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Exporting and firm performance: Evidence from India

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Apoorva Gupta, Ila Patnaik and Ajay Shah

National Institute of Public Finance and Policy
New Delhi

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Apoorva Gupta Ila Patnaik Ajay Shah *

Abstract

The positive correlation between firm productivity and export status is well established. This correlation can arise from multiple alternative casual models. We investigate these relationships, harnessing the transition of several firms from serving the domestic market to exporting, in a dataset of Indian firms from 1989 to 2015. Each firm which made the transition is matched against a control which did not. The transitions take place across many years, thus permitting a matched event study in firm outcomes. We find there is self-selection of more productive firms into exporting. Firms that make the transition become bigger, but there is little evidence of learning by exporting, of improvements in productivity right after exporting commences. However, there is evidence of improvement in productivity of export starters a couple of years before they begin to export.

JEL Classification: F43, L1, D24

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Contents

1 Introduction

Following Bernard et al. (1995), a growing body of empirical studies showed that exporters are more productive than non-exporters. The apparent correlation between exporting and productivity could, however, come about through alternative causal mechanisms. The standard models of modern trade theory, Melitz (2003); Helpman et al. (2004) are based on the notion that firms are heterogeneous, productivity is immutable, and the most productive ones self-select themselves into exporting. In this world, policies of export promotion yield no benefits in terms of within-firm productivity since it is immutable, but the reallocation of resources towards more productive firms can propel economic growth. Advocates of export promotion also argue that once a firm steps into the international market, learning and productivity growth takes place through exposure to better technology, increased competition in foreign markets, scale effects, etc. Thus, alternatively firms could 'learn by exporting' (LBE).

Export promotion policies ranging from microeconomic interventions (e.g. subsidised purchases of high technology or tax breaks) to macroeconomic interventions (e.g. exchange rate undervaluation or tariff reductions) occupy a prominent place in many countries, particularly when they have macroeconomic impacts through productivity growth. This has motivated a vibrant research literature in the last decade, examining the positive correlation between exports and productivity[1]. The quest of this entire literature has been to obtain persuasive causal identification of the impact of exporting upon productivity.

In this paper, we offer further refinement of the research designs of this literature. We study a large dataset of Indian manufacturing firms observed from 1989 to 2015. This is an interesting period as many firms made the *transition* from serving the domestic market to exporting. This permits the construction of a dataset with firms that transitioned into sustained exporting, matched against similar firms that did not. The event of *starting* to export is found across diverse years, which permits the use of the event study methodology in identifying the trajectory of parameters of interest – such as firm productivity – before and after the year when exporting commenced.

Datasets in this field have many firms that intermittently transition in and out of exporting. We impose requirements of clean trajectories. This yields 3391 firms which are sustained non-exporters: which do not export in any year of the sample. There are 465 firms which have two years of no export followed by three years of exporting. With matching techniques, we are able to compare export starters to non-exporters both before and after they commence exporting. This offers a unique

[1]Figure 5 shows the correlation between exporting and productivity at a sectoral level in Prowess data. It plots the average productivity for 23 sectors from 1994 to 2014 against the average number of exporters in the corresponding sector year. The correlation is positive.

opportunity to examine the phenomena of interest.

The strength of the paper lies in an opportunity for sound measurement: the opportunity to observe firms make a clean transition into exporting, spread over many years so as to support event study analysis, with matched controls who never exported.

Our findings may be summarised as follows. Our results are similar to numerous papers of the recent decade in that we find that export *starters* are different prior to becoming an exporter. They are bigger, younger, pay higher wages and are more productive, prior to exporting. We find evidence of 'learning to export' i.e. export starters show gains in productivity a couple of years before they begin to export, thus suggesting that new exporters make a conscious decision to improve their productivity before entering foreign markets. But there is no evidence of learning by exporting, that is new exporters do not become more productive after they start exporting. While they remain the same in terms of productivity, exporting has a positive impact upon size. This suggests that policies should aid firms that are *preparing* to export to realise productivity gains. Once firms enter export markets, there will be gains due to reallocation of resources to more productive firms.

The remainder of the paper proceeds as follows. Section 2 reviews the evidence thus far on self-selection and learning by exporting. Section 3 outlines the data and measurement of key variables. Section 4 discusses the methodology we have used to study the pre and post entry performance of exporters, and section 5 discusses the results. Section 6 discusses robustness tests. Section 7 concludes.

2 Empirical research on firm productivity and exporting

The empirical evidence for self-selection and LBE is drawn from datasets in many countries. Wagner (2007) reports that most studies find evidence for self-selection, while the debate on post-entry productivity growth remains active.

In this literature, one important dimension is the distinction between advanced economies and emerging economies. Exporting by a firm in an emerging economy may be particularly important, as this gives exposure to global technology, sophisticated inputs, and the pressure to produce sophisticated outputs. For instance, Goldberg et al. (2010a) show that Indian firms substantially gained from trade liberalisation through access to new imported inputs, and Bustos (2011) shows that Argentinian firms in industries facing higher reductions in tariffs upgrade their technology faster. In contrast, a purely domestic firm in an advanced economy faces competition from sophisticated firms, and hence may not gain knowledge by exporting. From the viewpoint of research design, datasets in advanced economies tend to have the property that most large firms are exporters; the transition to exporting is often not observed. In contrast, datasets in emerging economies have the advantage of seeing firms make the transition.

Another important dimension is the distinction between large and small countries. In a small country, firms that step out into the global market have a greater opportunity to achieve scale effects. On the other hand, this may not be an issue for firms in large countries.

The existing evidence for LBE from developed economies is mixed. Papers that have studied the US (Bernard and Jensen, 1999; Hung et al., 2004), Spain (Delgado et al., 2002), and Germany (Wagner, 2002; Arnold and Hussinger, 2005) find little or no evidence for LBE. On the other hand, papers which have studied Canada (Baldwin and Gu, 2003) and the UK (Girma et al., 2004; Greenaway and Kneller, 2008) found evidence for both self selection and LBE.

Evidence from emerging economies is also mixed. Papers which have examined Slovenia (De Loecker, 2007), Sub-Saharan Africa (Van Biesebroeck, 2005) and Indonesia (Blalock and Gertler, 2004) report gains in firm productivity after exporting commenced. Aw et al. (2000) shows that while learning by exporting is seen in Taiwan, this is not the case in Korea. On the other hand, Isgut (2001) for Colombia, and Clerides et al. (1998) for Colombia, Mexico and Morocco, do not find evidence in favour of LBE.

The lack of evidence for learning by exporting has often been attributed to the argument that learning is specific to a certain kind of firm, and studying average treatment effect can nullify these differences in learning. This has motivated exploration of heterogeneous treatment effects. Learning from exporting has been found to be more pronounced for firms that belong to an industry which has high exposure to foreign firms (Greenaway and Kneller, 2008), are younger (Delgado et al., 2002), or have a greater exposure to export markets (Kraay, 1999; Castellani, 2002).

Another line of thought suggests that firms do not learn *from* exporting but learn *to* export. Alvarez and Lopez (2005) argue that productivity changes occur after the decision to start exporting, and firms most likely invest in new technologies *before* entering foreign markets to be able to compete internationally. Iacovone and Javorcik (2012) find that firms improve quality exactly one year *prior* to entering export markets and there is no upgrade after entry. Hallward-Driemeier et al. (2002) find that the firms that explicitly target export markets make systematically different decisions and thus raise their productivity.

This literature provides the setting for three groups of questions about export starters. How do they differ from non-exporting firms? What changes in productivity are observed before commencing exports? What changes in productivity are observed after commencing exports?

Firm data in India is well developed, and a small literature has worked on related questions. Mallick and Yang (2013) and Ranjan and Raychaudhuri (2011) use a panel of Indian firms and find evidence for both self-selection and learning by exporting. Tabrizy and Trofimenko (2010) and Haidar (2012) find evidence for self-selection but not for learning by exporting. The paper that comes closest to our paper is Haidar (2012), and we improve upon it in many aspects of the research methodology. We use data on Indian manufacturing firms from 1989-2015, and are able to exploit the transition of many more firms into exporting to study this question. In addition, we use event study methodology, which shifts from physical time to event time, and thus yields improved causal interpretation. Moreover, in addition to examining if firms self-select, or learn by exporting, we examine if firms learn to export, that is we study both the pre and post-entry growth of export *starters*.

3 Data and descriptive statistics

We source firm level data from the Prowess database provided by the Centre for Monitoring Indian Economy (CMIE).[2] We restrict the analysis to manufacturing firms since their exporting activity is easily distinguishable, and the theoretical foundations of services exports may differ significantly from export of goods as transportation costs are zero (Bhattacharya et al., 2012; Wagner, 2014)[3].

CMIE Prowess currently has data for approximately 11,000 manufacturing firms from 1990 onwards. The data allows us to follow firms over time, and hence observe their transition into exporting. However, data is sometimes not available or are reported as missing[4]. After cleaning the data, we get 61892 observations for 8134 firms. Approximately 8 years of data is available for each firm. Table 1 provides summary statistics of the data. There is a lot of heterogeneity in the data in terms of firm size, age, capital intensity etc, and we see that exporters are larger, more capital intensive, older, and more productive[5].

In this sample, about 47-60% of the firms in each year report positive earnings from export. The mean export value to domestic sales ratio for the sample is stable at

[2]Many important papers in international trade have used this dataset previously (Goldberg et al., 2010b,a).

[3]Manufacturing firms are defined as firms for which revenue from industrial sales is atleast 50 percent of the total sales. Manufacturing companies in CMIE Prowess form 79% of the value of output of the registered manufacturing sector of India in 2008-09. CMIE also has a well-developed 'normalisation' methodology which ensures inter-year and inter-firm comparability of accounting data.

[4]We exclude observations for which data on sales, total assets, gross fixed assets, wage bill, and raw material expenses are missing. We also exclude observations where sales is less than Rs. 5 million.

[5]Firms are under no legal obligation to report to CMIE, and hence small, most likely domestic, firms are less likely to report their financial statements. However, since we focus on exporting in this paper which is generally observed in large firms, we need large domestic firms for our matching-based methodology. Thus the lack of representativeness of small domestic firms is not likely to affect our results.

Table 1 Summary statistics

All variables are in Rs. million. All nominal series have been converted to 2014 prices using the Wholesale Price Index. The distribution for all variables is positively skewed, which indicates that there is a large number of small firms in the dataset. The last column reports the percentage of exporters in the total number of observations for each variable.

Variable	Category	Median	IQR	Observed
Sales (INR Million)	All firms	826.84	2422.75	61892
	Exporters	1542.37	4221.56	54.7 %
	Non-Exporters	388.05	967.50	45.3 %
Total assets (INR Million)	All firms	727.32	2255.82	61892
	Exporters	1445.62	4197.45	54.7 %
	Non-Exporters	344.72	769.31	45.3 %
Gross fixed assets (INR Million)	All firms	401.62	1290.87	61892
	Exporters	771.40	2313.54	54.7 %
	Non-Exporters	204.20	467.63	45.3 %
Wage bill (INR Million)	All firms	36.06	114.21	61892
	Exporters	76.95	208.86	54.7 %
	Non-Exporters	13.95	36.11	45.3 %
Age (Years)	All firms	20.00	18.00	61665
	Exporters	22.00	21.00	54.8 %
	Non-Exporters	18.00	16.00	45.2 %
Raw material expenses (INR Million)	All firms	407.86	1234.78	61892
	Exporters	732.12	2030.43	54.7 %
	Non-Exporters	196.74	564.72	45.3 %
Power expenses (INR Million)	All firms	29.60	103.07	60620
	Exporters	50.53	175.22	55.1 %
	Non-Exporters	16.40	50.68	44.9 %
Export revenue (INR Million)	All firms	3.59	197.71	61892
	Exporters	152.58	655.98	54.7 %
	Non-Exporters	0.00	0.00	45.3 %
TFP (LP) ()	All firms	1.61	0.81	55107
	Exporters	1.65	0.78	54.8 %
	Non-Exporters	1.57	0.85	45.2 %

12-13% (see Table 9 in the appendix). There are exporters in all industrial sectors, but there is considerable variation in the internationalisation by sector. For the year 2007, 59% of the firms in Chemicals, 66% in Transport equipment and 71% in Non-electrical machinery industry were exporting, while only 30% in Paper and Pulp industry were exporting.

3.1 Productivity measurement

To measure firm level productivity, we assume that the production function at the firm level is the logarithm of the Cobb-Douglas function.

$$y_{it} = \beta_0 + \beta_1 k_{it} + \beta_2 l_{it} + w_{it} \tag{1}$$

where y_{it} represents the logarithm of firm output, k_{it} and l_{it} represent the logarithm of capital and labour respectively, and w_{it} is the productivity component. This equation cannot be estimated consistently using ordinary least squares regression since unobservable productivity shocks and input levels are correlated. We use the semi-parametric estimator for total factor productivity developed by Levinsohn and Petrin (2003) (TFP-LP henceforth). This measure uses intermediate inputs as a proxy, arguing that intermediaries may respond more smoothly to productivity shocks.

We estimate TFP-LP for each two-digit National Industrial Classification code separately. We use raw material expenses deflated by Core Wholesale Price Index (WPI-Core) as the proxy in the TFP-LP methodology.[6] Output is calculated as sales deflated by industry specific WPI series, and capital is calculated as gross fixed assets divided by WPI-Manufacturing. Labour is estimated by deflating total wage bill by Consumer Price Index for the Industrial Workers (CPI-IW). The productivity measure is made comparable across industries by demeaning it using industry means (Petkova, 2012).[7]. Figure 6 shows the growth of TFP, averaged over all firms in this data, since the early 1990s. Figure 1 compares the distribution of productivity for exporters and non-exporters in our sample. While there is a large overlap, on average exporters are more productive than non-exporters.

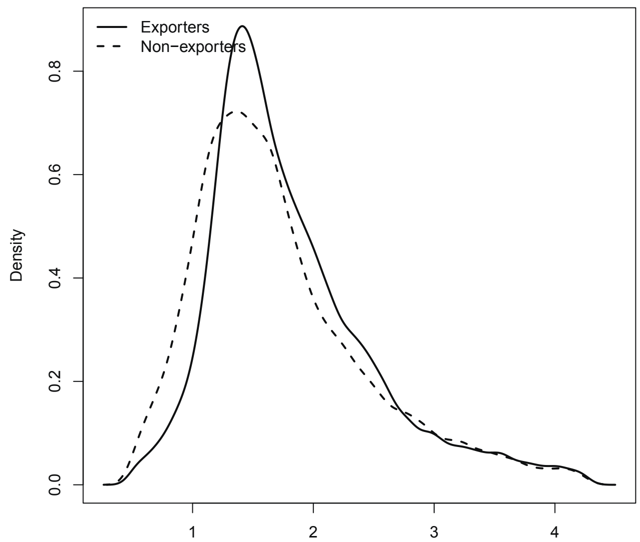
3.2 Defining export starter

We categorise firms in our dataset into one of the following sets.

[6]Core WPI is measured as WPI-All commodities minus WPI-Food articles and WPI-Fuel.

[7]We use the Stata command *levpet* for estimating TFP. The estimation methodology in Stata, when gross revenue is the dependent variable, is discussed in Petrin et al. (2004).

Figure 1 Productivity distribution by export status



- Constant exporters export continuously in the sample period.
- Constant non-exporters do not export in any year of the sample period.
- Entrants are non-exporters that become exporters and remain exporters for the duration of the sample.
- Quitters are exporters, who exit the export market and do not re-enter during the sample period.
- Flip-flops change export status more than once in the sample.
- Missing data includes firms for which we do not have a continuous time series of export sales. We cannot categorise these firms into any of the above sets, but neglecting this set can lead to sample selection bias and a reduction in our sample size.

Table 2 Categorisation of firms based on exporting trajectory

Category	Percentage of firms
Constant exporter	21.59%
Constant non-exporter	31.57%
Entrants: One switch from non-exporter to exporter	5.47%
Quitters: One switch from exporter to non-exporter	3.34%
Flip-flop	6.90%
Missing data	29.90%
Total	100%

Table 2 classifies the dataset into these categories based on the time period for which we observe a firm in our dataset. A large percentage of the firms consistently export (22%); these play no role in causal identification. There is a large group of firms who never export (32%); these contribute to the control pool. 6% of the firms make a one-time switch from non-exporter status to exporter status: these are the opportunities to identify the impact of exporting. 3% of the firms quit exporting once in the dataset. 7% of the firms enter or exit exporting more than once.

The fact that there is a large percentage of firms in the 'Constant exporter' and 'Constant non-exporter' category, and a small percentage of firms in 'Flip-flop' category suggests that there is something inherently different about exporters, as compared to non-exporters. Not many firms are trying and failing, or flip-flopping.

There are two distinct empirical questions. The first is: do firms raise their productivity prior to exporting? The second is: does productivity change after exporting commences? For the former, we require observations of a firm for a few non-exporting years prior to the first year of exporting. For the second question, the firm must undertake sustained exporting, through which there is a possibility of observing the impact on productivity over a multi-year period. This requires observing a clean trajectory of a firm which makes one jump into exporting, and then sustains exporting for several years.

Table 3 Are exporters different?

Outcome variable	Coefficient on export dummy
Log(Gross fixed assets)	1.34 (0.033) ***
Log(Wage bill)	1.43 (0.031) ***
Log(Sales)	1.44 (0.033) ***
Log(Total assets)	1.26 (0.03) ***
Total factor productivity (LP)	0.1 (0.01) ***

1)$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$
2)Robust clustered standard errors are reported in brackets

We define an 'export starter' as any firm that does not export for atleast two years, and then exports for atleast the next three consecutive years. This definition gives us 465 export 'starters' or 'treatment' firms, which begin to export in different years in our sample period. Firms which make the transition into exporting need to be compared against firms which have uncontaminated trajectories of zero export. There are 3391 firms in our dataset which do not export in any year during the sample period[8].

3.3 Superior exporter performance

The literature has established that exporters are different from non-exporters in important ways (Bernard et al., 1995). We replicate this analysis with our dataset using the following specification.

$$y_{it} = \alpha + \beta EXP_{it} + \delta_t + \lambda_k + \epsilon_{it} \tag{2}$$

where y_{it} is the firm characteristic for firm i at time t. EXP_{it} is an export dummy equal to one if firm i reports positive earnings from exports in period t. δ_t are year dummies, and λ_k are industry dummies. The estimate of β for different firm characteristics is reported in Table 3. It is clear that exporters are superior to non-exporters. They are bigger, have a higher wage bill, higher sales, and are also more productive than the non-exporters. This is a simple correlation and has no causal implication of exporting on firm performance. In the next section we outline our research design to casually estimate the impact of exporting on firm performance.

[8]The categorisation of firms into continous non-exporters group is based on the time period for which we observe a firm in the data. We can erroneously classify a firm as a continuous non-exporter if the firm starts exporting after we last observe it in the dataset, or was an exporter before we first observe it. We acknowledge the limitation of this classification scheme.

4 Research Design

To study the causal impact of a treatment, we need to evaluate $w_{is}^1 - w_{is}^0$, where w is the outcome of interest for firm i at time s, and the superscript is equal to 1 for treatment firm and 0 for the counterfactual, that is for a situation where the treated unit did not receive treatment. But we do not observe w_{is}^0. Hence, we need to find a counterfactual, and for that we use propensity score matching (PSM) to control for self-selection and construct a counterfactual for export starters. Our research strategy runs through six steps:

1. We use clean export status trajectories to define our treatment group. An export starter is defined as any firm that does not export for atleast two consecutive years, after which it exports for atleast three consecutive years.
2. Our control group includes firms that do not export in any year during our sample period.
3. We calculate the probability to *start* exporting for firms in the treatment and control group, using firm characteristics with a one-period lag[9]. This is done using a logit model with both time and industry fixed effects. We estimate the propensity to start exporting from this model, and remove observations with a propensity score in the 1% tails on both sides of the distribution[10].
4. We use the estimated propensity score from the logit model to do nearest-neighbour matching without replacement in each year such that if P_{it} is the predicted probability of entry at time t for firm i (a firm in the treatment group), a non-exporter j is chosen as its matched partner if its probability to enter export markets is closest to P_{it} amongst all non-exporters in year t (Rosenbaum and Rubin, 1983). That is, if a firm starts exporting in 2002, we look for a firm from the control pool that had a similar propensity to export in 2002, but did not export. It is important to note that PSM is based on the assumption of selection on observables, and does not control for bias due to non-observable characteristics affecting probability of treatment.
5. We use a caliper matching method to ensure a region of common support, that is, if for a treated firm we do not find a close enough control unit, we drop the firm from subsequent analysis. We check for the quality of

[9] A firm in the control group that never exports, and exists from 2004 to 2010 in the dataset has three overlapping time-periods of 5 years when it doesnt export, that is from 2004-2008, 2005-2009, and 2006-2010. This firm can be matched to a treatment firm that starts exporting in 2006, 2007 or 2008. Thus we need a propensity score for the control firm for these three years, and hence we estimate the propensity score for this control firm for 3 observations.

[10] Imbens (2015) suggests that removing observations with estimated propensity score values close to zero or one makes the estimates robust to the choice between logit and probit models.

our matching by calculating the standardised difference, and Kolmogorov Smirnov test for the treatment and control group firms. These tests tell us if the treatment and control group are balanced after matching as compared to before matching. If they are not balanced we repeat step 3 to respecify the propensity score regression, or repeat step 4 with a stronger caliper until we obtain good match balance.

6. Once matched pairs are obtained, they are all re-expressed in event time where the first year of exporting is the event for which $s = 0$. We study the difference in performance of these groups at $s \in 1, 2, 3$, that is one, two and three year horizon after treatment.

This research design incrementally improves upon the existing literature in numerous directions. Greenaway and Kneller (2008) and De Loecker (2007) define export-starters as firms that are non-exporters, and then switch to become exporters, and remain exporters henceforth. This definition of treatment only picks up firms that after becoming an exporter, survived in the export market for the full duration of the sample. Thus it ignores all firms that tried and failed, and this could lead to an upward biased estimate of learning by exporting. To estimate firm propensity to export, Mallick and Yang (2013) use a logit specification with contemporary firm characteristics, thus ignoring that starting to export itself can have an impact on firm characteristics contemporaneously. Haidar (2012) use a similar definition as ours to define export-starters, however for only 1994 and 2001, and they estimate learning by exporting for each of the two years separately . Our event-study design allows us to pool outcome variables for all export starters, from 1998 to 2010, and estimate the average treatment effect over many years.

While PSM allows us to match on multiple firm characteristics, recent work by King and Nielsen (2016) suggests that PSM could increase imbalance, model dependence, researcher discretion, and bias if not used judiciously. We report detailed match balance statistics, both before and after matching, to ensure that PSM is not making the imbalance in the data worse. In addition to checking the balance for variables used in estimating the propensity score, we also check balance for variables not included in the model. Our event study methodology allows us to use a large number of matched pairs for estimating treatment effects and therefore increases efficiency.

5 Results

In this section, we ask the following questions to establish how exporting and firm characteristics impact upon each other following the research design discussed above:

1. Do more productive firms self-select to become exporters?

2. Do firms learn to export?
3. Do firms learn by exporting?
4. Do export starters grow significantly after export market entry?

Do more productive firms self-select to become exporters?

To study if better firms self-select themselves into exporting, we look at how firm characteristics in $t-1$ affect the probability to *start* exporting for export starters. Here $START_{it}$ is a dummy variable which is equal to 1 for firm i that *begins* to export in year t, and 0 for firm i belonging to the control group in year t.[11] We estimate a logit model:

$$START^*_{it} = \alpha + \beta Productivity_{it-1} + \gamma wagebill_{it-1} + \delta age_{it-1} + \epsilon_{it} \tag{3}$$

such that
$START^*_{it} > 0$, if $START_{it} = 1$
$START^*_{it} <= 0$, if $START_{it} = 0$

where the controls are three year averages of productivity, wage bill (as a proxy for size of the firm) and age of i in $t-1$. To control for industry specific comparative advantage and proclivity to internationalise, we use industry fixed effects. We also use year fixed effects to control for macroeconomic changes. All variables are in logs.

The logit estimates are shown in Table 4. We show the results with five different measures of productivity: Total factor productivity using Levinsohn Petrin methodology, Labour productivity, Capital productivity, Cobb-Douglas OLS residual[12], and log-value of the ratio of Profit after tax (PAT) to sales of a firm. The calculation of labour and capital productivity is discussed in section 6. Our results indicate that the probability of starting to export is greater for more productive firms, across all measures of productivity. The probability is higher for younger firms and those that have larger wage bills.

[11] If we use the export dummy EXP_{it} as defined in equation 2 we would estimate the propensity to export of a firm i in any given year t. We are interested in the determinants of a firm *starting* to export. Hence, we use $START_{it}$ to calculate the propensity to *start* exporting.

[12] We estimate w_{it} in equation 1 using a simple linear regression model. We deflate the variables in the same way as done for TFP-LP estimation

Table 4 Self-selection

	Model 1	Model 2	Model 3	Model 4	Model 5
Intercept	-19.32	-17.13	-16.72	-17.42	-16.94
	(2399.54)	(1455.40)	(1455.40)	(1455.40)	(2399.54)
$Log(Age)_{it-1}$	-0.19^{*}	-0.12	-0.25^{**}	-0.24^{**}	-0.29^{***}
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
$Log(WageBill)_{it-1}$	0.40^{***}	0.51^{***}	0.41^{***}	0.44^{***}	0.45^{***}
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
$TFP(LP)_{it-1}$	0.58^{***}				
	(0.10)				
$LabourProd_{it-1}$		0.50^{***}			
		(0.07)			
$CapitalProd_{it-1}$			0.24^{***}		
			(0.06)		
$OLS - Residual_{it-1}$				0.82^{***}	
				(0.18)	
$Log(PAT/Sales)_{it-1}$					0.49^{***}
					(0.09)
N	5565	5301	5436	5478	4849
AIC	2984.75	2891.94	2973.14	3006.41	2709.62
BIC	4071.13	3996.65	4082.07	4116.64	3799.36
$\log L$	-1328.38	-1277.97	-1318.57	-1335.20	-1186.81

All variables are 3 year averages
† significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Do firms learn to export?

López (2009) proposed that selection of firms into exporting may involve an intermediate stage where firms undertake initiatives that increase their productivity with the explicit purpose of becoming exporters. Thus firms might be 'learning to export'. Firms that want to compete in global markets, especially those operating in developing countries, may have to buy new technology and upgrade the quality of their goods before they start exporting. This process could yield productivity gains for the firms ahead of time.

In order to explore this hypothesis, we study the productivity premium of export starters versus non-exporters before they begin to export. We need to compare export starters against similar non-exporting firms. We use Mahalanobis distance matching to match an export starter with a non-exporter, using firm characteristics from three years before the firm starts exporting. We match the firms on productivity, size, wage bill, and age. This gives us 213 matched pairs. We check for match balance using Kolmogorov-Smirnov test. The results, reported in table 6, show that the null of no difference before matching is rejected, while after matching it cannot be rejected for various firm characteristics. This suggests that we have succeeded in identifying two groups of firms who were similar in year -3 in event time.

Table 5 Treatment group, control group, and matched pairs

Since we impose a caliper, we get matches for a fewer number of treated firms than the total firms in the treatment group. For example, in 2006, the number of treated firms is 29, but we get matches for 24 firms only.

Year	Number of controls	Number of treated	Matched pairs
1999	78	11	6
2000	256	25	17
2001	282	34	26
2002	312	32	24
2003	398	34	29
2004	438	42	35
2005	515	29	28
2006	452	29	24
2007	367	18	15
Total	240	12	9
	3338	266	213

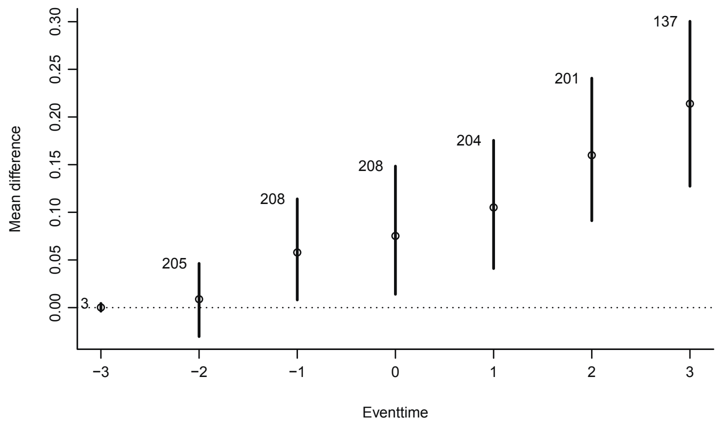
Table 6 Kolmogorov-Smirnov match balance test

This shows Kolmogorov-Smirnov test statistics for equality of distribution between the two groups of firms. P-values are in brackets. As an example, in the raw data, we can reject the null of equality of the distribution of the LP TFP, but after matching the null cannot be rejected.

	Before Matching	After Matching
$TFP(LP)_{i,t-1}$	0.1137	0.1048
	(0.0092)	(0.3306)
$Log(Size)_{i,t-1}$	0.195	0.0613
	(0)	(0.9141)
$Log(Salary)_{i,t-1}$	0.1832	0.0568
	(0)	(0.951)
$Log(Age)_{it-1}$	0.036	0.0492
	(0.937)	(0.9879)

Figure 2 Learning to export: Productivity premium

The black dot in the graph is the estimate of the statistic calculated using equation 4. The vertical black lines depict the bootstrapped 95% confidence interval. The dotted horizontal line is a reference line for no statistically significant difference between the matched pairs. The number of observations available for each event time s are mentioned on the top of each black line. Out of 213 matched pairs, we have data for 208 matched pairs at event time -1, 205 matched pairs at event time -2, and so on.



We calculate the productivity premium of export starters as follows:

$$\frac{1}{N_s}(w_{i,s} - w_{j,s}) \tag{4}$$

where w_i is the TFP of export starter i, w_j is the TFP of its matched non-exporter, and N_s is the number of matched pairs at s. We rescale time such that s=0 when an export starter exports for the first time. Productivity premium is the difference in the productivity of the treated firm and its matched control firm. Table 6 shows that we have good match balance on firm productivity in event time -3, and hence the difference in productivity in $s = -2, -1, 0, 1, 2, 3$ shown in figure 2 is the average treatment effect of exporting at different time horizons[13].

The black dot in figure 2 is the mean productivity difference of the 213 matched pairs. The vertical black lines represent the 95% confidence interval using bootstrapped standard errors. The point estimate shows a productivity gain of roughly 7.5% from $s = -3$ to $s = 0$, and an increase of 21% from $s = -3$ to $s = 3$. Thus export-starters experience a significant increase in productivity compared to similar non-exporters a couple of years before they start exporting. This could be because firms take the decision to export some years before they actually report sales from

[13] We have matched export starters and non-exporters in s=-3, and hence do not expect to see a significant difference in productivity

exporting, and this is the time when they invest in productivity enhancing technology. For instance, López (2009) find that increase in productivity of export starters is accompanied by increases in investment during the 2 years preceding entry. The delay between taking the decision and actually exporting can be large in developing countries like India due to the administrative clearances needed to start exporting.

Do firms learn by exporting?

Since exporters are *a priori* better than non-exporters (see table 4), we cannot compare the performance of export starters with non-exporters directly. To study the post-entry gains we need to match an export starter to a non-exporter that is similar to the exporting firm in the year prior to the year of entry. We use propensity score matching as discussed in section 4 to control for self-selection and construct a counterfactual for export starters.[14]

The export starters, as defined above, form the treatment group and the non-exporters form the control group. We estimate the probability to export for firms in the treatment group and control group using a logit model. We control for productivity, size, wage bill, age, and industry group in the logit. We get 430 matched pairs using this technique. Table 7 shows the number of firms in the control group and treatment group, and the number of matched pairs in each year.

The caliper matching ensures that we get good matches i.e. the difference in propensity scores of the treated firm and its counterfactual is not substantially different. Table 8 shows the match balance statistics. We use the Standardised difference and Kolmogorov Smirnov-test (KS-test) to check if the treatment and control group are not significantly different based on the calculated propensity score and firm characteristics in the year prior to treatment. We achieve good match balance with the distribution of the propensity scores, productivity, size and wage bill being very similar in both groups after matching. For example, the standardised difference for propensity score before matching is 0.91 and almost 0 after matching. Similarly, in the KS-test, while the p-value is 0 before matching, it is almost 1 after matching for the propensity score, showing that the distribution for the treated and the corresponding control firms is not significantly different.

For the matched pairs, we calculate the productivity difference as in equation 4 for $s = 0, 1, 2, 3$ where s is the rescaled time such that 0 is the time at which a treated firm starts exporting. w is firm productivity. We bootstrap the statistic in equation 4 to obtain standard errors.

Figure 3 shows the impact of exporting on productivity premium of export starters from the time they start exporting to three years after it. The mean difference in productivity (black dot in the figure) is not statistically different from zero at a

[14] Girma et al. (2004) and De Loecker (2007) use a similar methodology for UK and Slovenia, respectively, to study learning by exporting.

Figure 3 Mean difference in productivity of treatment and control

The black dot in the graph is the estimate of the statistic calculated using equation 4 for firm productivity. The vertical black lines depict the bootstrapped 95% confidence interval. The dotted horizontal line is a reference line for no statistically significant difference between the matched pairs. The number of observations available for each event time s are mentioned on the top of each black line. Out of 430 matched pairs, we have data for 390 matched pairs at event time 1, 371 matched pairs at event time 2, and so on.

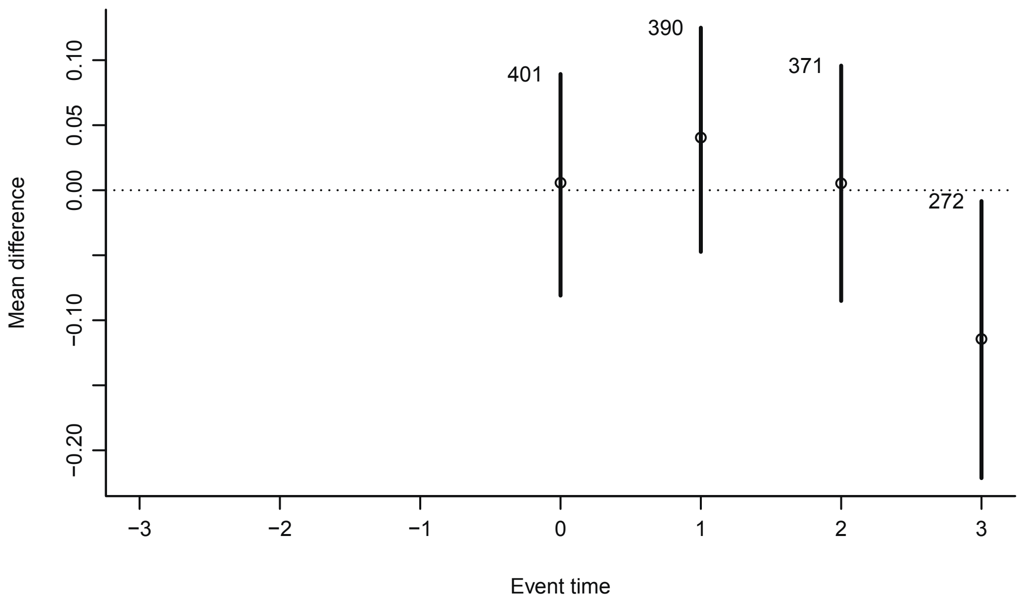


Table 7 Treatment group, control group, and matched pairs

Since we impose a caliper, we get matches for a fewer number of treated firms than the total firms in the treatment group. For example, in 2006, the number of treated firms is 54, but we get matches for 53 firms only. This leads to loss in data, but we get a better match balance and can do a robust analysis for the outcome variable.

Year	Number of controls	Number of treated	Matched pairs
1998	5	2	1
1999	11	3	1
2000	49	6	5
2001	98	11	11
2002	285	42	39
2003	318	37	32
2004	341	47	43
2005	426	45	40
2006	495	54	53
2007	620	69	66
2008	598	46	43
2009	470	40	36
2010	380	29	28
Total	250	20	19
2012	184	14	13
	4530	465	430

horizon of zero, one and two years after the firm starts exporting. The difference in the productivity of firms three year after the firm starts exporting decreases significantly. However, we do not observe this decrease in productivity in the robustness checks discussed in section 6.

Thus the above empirical analysis rejects the hypothesis of learning by exporting[15]. Exporter starters do not see a significant increase in productivity growth compared to matched non-exporters at a three-year horizon.

Do export starters grow significantly after export market entry?

$$\frac{1}{N_s}(Size_{i,s} - Size_{j,s}) \tag{5}$$

[15] The above analysis considers learning as an average treatment effect across all matched pairs. We explore if learning is heterogenous and if certain firm characteristics are correlated with high learning effects. We find that for quartiles based on age and size of firm in the period before entry, there is no learning by exporting at a horizon of one, two, and three years. There is mild evidence of export-starters in the first size quartile having lower productivity than matched counterfactuals at a horizon of three years. On the other hand, firms in the third size quartile show mild evidence of a positive differential in productivity premium of exporters and non-exporters. Detailed results are available upon request.

Table 8 Match Balance

The values in brackets are p-values. Both tests show that before matching treated and control firms are significantly different in terms of different firm characteristics, while after matching they are similar.

Standardised difference

	Before Matching	After Matching
Propensity Score	0.91	0.00
$TFP(LP)_{i,t-1}$	0.13	-0.01
$Log(Size)_{i,t-1}$	0.59	-0.05
$Log(Salary)_{i,t-1}$	0.55	-0.10
$Log(Age)_{it-1}$	-0.04	-0.04
$Log(Sales)_{it-1}$	0.62	0.00

Kolmogorov Smirnov test

	Before Matching	After Matching
Propensity Score	0.4424	0.0116
	(0)	(1)
$TFP(LP)_{i,t-1}$	0.1275	0.0607
	(0)	(0.4177)
$Log(Size)_{i,t-1}$	0.2563	0.0754
	(0)	(0.1769)
$Log(Salary)_{i,t-1}$	0.2633	0.0793
	(0)	(0.1351)
$Log(Age)_{it-1}$	0.0516	0.0282
	(0.1894)	(0.9959)
$Log(Sales)_{it-1}$	0.2528	0.0628
	(0)	(0.3648)

Figure 4 Mean difference in log size (at constant prices)

The black dot in the graph is the estimate of the statistic calculated using equation 4 for firm productivity. The vertical black lines depict the bootstrapped 95% confidence interval. The dotted horizontal line is a reference line for no statistically significant difference between the matched pairs. The number of observations available for each event time s are mentioned on the top of each black line. Out of 430 matched pairs, we have data for 422 matched pairs at event time 1, 419 matched pairs at event time 2, and so on.

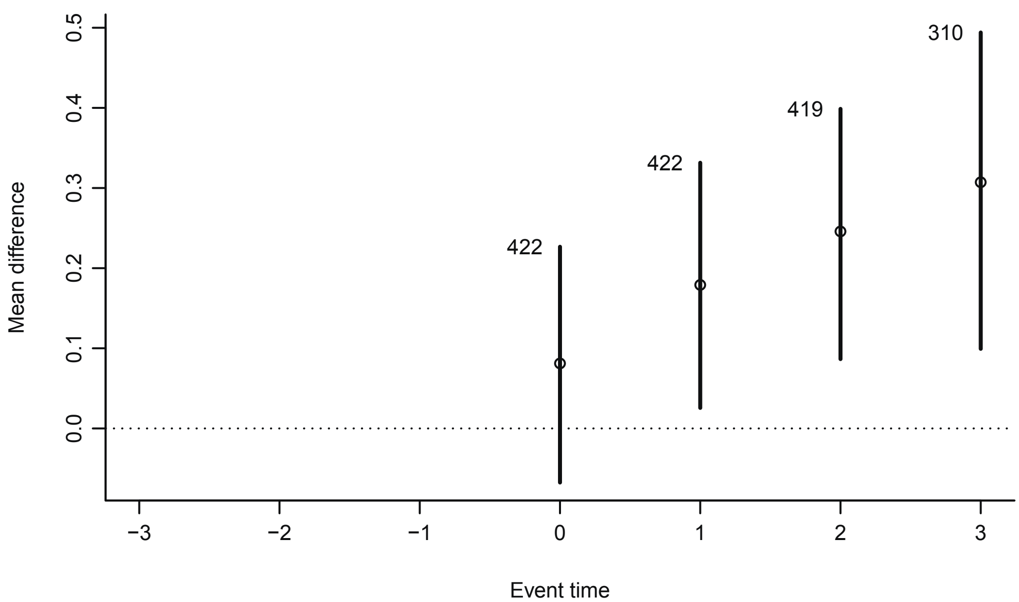


Figure 4 plots the mean difference in the log size of export starters and their matched counterfactuals at a horizon of one, two and three years after a firm starts exporting[16]. The mean difference in the size of treated and control firms increases by about 30% from $s = -1$ to $s = 3$. This is a substantial gain for export starters and is likely to lead to reallocation of resources in the industry the export starter belongs to.

6 Robustness Tests

To check the robustness of our results, we perform the following tests.[17]

6.1 Changing the definition of an export starter

Higher export-sales ratio to define export starter:

In the analysis above, we define exporters as firms with a positive value of export sales. We now define exporters as firms with atleast 2% of their sales coming from exporting. Using this definition, we find evidence for self-selection of firms into exporting, and for learning to export. We do not find evidence for learning by exporting, and do not find that exporters grow significantly more than non-exporters.

In the sections above, we define an export starter as a firm that does not export for atleast two consecutive years, and then exports for atleast the next three consecutive years. This definition of an export starter allows us to look at the impact of sustained exporting, and also gives us a large set of treatment firms. As a robustness test, we define an export starter as a firm that does not export for atleast one year, and then enters the export market and remains an exporter for atleast the next one year. Our results remain robust to this change[18]. Our results are also robust to defining an export starter as a firm that does not export for atleast three years, and then enters the export market and remains an exporter for atleast the next three years.

[16] This analysis uses the matching design used to estimate learning by exporting above. Thus we use 430 matched pairs to study the gains in size after exporting.

[17] Detailed results are available for all the robustness tests on request from the authors.

[18] This is the definition of export starter used by Mallick and Yang (2013). However, we do not find evidence for LBE and this could be because of differences in our research design, such as calculation of TFP for each industry separately with deflated values of output and capital, and calculation of propensity score with lagged firm characteristics.

6.2 Alternative measures of productivity

The main results of the paper were based on productivity estimates calculated using the Levinsohn Petrin methodology. We use two single-factor productivity ratios to assess the robustness of our results.

Labour productivity

As an alternate measure of productivity, we follow Tabrizy and Trofimenko (2010), who use the same dataset to build a proxy for labour productivity. CMIE Prowess does not report the number of employees or the number of hours worked, and hence we use wage bill as a proxy for labour input. We calculate labour productivity as follows:

$$log(VA_{it}) - log(L_{it}) \tag{6}$$

where VA_{it} is the firm-level value added, computed as total industrial sales plus change in stock minus power and fuel expenditures, and raw material expenses; and L_{it} is the total wage bill. Thus, labour productivity corresponds to value added by a firm in a year per unit spending on labour. It has an unconditional correlation of 0.39 with revenue-based total factor productivity. We acknowledge that unavailability of worker hours makes this an imprecise measure of labour productivity since it masks any systematic difference between skill composition and remuneration of workers across exporters and non-exporters.

We find evidence for self-selection of firms as reported in table 4, and mild evidence for learning to export. There are no gains in labour productivity after the firm enters export markets, however they do show substantially higher growth in terms of size than their matched counterfactuals.

Capital productivity

Capital productivity is value-added per unit of capital input used by a firm. We calculate it as follows:

$$log(VA_{it}) - log(K_{it}) \tag{7}$$

where K_{it} is the gross fixed assets of the company, and other definitions are the same as in equation 6. It has an unconditional correlation of 0.43 with revenue-based total factor productivity. Using this productivity measure, we find evidence for self-selection, but not for learning to export. There is no evidence for post-entry gains in productivity, however export starters grow at a significantly higher rate as compared to their matched counterfactuals.

We also check the results with two other productivity measures: Cobb-Douglas OLS residuals and the profit to sales ratio. We do not find evidence for learning to export with these measures, although there is evidence for self-selection. Learning by exporting is rejected, however export starters grow at a high rate.

6.3 Changing the matching methodology

In section 5, we match export starters to non-exporters in the same year. We check the robustness of our result to matching firms in the same year and same two-digit NIC industry. With industry and year level matching, we find that treated firms show significant gains in productivity at a horizon of one and two years, however this difference disappears by the third year of exporting. Exporters grow at a significantly higher rate compared to matched non-exporters. We also find evidence for learning to export when we match export-starters a couple of years before they start exporting.

Our results also remain robust to using a caliper 5 times stronger than the baseline to match firms.

6.4 Summarising the robustness checks

The basic character of our results are consistently obtained across the range of robustness checks shown above.

7 Conclusion and Policy implications

Do firms learn by exporting, learn to export, or do more productive firms self-select themselves into exporting? This is an important question which shapes our understanding of trade theory, and influences policy questions ranging from micro-economic interventions to support firms that export, to exchange rate undervaluation for economic growth.

The lack of consensus in this field suggests this is a question that requires further research. This paper explores this question, starting from a large database of firms in India, where many firms have made the transition into exporting. The unique feature of the paper is a clean research design using which the phenomenon of interest is identified. The paper examines the reasons for the differential performance of exporters as compared to non-exporters.

We start with a large database of 8134 Indian manufacturing firms from 1989 to 2015 sourced from CMIE Prowess, a period in which a large number of firms made the transition into exporting. We define export starters as firms who have been domestic for atleast two years, followed by entry into export markets and an export status for three years hence. We use propensity score matching to match an export starter with a non-exporter in the *same year* to control for any macroeconomic changes. The inference procedure is done in an event study framework with bootstrapping to study the outcome variable at a one, two, and three year horizon from the date of entry into exporting.

We find that more productive firms self-select themselves into participating in foreign markets, and that there is a significant increase in productivity before export market entry, that is firms learn to export. Firms experience large growth in terms of size after they begin to export, but rise in scale does not translate into higher productivity. Firms do not learn by exporting. However, since we find that firms grow faster after entering export markets, the gradual increase in market share of these firms would force the less productive firms to exit. This reallocation of resources towards more productive firms should propel growth in the economy (Melitz, 2003).

Our results thus reinforce macroeconometric evidence on the link between openness and economic growth. We find that firms consciously improve their productivity while preparing to enter foreign markets, and thus export promotion policies could focus on making entry into export markets easier for firms by reducing bureaucratic costs of registering as an exporter, improving market information through public support for trade missions etc. Entry of more productive firms into export markets would further lead to reallocation of resources towards them, and hence deliver a productivity boost.

One potential issue left for future research is to study the underlying changes that lead to an increase in productivity of new exporters. The literature has identified channels like technology adoption and product innovation (Bustos, 2011; Lileeva and Trefler, 2010), however improvements in management quality, or human capital are less studied. This can further direct policy in promoting trade-led-growth. Future research can also build on the average treatment effects presented in this paper for the manufacturing sector by looking at industrial heterogeneity, destination and market specific learning for firms, and business cycle dependent effects.

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Appendix

Table 9 Export Statistics by Year

	$EXPDUM = 1$	$EXPDUM = 0$
1989	53.49	46.51
1990	53.57	46.43
1991	50.00	50.00
1992	58.14	41.86
1993	53.27	46.73
1994	44.76	55.24
1995	52.57	47.43
1996	54.70	45.30
1997	49.84	50.16
1998	46.59	53.41
1999	43.14	56.86
2000	53.95	46.05
2001	54.64	45.36
2002	54.76	45.24
2003	54.54	45.46
2004	54.67	45.33
2005	52.65	47.35
2006	53.47	46.53
2007	53.84	46.16
2008	54.11	45.89
2009	53.39	46.61
2010	51.65	48.35
2011	54.22	45.78
2012	59.86	40.14
2013	65.54	34.46
2014	67.73	32.27
2015	81.82	18.18

Figure 5 Correlation between TFP and exporting: Sectoral level

On the X-axis is the average productivity of firms by NIC two-digit sectors from 1994 to 2014. On the Y-axis is the proportion of exporters in the corresponding sector year.

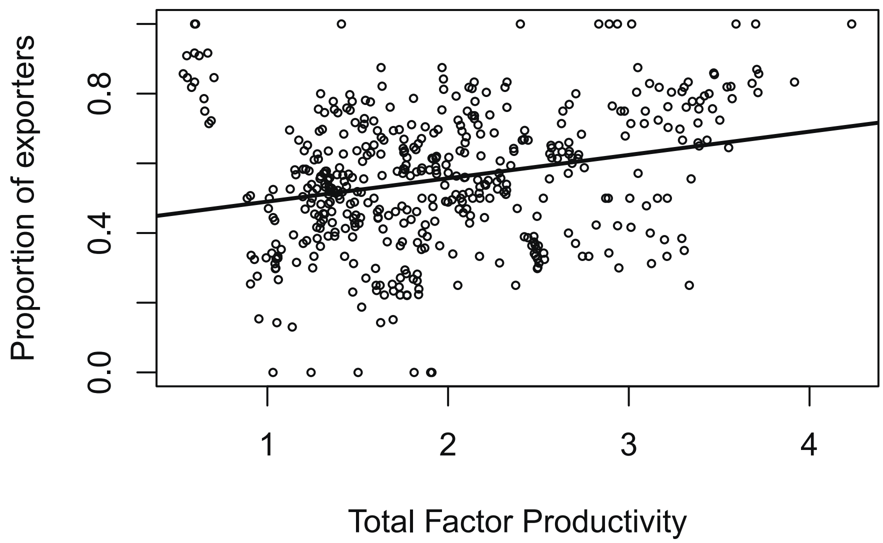

Figure 6 Average TFP over time

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Apoorva Gupta, University of Nottingham

Email: apoorvag87@gmail.com

Ila Patnaik, is Professor, NIPFP

Email: ila.patnaik@nipfp.org.in

Ajay Shah, is Professor, NIPFP

Email: ajay.shah@nipfp.org.in

National Institute of Public Finance and Policy,
18/2, Satsang Vihar Marg,
Special Institutional Area (Near JNU),
New Delhi 110067
Tel. No. 26569303, 26569780, 26569784
Fax: 91-11-26852548
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