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Which Portuguese Manufacturing Firms Learn by Exporting? [1]

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Abstract

Using a longitudinal database (1996-2003) at the plant level, this paper aims to shed light on the causal nexus between international trade engagement and productivity in Portugal. We analyse in particular the learning-by-exporting hypotheses. In line with recent empirical literature, we apply mainly the Propensity Score Matching and a differences-in-differences estimator. In post-entry years we find a higher growth of labour productivity and total factor productivity for new exporting firms when compared to firms that, although having similar characteristics, have decided not to begin exporting in that year. Moreover, in an attempt to uncover the channels through which the learning effects are driven to new exporters, we applied the same methodology to some sub-samples. We found that learning effects are higher for new exporters that are also importers or start importing at the same time. Other important factors influencing that learning ability are found in firms that export to more developed markets, in those that achieve a certain threshold of export intensity and particularly for those firms that belong to sectors in which Portugal is at a comparative disadvantage

[1]This paper contains statistical data from the National Institute of Statistics of Portugal (INE). The data is used under the permission of INE but does not imply the endorsement of INE in relation to the interpretation or analysis of the statistical data.

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1. Introduction

Since the 60s, cross-country macroeconomic literature has established a positive correlation between trade and growth. However, at the firm level, there is still an on-going debate on the relationship between trade and firms' performances, namely productivity. Pioneered by the works of Bernard and Jensen (1995) and Aw and Hang (1995), several works have been produced in recent years aiming to shed light on this issue.

There are two non-mutually exclusive theses to explain the observed high correlation between trade and productivity, at the firm level: the "self-selection" thesis (SS), assuming that most productive firms become exporters and the "learning-by-exporting" thesis (LBE), claiming that firms become more efficient by exporting and even experience an acceleration in productivity growth when compared with firms remaining non-exporters.

SS is based on the existence of important fixed costs of foreign market entry (e.g., Jovanovic, 1982; Roberts and Tybout, 1997). Thus, only the most productive firms would self-select to foreign markets. Hence, several theoretical models assume the higher productivity of some firms to be one of their intrinsic features, in an exogenous origin (e.g., Melitz, 2003; Bernard et al., 2003; Melitz and Ottaviano, 2008), and thus consider those firms had received a positive random draw from a productivity distribution. Other authors assume that some firms make a conscious decision to begin exporting (e.g., Yeaple, 2005) and thus, admit those firms deliberately "invest" to be exporters; in these cases, the productivity growth would be a result of such policies and preparation for future foreign market participation.

LBE is still often taken as a black-box function with an unclear learning mechanism behind the productivity growth; there are several mechanisms that could fill that gap: (i) exporting positively affects product and process innovation (e.g., Salomon and Shaver, 2005; Cassiman and Martinez-Ros, 2007); (ii) large and more competitive markets could give the necessary boost to exporters to become more efficient (competition effect); (iii) the wider network of contacts with several sources, such as clients, suppliers, competitors or even professional and scientific institutions could also enhance the generation of efficiency improvements or innovations, and (iv) the wider dimension of international markets could more likely offer the required scale economies. So, the absence of a coherent theory that supports and explains the LBE thesis may occur due to the difficulty in controlling the learning mechanisms in empirical works and this may block further theoretical advances.

Nevertheless, a growing body of literature has been claiming that exports produce learning effects, which would result from an adjustment in the process governing firm's productivity growth. The basic theoretical argument behind this LBE thesis is that firms operating in international markets can better capture knowledge and technological spillovers, from their international contacts. Such factors could generate a process of learning and efficiency gains which would end up by creating different productivity growth rates between exporter and domestic firms. LBE is then assumed to be a channel or a set of channels, beyond the use of economies of scale, generated or enhanced by wider foreign markets, that are associated with increased competition and technology transfers and which enable firms to improve products,[2] processes and organization structures.

The empirical literature seems to fully confirm only the self-selection thesis. Indeed, LBE tests have been produced for several countries (e.g., Wagner, 2007 reports studies for 34 countries) but overall, post-

[2] Some empirical studies have found positive effects of product market competition on productivity growth (e.g., Nickell, 1996).

entry effects seem weak or at most are mainly observed in less developed countries or are confined to groups of exporters. Moreover, the channels from which LBE could be generated are also not clear. Hence, there is a general assumption that LBE is probably more easily detected in firms of sectors or countries operating far away from the technological frontier and also on firms that export to more developed markets, which have a greater potential for “teaching” new exporting firms.

In order to contribute to this discussion, we tested the LBE thesis, for the first time for Portuguese firms. We used the largest sample of Portuguese manufacturing firms for the period 1996-2003 for which data is available, on both financial and international trade variables. Applying matching techniques with differences in differences estimators and other panel data techniques, we tried to uncover the effects on several variables (e.g., productivity, sales and wages), at the firm level, generated by the beginning of exports activity. Also for the first time for Portuguese firms, we analysed the connections between imports and LBE.

Assuming that post-entry mechanisms of exporting activity are heterogeneous and rely on several factors that work as transmission channels to LBE, then to uncover their role, we also tested the LBE thesis for some sub-samples: (i) technological differences between sectors; (ii) firms’ initial size, wage and efficiency; (iii) firms’ previous international trade status; (iv) types of markets firms trade with on export and import sides; (v) firms’ exporting intensity.

The remainder of the paper is organized as follows. Section 2 describes the data. In Section 3 begins we begin to test for LBE by using a Fixed Effects model (FE), but after that we focus on the analysis of post-entry effects, by implementing a matching approach; this approach allows us to discuss in more depth if exports improve firms’ performances or not. Section 4 presents some concluding remarks.

2. Data description

The empirical analysis relies on the same dataset used in Silva et al. (2010a, b); it combines two different data sources developed by the Portuguese National Statistics Institute (INE): balance sheet information (IAE) and external trade information (ECE). The two datasets are linked by firms’ non revealed fiscal number. IAE provides information of firms’ balance sheets,[3] and uses a survey sample of all the universe of manufacturing Portuguese firms, from 1996-2003. In this paper, we used: number of employees, turnover, value added, investment, labour cost, stock of capital assets, liabilities and earnings.[4] Firms are classified according to their main activity, as identified by INE standard codes for sectoral classification of business activities (CAE), which has a high correlation with EUROSTAT NACE 1.1 taxonomy.

We define an “active firm criteria” that involves firms experiencing three conditions: firms with at least 2 employees; firms with a global turnover of at least 1.000€; firms with a positive net fixed asset register. We also define “Exporter” as a firm which exports at least 1% of their turnover. Given those restrictions and the natural entry and exit of firms or the lack of information on some variables, the dataset is unbalanced. Nevertheless, it contains information for an average of 4,500 firms per year. Capital is proxied by tangible fixed assets at book value (net of depreciation).

[3] Since 2004, INE has changed its methodology and works with the universe of Portuguese manufacturing firms but before 2004 the only data available is the one we use. INE ensures the representativity of the sample used.
[4] Unfortunately, we do not have other data types that would have been useful, such as: innovation performance, workforce composition, workforce educational level or about affiliates of Portuguese multinationals.

In turn, ECE provides information of all Portuguese firms that exported and imported over the 1996-2003 period. For each firm, ECE supplies data on trade volume (exports and imports) aggregated by year and by country (destination of exports and origin of imports) and it also display information on the types of products/sectors traded for each transaction.[5] There is also information on the volumes (Kilograms) involved.

All nominal variables are measured in 1996 Euros and are deflated using 2 digit industry-level price indices provided by INE; for capital stock we use the same deflator for all sectors. The firm-level productivity is measured using two concepts: value-added per employee (LP) and Total Factor Productivity (TFP). Since it is highly probable that profit-maximizing firms immediately adjust their input levels (especially capital) each time they notice productivity shocks, then productivity and input choices are likely to be correlated and TFP estimation involves problems.

In line with several authors (e.g., Sharma and Mishra, 2009; Maggioni, 2009), TFP is estimated using the semi-parametric method of Levinsohn and Petrin (2003). This method recognizes the simultaneity bias as firms observe the productivity shocks but econometricians do not. Thus, like Levinsohn and Petrin (2003), we compute TFP as the residual of a Cobb-Douglas production function in which: the value added of each firm is the independent variable; capital, labour and unobservable productivity level are the dependent ones. Besides, this method assumes that intermediate inputs present a monotonic positive relationship with productivity and thus could be used as proxies. Given our data availability we use intermediate inputs as the values of “supplies and services from thirds” at book value. We estimate production function for every 2-digit sector separately[6].

3. Post-entry effects

In Silva et al. (2010.b) we have confirmed, in general, the presence of a Self Selection that drives the most successful, large and efficient firms into the export market. Nevertheless, SS doesn’t exclude the potential for LBE and even if new-exporters were already more productive, before exporting, they could further improve their performance differential, with non exporters, after the export entry.

To empirically prove LBE it requires that new exporters experience large, stable productivity improvements, comparing with those firms serving only the domestic market. Hence, to prove the existence of LBE upon entry into international trade, starters must present advances in productivity at a faster rate than non exporters. This faster rate of advance could be justified by: (i) advantages of higher competition in foreign markets, compared to domestic one; and (ii) benefits of knowledge sharing from foreign contacts and wider experiences (e.g., customers or suppliers of technical assistance) which further enhance efficiency and innovative performances.

In modelling terms, there are few attempts to formally describe LBE. Kostevc (2009) presents a two-country general equilibrium model, founded on the general equilibrium model of monopolistic competition of Fujita et al. (1999), in which, as market competition intensifies and firms previous price-cost mark-ups decrease, firms struggle to maintain their achieved profit levels. This scenario of competition intensification could lead firms to react by lowering marginal costs (through productivity increases) to sustain previous profit levels or just break even. One crucial assumption of these models lies in the fact that exporters, from less developed countries, face less intense competition in their home market, while in

[5] Our data includes 14 different sectoral types of traded products.
[6] Details on the Levinshon and Petrin’s methodology are shown in Maggioni, 2009.

foreign markets they face far more elastic demand for their products. Moreover, given the wide availability of varieties in those developed environments, the elasticity of substitution raises and generates a fall in the slope of their demand curves and a reduction of price-cost mark-ups. Thus, those firms have two alternatives: to improve their productivity or to exit the market.

In empirical terms and to assess LBE, several methodologies have been used: Granger causality tests (as the performed in Silva et al., 2010b, and which suggests the existence of a bi-directional causality between productivity and export intensity), random effects panel estimation, fixed-effects (FE) model, Generalized Methods of Moments (GMM) estimations, non-parametric techniques and, more recently, Matching techniques, especially a combination of Propensity Score Matching with Differences in Differences estimators (PSM-DID).

3.1. Post entry effects assessed by Fixed Effects model

The use of the FE model regression (as in McCann, 2009) does not allow perfect treatment for the endogeneity problems that are associated with LBE, as firms self-select to export, and thus the analysis of post-entry effects may be biased. However, an FE approach is considered useful as a first test of post entry effects. Assuming that "starter" is a firm that initiates exports at time *t* and keeps doing it for at least 2 years, and does not export at *t*-2 and *t*-1; additionally assuming that a "non-exporter" is a firm which does not export during the observed period, we performed the following regression:

$$\Delta TFP_{i,t+\delta} = \beta_0 + \beta_1 Starter_{i,t} + \beta_2 Controls_{i,t} + \varepsilon_{i,t} \qquad (1)$$

where we take time *t*-1 to be the last year in which the firm is a non-exporter, and *t* to be the year in which it becomes an exporter. The dependent variable may be taken as an average TFP growth rate, computed from the year previous to export. Since we use here the same concept of starter that was used for the SS test we have five different cohorts of average growth rates: (i) $\Delta tfp_{i,t+0} = tfp_{i,t} - tfp_{i,t-1}$; (ii) $\Delta tfp_{i,t+1} = \frac{tfp_{i,t+1} - tfp_{i,t-1}}{2}$; (iii) $\Delta tfp_{i,t+2} = \frac{tfp_{i,t+2} - tfp_{i,t-1}}{3}$; (iv) $\Delta tfp_{i,t+3} = \frac{tfp_{i,t+3} - tfp_{i,t-1}}{4}$; (v) $\Delta tfp_{i,t+4} = \frac{tfp_{i,t+4} - tfp_{i,t-1}}{5}$

(where *tfp* is the *ln*TFP).

The Controls variable refers to a vector including: size, a dummy for firms that possess skilled workers only devoted to R&D, a dummy for firms that report a share of foreign capital, a dummy for different sectors (CAE) and a time dummy.

Table 1. Learning effects of entry to exports on TFP, using FE model

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t-1*	*t*+3 / *t-1*	*t*+4 / *t-1*
tfp change (of all starters)	0.136^{*} (0.005)	0.033^{*} (0.019)	0.073^{*} (0.022)	0.005^{+} (0.023)	-0.012^{+} (0.030)
tfp change (of starters which do not import): NT – OE(a)	0.011^{**} (0.066)	-0.019^{+} (0.038)	0.085^{*} (0.035)	-0.017^{+} (0.036)	-0.047^{+} (0.042)
tfp change (of starters already importers):OI – TWT(b)	0.143^{*} (0.05)	0.060^{**} (0.030)	0.087^{**} (0.027)	0.021^{+} (0.029)	0.024^{+} (0.042)

ource: Own calculations.
lotes: (a) NT means non-trader becoming OE which means only exporter; (b) becoming TWT which means two-way tra
nd OI means only importer. Robust standard errors appear below the coefficients' estimates in parenthesis. * and ** m
tatistical significance at 10% and 5%, respectively; $^{+}$ means not statistically significant; if nothing is mentioned, estima
re significant at 1% level. Estimations obtained with Stata 10 software.

Table 1 shows clearly that beginning to export is associated with a significant increase in the TFP growth rate for starters, at least for a period of three years after the entry into foreign markets occurs, thus suggesting the presence of LBE. These effects are particularly stronger when starters were already importers (OI) and become two-way traders (TWT), suggesting that LBE may be enhanced by imports. In the case of firms that change from non-traders (NT) to only exporters (OE) the LBE effects are weaker and appear to be inconstant.

3.2. Post entry effects assessed by matching methods

Introduction to the use of matching methods in LBE assessment

In Silva et al. (2010b), we confirm that the Portuguese firms with best performances are more likely to become exporters, to assess the causality *nexus* from export status changes to firm performances, it is wise to control for this sample selection issue. That is, to properly evaluate the impact of exports on new exporters' performances, it must be considered that the group of export starters is not randomly selected. Thus, a comparison of performances between new exporters and never exporters, as was done in the first LBE studies (e.g., Bernard and Jensen, 1999) do not reveal the direction of causality between export status changes, on one hand, and efficiency improvements, on the other.

Matching methods are regarded, by many researchers, as a promising tool to cope with these statistical problems stemming from endogenous participation decision with regard to becoming an exporter. Besides, by using matching methods we are freed from the limiting functional forms on which regressions are based. The use of these methods applied to LBE was pioneered by Wagner (2002) for German firms and Girma et al. (2003) for UK firms.

Ideally, the evaluation of the effects of becoming an exporter would be made by comparing the firms' performance, some years after they begin to export, with the performance, at the same time, if they never began to export. Indeed, we would like to discover if, in the supposed counterfactual case of no exports, starters would have had worse or better outcomes. However, the observation of both outcomes is impossible.

Under the impossibility of such an ideal scenario, matching methods aim to evaluate the Average Treatment effect on the Treated (ATT), which means the difference for a "treated" firm between: (i) the effective outcome it obtains at a time *t*, after exports begins and (ii) the potential outcome it would have obtained if it had chosen not to export. In practice, we are considering the effects of a treatment model, where treatment is the export entry; however, we can only observe the outcome of exporters, provided that they had exported.[7] Moreover, if the group of treated firms were randomly chosen from the whole population of firms there would be no bias in computing the ATT, but as we have already confirmed this is not the case.

Conceptually, we aim to measure the ATT, meaning the average effects of a "treatment"; i.e., the decision to start exporting on starters' performances, this means computing:

$$ATT = E\left[Y_{i,t}(1) - Y_{i,t}(0) \mid D_i = 1\right] = E\left[Y_{i,t}(1) \mid D_i = 1\right] - E\left[Y_{i,t}(0) \mid D_i = 1\right], \tag{2}$$

where: $Y_{i,t}(1)$ is the outcome of a firm *i* in year *t* given it began exporting at a certain time; $Y_{i,t}(0)$ is the outcome of the same firm in the same year given it did not begin exporting at the referred time; *D* represents the decision made by the firm if it was starting to export (1) or not (0). Originally, *ATT* would consist of computing the differences in the outcomes of a firm either after it starts exporting or of not starting to export. In practise, we can compute $E\left[Y_{i,t}(0) \mid D_i = 0\right]$; i.e., the outcome for non-exporters provided that they have not exported, but we are unable to calculate the outcome of exporters if they had not started to export $E\left[Y_{i,t}(0) \mid D_i = 1\right]$. The solution is to replace the unobservable $E\left[Y_{i,t}(0) \mid D_i = 1\right]$ by the observable $E\left[Y_{i,t}(0) \mid D_i = 0\right]$, which means that, in computing ATT, we use $E\left[Y_{i,t}(0) \mid D_i = 1\right] - E\left[Y_{i,t}(0) \mid D_i = 0\right]$ (originating the selection bias in ATT computation).

As our study is not experimental and randomization is not possible,[8] bias is inevitable. To overcome this problem we must construct a proper counterfactual by considering certain assumptions and by using matching techniques. In fact, if we do not ensure that treated and non-treated firms are as similar to each other as possible, then the computation of ATT would be meaningless. By using matching techniques, we hope to build consistent counterfactuals to every starter, while using a generic non-exporter as the comparison group would not allow us to make causal inferences, since the observed differences after export begins could exist previously in a pre-export period and remain after it.

Matching techniques are based on the premise that it is possible to select a suitable control group, from among non-starters, that will be used as counterfactuals for starters. Ideally, it would be crucial, as a first step, to select firms that present the most identical features to the treated group of firms. That is, the control group of firms should have *n*-1 (out of *n*) similar features to the starters group and differ only in the n^{th} feature, which would be their decision to export in that year. In other words, the true purpose of matching is to pair each new exporting firm, in each year – on the basis of some observable variables, named as covariates – with a larger control group of firms that remain non-exporters until that year.

Given the potentially strong variety of firm observable variables that may be used to pair starters with non-starters (e.g., productivity, size, ownership, capital, sector or time effects), a problem of dimension of treatable variables arises. This problem is solved by computing an average index – the so called "propensity score" –, in line with the method of Rosenbaum and Rubin (1983). Using this propensity

[7] Obviously, the outcome of exporters if they had not exported is unknown.
[8] We have already confirmed that firms self select to export, which means there are substantial differences between treated firms (starters) and untreated firms (non starters).

score, from among a large group of non-treated firms we are able to find the ones which happen to be the most similar to the starters, on average terms and over the pre-treatment period.

Two main conditions must be observed in order to adequately use matching methods: (i) the conditional independence assumption (CIA) and (ii) the common support assumption (CSA). The former assumes that the covariables on which we match cannot be affected by the treatment, either ex-post or in anticipation of the treatment: if the exporting firms adjust their characteristics in anticipation of the beginning of the export activity, then we would end up matching on endogenous variables. This assumption is essential to overcome selection bias.

The CSA assumption assures that, depending on the chosen covariates, the potential outcome in the non-treatment scenario is independent of the treatment status. In line with Heckman et al. (1998), it is enough to assure only "mean conditional independence" between the control group and the treatment.[9] Besides, the CSA prevents the group of covariates from becoming a perfect predictor of the decision of a firm to begin exporting. Thus, we restrict matching to firms in "common support", meaning that we only "work" with firms where the propensity score belongs to the intersection of the supports of the propensity score of treated and controls (Becker and Ichino, 2002). Thus, we drop treated units which have a propensity score higher (lesser) than the *maximum* (*minimum*) propensity score of the controls.

The selection of Treated firms and Control firms

If we had chosen common exporters as the treatment group, it would not provide us with the necessary dynamics, as the effects of starting to export may already have dissipated some time previously. Thus, treated units must be export starters in a certain year and controls must be firms that do not decide to export in that same year. Nevertheless, there are several criteria to identify and select each of those two groups of firms.

Eliasson et al. (2009, p. 19) describe the difficulties involved: "the problem here is that we try to transform what is actually a process of dynamic treatment assignment (where some firms choose to enter the export market early, others decide to go in later, and some prefer to never enter) into a static one (where firms once and for all decide whether or not to enter)".

In fact, the definition of the treated and control firms may affect the computation of ATT and thus influence LBE assessment. If we define starters as only "successful" new exporters instead of all new exporters, or if we define controls as only never-exporters in the observed period, instead of considering firms that do not decide to begin exporting in that year, we will probably generate a bias favouring the LBE thesis. Indeed, if we deliberately forget firms that fail to survive as exporters, we also forget that export failure should be viewed as a possible outcome of the overall treatment effect of export entry and should not be considered as exogenous with regard to that same treatment. Also regarding control firms we must admit that using the group of never-exporters we establish a prejudice against firms that in each year choose not to export but in the near future may opt differently. Thus, in line with Eliasson et al. (2009) we think it would be more prudent to use as controls, in each year, all firm not-yet-entrants, regardless of what options they may assume in the future. Overall, it must be recognized that the composition of the treatment and comparison group involves conditioning on the future results as it produces samples that are selective in terms of the outcome of our interest. Thus, to analyse more deeply the implications of such options we decided to implement a triple classification of both starters and controls.

[9] This means that we are exempt of studying other moments of the conditional distribution of probability between starters and controls.

Concerning starters, we have used three concepts: (i) the more restricted one assumes starter is a firm that exports in year *t*, but not in *t*-1, *t*-2 and *t*-3; (ii) an intermediate definition assumes that starter is a firm that exports in year *t*, but not in either *t*-1 or *t*-2; (iii) a more flexible concept assumes that a starter is a firm that exports in year *t*, but not in *t*-1. Regarding the definition of controls, we also tested three concepts: (i) the more restricted one assumes that a control is a firm that does not export in years *t*, *t*-1, *t*-2 and *t*-3; (ii) an intermediate definition assumes that control is a firm that does not export in years *t*, *t*-1 and *t*-2; (iii) a more flexible concept assumes that control is a firm that does not export in years *t* and *t*-1. We also require that both groups of firms are observed at least for one year, after the treatment begins.

In the next sections we adopt the more flexible concept of starter[10] and of control; this option derives from the interest in using the widest sample possible. However, we also tested for other concepts of starters and controls and as we will see our main conclusions are not affected[11].

Moreover, studying the pattern of export starters during the period 1997-2002 (Appendix A), we can observe that the set composed by Food and Beverages, Wood, Printing and Machinery were the sectors with a highest propensity to start exporting, relative to the weight of each sector exporters in total exporters, for the same period.

3.3. Estimating the propensity score matching

The selection of covariates

The first step to implement PSM consists of estimating the propensity score. Thus, the choice of the covariates used to identify the probability of a firm beginning to export is the first task to perform. In line with several determinants of firms' selection to exporting activity (e.g., Silva et al., 2010b), our chosen covariates were: TFP, ULC of sales, size measured by the number of employees, a dummy (Dsmall) controlling for small firms (i.e., with fewer than 50 employees), capital stock, investment, wages, sales, a dummy indicating if the firm has workers in R&D activities (Skill), a dummy indicating if the firm has a foreign share of capital (Forcap), a dummy indicating if the firm imports (Imp), a dummy indicating if the firm imports machines (ImpMac), and also dummies for sector.[12] All these variables seem to influence both the export entry decision and the outcomes of both starters and control firms.

Regarding the regression, the choice of the functional form seems to be robust since the binary treatment with logit and probit regressions yields similar results.

$$\begin{aligned} Pr\left(Start_{i,t}=1\right) = f\Big(& TFP_{t-j}\,; N_{t-j}\,; Small_{t-j}\,; K_{t-j}\,; Invest_{t-j}\,; Wages_{t-j}\,; Sales_{t-j}\,; UCL_{t-j}\,; \\ & Skill_{t-j}\,; Im\ p_{t-j}\,; Im\ pMac_{t-j}\,; Forcap_{t-j}\,; Other\ control\ Variables_{t-j}\Big) \end{aligned} \quad (3)$$

The probit is estimated pooling all cohorts but we have also tested its validity separately for each year and sectoral group. To free up the functional form of the propensity score we also included higher order polynomials and interaction terms. In search for the proper specification of the probit model, we also tested the influence of other variables': level of profits, liquidity restrictions of firms (measured, at the end of each year, by firms' available cash balances and by firms' banking credit balances), weight of debt for each firm (measured by the ratio of banking debt to capital stock at the end of each year) and a dummy controlling for the imports of raw materials. However, these variables were excluded as they harmed the

[10] Using this definition of starter, on average for 1997-2002, 5% of the exporters are new entrants in each year.
[11] Details on the number os starters and controls in each definition are in Tables 2and 3 of Silva et al. (2010.b),
[12] The detail of these dummies depends on the type of data used; if using data for each cohort we used five digits sectoral codes but when performing pooled data we used two digit sectoral codes.

quality of matching and the balancing tests that are performed to ensure that the chosen specification balances the pre-treatment covariates between the treatment and the control group, conditional on the estimated propensity score.

The results of the subsequent matching performed on pre-treatment variables at year *t*-3, *t*-2 and *t*-1 were quite similar. Moreover, the risk of matching on the endogenous variable is, in our case, extremely low as many starters' pre-treatment features at *t*-3 closely resemble those at *t*-1. Thus, to benefit from a larger sample we chose to present the results from matching on covariates at *t*-1. The chosen probit specification respects the balancing test (Rosenbaum and Rubin, 1983; Becker and Ichino, 2002). This means that the decision to export must be random and treated and matched firms (not control firms in general but only matches) must be identical, on average, before exports. The common support option must also be selected, ensuring that firms with the same propensity to export have the same positive probability to become an exporter or to remain a domestic firm.

Performing Propensity Score Matching (PSM)

After propensity scores are obtained, the second phase consists of matching starters (treated firms) with controls (non-treated firms) by using the estimated propensity scores. There are several algorithms to establish that match; they differ due to the different weighting regimes to evaluate the importance of each control for each treated firm. We tested the use of two of those weighting schemes: kernel matching and nearest neighbour matching.

Kernel matching defines a neighbourhood for each treated observation and assigns a positive weight to all control observations within the neighbourhood while the weight is otherwise zero. In addition, there are different kernel weighting schemes (e.g., Gaussian, Biweight or Epanichnikov) that define different estimators; for example, uniform kernel attributes the same weight to each observation in the neighbourhood, while other forms of kernel make the weights dependent on the distance between the treated and the control being matched, where the weighting function is decreasing in distance. By using more observations, kernel matching reduces the variance of the estimator as compared to the nearest neighbour and produces less bias. The nearest neighbour algorithm matches a starter with a single "non starter" that has the closest propensity score (we allowed never exporters to be used as a match more than once – matching with replacement). Given their properties on variance, we will present results based on the Epanechnikov kernel.[13]

A crucial decision has to be made when performing the matching: (i) to do it separately in each year and for each sector (CAE), or (ii) to do it pooling all time cohorts and all sectors. We performed it both ways but when performing a pooled PSM we found that control-matches belong to same year and sector of starters only in 25% of all cases.

In line with De Loecker (2007), we argue that the option of such pooled PSM may have potential drawbacks as the marginal effects of various variables on the probability to start exporting may differ greatly between different sectors and even between different moments. This could be due to different technological and market conditions the firms face in different sectors or different years. This means that, conceptually, the separate estimation of the probability to start exporting for each sector and each year is a better procedure. On the other hand, given the narrowness of our database the use of a pooled PSM is of some interest.

[13] We use a bandwith of 0,001. Moreover, the results show little sensivity on the weighting regime used or on the bandwith interval.

Subsequently, aware of the importance of both keeping an ample dimension of our database and of the advantages of estimating the PSM cohort by cohort,[14] we implemented, as an intermediate solution, the data segmentation used in the final part of the section studying SS. Thus, we use the above-mentioned five group classification of sectors based on technological sophistication (in line with Pavitt, 1984 - adapted).

In this line, we estimated the PSM in two different ways; (i) for each year and for each of the five aggregated group of sectors and (ii) by pooling all cohorts (all years and all sectoral groups). At one hand, by using (i), we ensure that the matches come from the same year and from the same sectoral group. However, through this method we cannot produce an overall analysis of LBE for the period 1996-2003 and for the whole manufacturing sector, which is our main objective. At the other hand, by using the pooled PSM in (ii) we can exploit the information contained in the largest possible dataset, for modelling the export-starting decision; thus overcoming the eventual loss of efficiency of (i), since the number of starters in every separate cohort is sometimes relatively low. However, when using (ii) we may obtain estimations of lower quality since we cannot ensure that the ideal matches are obtained. Moreover, in line with Serti and Tomasi (2008) and Dehejia and Wahba (2002), there is no reason to believe that the same specification of the propensity score will balance the covariates in all samples. Thus, when we apply the matching to the pooled sample, additional precaution is needed and some “compensatory measures” must be taken, for example by using relative variables that measure deviations from average sectoral and year performances.

In an essay to reach the “best of both worlds”, we also used another PSM performance where the propensity scores are initially obtained from each separate year and sectoral group and then keeping those propensity scores and their corresponding weights, we perform the matching by pooling all cohorts of sectoral groups and years but ensuring that matches come from the same year and sectoral group of each starter. In order to distinguish the PSM applied to the two pooled datasets, we will name the latter as the “fine pooled PSM” and the former as the “general raw pooled PSM”.

To sum up, we applied PSM to four different samples: (i) we begin by performing PSM for each cohort of different year and sectoral group; and then, we implement matching on the pooled sample using three complementary ways: (ii-a) by pooling together the treated and matched control firms of different calendar years and of different sectoral groups– the general raw pooled PSM; (ii-b) by adding to the general raw pooled PSM the use of relative variables, computed as deviations from sector-year means, instead of absolute variable levels; (ii-c) by pooling together treated and matched controls but making sure to perform PSM on firms of the same year and sectoral group (fine pooled PSM) using the methodology described.

In all pooled samples we ensure that starters that repeat their status during the time span are only accounted for the first time they are starters; i.e., we eliminated the records of firms that are starters more than once[15]. Thus, we eliminated 49 records of repeated starters (Appendix B) presents the number of repeated starters of the various cohorts.

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[14] Now, meaning separately for each year cohort and also for each sector.
[15] Details on this procedure are in Appendix B, Silva et al. (2010.b)

3.4. Assessing matching quality

The basic assumption to evaluate matching quality is to compare the average level of the covariates before and after matching and looking for differences between treated and control units. If there are differences for the matched sample, the matching was not fully successful.

To test our matching we implemented a balancing test proposed by Becker and Ichino (2002) and a standard *T*-test for equality of means. In the former test, we split the sample into intervals so that the average propensity score for the treated and the control does not differ in each interval. Then, within each interval, we checked that the means of each feature do not differ between treated and control units. We made sure that the balancing property is satisfied for every specification of the propensity score (and thus for each cohort of starters and controls separately). Regarding the second test, we performed a standard *T*-test for equality of means for the covariates to check if significant differences remain after conditioning on the propensity score. We computed the *T*-test for the mean values at *t*-1[16]. Results shown in Table 2 refer to the concept of starters and controls considered earlier, and make us confident that we have identified the appropriate matched control group. Indeed, after matching no differences were found in covariate means of treated and untreated firms; thus, we are not able to reject the null hypothesis of equality of means for all relevant variables.

Table 2. Assessing the matching quality
Comparison between treated and control at *t*-1

Unmatched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports Machine	All imports
Treated	1.052	102.7	6.062	0.645	10.489	0.256	0.072	0.059	0.355	7.90
Control	0.959	69.0	3.800	0.355	10.182	0.195	0.043	0.023	0.199	4.90
T-test	3.44	8.42	4.63	5.92	1.10	15.03	2.81	4.95	8.93	2.02

Matched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports Machine	All imp
Treated	1.033	95.1	4,98	0.557	10.482	0.251	0.069	0.055	0.349	7.90
Control	1.050	92.3	5,08	0.554	10.601	0.247	0.067	0.051	0.343	7.01
T-test	-0.50	0.48	-0.31	0.05	-0.55	0.49	0.04	0.41	0.19	0.41

Source: Own calculations.

3.5. Propensity Score Matching with Differences in Differences estimator

PSM options

In spite of all precautions when performing PSM, the self-selection bias may still exist, due to the bias coming from unobservables. In fact, if there are unobservable variables affecting both "assignment" into exporting and the outcome variable simultaneously, a hidden-bias arises.

[16] Similar results are available for *t*-2., in Table 4.b of Silva et al. (2010.b). We have also assessed the matching quality for the other concepts of starters and controls (Appendix C.1 and C.2 of Silva et al. 2010.b).

A practical and common procedure to deal with time-invariant unobservable bias is to add a Differences in Differences (DID) estimator to PSM (Blundell and Costa Dias, 2000 assume that the use of matching estimator with differences in differences approach can improve the quality of non-experimental evaluation). Using DID,[17] we compare the differences in outcomes after and before the treatment (in our case, before and after export entry) for the treated group (export starters) to the same differences computed for the untreated group (controls). Given the previous assumptions, without the treatment, the differences across the two groups would not exist.

Thus, to finally evaluate the impact of exporting on new exporters´ performances (ATT), we performed the PSM-DID estimator applying it to the database built on the more flexible concept of starters and controls. We computed the PSM-DID estimator at every period *k* after the entry into the export markets, with respect to the year prior to entry (*t*-1). The implemented estimator could be written as:

$$M^{PSM-DID} = \frac{1}{n_i}\sum_{D_i=1}\left[\left(Y_{i,Post}-Y_{i,pre}\right)-\sum_{D_j=0} w_{i,j}\left(Y_{j,Post}-Y_{j,pre}\right)\right] \qquad (4)$$

In (4), *Y* is the required outcome (in our case we used logarithms – *ln* - instead of absolute values to obtain differences in growth rates between starters and non-starters); subscripts *Post* and *pre* denote that variable refers to the period post-entry and pre-entry; D_i=1 denotes the group of starters in the region of common support, while D_j=0 denote the group of non starters in the region of common support; n_i is the number of treated units on the common support; $w_{i,j}$ is the weight given to the j^{th} observation of controls in constructing the counterfactual to the i^{th} treated firm. The number of control firms that are matched with a starter *i* is always 1 when we use a single nearest neighbour or all matched controls in common support, when we use kernel algorithm. Given the assumptions made earlier about the database, namely the definition used for starters and controls, we considered a maximum of six years after the starting year and thus we calculated ATT effects for: the entry period *t*, *t*+1, *t*+2, *t*+3, *t*+4, *t*+5 and *t*+6.

Moreover, given that we used *ln* for each variable, values in Table 5 are expressed as percentual point differences in growth rates between starters and matched controls, for each of the variables considered and observed cumulatively from the year *t-1* to the end of that year. The standard errors are obtained by bootstrapping[18] the entire estimation framework, including the stage of propensity score computation.

The first PSM-DID was applied to the sample obtained by pooling all cohorts without special care for matches coming from different sectors and years (general raw pooled PSM-DID). Table 5 shows that the effect of exporting on TFP is positive and statistically significant since one year after entry into foreign markets up to four years later. The same idea is confirmed by using LP: the labour productivity of starters grows faster than for non-starters.

From Table 5 LBE seems to be confirmed with both productivity indicators. These results are broadly similar to those obtained by the FE model in the previous section; similar results arise when we match the pooled sample using nearest neighbour algorithm[19]. LBE effects seem to be robust enough whatever the methodology used.

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[17] According to Heckman et al. (1997), the difference-in-difference (DID) matching estimator removes the effects of common shocks on treated and controls, and thus provides a more accurate description of the impact of exporting.
[18] The use of bootstrapping is justified as an improvement of the accuracy of standard errors computed by module *psmatch2*; in performing bootstrapping, we used 200 replications.
[19] We do not report estimates based on nearest neighbour algorithm for reasons of brevity.

The positive effects of exporting seem to spread to other variables such as capital, number of employees and sales, while wages do not present different growth rates between starters and non-starters. Starters also appear to become even more capital intensive than non-starters, especially three and four years after the beginning of export. Moreover, starters do not present a higher growth of investment, which could be explained by the larger investment waves that starters performed some years before internationalization.

Moreover, the ULC of sales for starters presents lower growth rates in relation with non-starters, in the first two years after exports beginning, which can be explained by starters´ superior overall efficiency growth. In addition, starters present, from two years after beginning to export, a significantly higher growth rate of the weight of employees exclusively involved in to R&D activities (R&D personnel), indicating for those firms a superior attention to innovation activities and their preparation; this, in turn, helps them to deal with the more competitive environment faced in foreign markets. Regarding profits (earning after taxes), in the first three years after exporting, starters perform worse than non-starters but after that time interval the growth rate of earnings for starters is much more higher, which suggests that only after some time exporters are compensated for their option of internationalization.

Table 5. ATT effects: PSM-DID estimations (raw version)

	t / t-1	*t*+1 / *t-1*	*t+2 / t-1*	*t+3 / t-1*	*t+4 / t-1*	*t+5 / t-1*	*t+6 / t-1*
TFP	0.008^{+}	0.026*	0.045*	0.039*	0.059**	-0.002^{+}	-0.071^{+}
	(0.018)	(0.013)	(0.025)	(0.027)	(0.027)	(0.044)	(0.067)
LP	0.003^{+}	0.036	0.029^{+}	0.038*	-0.019^{+}	-0.039^{+}	-0.070^{+}
	(0.027)	(0.018)	(0.024)	(0.022)	(0.045)	(0.042)	(0.071)
Capital	0.009^{+}	0.058	0.048	0.358	0.064**	0.009^{+}	-0.027^{+}
	(0.01)	(0.011)	(0.016)	(0.001)	(0.026)	(0.04)	(0.077)
Employees	0.006^{+}	0.028	0.052	0.035**	0.046**	0.071**	-0.027^{+}
	(0.008)	(0.011)	(0.016)	(0.020)	(0.026)	(0.040)	(0.067)
Investment	0.014^{+}	0.081^{+}	-0.016^{+}	0.016^{+}	0.018^{+}	0.014^{+}	0.001^{+}
	(0.057)	(0.067)	(0.085)	(0.091)	(0.011)	(0.17)	(0.26)
Sales	0.022**	0.032**	0.053	0.054**	0.076**	0.017^{+}	-0.077^{+}
	(0.013)	(0.011)	(0.020)	(0.022)	(0.031)	(0.052)	(0.093)
Wages	0.001^{+}	-0.004^{+}	-0.014^{+}	0.002^{+}	0.007^{+}	-0.032^{+}	-0.034^{+}
	(0.012)	(0.072)	(0.013)	(0.011)	(0.023)	(0.026)	(0.045)
ULC	-0.015*	-0.017*	-0.014^{+}	-0.016^{+}	-0.022^{+}	0.021^{+}	0.016^{+}
	(0.011)	(0.012)	(0.022)	(0.024)	(0.026)	(0.037)	(0.042)
CI	0.004^{+}	0.022*	0.041	0.275	0.018^{+}	-0.061^{+}	-0.002^{+}
	(0.011)	(0.012)	(0.013)	(0.012)	(0.032)	(0.048)	(0.076)
Earnings after taxes	-0.055*	-0.071**	-0.162**	0.125	0.271	0.217*	0.021^{+}
	(0.023)	(0.042)	(0.073)	(0.041)	(0.058)	(0.131)	(0.023)
R&D personnel $^{(a)}$	-0.173^{+}	0.078^{+}	0.354**	0.423**	-	-	-
	(0.191)	(0.271)	(0.211)	(0.242)			
Number Treated	725	723	489	381	281	181	111
Number Controls	2,751	2,747	1,822	1,298	869	509	233

Source: Own calculations.

Notes: We report bootstrapped standard errors (200 replications), the number of treated on the common support and the number of matched controls. If nothing mentioned coefficients are significant at 1%.**: mean significant at least at 5%. * means coefficients are significant at least at 10%. $^{+}$ means coefficients are not significant. $^{(a)}$: PSM-DID for the quote in total employees of those designated exclusively to R&D activities.

Robustness of PSM-DID to different pooling methods

As an initial robustness test, we performed a similar procedure for PSM-DID but in this case we use the two years before starting to export (instead of one year) as the base years for computing the LBE effects. Appendix D shows that conclusions are very similar to those reported in Table 5 which confirms the robustness of those findings.

Moreover, in the previous PSM-DID we applied PSM-DID in a broad criteria of pooling to all treated and controls across sectors and years. As mentioned before, this procedure has some limitations and thus, in order to overcome them, we computed ATT (LBE effects) using relative values instead of absolute values. Thus, we now use all variables expressed as a deviation from their respective sector-year mean, to take into account sectoral and time specificities. The results of the respective ATT (Table 6), concerning the two productivity measures, are quite similar to the previous ones obtained with absolute variables. In this line, these results also suggest that applying PSM-DID estimators to the general raw pooled sample, as used in the first place, is an acceptable procedure.

Table 6. ATT effects: PSM-DID estimations using relative variables

	t / t-1	t+1 / t-1	t+2 / t-1	t+3 / t-1	t+4 / t-1	t+5 / t-1	t+6 / t-1
TFP	0.010^{+} (0.015)	0.026^{*} (0.015)	0.052^{**} (0.022)	0.044^{**} (0.020)	0.055^{*} (0.030)	0.021^{+} (0.015)	-
LP	0.013^{+} (0.015)	0.031^{*} (0.015)	0.045^{*} (0.022)	0.039^{*} (0.021)	0.052^{*} (0.031)	0.014^{+} (0.015)	-

Source: own calculations.

Notes: see Table 5. Given data narrowness estimates for six years are not possible.

Additionally, in a superior form of robustness, we estimated ATT effects ensuring we performed PSM-DID strictly matching firms exclusively of the same year and sectoral group.

To execute such “fine” pooled PSM: (i) we performed thirty different PSM, using nearest neighbour algorithm and controlling for common support, for each of the 30 cohorts;[20] (ii) we saved the propensity scores and the weights, in each separate PSM, of the treated and controls matched by using a nearest neighbour algorithm; (iii) we eliminated all treated and all controls that were out of the common support region; we also ignore all controls that were not used as matches in each separate PSM; (iv) we pooled all the remaining firms (the treated and the controls that were matched) and computed the correspondent ATT.

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[20] The 30 cohorts are the result of 6 years (1997 to 2002) multiplied by 5 groups of aggregated sectors.

Table 7. ATT effects: PSM-DID “fine” estimations

(PSM performed cross-section by cross section and year by year)

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
TFP	0.009^{+}	0.077^{**}	0.031^{**}	0.045^{+}	0.118^{*}	0.005^{+}	-0.014^{+}
	(0.023)	(0.033)	(0.021)	(0.043)	(0.056)	(0.07)	(0.014)
LP	0.012^{+}	0.072^{**}	0.025^{**}	0.035^{+}	-0.037^{+}	-0.045^{+}	-0.061
	(0.023)	(0.031)	(0.013)	(0.042)	(0.07)	(0.067)	(0.111)
Capital	0.003^{+}	0.049	0.029^{+}	0.572^{*}	0.024^{+}	0.019^{+}	-0.053^{+}
	(0.023)	(0.019)	(0.023)	(0.023)	(0.023)	(0.083)	(0.153)
Employees	-0.002^{+}	0.040^{**}	0.056	0.028^{+}	0.063^{*}	0.061^{+}	-0.007^{+}
	(0.013)	(0.017)	(0.019)	(0.023)	(0.023)	(0.053)	(0.111)
CI	0.007^{+}	0.008^{+}	0.028^{*}	0.641	-0.047^{+}	-0.042^{+}	-0.047^{+}
	(0.011)	(0.021)	(0.013)	(0.013)	(0.043)	(0.053)	(0.131)
Sales	0.032^{**}	0.067	0.048^{*}	0.007^{+}	0.107^{*}	0.016^{+}	0.018^{+}
	(0.016)	(0.025)	(0.028)	(0.053)	(0.056)	(0.088)	(0.151)
Invest	-0.026^{+}	0.031^{+}	-0.129^{+}	-0.119^{+}	0.319^{*}	-0.057^{+}	0.331^{+}
	(0.098)	(0.017)	(0.146)	(0.221)	(0.019)	(0.311)	(0.691)
Wages	0.008^{+}	-0.017^{*}	-0.011^{+}	-0.019^{*}	-0.025^{+}	-0.057^{*}	-0.027^{+}
	(0.008)	(0.010)	(0.014)	(0.091)	(0.020)	(0.031)	(0.053)
ULC	-0.026^{*}	-0.043^{**}	-0.009^{+}	-0.029^{+}	-0.075^{**}	0.002^{+}	-0.059^{+}
	(0.015)	(0.021)	(0.022)	(0.024)	(0.037)	(0.05)	(0.076)

Source: Own calculations. Number of firms: 944

Notes: See Table 5.

Robustness of PSM-DID to firms' history

In a last effort to test the robustness of PSM implemented on the general raw pooled sample, we performed a PSM splitting our treated and control firms according to the time span in which they are observed, both before and after the year of treatment. Table 5 shows that the sample size drops when we focus on periods more distant from the export entry year; this can be due to different reasons: (i) some starters (the less successful starters) stop exporting after some years; (ii) the controls, the starters or both exit the market; (iii) despite being in the market the controls or the starters did not respond to the query of INE. Assuming a time span of one year after export entry, to admit firms in our database, we cannot observe the whole history of firms after the export entry. This option, although enlarging our database could bias the results. Indeed, by imposing only a single year of observation after export entry and not a longer period, we are ignoring that several starters quit after one year and so we could be biasing the results against LBE as we would be overweighting unsuccessful starters.

To check on this possible sample selection effect, following De Loecker (2007) and Maggioni (2009), we recalculated the post-entry effects for different groups of firms according to the number of consecutive years we can observe the starters, after the export entry, and the correspondent matched controls; this means the number of years of consecutive data – not necessarily of exporting – after a firm starts to export. Table 8 shows the results.

Table 8. PSM-DID (TFP) firms splitted by consecutive number of years observed after "start"

Years of consecutive observations after starting year	*t* / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
1 year (firms: 988)	0.021^{+} (0.024)	0.060^{*} (0.034)	-	-	-	-	-
2 year (firms: 676)	-0.005^{+} (0.034)	0.020^{+} (0.041)	0.064^{*} (0.041)	-	-	-	-
3 year (firms: 648)	0.006^{+} (0.023)	0.006^{+} (0.021)	0.052^{*} (0.029)	0.069^{*} (0.021)	-	-	-
4 year (firms: 510)	-0.029^{+} (0.035)	0.043^{+} (0.059)	0.069^{**} (0.041)	0.117^{**} (0.051)	0.136^{**} (0.061)	-	-
5 year (firms: 355)	0.004^{+} (0.036)	0.040^{+} (0.034)	0.049^{*} (0.035)	0.076^{*} (0.046)	0.051^{+} (0.065)	0.101^{*} (0.061)	-
6 year (firms:296)	0.022^{+} (0.034)	-0.061^{+} (0.041)	-0.035^{+} (0.061)	-0.049^{+} (0.061)	-0.048^{+} (0.058)	-0.081^{+} (0.061)	-0.063^{+} (0.079)

Source: Own calculations.

Notes: See Table 5. We also report the maximum number of firms for each row.

We must highlight the differences between these results and those of Table 5. The results on productivity growth in Table 5 were obtained with a less restrict criteria as it includes firms having at least one consecutive observation after the export entry. Given that condition, we now check whether the effects attributed to exporting are not driven by the selection process over time. Thus, we looked at the effects of export entry at every *t* for the various groups of firms split by the different number of years the firm was observed, after the decision to start exporting was made; results of Table 8 show no substantial differences when compared to correspondent cases of Table 5; thus confirming there is no systematic selection process over time. For instance, at *t* and *t*+1 no LBE is detected whatever the sample duration while at *t*+2 and *t*+3 is possible to detect LBE whatever the sample duration; the exception is the group of firms having six observations after starting to export, yet, we must consider the small dimension of that particular sample which may difficult LBE detection.

Additionally, we studied LBE, splitting firms by the number of years with complete data before exports start, thus adding firms´ history" to LBE assessment.

Table 9. PSM-DID (TFP) firms splitted by the number of years observed before "start"

Years of consecutive observations **befor**e starting year	*t* / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
1 year (firms: 1017)	-0.016^{+} (0.021)	-0.027^{+} (0.025)	0.043^{+} (0.049)	0.019^{+} (0.037)	0.033^{+} (0.050)	0.002^{+} (0.065)	-0.067^{+} (0.078)
2 years (firms: 783)	0.006^{+} (0.029)	0.094^{**} (0.042)	0.032^{+} (0.043)	0.081^{*} (0.046)	0.063^{+} (0.063)	0.078^{+} (0.069)	-
3 years (firms: 594)	-0.003^{+} (0.062)	-0.012^{+} (0.034)	0.075^{*} (0.041)	0.067^{*} (0.041)	0.064^{+} (0.10)	-	-
4 years (firms: 463)	0.125^{+} (0.089)	0.151^{*} (0.091)	0.125^{*} (0.08)	0.008^{+} (0.099)	-	-	-
5 years (firms:319)	-0.032^{+} (0.121)	0.024^{+} (0.131)	0.008^{+} (0.151)	-	-	--	-
6 years (firms: 245)	-	-	-	-	-	-	-

Source: Own calculations. We add one more covariate to this PSM-DID: the year of start to export.

Notes: See Table 5. We also report the maximum number of firms for each row.

Results of Table 9 show that LBE is observed for firms having between two and four years of "historical data" but is not confirmed for firms with a small history of just one year before exports start. Although these results require further investigation they may suggest that firms in order to benefit from LBE need time to build some capacities and competences. Moreover, given the small number of observations with five and six years before exports start, it is not possible to confirm LBE for those time spans.

Robustness of PSM-DID to yearly computation

Although the results of Table 5 are quite expressive and seem also robust, we could consider, in line with De Loecker (2007) and Maggioni (2009) that in the entry year firms place themselves on a higher TFP path and then they "simply" stay on this "superior" level of efficiency. If this is true, the annual growth rates would be higher for starters only for the entry period. To check this hypothesis we then computed the ATT effects on yearly TFP growth rates, changing for each computation the year basis of comparison. Table 10 shows that starters present a significant higher annual growth rate of TFP than non-treated firms only for the second complete year after export entry. Considering this table together with previous results on PSM-DID estimates, we can argue that, even if exporting has positive effects on firm performance and even if it lasts for some years following the export entry, it is not in the entry year that starters go on a higher TFP path. These results are different from those found for Slovenian (De Loecker, 2007) and Turkish firms (Maggioni, 2009).

Table 10. ATT effects: PSM-DID estimations of yearly growth rates

	t / t-1	*t*+1 / t	*t*+2 / t+1	*t*+3 / t+2	*t*+4 / t+3	*t*+5 / t+4	*t*+6 / t+5
TFP	0.008^{+} (0.018)	0.017^{+} (0.015)	0.034^{**} (0.017)	0.019^{+} (0.019)	0.048^{**} (0.021)	0.044^{+} (0.031)	-0.005^{+} (0.067)

Source: Own calculations.

Notes: See Table 5.

3.6. Learning channels and detailed Learning-by-Exporting analysis

The link between LBE and imports

Empirical evidence on LBE shows, as already noticed for other countries (e.g., Maggioni, 2009, for Turkey), the existence of a close link between exporting and importing: export starters often also start importing in the same year; in our sample, almost 40% of export starters are simultaneously export and import starters (Table 11).

Table 11. Export and import starters

	1997	1998	1999	2000	2001	2002
Export starters	166	132	105	125	86	118
Import starters	253	236	179	203	178	194
Common starters	75	58	41	36	31	51

Source: Own calculations.

Based on Table 11, we decided to perform a double test concerning the importance of imports: (i) we tested if post-entry effects of exports are larger for firms start importing simultaneously with exporting; (ii) we also checked for any difference in ATT effects between two distinct groups of firms: (a) firms that change their international status from non-trader (NT), in the pre-entry year, to only exporters (OE) and (b) firms that change from only importers (OI), in the pre-entry year, to two-way-traders (TWT).

Concerning the first objective we split our database[21] into two mutually exclusive groups: (i) one group formed by common starters in each year, i.e., firms which were NT in *t*-1 and become TWT in year *t*; (ii) another group containing all export starters for each year, independently of being NT or OI before that moment.[22] Then, we applied the usual PSM-DID estimator to each group and also checked the quality of the matching. Results, in Table 12, strongly suggest that imports enhance LBE effects. When export starters simultaneously start importing firms experience strong positive effects in their productivity, whereas if export starters do not simultaneously start importing LBE is not observed.

Table 12. ATT effects: PSM-DID estimations on TFP controlling for import starters

	t / t-1	t+1 / t-1	t+2 / t-1	t+3 / t-1	t+4 / t-1	t+5 / t-1	t+6 / t-1
Import and export starters	0.116^{**} (0.022)	0.159^{**} (0.033)	0.215^{**} (0.069)	0.083^{*} (0.059)	0.109^{+} (0.092)	0.192^{+} (0.152)	-
Only export starters	-0.001^{+} (0.032)	0.014^{+} (0.034)	0.062^{+} (0.048)	0.063^{+} (0.051)	0.056^{+} (0.046)	0.048^{+} (0.049)	-0.097^{+} (0.089)

Source: Own calculations.

Notes: See Table 5. Given the narrowness of each sub-sample it is not possible to compute ATT for six years.

In a complementary analysis, we split that second group of firms, which become exporters, into two sub-groups: (i) one sub-group contains OI, that is the firms that are also importers in the export starting year, independently of when those imports began, and (ii) another sub-group is composed of purely NT in the export starting year. Results, in Table 13, in line with McCann (2009) for Irish firms, suggest that LBE is faster for NT than for OI. In fact, NT benefit more rapidly from LBE while OI seem to benefit only after a certain time lag.

Table 13. ATT effects: PSM-DID estimations with different import status

	t / t-1	t+1 / t-1	t+2 / t-1	t+3 / t-1	t+4 / t-1	t+5 / t-1	t+6 / t-1
NT – OE	0.012^{+} (0.018)	0.032^{*} (0.021)	0.052^{*} (0.021)	0.023^{+} (0.037)	0.067^{*} (0.043)	0.029^{+} (0.071)	-0.021^{+} (0.121)
OI – TWT	-0.001^{+} (0.032)	0.014^{+} (0.034)	0.051^{+} (0.047)	0.063^{*} (0.039)	0.056^{+} (0.046)	0.048^{*} (0.029)	-0.097^{+} (0.141)

Source: Own calculations.

Notes: See Table 15.

From the previous analyses we can state that imports perform two complementary roles for LBE assessment. At one hand, we must distinguish between LBE (which effects are not confirmed in Table 12)

[21] Given verifications performed we argue that the use of general raw pooled sample is acceptable from now on.
[22] This group of firms includes all export starters in each year which do not import in that year, whether or not they have imported previously.

and Learning-by-Importing, LBI, effects (which effects are noticed in Table 12). These LBI effects could be obtained from imported goods, the preparation for imports and imports use. At the other hand, comparing the first and the second rows of Table 13, in which any LBI effects are excluded, we can argue that previous imports may act for new exporters as a catalyser to LBE, in the sense that they extend the LBE effects in time.

The link between LBE and comparative advantage sectors

In line with Greenaway and Kneller (2007), we tested the hypothesis that the potential for LBE depends on some sectoral features that proxy for the differences between countries regarding the development of their productive systems. Those authors studied the importance of the level of exposure to trade in several sectors and also of sectoral R&D intensity levels. They found that LBE was weaker in firms that belong to sectors already highly exposed to international trade and in firms belonging to sectors that already have a high R&D intensity. Other authors (e.g., De Loecker, 2007 or Maggioni, 2009) argue that the potential for learning (and thus for LBE) depends upon the productivity gap between the domestic productive system and the foreign productive systems, which new exporters enter.

We also admit such theses and we assume that there is a different scope for learning relying on the productivity and technological gap between: (i) the home country and export destinations' countries; (ii) sectors of different countries.

This framework would lead us to study both: (i) the importance of overall efficiency differences between Portugal and market destinations of Portuguese exports; (ii) and the technological gap between Portuguese and foreign sectors or industries. In respect to the first issue, we assume that there is a "uniform" technological gap between countries, which affects all sectors with the same intensity; in the second approach we argue that the main objective is to take care of sectoral differences given that countries are likely to have sectors with comparative advantages and others with comparative disadvantages.

In sectors where Portugal has no comparative advantage, Portuguese firms are likely to be less productive, on average, than foreign firms; however, in comparative advantage sectors, the Portuguese productive system is probably more efficient (in absolute and relative terms) than average foreign productive systems. Thus, in comparative advantage sectors, Portuguese firms could be more productive than firms of trade partners or, even if they are less efficient than foreign firms, that differential of productivity should be lower than in comparative disadvantage sectors. Thus, we want to check if learning effects are stronger and more significant for new exporters in comparative disadvantage sectors since in these sectors the productivity gap between the domestic productive system and foreign productive systems should be higher than in comparative advantage sectors. In fact, new exporters, in comparative disadvantage sectors, could more likely be exposed to a more competitive environment than in their domestic context and thus they could more probably receive positive spillovers, which could explain their potentially larger post-entry effects. In this line, we expect LBE to be more intensive in comparative disadvantage sectors.

In order to classify sectors according to international comparative advantages, we assume that trade patterns reflect this status. Using the Balassa (1965) Index of Revelead Comparative Advantage and the sectoral classification produced by Amador et al. (2007), we assume that Portugal has a comparative advantage in sectors in which it is more specialized than the world average;[23] in these cases, this would imply a Balassa index higher than one, which refers to sectors whose weight in total Portuguese exports is higher than its corresponding weight in total world exports (Appendix E).

Hence, we tested the argument that LBE is more effective in comparative disadvantage sectors. Thus, after the PSM-DID for the whole sample, we defined Post_CA a vector of dummy variables for the post-entry period for starters in comparative advantage (CA) sectors, and Post_CD a similar vector for the post-entry period for starters in comparative disadvantage sectors (CD). Finally, we computed ATT effects running an OLS of the following equation:

$$\Delta TFP_{i,s} = \alpha + \beta_1 Post\_CA_{i,s} + \beta_2 post\_CD_{i,s} + \varepsilon_{i,s} \ , \qquad (5)$$

where: $\Delta TFP_{i,s}$ is the productivity growth between the post-entry and pre-entry (*t*-1) period. To correct for specific effects linked to comparative advantage or disadvantage sectors, we use relative $TFP_{i,s}$ expressed as a deviation from the sector-year mean, to capture and correct for effects common to all firms belonging to the same sector.

Results obtained in Table 14 seem to confirm our argument. In comparative disadvantage sectors, new exporters present significant effects of LBE since the first year of exporting and they increase their productivity substantially more than never exporters and somewhat more than starters in comparative advantage sectors.

Maggioni (2009) in a study for Turkish manufacturing firms had remarked that firms belonging to comparative advantage sectors could immediately take advantage of the export activity when they enter foreign markets; she also noticed that firms in comparative disadvantage sectors needed some time to exploit the opportunities offered by foreign markets. That is, the magnitude of post-entry effects would depend on comparative advantage and also on the timing for benefiting from LBE effects according to the groups firms belonged to. Our study does not prove that claim as we observed significant higher LBE effects on starters that belong to comparative disadvantage sectors.

Table 14. PSM-DID estimations of TFP according to starters comparative advantage

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
Starter in CA sectors	-0.006 (0.018)	0.030* (0.017)	0.034* (0.022)	0.034+ (0.031)	0.045+ (0.039)	-0.077+ (0.054)	0.016+ (0.091)
Starter in CD sectors	0.045* (0.031)	0.039* (0.027)	0.072* (0.049)	0.002+ (0.057)	0.024+ (0.063)	-0.035+ (0.083)	-0.162+ (0.191)

Source: Own calculations.

Notes: See Table 5.

LBE at industry level

Tables 15a and 15b show the differences on LBE effects depending on the sectors new exporters belong to, although we use then two different aggregations. Nevertheless, we notice that wearing apparel (CAE 18) and non-metallic products (CAE 26) show hints of negative effects on TFP from their new exporting activity. On the contrary, starters from leather, leather products (CAE 19) and electrical machinery (CAE 31) are the ones that present more obvious LBE positive effects. Some other sectors present only partial hints of LBE: textiles (CAE 18), wood (CAE 20), fabricated metal products (CAE 28).

[23] The ideal Index of Revealed Comparative Advantage should be computed in a bilateral basis given that it is comparative advantage is influenced by bilateral trade policy. Yet, the computation of such Index would be extremely demanding in the context of the present essay.

Finally, for some sectors we do not obtain any evidence of LBE effects (e.g., machinery – CAE 29). However, bearing in mind the limitations of data for some sectoral groups several computations were not possible.

Table 15a: PSM-DID estimations of TFP according to sectoral groups

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
Gr 1	0.060* (0.311)	0.031^{+} (0.047)	0.035^{+} (0.065)	0.013^{+} (0.061)	-0.039^{+} (0.081)	-0.092^{+} (0.131)	-0.351* (0.188)
Gr 2	0.012^{+} (0.021)	0.041^{+} (0.043)	0.166** (0.071)	0.098* (0.055)	0.112^{+} (0.101)	0.012^{+} (0.013)	-0.291^{+} (0.212)
Gr 3	0.004^{+} (0.076)	0.066* (0.034)	0.032^{+} (0.046)	0.026^{+} (0.054)	0.052^{+} (0.071)	0.156^{+} (0.131)	0.049^{+} (0.311)
Gr 4	-0.069* (0.035)	-0.026^{+} (0.037)	0.004^{+} (0.051)	0.017^{+} (0.063)	0.098* (0.065)	0.010^{+} (0.012)	-0.121^{+} (0.142)
Gr 5	0.084* (0.056)	0.038^{+} (0.059)	0.126* (0.081)	0.177* (0.080)	0.056^{+} (0.075)	0.019^{+} (0.101)	0.001^{+} (0.382)

Source: Own calculations.

Notes: See Table 5.

In Table 15b, we use two digit CAE sectoral aggregations. Given the narrowness of data for some sectors, conclusions are risky.

Table 15b. PSM-DID estimations of TFP according to CAE sectors

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
CAE 15	0.060* (0.33)	0.031^{+} (0.047)	0.035^{+} (0.065)	0.013^{+} (0.061)	-0.039^{+} (0.081)	-0.092^{+} (0.131)	-0.351* (0.188)
CAE 17	-0.016^{+} (0.031)	-0.021^{+} (0.055)	0.043^{+} (0.064)	0.179* (0.102)	0.086^{+} (0.142)	-0.108^{+} (0.241)	-
CAE 18	0.044^{+} (0.045)	0.149^{+} (0.113)	0.046^{+} (0.211)	-0.188^{+} (0.312)	-0.131* (0.104)	-	-
CAE 19	0.015 (0.011)	0.542* (0.292)	0.095* (0.065)	-	-	-	-
CAE 20	-0.043^{+} (0.065)	0.113* (0.059)	-0.049^{+} (0.164)	-0.053^{+} (0.173)	-	-	-
CAE 21	0.089^{+} (0.196)	-0.085^{+} (0.253)	-0.241^{+} (0.356)	-	-	-	-
CAE 22	0.036^{+} (0.074)	0.007^{+} (0.075)	-0.018^{+} (0.132)	0.082^{+} (0.188)	-0.061^{+} (0.187)	-	-
CAE 24	-0.019^{+} (0.068)	-0.029^{+} (0.091)	0.012^{+} (0.014)	-0.053^{+} (0.217)	-0.022^{+} (0.039)	-	-
CAE 25	-0.014^{+} (0.091)	-0.002^{+} (0.097)	0.209^{+} (0.208)	0.149^{+} (0.142)	-	-	-
CAE 26	-0.122^{+} (0.122)	-0.159* (0.093)	0.018^{+} (0.093)	-0.089^{+} (0.112)	0.049^{+} (0.163)	0.035^{+} (0.175)	-
CAE 27	-0.059^{+}	-0.186^{+}	-0.003^{+}	-	-	-	-

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
	(0.079)	(0.178)	(0.267)				
CAE 28	0.011^{+} (0.038)	0.004^{+} (0.073)	-0.079^{+} (0.088)	-0.012^{+} (0.142)	0.339* (0.242)	0.094^{+} (0.231)	-
CAE 29	0.028^{+} (0.043)	-0.004^{+} (0.064)	0.027^{+} (0.074)	0.045^{+} (0.102)	0.044^{+} (0.123)	-0.141^{+} (0.195)	
CAE 31	0.002^{+} (0.091)	0.019^{+} (0.086)	0.141* (0.092)	0.137* (0.091)	-	-	-
CAE 34	-0.067^{+} (0.087)	-0.031^{+} (0.065)	-0.091^{+} (0.121)				
CAE 36	0.037^{+} (0.082)	0.011* (0.061)	0.023^{+} (0.187)	-0.055^{+} (0.171)	-0.094^{+}		

Source: Own calculations.

Notes: See Table 5.

The link between LBE, foreign capital and R&D workforce

Since firms that are affiliates or subsidiaries of multinational firms are in a certain sense internationalized, it is not clear whether the LBE theory could be applied to such firms. Those firms can access internal knowledge information spillovers within the multinational group and probably have already benefited from the international experience and knowledge flows of their international investors. Thus, we expect to be less room for learning effects in starters that share capital with foreign multinationals. Moreover, according to Helpman et al. (2004), the most productive firms choose to engage in foreign direct investment and most probably share their technology and expertise with their affiliates or subsidiaries.

Given that in our database there was a low share of starters that report to have foreign capital, we could not perform an appropriate PSM-DID for the sub-sample of such firms; thus, we decided to pool this group with the group composed by firms that report having workers assigned to R&D activities. Hence, we split our database of firms into two mutually exclusive groups: (i) firms that, in the first year of exporting, report to have a share of foreign capital or a share of specialized workers; (ii) firms that do not report any of these two features. Then, we applied the usual PSM-DID estimator to each group and checked for the matching quality.

Results in Table 16 show that LBE effects, in starters with foreign capital or skilled workers, are limited to the year of entry and after that year are not significant. On the other hand, after the second year, firms domestically owned and without a specialized workforce seem to experience a persistent higher growth of efficiency compared with non-starters. These outcomes are in line with similar tests performed by Ma and Zhang (2008) for Chinese firms.

Table 16. ATT effects: PSM-DID estimations according to starters foreign capital or skilled labour

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
Starter with Skill or Forcap	0.174* (0.098)	-0.044^{+} (0.065)	-0.054^{+} (0.054)	-0.033^{+} (0.087)	-	-	-
Starter without Skill or Forcap	0.016^{+} (0.023)	0.051* (0.036)	0.082* (0.053)	0.054^{+} (0.076)	0.122* (0.087)	0.146^{+} (0.187)	-

Source: Own calculations.

Notes: See Table 5. Given the low numbers of starters with foreign capital or specialized employees it is not possible to compute ATT effects for years *t*+4, *t*+5 and *t*+6.

The link between LBE and firms' size

We also tested the size importance in explaining the LBE effects. Using the dummy covariate "small", equal to one for firms with less than 50 workers and zero otherwise, we split our database of starters and of controls into the corresponding two mutually exclusive sub-groups. Then, we applied the usual PSM-DID estimator to each group and checked the quality of the matching. Results in Table 17 reveal that "big" firms have significant LBE effects that are in contrast with "small" firms that do not observe LBE. This could be due to the fact that firms need a certain dimension to benefit from external learning – the so called absorptive capacity.

Table 17. ATT effects: PSM-DID of TFP estimations according to starters size

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
Small Starter	-0.038^{+} (0.030)	-0.013^{+} (0.012)	-0.011^{+} (0.0290)	0.009^{+} (0.059)	0.054^{+} (0.031)	0.057^{+} (0.071)	-0.058^{+} (0.052)
Big Starter	0.027^{+} (0.025)	0.035^{*} (0.021)	0.077^{**} (0.034)	0.051^{*} (0.031)	0.062^{*} (0.040)	-0.019^{+} (0.058)	0.009^{+} (0.091)

Source: Own calculations.

Notes: See Table 5.

The link between LBE and export intensity of starters

Following several studies (e.g., Castellani, 2002; Andersson and Lööf, 2009), one can argue that starters are able to overcome the sunk costs of entering foreign markets when they achieve a certain threshold of export intensity. Moreover, if we assume that higher export intensity firms may have a higher degree of commitment to foreign operations and also a higher frequency of foreign sales, this would justify, for those firms, the existence of a more sophisticated structure and of a superior organisational capability.

All these facts would explain the higher capacity for learning and the higher productivity growth for more intensive exporters. Besides, low export intensity firms may explain the existence of occasional exports, without a clear exporting strategy that limits the option to profit from a higher productivity growth. In a study for Singapore, Chongvilaivan (2008) also stated that LBE effectiveness depends more on the intensity of the exporting activities than on the simple exporting status. Only persistent and frequent exporters find the need and the opportunity for the transfer of technology and of administrative or organisational knowledge. Fernandes and Isgut (2007) for Colombian firms also noticed that the LBE effects were negligible for firms that only participate marginally in foreign markets.

In line with these studies, we tested the importance of export intensity - % of exports in turnover - in Portuguese new exporters. We split our sample of starters into three mutually exclusive groups: (i) starters with an export intensity inferior to 5%, in the starting year and in the next two following years; (ii) starters that reach an average export intensity higher than 5% but always inferior to 35%, in that period; (iii) starters with an average export intensity higher than 35% in the three years span. We kept all controls for each group.

Results in Table 18 seem to confirm previous hypotheses.[24] Starters with lower export intensity take more time to benefit from their exporting activity since positive and significant effects on their

[24] Given the fact that we do not control for export intensity after three years of entry the results for *t*+4 and *t*+5 must be read with particular caution.

efficiency occur just four years after starting to export. In contrast, starters with high export intensity take advantage of their exports faster, in a longer period and with a higher level. Moreover, results of medium export intensity group suggest the relationship between export intensity and LBE is not a linear one, in line with Fryges and Wagner (2008).

Table 18. ATT effects: PSM-DID estimations of TFP according to starters export intensity in the first three years after exporting begins

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
High export intensity	0.016^{+} (0.041)	0.064^{*} (0.035)	0.109^{*} (0.062)	-0.075^{+} (0.065)	0.179^{*} (0.231)	0.111^{+} (0.121)	
Medium export intensity	0.065^{+} (0.100)	0.107^{+} (0.058)	0.085^{+} (0.141)	0.071^{+} (0.059)	0.152^{+} (0.241)	-	
Low export intensity	0.031^{+} (0.046)	0.040^{+} (0.040)	0.060^{+} (0.06)	0.056^{+} (0.053)	0.114^{*} (0.077)	0.063^{+} (0.111)	

Source: Own calculations.

Notes: See Table 5. Given the narrowness of each subsample it is unfeasible to compute ATT for the 6th year after export entry.

The link between LBE and the initial TFP level

In another strand of these studies, we could argue that starters with lower TFP levels are in a better position to benefit from the LBE effects. In order to test this idea, we split our database into two mutually exclusive groups of firms: (i) firms with a lower TFP, in the year before export entry; lower means inferior to the average level of all firms in the same year; (ii) firms with a TFP, in the year before export entry, higher than the average level for all firms. Results in Table 19 show, on one hand, starters with lower TFP levels can benefit more rapidly from the LBE positive effects; on the other hand, starters with an initial superior TFP level benefit from LBE three years after starting to export but this positive effect lasts for two years.

Table 19. ATT effects: PSM-DID estimations of TFP according to TFP initial level

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
High initial TFP level	0.006^{+} (0.016)	0.004^{+} (0.021)	0.008^{+} (0.029)	0.058^{*} (0.023)	0.077^{**} (0.036)	0.062^{+} (0.068)	-0.067^{+} (0.092)
Low initial TFP level	0.012^{+} (0.017)	0.047^{**} (0.021)	0.041^{*} (0.028)	0.055^{**} (0.027)	0.084^{*} (0.040)	0.051^{+} (0.042)	0.001^{+} (0.023)

Source: Own calculations.

Notes: See Table 5.

The link between LBE and the initial wage level

Since wages may reflect the labour skill and firms' technological capacity, we could speculate that starters with lower wages have a greater potential to benefit from the LBE effects because of the distance between their knowledge level and the corresponding level of their trader partners. Moreover, we could make the opposite assumption as lower wage firms more likely have lower absorptive capacity and then may not have the requirements for benefiting from LBE potential effects. To test these arguments we split our database into quartiles.

Table 20. ATT effects: PSM-DID estimations of TFP according to initial wage level

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
25% of firms with the highest initial wage level	0.074^{+} (0.055)	0.047^{+} (0.059)	0.099^{+} (0.088)	0.047^{+} (0.111)	0.045^{+} (0.145)	0.049^{+} (0.421)	-0.111^{+} (0.213)
25% of firms with the lowest wage level	0.035^{*} (0.024)	0.047^{*} (0.027)	0.099^{**} (0.051)	0.018^{+} (0.071)	0.059^{+} (0.113)	-0.064^{+} (0.071)	-0.119^{+} (0.185)

Source: Own calculations.

Notes: See Table 5.

Results in Table 20 show that starters with the lowest wage levels benefit immediately and during three consecutive years from positive LBE[25] effects; on the other hand, starters with an initial higher wage level do not benefit from exporting activities. Thus, wage levels in Portuguese starters seem to proxy correctly for firms´ technological level and may also mean that firms with low technological levels seem to learn when trading abroad, probably with more developed knowledge environments.

The link between LBE and exports destinations

In line with De Loecker (2007), we analysed LBE according to exports market destination. For that purpose, we split our database of starters into several groups, according to the countries they export to in their first two years of international sales: the year of entry and the following year,[26] and then we computed ATT separately for each cohort. Our division into groups comprised the following cases: (i) firms that export only to the European Union (EU); (ii) firms that export only to Portuguese Language countries (PL); (iii) firms that export only to other Developed countries outside the EU (ODev); (iv) firms that export only to non-developed countries (NDev); (v) firms that export only to EU+PL; (vi) firms that export only to EU+ODev; (vii) firms that export to more than one of the groups of countries mentioned before (Multiple). Moreover, bearing in mind the importance of Spain as our main trade partner, we were also able to study the LBE effects, in the first four years, for firms that only export to Spain. Moreover, since few observations existed for groups (v) and (vi), it was not possible to obtain estimates of ATT for those cases.

Results in Table 21 present some important features: (i) for firms that export exclusively to non Developed markets we cannot confirm LBE effects; (ii) firms that export to the EU seem to obtain fast LBE; moreover, those effects last for 4 consecutive years; (iii) firms that export to PL seem to obtain positive LBE effects but not so consistently as for exports to EU; (iv) firms that export only to other Developed countries only obtain positive LBE effects from the third and fourth complete years after beginning to export; (v) for firms that export exclusively to Spain we cannot confirm the existence of positive LBE effects; (vi) firms that mix several types of destinations seem to get moderately positive LBE effects. Thus,

[25] Following Table 10 analysis, we also uncover for firms of Group 2 that no significant effect is observed in the wage level for such starters until the fifth year; then a decrease is observed.

[26] For some starters (about 20% of the total) it was not possible to identify the group of countries to which they had exported in that two years period; this situation was the result of two different factors: firms that did not present a constant pattern of export destinations along the two years and the mismatch between the two main datasets used.

we could argue that these results show clearly that LBE depends both on the teaching potential of the exports destination markets but also on the absorptive capacity of exporters to make suitable use of such benefits.[27]

Table 21. ATT effects: PSM-DID (TFP) according to exports´ destinations

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
Firms that export to multiple destinations	-0.022^{+} (0.035)	0.053^{+} (0.051)	0.021^{+} (0.071)	0.117* (0.075)	0.091^{+} (0.192)	0.091^{+} (0.221)	-
Firms that export only to EU	0.050* (0.031)	0.152** (0.061)	0.097* (0.061)	0.122* (0.021)	0.089^{+} (0.012)	0.055^{+} (0.042)	-
Firms that export only to Spain	0.003^{+} (0.012)	0.032^{+} (0.030)	-0.106^{+} (0.111)	0.064^{+} (0.108)	-	-	-
Firms that export only to PL	-0.022^{+} (0.059)	0.079* (0.047)	0.064^{+} (0.050)	0.063^{+} (0.056)	0.141** (0.067)	0.116^{+} (0.099)	0.098^{+} (0.261)
Firms that export only to NDev	-0.079^{+} (0.098)	-0.025^{+} (0.171)	-0.061^{+} (0.199)	-0.074^{+} (0.089)	0.007^{+} (0.081)	-0.147^{+} (0.126)	-
Firms that export only to ODev	-0.028^{+} (0.030)	0.036^{+} (0.171)	-0.111* (0.061)	0.098* (0.061)	0.188* (0.121)	-	-

Source: Own calculations.

Notes: see Table 5.

The link between LBE and the specific year of entry (economic cycle)

Assuming the higher accuracy of estimates of LBE when performed for samples of firms that start to export in the same year and given that LBE effects may depend on the cycle of world trade and also on the economic cycle of the countries that buy Portuguese exports, we perform estimations of the ATT effects for TFP, splitting firms by their entry year in export markets.

Table 22. ATT effects: PSM-DID for TFP according to the entry year of starters

	t / *t*-1	*t*+1 / *t*-1	*t*+2 / *t*-1	*t*+3 / *t*-1	*t*+4 / *t*-1	*t*+5 / *t*-1	*t*+6 / *t*-1
1997	-0.008^{+} (0.026)	-0.015^{+} (0.035)	0.061* (0.033)	0.006^{+} (0.048)	0.047+ (0.061)	-0.041 (0.069)	-0.067^{+} (0.071)
1998	-0.008^{+} (0.033)	0.077** (0.047)	0.012^{+} (0.051)	0.047^{+} (0.064)	0.046^{+} (0.061)	0.094* (0.049)	-
1999	0.018^{+} (0.051)	0.012^{+} (0.041)	0.069* (0.047)	0.107* (0.057)	0.126* (0.081)	-	-
2000	0.062^{+} (0.071)	0.094* (0.052)	0.051^{+} (0.071)	0.061^{+} (0.053)	-	-	-
2001	-0.011^{+} (0.049)	0.039^{+} (0.054)	0.082* (0.056)	-	-	-	-
2002	0.008 (0.071)	0.023 (0.017)	-	-	-	-	-

Source: Own calculations.

Notes: see Table 5

[27] In this line it would be recommended a mixed study combining both export destination and firms´ level of initial TFP. Given the narrowness of our database such test was not possible.

Results of Table 22 show that LBE is present for every cohort of starters whatever the year they initiate exports; nevertheless, we noticed distinct LBE "strengths" across years. Although comparisons are difficult given the different time spans involved in each cohort of starters, we can observe strong LBE effects for starters in 1999 relative to other years. In order to uncover such fact, we performed a non-exhaustive study on the relative performances of each year starters. The results of Appendix F show clearly that 1999 starters achieved the highest levels in several factors that enhance LBE, namely: (i) import share; (ii) export intensity; (iii) weight of exports to EU and ODev countries; (iv) weight of firms of Group 5, the one with superior technological sophistication.

4. Concluding remarks

In this study, for the first time for Portuguese firms, we analysed Learning-by-Exporting thesis, for the period 1996-2003. In this respect, we noticed that export starters present, relative to non-starters, a higher growth rate in some important variables, after exports begin. This conclusion is robust to the use of an FE model or a PSM-DID estimator. With regard to PSM-DID estimations we used several robustness checks: we tested the LBE effects using three different concepts of export starters and export controls, and in addition, we used complementary methodologies to assure that when pooling several cohorts of sectoral groups and years the quality of the PSM was not compromised.

Looking at the results, we found that, overall, new exporters present (in comparison with non-starters) a higher growth rate on the majority of performance variables: efficiency, turnover, number of employees and capital intensity. Nevertheless, sales seem to be the only variable that presents immediate higher growth rate, while the others take some time to display that superiority: e.g., the growth rate of profits for starters only shows superiority four years after exports begin. Moreover, it seems that exports do not place Portuguese starters on a higher productivity path from the entry year but only after a two year period. Our analysis also confirms a strict linkage between export and import entry as the strongest LBE effects are obtained by firms that also start importing at the same time.

The "heterogeneity" analysis allowed us to understand that the "treatment effects" are not homogeneous, but rather that they vary with respect to firms' features (size, sector, future export intensity, foreign-capital share, specialized-workers share, initial wage level, initial productivity level, destination of exports and comparative-advantage level of exports). In fact, for the first time for Portuguese firms, we shed some light on the channels of LBE; in that respect, we noticed that new exporters in comparative disadvantage sectors benefit more from export participation, which could support the hypothesis that competition and technology spillovers, are significant channels through which exports may affect firm's productivity. We also observed that LBE effects are not noticed for firms that export only to Non-Developed countries; it seems to exist a hierarchy of LBE effects as Portuguese firms move their exports to countries of high development levels or as they obtain superior export intensity.

In future studies on this line of research, it would be important to clarify some issues. We stress two of them: (i) in line with Fryges and Wagner (2008), that apply the generalised propensity score (GPS) methodology (which allows for continuous treatment of different export intensity) it would be interesting to test it for Portuguese firms and in that way to test if LBE is a linear relationship or not; (ii) by joining our data to data on innovative performance of firms, it would be possible to test the connections between LBE and firms´ innovation ability. A final question arises: is LBE the result of active learning that firms perform accordingly to their learning capacities or does the very fact of being in international markets generate passive learning that always occurs in those competitive environments even if the firm does not have the ability to enhance it? Can we find merit on firms that achieve LBE?

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APPENDIXES

APPENDIX A. Percentual differential between the weight of each industrial sector in export starters and in all exporters (1997-2002)

CAE	15	17	18	19	20	21	22	24	25	26
Dif (p.p.)	+3	-2	-3	-2	+3	0	+3	0	0	-1
CAE	27	28	29	31	32	33	34	35	36	37
Dif (p.p.)	+2	+1	+2	+1	0	0	0	+1	0	0

Source: Own calculations.

APPENDIX B. Number of repeated starters

	Starter 1999	Starter 2000	Starter 2001	Starter 2002
Starter 1997	9	6	8	10
Starter 1998	-	6	2	9
Starter 1999	-	-	3	7
Starter 2000	-	-	-	7

Source: Own calculations.

APPENDIX C.1. Treated and controls using the more restrict concept:

Starter is a firm that exports in year t but does not export in years: t-1, t-2 and t-3

Control is a firm that does not export in years: t, t-1, t.2 and t-3

Assessing the matching quality - values at t-1

Unmatched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.093	91.98	5.345	0.722	12.09	0.116	0.023	0.011	0.337
Control	1.048	78.10	4.845	0.425	11.43	0.079	0.042	0.022	0.212
T test	0.57	1.33	0.49	2.82	1.08	6.60	-0.86	-0.68	2.67

Matched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.096	81.54	5.406	0.731	12.16	0.111	0.023	0.012	0.341
Control	1.098	85.45	5.398	0.665	11.99	0.104	0.027	0.013	0.309
T test	-0.08	-0.31	0.01	0.34	0.20	0.66	-0.15	-0.09	0.45

Source: Own calculations

Assessing the matching quality – values at t-2

Unmatched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.111	89.26	4.990	0.523	12.49	0.108	0.061	0.012	0.086
Control	1.060	78.16	4.703	0.444	11.93	0.089	0.043	0.021	0.059
T test	1.32	1.15	0.34	0.73	1.09	4.44	0.99	-0.71	1.41

Matched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.111	89.26	4.990	0.523	12.46	0.108	0.061	0.010	0.086
Control	1.059	82.14	4.991	0.491	12.29	0.102	0.050	0.009	0.09
T test	0.53	0.45	0.00	0.43	0.15	0.80	0.35	0.01	-0.23

Source: Own calculations

Assessing the matching quality – values at t-3

Unmatched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.037	76.11	4.56	0.452	12.45	0.102	0.061	0.010	0.058
Control	0.986	79.43	4.50	0.433	12.00	0.091	0.039	0.023	0.052
T test	0.76	-0.29	0.11	0.76	1.05	3.60	1.05	-0.70	0.17

Matched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.037	76.11	4.56	0.452	12.46	0.102	0.061	0.013	0.058
Control	1.011	77.79	4.56	0.444	12.32	0.097	0.051	0.012	0.055
T test	0.29	-0.10	0.00	0.32	0.09	1.15	0.33	-0.17	0.06

Source: Own calculations

APPENDIX C.2. Treated and controls using the intermediate concept:

Starter is a firm that exports in year t but does not export in years t-1, t-2

Control is a firm that does not export in years t, t-1, t-2.

Assessing the matching quality - values at t-1

Unmatched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.011	86.74	4.89	0.475	10.86	0.149	0.057	0.354	0.314
Control	1.000	73.83	4.35	0.406	10.97	0.119	0.422	0.236	0.198
T test	0.80	2.08	0.87	0.97	-0.24	7.30	1.04	1.06	4.00

Matched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.011	86.74	4.89	0.475	10.86	0.149	0.057	0.354	0.314
Control	1.019	78.50	4.43	0.447	10.90	0.145	0.559	0.303	0.291
T test	-0.17	0.88	0.47	0.32	-0.10	0.99	0.07	0.30	0.53

Source: Own calculations

Assessing the matching quality - values at t-2

Unmatched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.009	86.45	4.431	0.399	10.32	0.149	0.070	0.035	0.313
Control	0.978	73.89	4.171	0.401	10.33	0.121	0.043	0.023	0.204
T test	0.69	2.03	0.43	-0.03	-0.02	7.30	1.84	1.04	3.72

Matched sample

	TFP	Employees	Capital	Invest.	Wages	Pscore	Skill	Forcap	Imports
Treated	1.009	86.48	4.432	0.399	10.32	0.149	0.070	0.035	0.313
Control	1.008	84.85	4.880	0.409	10.37	0.145	0.060	0.301	0.288
T test	0.02	0.15	-0.35	-0.13	-0.13	0.99	0.45	0.26	0.57

Source: Own calculations

APPENDIX D. ATT effects: PSM-DID estimations; covariates at *t*-2

	t / *t*-2	*t*+1 / *t*-2	*t*+2 / *t*-2	*t*+3 / *t*-2	*t*+4 / *t*-2	*t*+5 / *t*-2	*t*+6 / *t*-2
TFP	-0.004^{+}	0.012^{+}	0.037^{+}	0.047^{+}	0.105*	0.111*	0.065*
LP	-0.003^{+}	0.012^{+}	0.041^{+}	0.052^{+}	0.095*	0.103*	0.053*
Sales	0.032*	0.042*	0.048^{+}	0.117**	0.125*	0.047^{+}	0.037^{+}
ULC	-0.008^{+}	0.003^{+}	0.009^{+}	-0.047*	-0.025^{+}	0.002^{+}	0.001^{+}
CI	0.017^{+}	0.023^{+}	-0.014^{+}	0.017^{+}	0.089*	0.012^{+}	0.010^{+}
Number Treated	705	703	468	361	267	141	101
Number Controls	2550	2547	1702	1185	798	481	203

Source: Own calculations.
Notes: See Table 15.

APPENDIX E. Relative specialization of Portuguese exports (1995-2004);

Balassa Index for Portuguese industries (average for 1995-1999 and 2000-2004)

Sector code	Sector Description	Balassa index	Technological level
15, 16	Food, beverages and Tobacco	1.0	Low
17, 18, 19	Textiles, wearing apparel and leather	3.4	Low
20, 21, 22, 36	Wood, pulp and paper; printing; furniture	2.2	Low
24	Chemicals	0.5	Medium
25	Rubber, plastic	0.9	Medium
26	Non-metalic mineral product	2.6	Medium
27	Basic metals	0.3	Medium
28	Fabricated metal products	1.1	Medium
29	Machinery	1.3	Medium-High
30	Office machinery and computers	0.3	High
31	Electrical machinery	1.3	Medium-High
32	TV and communication equipment	0.6	High
33	Medical, precision and optical instruments	0.3	High
34	Motor vehicles	1.3	Medium-High
35	Other transport equipment	0.7	Medium-High
37	Recycling	0.8	Low

Source: Adapted from Amador et al. (2007).

APPENDIX F. Comparative level of Starters in each year

	Weight of firms of Group 5	**Export intensity**	**Weight of EU and Dev export destination**	**Import share**	**Initial average TFP level**	**Initial size (number of employees)**
1997	65	93	81	58	79	94
1998	91	94	90	93	85	91
1999	100	100	100	100	90	84
2000	97	60	91	96	89	85
2001	79	76	90	93	83	100
2002	85	83	100	95	100	92

Source: Own calculations.
Note: In each column, values in percentage relative to the year of highest performance (100).