in a difficult environment characterized by very high 'infant-mortality'. Using this, we investigate the determinants of success beyond their first year.

The most striking finding coming out of our analysis —one which could not be observed on product-level data—is that exporters of similar goods to the same destination exert a *positive* externality on new entrants. That is, the more they are, the higher the success probability of new entrants. Strikingly, the externality effect is much weaker across origin-country borders. If we proxy the externality by the number of firms exporting the same product to the same destination from *neighboring* countries, the effect is still present but only weakly identified (significant at 10%). If we proxy it by the number of firms from non-neighboring countries (say, Tanzania for Senegal), it disappears completely. That is, the synergy effect is largely national or, at least, conditional on proximity.

This positive externality is surprising, as one might expect that exporters of a given product to the same destination may crowd out each other, either through price competition or simply by offering more choice to buyers who could then 'hop' from one to the other, reducing survival at the individual level. We have explored a number of robustness tests in order to establish, at least tentatively, that our finding is not a statistical illusion. First, it could be driven by omitted-variable bias (e.g. supportive infrastructure at the national level or comparative advantage). We control for this by including as regressors either the country's revealed-comparative advantage index or product-origin fixed effects, without altering the results. We also control for selection into product-destination markets by including exports from other countries in those markets in the first stage of a Heckman extimator. Finally, we explore whether both better survival and 'crowding in' could reflect national capabilities at the sectoral level, following the idea developed by Hausmann and Klinger (2006). Our synergy effect does not disappear when controlling for these capabilities through various proxies.

Beyond robustness tests, we explore a number of conjectures that could help understand the economic channels through which these externalities could operate, focusing on information asymmetries and access to finance. For instance, access to credit may be easier when many exporters of the same product from the same country simultaneously operate in the same destination, as larger numbers may provide signals about profitability to both new entrants and banks. First, an exporter may obtain information through the network of other exporters of the same product, potential buyers, relatives or friends involved in the same manufacturing activity and exporting to the same market. Second, an isolated exporter may have more difficulties convincing banks that the risks she faces are manageable given the uncertain environment of export relations. If other firms are successfully in operation, by contrast, the bank can use the success of others as a predictor of its client's potential. We verify these conjectures in different ways. First, we interact the externality effect with quality heterogeneity as a measure of informational asymmetries, proxied by the cross-country dispersion in unit values at the product level. Second, we interact it with indicators of dependence on outside finance and the importance of physical assets proxied respectively by Rajan-Zingales and Braun indices. Interaction terms with the former two (unit-value dispersion and Rajan-Zingales index) are positive and strongly significant, suggesting that externality effects are stronger in sectors where informational asymmetries are higher, and dependence on external finance is more pronounced. The interaction term with the latter (Braun index of asset structure) is negative, suggesting a weaker synergy effect in sectors where the scope for moral hazard is reduced (and accordingly access to finance tend to be easier).

Our results are suggestive of a potential market failure if exporters fail to internalize the positive externality that they exert on new entrants. This may be taken as an argument in support of government-sponsored export promotion. However, policy implications should be interpreted cautiously, as the record of export promotion in developing countries is uneven. In addition, it may well be that exporters could internalize the externality through mutual-support professional associations.

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