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Exporting and performance: The impact of destination characteristics on learning effects

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Abstract

We examine the effect of export market entry on firm performance, including productivity. Our novel contribution to the literature is the treatment of export status as an incremental process, in which firms may export to one or more markets and each of these markets provides additional potential for learning to occur. Using propensity-score matching techniques on the population of New Zealand manufacturing exporters, we test whether entry into additional markets imparts new performance-enhancing knowledge. Focussing on new markets provides several benefits. Most importantly, since we match on firms' export history as well as their current characteristics, we reduce the problem of selection on unobservables (such as managerial preferences) which would confound a causal interpretation. This formulation provides a more stringent test of performance improvements from entry, as well as a much larger number of entry events on which to test our model. Finally, by splitting our sample according to characteristics of destination countries (specifically, per capita income), we are able to test whether potential performance improvements from exporting are dependent on the foreign market involved.

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This research uses data that was accessed while the authors were on secondment to Statistics New Zealand in accordance with security and confidentiality provisions of the Statistics Act 1975. Only people authorised by the Act are allowed to see data about a particular business or organisation. The results of this work have been confidentialised to protect individual businesses from identification. The analysis and interpretation of these results were undertaken while the authors were at the Reserve Bank of New Zealand. The opinions, findings, recommendations and conclusions expressed in this report are those of the authors. Statistics New Zealand, the Reserve Bank of New Zealand, Motu and the University of Waikato take no responsibility for any omissions or errors in the information contained here.

The results are based in part on tax data supplied by Inland Revenue to Statistics New Zealand under the Tax Administration Act 1994. This tax data must be used only for statistical purposes, and no individual information is published or disclosed in any other form, or provided back to Inland Revenue for administrative or regulatory purposes. Any person who had access to the unit-record data has certified that they have been shown, have read and have understood section 81 of the Tax Administration Act 1994, which relates to privacy and confidentiality. Any discussion of data limitations or weaknesses is not related to the data's ability to support Inland Revenue's core operational requirements.

Statistics New Zealand protocols were applied to the data sourced from the New Zealand Customs Service. Any discussion of data limitations is not related to the data's ability to support that agency's core operational requirements.

1 Introduction

That exporters outperform domestically focused firms has become something of an established fact in the empirical trade literature. Exporters have been found to be larger, more productive and to pay higher wages than their domestically focused counterparts. Moreover, this "exporter premium" is found to exist before firms begin exporting, suggesting that it can be largely explained as self-selection of productive firms into export markets (eg, Bernard and Jensen 1999; Greenaway and Kneller 2004; Van Biesebroeck 2005; Greenaway and Kneller 2007; Wagner 2007).

In contrast, the jury remains out on what (if any) additional benefits exporting imparts on firms. Theoretical models suggest three broad channels through which exposure to offshore markets in general, and exporting in particular, may lead firms to improve their productivity: forced efficiency gains due to increased competition;[1] improved access to new knowledge and technologies through greater contact with offshore suppliers, customers and competitors; and higher profits, economies of scale and greater incentives to develop specialised products for larger markets. Despite the variety of possible channels through which it may occur, the notion of firms improving their productivity performance through exporting has been generally referred to as "learning-by-exporting" (LBE) and we follow that convention in this paper.

Reviews of the LBE literature suggest that while many studies find evidence in support of "learning", almost an equal number fail to find any such effect (Greenaway and Kneller 2007; Wagner 2007). Comparison of these papers is complicated by differences in methodology, data availability, explanatory variables controlled for and the wide range of countries which have been studied. However, a broad view of the literature, in consideration of the hypothesized channels through which exporting might impact on productivity, suggests a number of possible explanations for conflicting results.

In this paper we examine two options: (1) that the finding of LBE depends on the relative sophistication of the exporting and destination countries; and (2) that methodological differences explain much of the variation in overall

[1] This channel is hard to reconcile with standard assumptions of profit maximising firms, but fits within the literature on X-inefficiencies. In practice, almost all empirical considerations of the effect of exporting on productivity use revenue-based estimates of productivity, which conflate changes in the prices received by firms with changes in the efficiency of their production processes. An increase in competition might therefore be observed as a fall in productivity, due to reduced profit margins even if the firm is making improvements in their underlying efficiency.

results. In order to address these questions, we utilise administrative data on the population of New Zealand manufacturing exporters linked to detailed firm-level data on exports by destination. To address the methodological concerns noted above, we test a range of approaches, from a simple regression model to our preferred approach of using propensity score matching with controls for firms' past export history. We document the extent of the exporter premium among New Zealand firms and test for observable "learning" in both the full population of exporters and in sub-populations determined by the degree of sophistication of the destination country, as proxied by per capita income.

While much of the literature on the firm-level consequences of exporting has focused on identifying productivity improvements, we follow the early work of Bernard and Jensen (1999) in expanding the set of variables of interest to include employment and the capital-labour ratio as well as labour productivity and multi-factor productivity (MFP). Hence we address not only the question of whether entry into export markets leads to firm-level productivity improvements, but also whether exporting impacts on aggregate productivity or income, through the reallocation of resources towards firms which were already productive or through increasing the returns to labour.

Finally, this paper provides the first detailed study of the learning-by-exporting hypothesis using New Zealand data. On theoretical grounds, New Zealand appears to be a prime candidate for observing both a strong self-selection effect and productivity improvements due to exporting. The vast geographic distances between New Zealand and potential export markets (particularly the developed markets of Europe and North America) impose relatively high costs on exporters compared to many of the countries which have been studied previously, and hence may lead to more pronounced self-selection. Meanwhile, all three hypothetical channels for productivity improvements through exporting may be relevant for New Zealand firms. While New Zealand is open in the sense of having relatively low barriers to foreign trade and investment, domestic market size and distance from other major markets are likely to have reduced the degree of effective competition in the domestic market. It is also likely that New Zealand firms reach the limit of domestic expansion possibilities at an earlier stage than firms in larger markets, thus enhancing the probability that exporting will be important for expansion, if not for productivity performance. Finally, New Zealand's relatively low level of productivity compared to other advanced economies and low observed investment in research and development (R&D) suggest that New Zealand firms may have plenty to learn from competitors, suppliers and customers offshore.

Our results suggest that when standard methodologies are used New Zealand firms do exhibit some gains from exporting, at least for employment, labour-productivity and the capital-labour ratio. However, when we introduce our most stringent matching methodology much of this effect disappears. In the next section we outline the existing empirical literature on exporting and productivity and review methodological options for identifying LBE effects. Section 3 describes the data and our empirical approach. Section 4 gives the results, while section 5 summarises and discusses areas for further work.

2 Literature review

Over the past 15 years, since the publication of seminal works by Bernard and Jensen (1995) and Roberts and Tybout (1997), research on the determinants and consequences of firm-level export performance has flourished. Studies of the exporting-productivity relationship in particular have been completed for over 30 countries, from Austria to Zimbabwe.[2] A wide range of studies have focused on identifying the proximate determinants of exporting ability, from innovative ability (eg, Roper and Love 2002) to government support (eg, Görg et al 2008) and demonstration effects from other local firms (Greenaway et al 2004; Greenaway and Kneller 2008). Other studies have considered whether firm-specific characteristics interact with exporting to determine the existence and extent of productivity benefits from exporting.[3] Finally, methodological developments have allowed for a reassessment of a number of early results, using more sophisticated techniques (eg, Wagner 2002; Girma et al 2004). This review focusses on three areas in which we hope to add to the existing understanding: the inclusion of multiple measures of firm performance; incorporation of destination country characteristics; and methodological issues in the selection of appropriate control groups.

2.1 Exporting, productivity and reallocation

Because most LBE studies only consider productivity growth within the firm, it is often difficult to evaluate potential benefits to the aggregate economy

[2] Austria is one of 14 countries included in a cross-country comparative study by ISGEP (International Study Group on Exports and Productivity) (2008). Zimbabwe is one of nine sub-Saharan African nations studied by Van Biesebroeck (2005).

[3] For example, Baldwin and Gu (2003) consider differences between foreign- and domestically-controlled, and between young and older plants, while Fryges and Wagner (2008) focus on export intensity.

through resource reallocation. However, a number of studies provide convincing evidence that reallocation from less productive domestically-focussed firms towards more productive export-oriented firms is a significant source of aggregate productivity growth.

Bernard and Jensen (2004) compare the performance of exporters and non-exporters in the US on a number of dimensions and find that, while exporting does not lead to productivity improvements in firms, it does have a significant effect on firm growth in employment and sales (both domestic and foreign). They find that over 40 percent of TFP growth in the US manufacturing sector can be attributed to the impact of exporting on resource reallocation. Baldwin and Gu (2003) find that continuing and new exporters together account for almost all aggregate productivity growth in Canadian manufacturing, through a combination of intra-firm improvements in productivity and inter-firm reallocations towards more productive firms. Meanwhile, Pavcnik (2002) considers the impact of trade liberalisation in Chile between 1979 and 1986. Characterising industries according to their trade orientation (export-oriented, import-competing, or non-traded goods sector) and comparing firm-level performance between sectors and over time, Pavcnik (2002) suggests that reallocation of resources within the economy accounted for around two-thirds of aggregate growth in Chilean manufacturing.

These results, alongside similar findings from Falvey et al (2004), Hansson and Lundin (2004) and others, imply that even if there is no firm-level productivity benefit from exporting, aggregate productivity may well be enhanced through resource reallocation and the expansion of already productive export-oriented firms. Such impacts are likely to be particularly important for New Zealand, where the small domestic market is likely to limit growth opportunities.

2.2 Heterogeneous destination markets

Given the three key channels through which exporting is expected to impact upon firm performance, it seems that many of these benefits are more likely for exports to large, highly developed destinations. First, the competitive disciplines imposed upon exporting firms are likely to be more severe when exporting to large, developed markets, as these markets can be expected to have a significant number of local suppliers already and may also attract a broader range of suppliers from abroad. At the same time, more sophisticated consumers are likely to place greater demands on exporters in terms of product quality and timeliness. Second, opportunities to learn from offshore

contacts will be more beneficial the greater the degree of sophistication of those contacts.[4] Finally, in imperfectly competitive markets, firms may be able to charge higher prices to consumers in wealthy countries, leading to higher observed value-added with no change in the underlying efficiency of the firm.[5]

If learning relies on the destination country having superior economic performance to the exporting country, we would expect to find that LBE is more commonly observed in less developed countries (LDCs). Such evidence can be garnered "by feel" – the unsystematic approach of reading many studies and looking for general patterns in the results – or through statistically robust methods. One paper which takes the latter approach is that of Martins and Yang (2009). These authors perform a meta-analysis of 218 estimates drawn from 32 studies on the productivity impacts of exporting. They investigate the impact of both methodological and contextual differences in the studies performed in explaining which studies are most likely to find LBE effects. The methodological issues they consider include whether the study uses a matching approach to the evaluation of impacts, whether the measure of productivity used is TFP or labour productivity, and whether the impacts are estimated for the year of entry or longer term.[6] Contextual issues are limited to the sample size (number of firm-year observations), the years covered by the data, and the development level of the source country.[7]

Of the 218 estimates in the paper, 55 percent used data from developed countries and 41 percent used propensity score matching techniques. The authors perform meta-regressions using four different weighting systems, based on the rankings of the journals in which the studies are published. They provide two specifications – their standard results and one controlling for the standard error of the initial estimate. Over the eight resulting specifications of the model, only one result came through consistently in all specifications: firms in LDCs are more likely to experience a stronger impact of LBE than those in developed countries. While this is far from conclusive evidence that destination country characteristics matter, it is consistent with a model in

[4] Providing that the exporting firm is itself sophisticated enough to benefit from these contacts. See Sanderson (2004) on the role of "absorbative capacity" in determining firms' ability to benefit from international engagement.

[5] Fabling, Joyce, and Sanderson (2009) find some indications of pricing-to-market based on destination GDP per capita. The extent to which charging higher prices to foreign markets affects observed productivity will depend on the degree to which these gains are offset by higher marginal costs (eg, transportation and insurance).

[6] Methodological issues are discussed further in Section 2.3, below.

[7] Development level is a binary variable based on UN definition of a developed economy, but the authors note that their results are robust to alternative definitions.

which firms are more likely to learn from exporting if their exports put them in contact with firms or consumers in countries more developed than their own.

There are very few papers which have directly addressed the question of destination market characteristics, perhaps due to a lack of detailed destination data for many countries. However, four recent papers have attempted to answer this question.

Trofimenko (2008) provides perhaps the most comprehensive treatment of heterogeneity in learning responses to exporting, allowing for the relationship to depend not only on the destination of exports, but also on the relative (*ex ante*) productivity of the firm and the level of sophistication of the industry (based on the share of highly skilled employment). Her results suggest that exporting does impart a productivity benefit to firms and that more productive firms gain an additional benefit from exporting to advanced economies. However, her ability to provide conclusive answers on the impacts of destination characteristics is limited by a lack of firm-specific data on export destinations. In the absence of firm-level destination data, Trofimenko links industry-year aggregate shares of exports by destination to firm-level productivity and performance data, which includes an indicator of whether the firm exported in a given year. She thus tests whether LBE effects are stronger for exporting firms in industries which export to high-income countries, rather than for firms exporting to high-income countries.

Two papers using Slovenian data (Damijan et al 2004 and De Loecker 2007) also consider the relationship between productivity growth and the characteristics of export destinations. Both papers find that exporting has a positive effect on productivity growth, but that this impact is limited to firms exporting to high-income countries.[8] The two papers differ in their estimates of the timing of such effects – Damijan et al suggest that the productivity boost from exporting is strong but short-lived, observed only in the first and second years of exporting, while De Loecker finds ongoing productivity gains as much as five years out from export entry.

It is worth noting, however, that the economic environment in Slovenia over the period covered by these studies was somewhat exceptional. Since gaining

[8] Damijan et al (2004) compare three possible destination groups: countries of the former Yugoslavia, OECD countries, and all others. De Loecker (2007) instead divides export destinations on regional boundaries, classifying North America, Western and Southern Europe as high income regions. Some noise may be introduced by this latter method. For example, under De Loecker's definition Japan would be counted as a low-income export destination.

independence from Yugoslavia in 1991, Slovenia has undergone a substantial programme of privatisation and trade liberalisation. Between 1994 and 2000, the value of manufacturing exports increased by 42 percent and the number of exporting firms quadrupled (De Loecker 2007). Given the specific circumstances faced by Slovenian firms over this period, it is not surprising that exports to developed countries may have provided rapid access to technologies and management styles not easily available domestically,[9] nor that exports to former-Yugoslav countries (which up until the 1989 would not have even counted as exports) did not provide such benefits.[10]

The question therefore arises as to whether the results of Damijan et al (2004) and De Loecker (2007) can be realistically assumed to apply for firms in open, developed economies. Evidence from Pisu (2008) suggests that the answer is no. Using data on Belgian manufacturing firms between 1998 and 2005, Pisu finds that while initial examination suggests that firms which enter export markets experience productivity gains relative to those that remain domestically focussed and that this relationship is stronger among firms that export to high-income destinations, these results are not robust to more formal empirical tests. Specifically, when matching methods are applied to determine a suitable control group of non-exporting firms, all significant LBE effects disappear and the positive relationship between exporting and productivity is shown to be due entirely to self-selection.

2.3 Methodological issues

One of the key methodological questions in empirical investigations of causal relationships is the identification of a suitable control group. The goal of any evaluation is to compare observed outcomes with the counterfactual outcome – in our case, to compare the productivity growth of firms which entered new export markets with the productivity growth that same firm would have experienced had they not entered. Clearly, a direct comparison is impossible, as we can only ever observe a firm in one state or the other.

[9] Damijan and Majcen (2003) also note that Slovenia received relatively low levels of inward FDI over the 1990s and that FDI does not seem to have had the strong effect on growth that it did in other transition economies.

[10] Weak product market competition and less demanding consumers in the former Yugoslav countries, especially combined with free-trade agreements between Slovenia and three of the four former Yugoslav countries imply that entry barriers to these countries are low (Damijan 2001), while at the same time providing little scope for learning-by-exporting.

If selection into exporting were random, a simple comparison of exporting and non-exporting firms would provide an appropriate test of the impacts of exporting. However, it has been widely shown that selection into exporting is non-random: exporting firms have superior performance prior to entry. A simple comparison of productivity outcomes for exporting firms relative to non-exporting firms would therefore pick up not only differences due to exporting, but also pre-existing differences in productivity levels and growth rates between exporters and non-exporters. One of the more contentious questions in quantifying LBE effects has been the appropriate implementation of controls for non-random selection. That is, robust evaluation of the causal relationship between exporting and productivity requires the identification of a plausible counterfactual.

Although authors have approached the question in various ways, two core methods can be identified in the literature to date. Most early papers (and many more recent ones) build on the approach introduced by Bernard and Jensen (1999), using a series of panel regressions (with or without controls for unobserved firm fixed effects) of the form

$$\ln(PROD_{it}) = \alpha + \beta(EXPORT_{it}) + \gamma(Z_{it-1}) + \epsilon_{it} \tag{1}$$

or,

$$\Delta \ln(PROD_{it}) = \alpha + \beta_1(START_{it}) + \beta_2(STOP_{it}) + \beta_3(CONTINUE_{it}) + \gamma(Z_{it-1}) + \epsilon_{it} \tag{2}$$

where EXPORT, START, STOP and CONTINUE are dummies representing the export status of the firm and the excluded category is non-exporters. Hence, the estimated impact of exporting is the difference in the productivity growth rate of firms which have recently entered relative to those which do not export, beyond that which can be explained by differences in a set of control variables. The control variables used differ across studies, with most studies including at least industry and year dummies, and often some measure of firm size. Some papers then allow for the impact of exporting to differ depending on firm characteristics (eg, Baldwin and Gu 2003; Fryges and Wagner 2008). Others also allow for export intensity (the share of exports in total sales) to matter (eg, Hansson and Lundin 2004; Fryges and Wagner 2008). More sophisticated regression frameworks have also been applied, including the

use of instrumental variables and system-GMM (eg, Baldwin and Gu 2003; Van Biesebroeck 2005).

Critics of the standard regression approach argue that a comparison of all exporting firms with all non-exporting firms gives a flawed picture of the returns to exporting. In particular, these papers suggest that a matched firm model provides a more robust control for the differences between exporting and non-exporting firms. These methodologies draw heavily on the literature on programme evaluation (eg, Smith 2004; Imbens and Wooldridge 2008) and consider export entry as a "treatment".

Although the exact details of matching estimators differ, there is a standard two-step procedure which is common across all models. The first step involves determining a suitable control group of firms which look similar to the treatment group *ex ante* but which do not receive treatment. Firms which are not "similar" to the treated firms are then discarded or down-weighted.[11] The most common approach is to match firms based on the probability of receiving treatment conditional on pre-treatment characteristics – the "propensity score".[12] Rosenbaum and Rubin (1983) prove that as long as there are no (unobserved) characteristics which are associated with both the potential outcome and the probability of treatment ("unconfoundedness") and suitable control cases can be found for each treated case ("overlap"), conditioning on the propensity score is sufficient to remove all the bias associated with differences in pre-treatment characteristics between the treated and untreated groups. Thus, all systematic differences in outcomes between the treated and controls are attributable to the treatment. Once propensity scores have been calculated, a number of possible matching techniques may be applied, differing with respect to the number of control firms matched to each treated firm and vice versa and the requirements for determining how similar two firms must be to be considered a valid control.[13] The second step is a comparison of the outcome variables of interest between the two groups some time after treatment.

A key question in matching models then is whether the observable differences between firms are sufficient to control for self-selection bias or whether there

[11] Some treated firms may also be discarded if no suitable match can be found. As such, matching models have the additional advantage of forcing researchers to confront the issue of "common support", and hence to consider the extent to which their findings can be generalised to the entire population.

[12] Alternatives include matching firms based on the underlying observable characteristics (eg, firm size, industry, foreign ownership) or on a combination of propensity score and other characteristics (known as Mahalanobis matching).

[13] See Caliendo and Kopeinig (2008) for a practical discussion of matching methods.

is instead some unobserved factor which determines both the probability of treatment and the later outcomes. For the purposes of our work on exporting and productivity, one possible such factor might be managerial incentives and ability. Managers with a strong focus on growth might be expected to be both more likely to enter into export markets and more likely to exhibit higher productivity growth in future periods, regardless of export market entry.

A range of options to help control for unobservable, time-invariant differences between treated and untreated firms have been developed. One option is to implement a difference-in-difference (DID) matching estimator, as suggested by Heckman et al (1998). This method has been implemented in a number of studies of exporting and productivity (eg, Girma et al 2004; Alvarez and López 2005; De Loecker 2007). While a standard matching model simply compares the ex-post performance of new exporters with that of matched non-entrants, the DID estimate instead compares the change in performance between the two groups in the period following market entry. Further alternatives include using a regression-adjusted or bias-corrected matching estimator, including relevant covariates in the second stage outcome regressions to capture any remaining observable differences between matched pairs. Finally, Imbens (2004) suggests weighting on propensity scores to obtain a balanced sample of treated and untreated firms.

The empirical evidence suggests that matching models may provide a more stringent test of the LBE hypothesis. Studies using matching methods are less likely to find significant LBE effects than those using panel methods (Greenaway and Kneller 2007; Martins and Yang 2009). However, it is also possible that the inability to find significant effects of exporting is due to the reduction in sample size when using matching methods.[14]

Other methodological issues discussed in the literature include the timing of any potential gains from exporting. As well as identifying whether future exporters already showed superior performance levels to non-exporters in the period prior to export market entry (the self-selection hypothesis) a number of papers have considered whether future exporters see a boost in productivity growth in the years leading up to market entry (the "learning-to-export"

[14] For example, if only 3 percent of firms commence exporting over the study period and each is matched to a single control firm, 94 percent of all observations are discarded.

hypothesis) (eg, Alvarez and López 2005).[15] Similarly, many authors distinguish between the contemporaneous impacts in the year of export market entry and longer term effects (eg, Greenaway and Kneller 2008). Timing aspects of the export-productivity relationship are important as they provide some insight into the channels through which exporting may be acting. As efficiency gains due to learning or competitive pressures are likely to take some time to realise, instantaneous productivity boosts may reflect instead the benefits of improved capacity utilisation. In contrast, longer term ongoing growth benefits are more likely to imply an evolutionary learning process. In this paper we focus on the productivity differential between entrants and non-entrants two years after the entry decision – a choice driven by sample size considerations. Future versions of the paper will investigate timing issues more fully.

3 Data and empirical strategy

The key goal of this paper is to identify whether there are observable benefits attributable to exporting among New Zealand firms and whether the existence and/or magnitude of such benefits depends upon the development level of the firm's export destinations.

To address this question we use data from the prototype Longitudinal Business Database (LBD). The LBD is based on Statistics New Zealand's Longitudinal Business Frame and draws together administrative and survey data on all economically significant enterprises (firms) in the New Zealand economy. Firm performance measures used in this paper are derived from Inland Revenue Department and Annual Enterprise Survey data,[16] while detailed export activity records are obtained from shipment level data from the New Zealand Customs Service, probabilistically matched to firms on the Longitudinal Business Frame. A full list of the variables used and their definitions is included in Appendix A.

[15] Productivity improvements in the years prior to export market entry are often explained in the literature as firms actively gearing up to enter foreign markets. In some cases they may also reflect active involvement by offshore potential customers (particularly for firms from developing countries). Questions of causality remain an issue here, however, as an unexpected positive productivity shock may also push firms into export markets. Finally, it is possible that a strategy of actively moving towards export markets may lead instead to a fall in productivity in the years prior to entry if firms are investing in capital equipment or R&D which will not be fully utilised until they expand into offshore markets. See eg, Bellone et al (2008).

[16] Following the method of Fabling and Grimes (forthcoming).

We restrict attention to employing, ever-manufacturing firms. In some cases, observed exports have been reallocated from non-manufacturing enterprises to manufacturing enterprises within the same parent-subsidiary group.[17] For the remainder of this paper, we use the term *firm* to refer to both individual enterprises and groups of manufacturers within a parent-subsidiary relationship. We further restrict to those firm-year observations for which we have a full set of outcome variables: MFP, labour productivity, employment and the capital-labour ratio. This reduces the population from 175,449 to 116,529 observations, or 31,083 firms over seven years (2000-2006) for cross-sectional statistics.

Our preferred methodological approach is to implement propensity score matching based on firm characteristics and to compare outcomes between entrants and non-entrants two years following market entry. In order to implement this approach we need to be able to follow firms over a four year period: the *"control year"* ($t = 0$) provides the *ex ante* performance metrics on which we match; the *"treatment year"* ($t = 1$) is the year in which we either observe an export market entry (treatment) or not (control); and the second year following treatment ($t = 3$) is the point at which we compare outcomes between the treated and non-treated groups. When the matching involves export history variables these are additionally calculated over $-4 \leq t \leq 0$.

Throughout the analysis we use data from only those firms which can provide performance data at $t = 0$ and $t = 3$. Matching on lagged performance and considering two-year forward outcomes further restricts the treatment years we consider to 2001-2005. Table 1 shows the incidence of missing financial performance data in the treatment and outcome years relative to the control year.[18] This attrition forces us to qualify our conclusions regarding the quality of match between treatment and control firms. Specifically, table 2 shows marginal coefficients from a probit regression of MFP attrition at $t = 3$ for the population that has MFP at $t = 0$. New and continuing exporters are observed to have a lower attrition rate than either non-exporters or firms which cease exporting (negative coefficients on transition01 and transition11). If this is due in part to the effect of treatment, we may underestimate causal effects (provided some sub-optimal matches have occurred where the *ex ante* best matched control firm is precluded because it cannot provide outcome data).

[17] The methodology and rationale for this allocation is documented in Fabling and Sanderson (forthcoming).

[18] As there are a number of possible reasons for firms to have missing performance data we cannot be certain that this attrition is due entirely to firm closures.

Table 1
Proportion of missing MFP data conditional on MFP at $t = 0$

	Observations	Firms
Treatment year (t=1)	0.215	0.188
First year outcome (t=2)	0.295	0.268
Second year outcome (t=3)	0.365	0.334

Table 2
Probit regression for absence of MFP data at $t = 3$

	Marginal effects	p-value
rc01 (Northland)	0.020	0.108
rc02 (Auckland)	0.019***	0.006
rc03 (Waikato)	0.019**	0.034
rc04 (Bay of Plenty)	0.035***	0.000
rc05 (Gisborne)	-0.029	0.241
rc06 (Hawke's Bay)	-0.010	0.416
rc07 (Taranaki)	0.002	0.877
rc08 (Manawatu-Wanganui)	-0.014	0.198
rc09 (Wellington)	-0.001	0.868
rc12 (West Coast)	0.062**	0.021
rc13 (Canterbury)	-0.025***	0.001
rc14 (Otago)	-0.024**	0.042
rc15 (Southland)	0.003	0.863
rc16 (Tasman)	-0.015	0.444
rc17 (Nelson)	0.009	0.608
rc18 (Marlborough)	-0.034*	0.058
MFP	-0.049***	0.000
ltotemp	-0.071***	0.000
klratio	-0.019***	0.000
FDI	0.052***	0.003
transition01	-0.059***	0.000
transition10	0.071***	0.000
transition11	-0.048***	0.000
N	82,392	
Pseudo R^2	0.040	

Robust p-values in brackets (clustered on groups).
Significant at: * 10%; ** 5%; *** 1%
Regression includes a full set of (unreported) industry-year dummies. TransitionXY is a dummy equal to 1 if export status is X at $t = 0$ and Y at $t = 1$. Transition00 is the omitted category.

Several alternative measures of entry are examined. The simplest definition uses a single year of export history, and defines the treatment variable as "the firm exports (to any destination) in period $t = 1$, and did not export (to any destination) in $t = 0$". This definition of *"year-on-year"* entry has been used by a number of authors (eg, Aw et al 2000), but does not, in our view, provide a suitable distinction given the variety of observed export histories. However, it does provide the opportunity to control specifically for those differences, giving some insight into the importance of controlling for the unobserved differences which help to determine a firm's export propensity.

In our preferred specifications we base our measures of export market entry on five years of historical data, with a firm counted as an entrant only if they have not been observed to export over the preceding five years. We then consider two alternative forms of entry – *"first-time"* entry into exporting and *"incumbent"* entry into new markets by incumbent or past exporters. The former provides the cleanest definition of entry into exporting, with entrants defined as firms which export in period $t = 1$, and did not export (to any destination)in the five years from $t = -4$ to $t = -0$.

Meanwhile, the latter definition allows for a more stringent test of the learning-by-exporting hypothesis in its purest form. Matching incumbent exporters with other incumbent exporters gives us a much better match quality, reducing the chance that unobserved differences between our treatment and control groups will bias our results. For example, the move into exporting can represent a significant increase in risk for New Zealand firms and many firms may have no interest in making that leap, even if their observable characteristics suggest they should have the ability to export.[19] If we have already observed a firm to export at some point in the past, we can be more confident that they have at least some desire to export. Entry into new markets can then be used as an indicator of first access to the technologies and consumer demands associated with a different destination.

By distinguishing between new markets according to their per capita income level, we are able to separately consider the effect of entry into a high-income country as opposed to entry into exporting more generally. Relatively more sophisticated export markets should imply greater opportunities for learning. We therefore distinguish between entry into high- and low-income markets,

[19] According to the Business Operations Survey 2007, even among manufacturing firms, over half do not earn any overseas income. Of those firms, 21% cite prohibitive costs or barriers to exporting. 54% note that the New Zealand market is sufficient, while 59% state that their requirement for physical proximity to customers prevents them from entering offshore markets (Statistics New Zealand 2008).

Table 3
Share of exporters and entrants by year and by industry, alternative definitions of entry

Year	Current exporters	First-time entrants	Year-on-year entrants	Incumbent entry into new markets
2000	0.131	0.015	0.024	0.473
2001	0.135	0.014	0.024	0.494
2002	0.141	0.016	0.026	0.474
2003	0.147	0.018	0.029	0.490
2004	0.152	0.018	0.028	0.512
2005	0.151	0.016	0.027	0.504
2006	0.154	0.014	0.025	0.484
Total	0.14.5	0.016	0.026	0.491

Year	Current exporters	First-time entrants	Year-on-year entrants	Incumbent entry into new markets
C21	0.238	0.019	0.028	0.562
C22	0.164	0.015	0.026	0.462
C23	0.091	0.009	0.014	0.432
C24	0.102	0.017	0.027	0.442
C25	0.379	0.023	0.044	0.503
C26	0.098	0.017	0.026	0.546
C27	0.109	0.014	0.023	0.480
C28	0.157	0.017	0.031	0.510
C29	0.094	0.016	0.024	0.426
Total	0.145	0.016	0.026	0.491

where high-income is defined as countries having GDP per capita greater than USD17,000. This cut-off point roughly reflects the per capita income of New Zealand in our dataset and also splits the sample of New Zealand exports approximately equally between high- and low-income destinations.[20]

At the same time, as expansion into new markets is a more common event than first-time entry into exporting, we also increase the size of our sample in the matching model, making it more likely that we will be able to identify significant differences in performance if they do exist. If the potential performance benefits from exporting are due to learning from contact with competitors, suppliers or consumers in offshore markets we would expect that entry into additional offshore markets should provide new learning opportunities.

Table 3 summarises the prevalence of entry in each year and industry, according to each definition. This table shows a general increase in the proportion of manufacturers exporting over time from 13 to 15 percent (as found by Fabling and Sanderson (2008)). First-time entry is a rare event, with less than two percent of firms entering in any given year. In contrast, incumbent en-

[20] GDP per capita data comes from United Nations Statistics (GDP) and the US Census Bureau (population).

Table 4
Hysteresis in exporting

	Exporting at t=1		Proportion
export_year_count	No	Yes	(Yes/(Yes+No))
0	0.796	0.016	0.020
1	0.032	0.015	0.320
2	0.012	0.014	0.534
3	0.007	0.015	0.689
4	0.005	0.015	0.759
5	0.004	0.070	0.951

try is not rare, though Fabling and Sanderson (forthcoming) show that most firms only trade to one country at a time. Clearly, the year-on-year entry measure provides an inaccurate picture of the number of true entrants, as almost 40 percent of year-on-year entrants have in fact exported previously (table 3).

Table 4 tabulates export status in the treatment year with status in the previous five years. As expected, participation rates at $t = 1$ rise rapidly with past export participation. In particular, firms which have at least some export experience are an order of magnitude more likely to export at $t = 1$ than those with no export history. This creates difficulties for the analysis of learning effects, as re-entrants may differ from first-time entrants in both their *ex ante* characteristics and their response to exporting (eg, re-entry may provide fewer learning opportunities than first-time entry). However, the use of year-on-year entry allows us to directly examine the impact of using export history variables as part of the matching model.

We therefore compare, in turn, outcomes of the following control and treatment groups, working our way from the simple entry definitions most commonly used in the literature to our more sophisticated matching method:

	Entry type	Population	Treatment	Control for export history
(1)	First-time	Firms which have not exported in the past five years	Entry into exporting	N
(2)	First-time	Firms which have not exported in the past five years	Entry into exporting to a high-income country	N
(3)	Year-on-year	Firms which did not export in the control year	(Re-)entry into exporting	N
(4)	Year-on-year	Firms which did not export in the control year	(Re-)entry into exporting	Y
(5)	Incumbent	Prior exporters	Entry into a new export destination	Y
(6)	Incumbent	Prior exporters	Entry into a new high-income export destination	Y

In each case we estimate the average treatment effect on the treated (ATT) from a matching model, in which the probability of treatment is estimated

separately for each two digit industry based on initial firm characteristics. The performance characteristics used in specifications 1-3 are MFP, log employment, the capital-labour ratio and a foreign ownership dummy, plus a full set of year and region dummies. In specifications 4-6 we also add a full set of export history variables, as defined in Appendix A. The estimated probability of treatment (or propensity score) is then used to match each treated firm to a single control firm within the same industry, using nearest neighbour matching and restricting to the common support.[21] We then pool observations across all industries to compare industry-year demeaned outcomes between the treated and control groups, adding additional controls for industry.[22] Standard errors are calculated through boot-strapping across both the first stage propensity score estimation and the second stage estimation of the treatment effect. The boot-strapped sample is drawn independently from the treated and untreated groups to maintain the same matched sample size across repetitions.

For comparison, we also report the results of a simple regression, in which firm outcomes (in year $t = 3$) are regressed on pre-treatment outcomes ($t = 0$) and a dummy variable indicating treatment, as well as a full set of industry and year dummies ($Z_{i,t=0}$):

$$OUTCOME_{t=3} = \alpha(OUTCOME_{t=0}) + \beta(TREATMENT_{t=1}) + \gamma(Z_{i,t=0}) + \epsilon_{t=3}. \quad (3)$$

4 Results

4.1 Exporters are different in cross-section

To set the scene, we first provide a visual comparison of the four performance variables across three groups of firms – current exporters, past exporters and non-exporters. Figure 1 shows kernel densities of the performance metrics with all industries pooled together. Industry-year averages are removed from all performance metrics to allow comparability and to enable matching to be pooled across years (within an industry). Like studies in other countries, we

[21] We use PSMATCH2 (Leuven and Sianesi 2003) to generate propensity scores and test the balancing of the matched sample.

[22] Industry ATTs were also produced but the estimated effects were seldom significantly different from zero, perhaps due to the small sample available for each industry.

find that current New Zealand exporters are larger, more capital-intensive and have higher labour productivity than past exporters (ie, their distributions sit to the right), who in turn perform better on these three measures than non-exporters.[23] In contrast, however, we see little difference in the MFP levels of the three groups.

In contrast to other papers considering differences across destination markets, we see little to suggest that high-income destinations present a greater challenge for prospective exporters. Figure 2 compares the distribution of observed performance measures across three groups: exporters to low-income countries only; exporters to high-income countries only; and exporters which target both low- and high-income countries. We see little to distinguish between exporters to low- and high-income countries. Rather, the substantive differences are between firms which export to only one group of countries and those which export to both. Given the relative concentration of firms which only export to a single destination (shown in Fabling and Sanderson forthcoming), it seems likely that this distinction is due more to the comparison of single-market exporters and multiple-market exporters, rather than the performance requirements to export to both high- and low-income countries specifically.

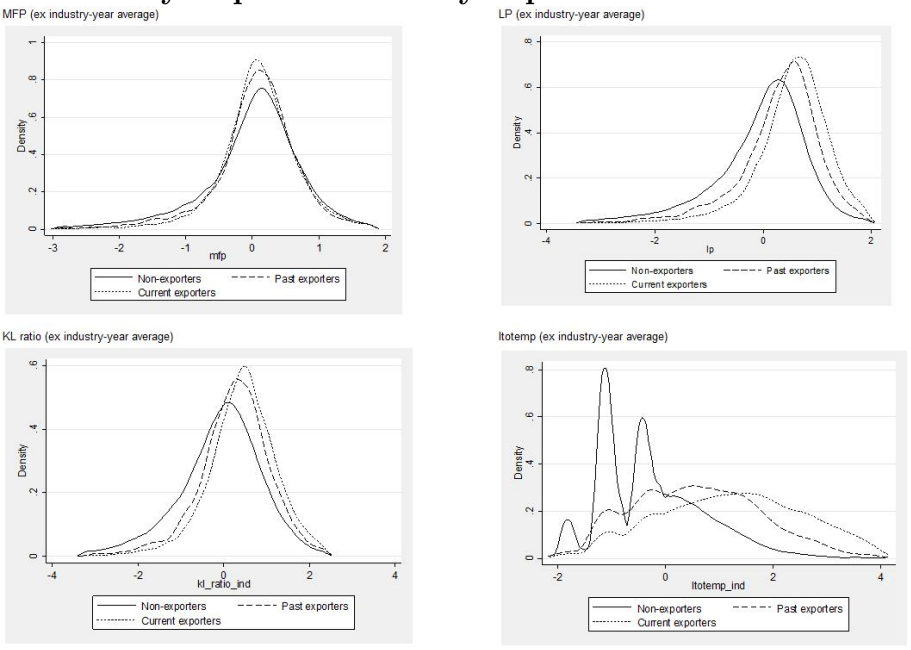
To quantify the relationship between performance and export history we perform simple regressions which control for additional firm characteristics that may impact on performance. Table 5 presents the results of these regressions. Columns A,C,E and G present results estimated over all firms, while other columns show results for the sub-sample of firms with some past export experience. Regional Council dummies are included to control for regional differences in, eg, infrastructure, agglomeration or land quality which may affect both firm performance and the probability of exporting.[24]

These estimates suggest that there are indeed differences between exporters and non-exporters on a range of performance dimensions. Firms which have exported more frequently over the prior five years (export_year_count), those which have exported more recently (inv_export_time) and those which have been increasing their share of exports (d_export_share_incum_xbar) perform better across most of our measures than those with less export experience. Meanwhile, firms which export a larger share of their sales on average over the five year period perform consistently less well.

[23] Employment comparisons are affected by the prevalence of very small firms among non-exporters, in particular working proprietor only firms.

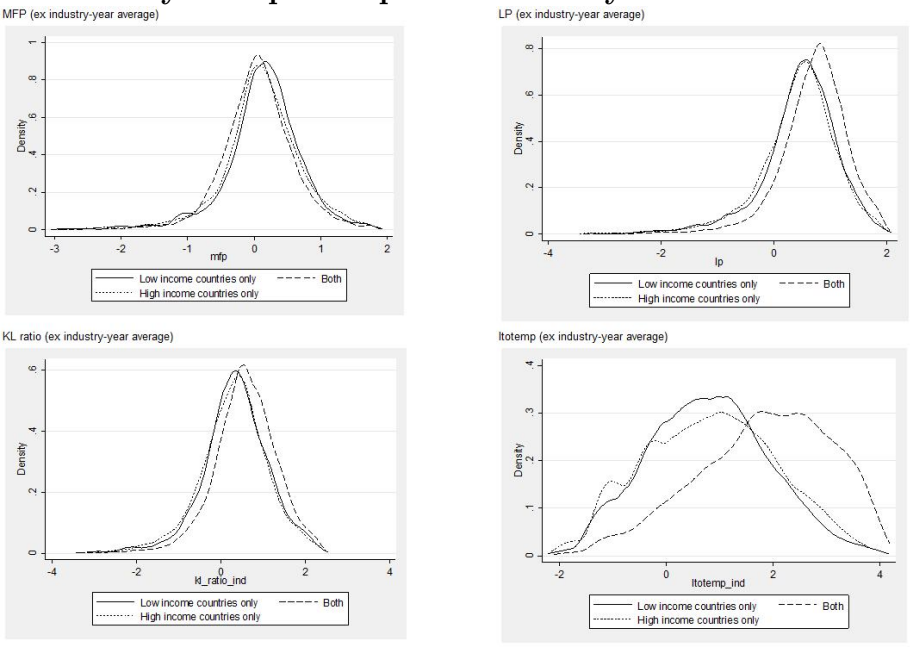
[24] See Maré (2008) and Fabling, Grimes, and Sanderson (forthcoming) for New Zealand evidence of agglomeration and localised export learning effects respectively.

Figure 1
Kernel density of performance by export status



All plots exclude top and bottom 1% of observations in accordance with Statistics New Zealand confidentiality requirements

Figure 2
Kernel density of exporter performance by destination income



All plots exclude top and bottom 1% of observations in accordance with Statistics New Zealand confidentiality requirements

Table 5
Cross-sectional performance and export experience

	MFP		LP		klratio		ltotemp	
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)
	All	Exports	All	Exports	All	Exports	All	Exports
rc01 (Northland)	-0.179***	-0.058	-0.099***	-0.009	0.084***	0.052	0.248***	0.158
	[0.000]	[0.203]	[0.000]	[0.895]	[0.007]	[0.496]	[0.000]	[0.152]
rc02 (Auckland)	0.115***	0.086***	0.247***	0.214***	0.160***	0.190***	0.365***	0.314***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
rc03 (Waikato)	-0.061***	-0.055**	0.037**	0.037	0.091***	0.058	0.322***	0.376***
	[0.000]	[0.047]	[0.034]	[0.285]	[0.000]	[0.161]	[0.000]	[0.000]
rc04 (Bay of Plenty)	-0.042***	-0.048	0.045**	0.047	0.057**	0.037	0.322***	0.393***
	[0.008]	[0.133]	[0.020]	[0.227]	[0.017]	[0.422]	[0.000]	[0.000]
rc05 (Gisborne)	-0.111***	-0.306***	-0.022	-0.248***	0.107*	0.146	0.255***	0.000
	[0.009]	[0.000]	[0.664]	[0.003]	[0.078]	[0.251]	[0.001]	[1.000]
rc06 (Hawke's Bay)	-0.028	-0.039	0.064***	0.059	0.086***	0.146**	0.282***	0.209**
	[0.160]	[0.343]	[0.009]	[0.243]	[0.005]	[0.022]	[0.000]	[0.014]
rc07 (Taranaki)	-0.022	0.019	0.034	0.076	0.013	0.060	0.257***	0.167
	[0.360]	[0.685]	[0.251]	[0.284]	[0.731]	[0.498]	[0.000]	[0.107]
rc08 (Manawatu-Wanganui)	-0.086***	-0.058**	-0.020	-0.004	-0.016	-0.033	0.386***	0.376***
	[0.000]	[0.044]	[0.341]	[0.910]	[0.521]	[0.501]	[0.000]	[0.000]
rc09 (Wellington)	0.024	0.028	0.122***	0.122***	0.098***	0.077**	0.309***	0.323***
	[0.107]	[0.267]	[0.000]	[0.000]	[0.000]	[0.031]	[0.000]	[0.000]
rc12 (West Coast)	-0.076*	-0.098*	0.014	-0.040	0.103*	0.018	0.272***	0.323**
	[0.060]	[0.090]	[0.755]	[0.538]	[0.053]	[0.853]	[0.000]	[0.036]
rc13 (Canterbury)	-0.050***	-0.086***	0.037**	0.009	0.012	0.015	0.435***	0.485***
	[0.000]	[0.000]	[0.011]	[0.690]	[0.482]	[0.601]	[0.000]	[0.000]
rc14 (Otago)	-0.055***	-0.050	0.026	0.008	0.011	-0.012	0.400***	0.343***
	[0.005]	[0.183]	[0.273]	[0.849]	[0.685]	[0.819]	[0.000]	[0.000]
rc15 (Southland)	0.034	-0.018	0.099***	0.031	-0.078**	-0.161*	0.497***	0.611***
	[0.154]	[0.660]	[0.001]	[0.574]	[0.043]	[0.086]	[0.000]	[0.000]
rc16 (Tasman)	-0.206***	-0.094*	-0.150***	0.003	0.039	0.114	0.173***	0.185
	[0.000]	[0.092]	[0.001]	[0.968]	[0.449]	[0.327]	[0.001]	[0.193]
rc17 (Nelson)	-0.059*	-0.024	-0.023	-0.078	0.026	-0.086	0.139***	-0.081
	[0.057]	[0.608]	[0.538]	[0.153]	[0.562]	[0.256]	[0.009]	[0.527]
rc18 (Marlborough)	-0.106***	0.008	-0.023	0.003	0.092*	0.018	0.205***	-0.151
	[0.002]	[0.892]	[0.565]	[0.961]	[0.070]	[0.859]	[0.000]	[0.147]
fdi	0.086***	0.041	0.452***	0.339***	0.553***	0.479***	0.828***	0.603***
	[0.000]	[0.122]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
export_year_count	0.008	0.004	0.082***	0.060***	0.066***	0.044***	0.260***	0.209***
	[0.207]	[0.571]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
inv_export_time	0.114***	0.077**	0.247***	0.089***	0.180***	0.034	0.375***	-0.014
	[0.000]	[0.010]	[0.000]	[0.007]	[0.000]	[0.382]	[0.000]	[0.772]
n_countries_5yr	-0.003*	-0.004***	0.003	0.002	0.005**	0.006**	0.023***	0.024***
	[0.067]	[0.001]	[0.120]	[0.418]	[0.025]	[0.018]	[0.000]	[0.000]
cty_entry	-0.002	0.001	0.002	0.008*	-0.002	0.002	0.018**	0.028***
	[0.666]	[0.784]	[0.675]	[0.051]	[0.768]	[0.771]	[0.048]	[0.001]
cty_exit	-0.005	-0.001	-0.005	0.003	-0.007	-0.003	0.011	0.025*
	[0.327]	[0.821]	[0.331]	[0.626]	[0.334]	[0.708]	[0.438]	[0.061]
avg_export_share	-0.184***	-0.217***	-0.548***	-0.633***	-0.224**	-0.269***	-1.613***	-1.755***
	[0.002]	[0.000]	[0.000]	[0.000]	[0.016]	[0.004]	[0.000]	[0.000]
d_exports_incum_xbar	0.062***	0.051**	0.128***	0.118***	0.041	0.039	0.254***	0.276***
	[0.006]	[0.024]	[0.000]	[0.000]	[0.194]	[0.216]	[0.000]	[0.000]
d_exports_entry_xbar	0.032	-0.003	0.097***	-0.013	0.097***	0.014	0.144***	-0.084**
	[0.116]	[0.909]	[0.000]	[0.607]	[0.000]	[0.639]	[0.000]	[0.032]
d_exports_exit_xbar	-0.048**	0.001	-0.202***	-0.002	-0.229***	-0.058*	-0.354***	0.130***
	[0.016]	[0.972]	[0.000]	[0.938]	[0.000]	[0.065]	[0.000]	[0.003]
Constant	-0.005	0.282***	-0.221***	0.299***	-0.238***	0.097	-0.713***	-0.197**
	[0.847]	[0.000]	[0.000]	[0.000]	[0.000]	[0.123]	[0.000]	[0.024]
N	116,529	21,915	116,529	21,915	116,529	21,915	116,529	21,915
R^2	0.017	0.039	0.089	0.136	0.055	0.096	0.367	0.483

Robust p-values in brackets (clustered on groups).
* significant at 10%; ** significant at 5%; *** significant at 1% level.
Regressions include a full set of (unreported) industry-year dummies.

4.2 Self-selection or Learning-by-Exporting?

In this section, we begin by reporting the probit models on which the matching is conducted. We then present ATT results based on that matching.

Table 6 contains the critical propensity score (probit) models on which the matching is performed.[25] The four reported models conform to the four potential treatments (first-time, year-on-year with and without export history controls, and incumbent). If selection into treatment is non-random, as previous studies have shown, these models must adequately capture the systematic differences between entrants and non-entrants in order to validate the estimated ATT. The results confirm our *ex ante* expectations: firms which are larger, more productive and more highly capital intensive are more likely to commence exporting (Column A), and also more likely to expand into new markets (Column D).

There is little difference between the results in Columns A (first-time entrants) and B (year-on-year entrants), with higher coefficients for the latter regression reflecting the higher overall probability of observed entry when re-entrants are included. Comparison of Columns B and C provides justification for the inclusion of export history variables in the propensity score regression and reinforces our concerns about the legitimacy of the year-on-year treatment variable. The impact of including these variables is to mildly reduce the magnitude of the coefficients on most of the firm characteristics, while increasing the explanatory power of the model from a Pseudo R^2 of 0.105 to 0.188. That is, the model including detailed export history variables provides a substantially better quality match between control and treatment groups than that using *ex ante* performance characteristics alone.

At this point, it is important to note that within each of our industry-specific propensity score matching models there were occasional variables which did not balance.[26] That is, our match on the propensity score has left us with differences in the distribution of some covariates between the control and treatment groups. In principle, a high quality match requires that after conditioning on the propensity score, additional conditioning on the firm-level

[25] These results are presented for pooled industries with industry dummies. Table 6 is therefore a summary version of the propensity score calculations used in the causal analysis in which we estimate propensity scores separately for each industry and match within industries.

[26] Inability to balance was more pronounced in some industries than others, with C28 (Machinery and Equipment Manufacturing) in particular being unbalanced on a number of outcome variables.

Table 6
Probit regressions of export market entry

	first-time	**year-on-year**	**year-on-year**	**incumbent**
rc01 (Northland)	-0.001	-0.004	-0.002	-0.035
	[0.457]	[0.111]	[0.252]	[0.134]
rc02 (Auckland)	0.004***	0.007***	0.005***	0.006
	[0.001]	[0.000]	[0.000]	[0.452]
rc03 (Waikato)	-0.003**	-0.005***	-0.003**	0.011
	[0.026]	[0.007]	[0.017]	[0.390]
rc04 (Bay of Plenty)	-0.002	-0.003	-0.002	-0.020
	[0.184]	[0.206]	[0.197]	[0.175]
rc05 (Gisborne)	-0.007**	-0.011***	-0.009**	-0.076*
	[0.022]	[0.006]	[0.012]	[0.087]
rc06 (Hawke's Bay)	-0.002	-0.004*	-0.004*	0.029
	[0.293]	[0.084]	[0.057]	[0.138]
rc07 (Taranaki)	-0.003	-0.005*	-0.004	-0.035
	[0.238]	[0.057]	[0.106]	[0.128]
rc08 (Manawatu-Wanganui)	-0.005***	-0.007***	-0.006***	-0.042**
	[0.004]	[0.001]	[0.000]	[0.014]
rc09 (Wellington)	-0.003**	-0.003*	-0.003**	0.013
	[0.023]	[0.063]	[0.035]	[0.263]
rc12 (West Coast)	0.005	0.001	0.003	-0.024
	[0.232]	[0.789]	[0.481]	[0.532]
rc13 (Canterbury)	-0.001	-0.001	-0.001	-0.006
	[0.565]	[0.467]	[0.270]	[0.507]
rc14 (Otago)	-0.002	-0.004*	-0.004**	-0.004
	[0.199]	[0.090]	[0.042]	[0.804]
rc15 (Southland)	-0.006***	-0.007**	-0.007***	-0.035
	[0.003]	[0.022]	[0.005]	[0.151]
rc16 (Tasman)	0.000	-0.003	0.000	-0.017
	[0.970]	[0.493]	[0.917]	[0.593]
rc17 (Nelson)	0.001	0.003	0.003	0.057**
	[0.657]	[0.407]	[0.430]	[0.037]
rc18 (Marlborough)	-0.005**	-0.006*	-0.005*	0.033
	[0.037]	[0.082]	[0.081]	[0.203]
MFP	0.003***	0.005***	0.004***	0.019***
	[0.000]	[0.000]	[0.000]	[0.001]
ltotemp	0.009***	0.015***	0.010***	0.046***
	[0.000]	[0.000]	[0.000]	[0.000]
klratio	0.005***	0.008***	0.005***	0.014***
	[0.000]	[0.000]	[0.000]	[0.001]
FDI	0.003	0.003	0.002	0.004
	[0.380]	[0.554]	[0.484]	[0.789]
export_year_count			0.003*	0.021***
			[0.051]	[0.000]
inv_export_time			0.111***	0.209***
			[0.000]	[0.000]
n_countries_5yr			-0.002	0.009***
			[0.132]	[0.000]
cty_entry			0.005*	0.032***
			[0.053]	[0.000]
cty_exit			0.002	-0.005
			[0.130]	[0.166]
avg_export_share			0.014***	0.007
			[0.007]	[0.788]
d_exports_incum_xbar			0.010	0.101***
			[0.107]	[0.000]
d_exports_entry_xbar			-0.014***	0.055***
			[0.004]	[0.001]
d_exports_exit_xbar			-0.004	-0.021
			[0.111]	[0.268]
N	94,611	99,852	99,852	21,915
$N(LHS = 1)$	1,860	3,066	3,066	7,353
Pseudo R^2	0.078	0.105	0.188	0.181

Robust p-values in brackets (clustered on groups).
* significant at 10%; ** significant at 5%; *** significant at 1% level.
Regressions include a full set of (unreported) industry-year dummies.

covariates should not provide any extra information about the treatment decision. In general, the variables that we were unable to balance are likely to be fairly marginal in the differentiation between firms. Many of the unbalanced results were on regional dummies, suggesting that the issue was mainly due to small numbers of firms in some areas. However, there were also some industries and definitions of entry in which output variables were unbalanced. In particular, the capital-labour ratio is unbalanced in several specifications, implying that higher capital intensity is a key factor differentiating exporters from non-exporters. Balancing was also more difficult in the model of incumbent new entry, probably due to the high share of treated firms on this measure of entry.[27]

Having convinced ourselves that we can at least partially explain treatment using $t = 0$ variables, we examine the causal effects of exporting on performance. Table 7 presents two sets of results, the naïve regression model of Equation 3 and the results using matched firms. To test the robustness of the estimated ATT we reestimate the model dropping industries which fail to balance on one or more outcome variables (changed statistical significance in this test is represented by † and ‡ in table 7).

In all cases, the naïve (ie, biased) regressions suggest that after controlling for initial performance, firms which enter exporting perform significantly better on all four metrics than those which do not. Estimated coefficients on the treatment effect for new exporters range between 0.135 and 0.229 for labour productivity, the capital-labour ratio and total employment and are always highly significant. In contrast, the effect appears to be much lower and less significant when the outcome in question is MFP, in keeping with the observed lack of difference between exporters and non-exporters shown in Figure 1, and smaller coefficients in the earlier cross-sectional regressions (Table 5). The estimated impact of entry into new export markets by incumbent exporters is weaker but still significant across labour productivity, the capital-labour ratio and employment. Thus, it would appear that there is potentially a positive causal relationship between both new entry into exporting and expansion of export markets and firm performance, at least in terms of employment, capital deepening and labour productivity.

Turning to the results of the matching model, we see that the more robust estimates based on matched firms suggest a weaker – and in many cases insignificant – relationship between export entry or expansion and firm performance. Following the standard methodology of comparing first-time entrants

[27] In any given year between 27 percent and 50 percent of all observations of incumbent exporters showed entry into new markets.

with matched non-exporters (the top row of table 7) shows a positive causal relationship between exporting and three of the four performance variables. Two years after entry, new exporters exhibit a premium of around eight percent in labour productivity and the capital-labour ratio, and a 16 percent employment premium relative to non-exporters, but are not significantly different from non-exporters with respect to MFP. When we restrict the treatment definition to include only first-time entries in which the destination is a high-income country, coefficients become weaker and lose significance. This may reflect the smaller sample available (774 treated firms under the high-income definition, compared to 990 firms for all first-time entry). However, we continue to see a positive impact of treatment on employment.

Table 7
Causal Models of Exporting and Firm Performance

	Naïve regression approach				Matched firms					Treatment
	MFP	LP	klratio	ltotemp	MFP	LP	klratio	ltotemp	N	rate
First-time entry	0.044**	0.177***	0.135***	0.218***	0.026	0.087**	0.089*	0.161***	41,286	0.024
	[0.024]	[0.000]	[0.000]	[0.000]	[0.478]	[0.041]	[0.055]	[0.001]		
First-time entry	0.043*	0.183***	0.144***	0.229***	0.025	0.067	0.054	0.125**†	41,286	0.019
(High Income only)	[0.051]	[0.000]	[0.000]	[0.000]	[0.549]	[0.101]	[0.309]	[0.012]		
Year-on-Year entry	0.040**	0.172***	0.138***	0.211***	0.056**	0.092***	0.054*†	0.114***	43,689	0.037
(no history)	[0.041]	[0.000]	[0.000]	[0.000]	[0.027]	[0.001]	[0.096]	[0.002]		
Year-on-Year entry			[As above]		0.032	0.050	0.052	0.014	43,689	0.037
(history)					[0.293]	[0.139]	[0.187]	[0.778]		
Incumbent Entry	-0.020*	0.084***	0.065***	0.108***	0.010‡	-0.000	-0.059*†	0.062	11,043	0.367
	[0.098]	[0.000]	[0.000]	[0.000]	[0.695]	[0.996]	[0.057]	[0.136]		
Incumbent Entry	-0.011	0.060***	0.056***	0.099***	-0.014	-0.009	-0.026	0.082*	11,043	0.215
(High Income)	[0.418]	[0.000]	[0.000]	[0.000]	[0.569]	[0.745]	[0.416]	[0.086]		

Regression populations are matched industry subsamples using PSMATCH2 (Leuven and Sianesi 2003).
All regressions include (unreported) industry dummies.
* significant at 10%; ** significant at 5%; *** significant at 1% level. P-values reported in brackets.
Standard errors bootstrapped (100 reps) for matched firm model.
Robustness checks were performed by dropping those industries which did not balance on one or more outcome variable. †(‡) indicates estimates that become insignificant (significant) when those industries were dropped.

However, the first-time entry definition is subject to the critique that they are not able to control for unobserved differences between exporters and non-exporters, as discussed above. Turning to the year-on-year definition, we find that without controls for export history entry has a positive and significant impact on all four performance variables. However, when we control for past export history, none of the effects is significant at the 10 percent level.

Comparison of first-time and year-on-year results requires an understanding of population differences. Using five years of export history, we select new first-time entrants (treated firms) from a population which includes only firms which have not exported at all over the past five years. With the year-on-year definition, sporadic and exited exporters enter into both the treated group and the pool of potential controls. If firms which have exited exporting are already on a downward performance trajectory (as has been suggested by eg, Girma et al 2003), these firms can be expected to see a continued fall in performance relative to the control year, pushing up the observed difference between the control and treatment groups as we match new exporters and re-exporters to failing past exporters. When we introduce controls for past history into the matching model, we reduce the potential for new exporters to be matched with past exporters and this observed impact disappears.[28]

Finally, we consider entry into new destination countries by incumbent exporters. Here, we match on a full set of firm and export history variables. The results suggest no impact of new market entry on employment, labour productivity and multifactor productivity, and a mildly negative effect on the capital-labour ratio. The capital-labour ratio is the outcome variable on which we have most trouble achieving a balanced sample of matched firms, and the negative result is not robust to the exclusion of industries which do not balance. In turn, the estimated impact of MFP becomes mildly significant (and positive) when we exclude those industries which do not balance. The failure to balance is likely due to the high share of treated firms under this definition (on average, each year around 37 percent of all incumbent exporters entered a new market) reducing the pool of potential control firms. Restricting the treatment variable to entry into high income markets reduces the balancing problem somewhat (averaged across years, only 21 percent of firms enter a high income country). Two years after entry, firms entering new high-income countries exhibit an employment premium of around eight percent relative to matched incumbent exporters which did not enter a new

[28] There are also arguments for underestimation of the causal effect. For example, re-entering exporters may have already gained the performance premium (if this is a one-off effect). However, the empirical results suggest the upward bias dominates.

market, though this coefficient is quite imprecisely estimated.

5 Conclusion

Our results suggest that common models of the impacts of exporting on firm performance may be upwardly biased due to a lack of appropriate controls for differences between entrants and non-entrants. In particular, propensity score matching can be significantly improved by including export history variables. When this more stringent control is applied almost all evidence of performance premia due to exporting disappears.

We find no evidence to suggest that exporting to high-income countries has a stronger effect on firm performance than exporting to low-income countries. While naïve regression estimates suggest slightly higher point estimates when considering only high-income entries, matching negates this finding.

Further, we find that the positive impacts of entry into exporting are due to an increase in employment and capital investment, rather than an improvement in the efficiency with which firms utilise inputs. This suggests that scale and reallocation effects may be of more importance to aggregate productivity benefits from exporting, rather than efficiency improvements at the firm-level. However, it must be noted that increases in capital intensity may in part reflect learning from offshore, if exporting helps firms to identify new technology in which to invest. Capital-intensity is a key difference between exporting and non-exporting firms, so much so that it creates difficulties in balancing the sample. This finding has implications for the interpretation of our work, where alternative matching methods may be necessary, and for other published papers, which focus on labour productivity and do not control for capital use.

A number of other issues remain unaddressed in this paper. These include methodological issues common to all analyses and others which are specific to the question at hand. The more general matters include ignoring endogeneity of inputs when measuring productivity. This issue may be material because simple OLS estimates of production functions bias capital coefficients down and exporters have higher capital-labour ratios. Possible solutions to this problem have been proposed by Olley and Pakes (1996), Levinsohn and Petrin (2003) and Ackerberg et al (2006) but have not yet been implemented using New Zealand data.

There remain issues around the continuity of firm identifiers in the LBD.

Firms which experience changes in ownership or legal structure are often allocated a new firm identifier by Statistics New Zealand. Such changes imply some mismeasurement of export histories and that some firms falsely appear to exit.[29] Our method of allocating exports within parent-subsidiary groups reduces this problem, but does not completely eliminate it.

More specific to this analysis are issues around the channels through which firms are believed to benefit from exporting. In this paper we consider only entry into new destination markets, but it is possible that market entry may interact with other characteristics of the firm, including the type of products traded, in determining the impact of exporting on performance. For example, it may be that learning only provides a benefit for exporters of sufficiently sophisticated products. Future work will explore in more detail differences across both markets and products. With respect to possible scale effects from exporting, it is also worth noting that most New Zealand exporters export only a very small proportion of their sales (Fabling et al 2008). If scale effects are indeed a major factor in firms' ability to improve their performance through exporting, these benefits may be restricted to a small subset of the population.

Further work is also needed to identify the dynamic effects on performance. In both the matching and the naïve regression models we can make no conclusions regarding the timing or dynamic aspects of the performance improvements based on the results outlined above. For example, our results show an employment premium of 16 percent in the second year after entry. However, we cannot tell whether this is due to a sustained higher growth rate of 8 percent per annum or a one-off jump of 16 percent when the firm starts exporting. Clearly such a distinction is important to understanding the long-term effects of LBE. Further analysis of timing may help explain the mechanisms through which performance improvements occur (eg, capacity utilisation versus access to technology).

Finally, (like other researchers before us) we have ignored the possibility of multiple (endogenous) treatments. That is, we have shown in previous papers that firms which are already in an export market are more likely to expand into new ones (Fabling et al forthcoming) and that firms with prior experience are more likely to survive when they enter markets (Fabling and Sanderson forthcoming). Further work is required to identify the relative impact of sequential entries.

[29] See Fabling and Sanderson forthcoming and Fabling and Grimes forthcoming for how this affects export history variables and firm attrition respectively.

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Table 8
Appendix A: Data definitions

Label	Description	Source
	OUTCOME VARIABLES	
	All outcome variables are calculated following Fabling and Grimes (forthcoming) and have industry-year averages removed	
ltotemp	Log Total Employment ($\ln L$) where L is working proprietors plus average monthly employees	LEED
klratio	Capital-labour Ratio ($\ln K - \ln L$) where K is capital services	AES, IR10, LEED
LP	Labour productivity ($\ln Y - \ln L$) where Y is value-added	AES, IR10, LEED
MFP	Multi-factor productivity ϵ from the regression: $\ln Y = \alpha \ln L + \beta \ln K + \text{ind\_year\_dummies} + \epsilon$	AES, IR10, LEED
	FIRM VARIABLES	
FDI	Dummy=1 if firm is foreign-owned (LBF foreign ownership $\geq$25% or IR4 foreign control)	LBF, IR4
rc01–rc18	Regional Council dummies set to one if the firm has some employment in the area[30]	LBF
	EXPORT HISTORY VARIABLES	
	Change variables are defined as differences across the two time periods $t \in \{-4, -3\}$ and $t \in \{-1, 0\}$	
export_year_count	Number of past five years in which the firm exported	Customs
inv_export_time	Inverse of number of years since firm last exported (set to zero if firm hasn't exported in past five years)	Customs
n_countries_5yr	Number of countries exported to over past 5 years	Customs
cty_entry	Number of countries entered in past 5 years	Customs
cty_exit	Number of countries exited in past 5 years	Customs
avg_export_share	Total exports over total sales in years of exporting	Customs, BAI
d_exports_incum_xbar	Export value growth in countries that are exported to at both the start and end of past five year period relative to average export value across the period	Customs
d_exports_entry_xbar	Export value growth in countries that are entered relative to average export value across the period	Customs
d_exports_exit_xbar	Export value growth in countries that are exited relative to average export value across the period	Customs