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Exporting and performance: Market entry, expansion and destination characteristics*

Richard Fabling and Lynda Sanderson†

Abstract

We examine the effect of export market entry on New Zealand firm performance. 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 with each of these markets providing additional potential for learning to occur. Focussing on new markets provides several benefits. Since we use matching techniques to account for self-selection, controlling for firm export histories reduces the problem of selection on unobservables (such as managerial preferences) which would confound a causal interpretation. Also, most new market entry is undertaken by incumbent exporters, providing a large number of events on which to test the learning-by-exporting (LBE) hypothesis. Our results suggest a powerful self-selection mechanism into exporting, with strong employment growth for first-time exporters and smaller (but still significant) gains associated with incumbent entry. Capital deepening is also important, though the dynamics differ between first-time and experienced exporters. Together these findings suggest a potentially important aggregate productivity growth effect arising from reallocation of resources to exporters.

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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 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 is generally referred to as "learning-by-exporting" (LBE) – a convention we follow.

While many empirical studies find evidence in support of LBE, many also fail to find such an effect (Greenaway and Kneller 2007; Wagner 2007). Comparison of results is complicated by differences in methodology, data availability, explanatory variables controlled for and the wide range of countries which have been studied.

This paper represents the first detailed study of LBE for New Zealand firms. 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 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

[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.

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. Finally, New Zealand's relatively poor aggregate 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.

Methodologically, we make use of propensity score matching to control for self-selection – not in itself a novel contribution. However, in our most stringent test, we subset on firms already exporting and look at the performance effect of entering new markets, controlling for past export experience. This approach has the benefit of focussing on a more homogenous group of firms (ie, exporters), and the ability to add variables with substantial explanatory power over the treatment variable (ie, export histories). From a theoretical perspective, we also expect that subsequent market expansion may yield learning effects.

While much of the literature on the firm-level consequences of exporting has focused on identifying labour or multifactor productivity improvements, we choose to 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. 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.

We find that New Zealand firms do exhibit gains from exporting, though these are largely limited to input growth. Employment effects appear to be persistent and exist for both first-time exporters and those expanding into new destinations. Contemporaneous labour productivity effects are also apparent for first-time exporters, driven largely by capital deepening in the year of entry. While there is tentative evidence of multifactor productivity (MFP) improvements following from new market expansion, these results are

not robust to subsetting on (high-income) markets where LBE effects might be expected to be largest.

In the next section we outline the existing empirical literature on exporting and productivity and review methodological options for identifying LBE, before describing the data and our empirical approach (section 3). Section 4 presents results and robustness tests, while section 5 summarises our findings.

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 attempted to identify the proximate determinants of exporting ability, including innovative ability (eg, Roper and Love 2002), government support (eg, Görg et al 2008) and demonstration effects from other local firms (Greenaway et al 2004; Greenaway and Kneller 2008; Fabling et al 2009a). 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 re-assessment of a number of early results, using more sophisticated techniques (eg, Wagner 2002; Girma et al 2004). This review focusses on three areas particularly relevant to this paper: the inclusion of multiple measures of firm performance; incorporation of destination country characteristics; and the appropriate method for establishing causal effects.

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 old 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 total factor productivity 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. Similarly, 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

It seems plausible that many of the potential benefits from exporting are stronger for exports to large, highly developed destinations. First, the competitive disciplines imposed upon exporting firms are likely to be more severe in markets with a significant number of local suppliers already and which 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 sophisti-

cation 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). Martins and Yang (2009) 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 on the likelihood of finding 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 labour or multifactor productivity, and whether the impacts are estimated for the year of entry or longer term.[6] Contextual issues are restricted to the sample size, the years covered, 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 comes 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 not conclusive evidence that destination country characteristics matter, it is consistent with a model in 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.

Five recent papers directly address the importance of destination market

[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 et al (2009b) 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.

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

characteristics using firm-level micro data. Trofimenko (2008) and Park et al (forthcoming) provide perhaps the most comprehensive treatment of heterogeneity in LBE outcomes.

Trofimenko (2008) allows 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.

Park et al (forthcoming) use exchange rate shocks during the Asian financial crisis as instruments to identify exogenous variation in the export levels of Chinese firms. They allow for the effect of exporting to vary continuously with destination country GDP per capita, finding a positive relationship between subsequent multifactor productivity growth and initial trading partner characteristics.[8] Park et al (forthcoming) suffers from similar data issues to Trofimenko. In particular, destination of trade is only known prior to the period over which productivity growth is measured, and almost half the firms in the study initially trade through Hong Kong, in which case industry averages must be used to identify final export destinations. Data also constrains the analysis to foreign-owned firms already exporting in the initial time period.

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 ex-

[8] Since the power of the instruments largely derives from an (unforseen) exchange rate shock, it is not clear whether there is a weak instrument problem for the subsample of firms that initially traded to developed economies (since China maintained a peg with the US Dollar throughout the crisis). If such a problem exists, positive productivity-based self-selection into exporting could result in a biased positive relationship between productivity growth and destination characteristics.

porting to high-income countries.[9] 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 independence from Yugoslavia in 1991, Slovenia has undergone a substantial programme of privatisation and trade liberalisation. 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,[10] nor that exports to former-Yugoslav countries (which up until the 1989 would not have even counted as exports) did not provide such benefits.[11]

The question therefore arises as to whether the results of Damijan et al (2004) and De Loecker (2007) – or, for that matter, Park et al (forthcoming) – 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.

[9] 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.

[10] 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.

[11] 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 LBE.

2.3 Methodology

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.

Figures 1 and 2 illustrate this point using New Zealand manufacturing data for the four performance metrics we consider in this paper – multifactor and labour productivity, the capital-labour ratio and log total employment.[12] Figure 1 provides kernel densities for three sub-populations – current exporters, past exporters and non-exporters. As in other countries, current New Zealand exporters are larger and more capital-intensive and, consequently, 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.[13] In contrast, however, we see little difference in the multifactor productivity levels of the three groups. The question is whether differences in performance reflect LBE or some selection mechanism.

Figure 2 addresses this question by comparing the distribution of *pre-entry* performance of firms that subsequently enter into exporting for the first time (into either low- or high-income countries) with those firms that do not. Firms about to enter into exporting have higher average labour productivity, capital intensity and employment, suggesting that at least some of the cross-sectional performance differences observed in figure 1 are due to the self-selection of larger, more capital-intensive firms into exporting.

Accounting for selection bias

One of the more contentious questions in quantifying LBE effects has been the appropriate implementation of controls for non-random selection. That

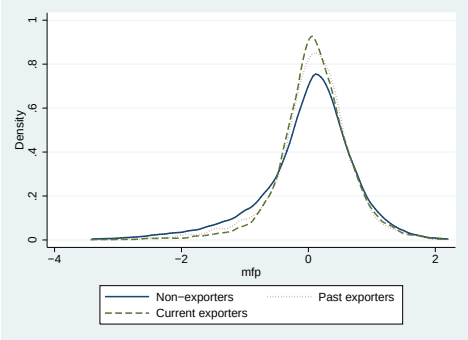
[12] All (two-digit ANZSIC) manufacturing industries are pooled together with industry-year averages removed.

[13] Employment comparisons are affected by the prevalence of very small firms among non-exporters, in particular working-proprietor only firms. For this reason we include a working-proprietor only dummy in subsequent analysis.

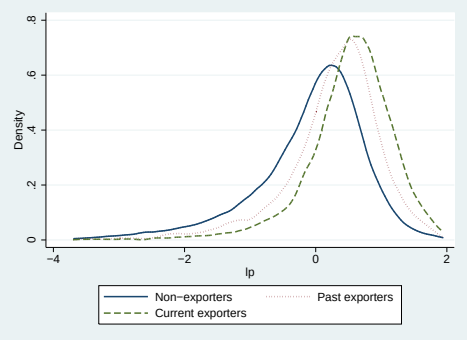
Figure 1

Kernel density – performance by current export status

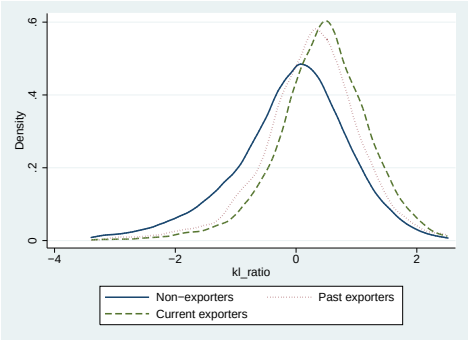
A. Multifactor productivity



B. Labour productivity



C. Capital-labour ratio



D. Employment

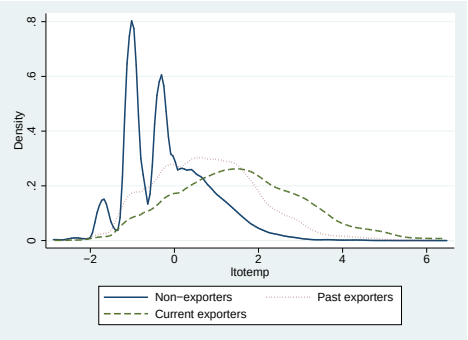
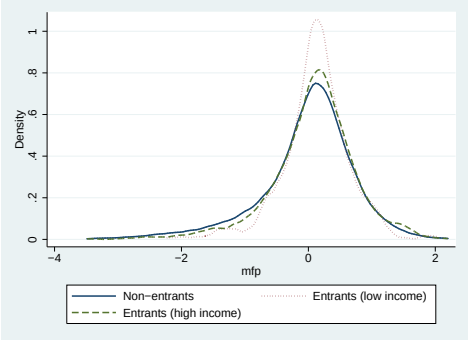


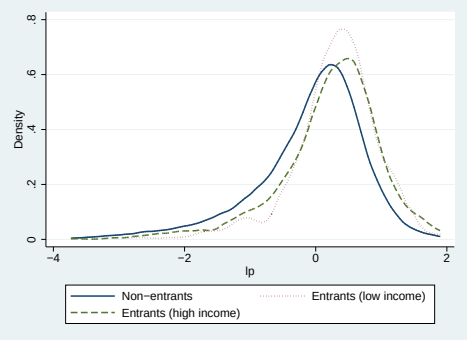
Figure 2

Kernel density – non-exporter performance by future export status

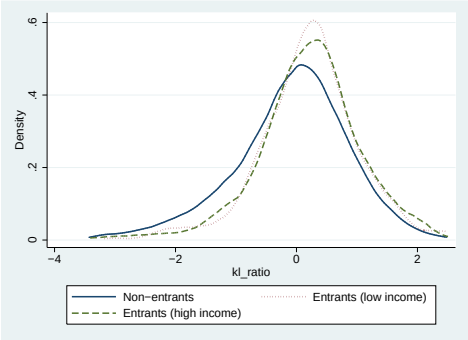
A. Multifactor productivity



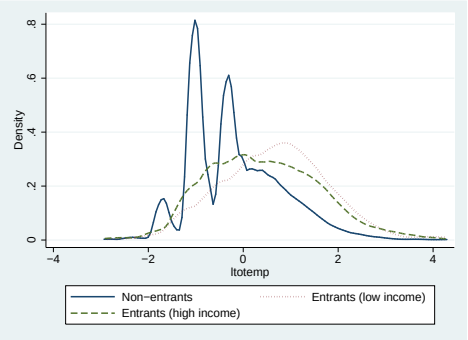
B. Labour productivity



C. Capital-labour ratio



D. Employment



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

$$\ln(PROD_{it}) = \alpha + \beta(EXPORT_{it}) + \gamma(Z_{it-1}) + \epsilon_{it} \quad \text{or,}$$

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

where $EXPORT_{it}$, $START_{it}$, $STOP_{it}$ and $CONTINUE_{it}$ 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 (Z_{it-1}). 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; Park et al forthcoming).

Critics of the standard regression approach argue that a comparison of exporting firms with all non-exporting firms gives a biased estimate of the returns to exporting. In particular, these authors 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 methods. 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.[14] The most common approach is to match firms based on the probability of receiving treatment conditional on pre-treatment characteristics – the "propensity score."[15] Rosenbaum and Rubin (1983) prove that as long as there are no

[14] Some treated firms may also be discarded if no suitable match can be found.

[15] 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 (Mahalanobis matching).

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 matches between treated and control firms and the requirements for determining how similar two firms must be to be considered a valid match.[16] 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 selection bias or whether there is instead some unobserved factor which determines both the probability of treatment and the later outcomes. For exporting and productivity, one possible such factor might be managerial incentives and ability. Managers focused on growing their firm might be expected to be both more likely to enter export markets and to pursue performance-improving technologies 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 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.

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

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

the reduction in sample size when using matching methods.[17] We follow the guidance of the literature and implement a mixed matching-DID approach, paying particular attention to the adequacy of the matching variables.

Other methodological issues

Other methodological issues discussed in the literature include the timing of any potential gains from exporting. As well as identifying whether future exporters show superior performance levels to non-exporters in the period prior to export market entry (the self-selection hypothesis) researchers have considered whether future exporters see a boost in productivity growth in the years leading up to market entry – the "learning-to-export" hypothesis (eg, Alvarez and López 2005). 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 (eg, Bellone et al 2008).

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 insight into the channels through which exporting affects performance. For example, since efficiency gains due to learning or competitive pressures may take some time to realise, instantaneous productivity effects are more likely to reflect improved capacity utilisation.

We provide empirical estimates of performance benefits from exporting up to two years after the entry decision, after identifying and controlling for pre-entry investment decisions.

[17] For example, if only three 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.

3 Data and empirical strategy

Estimates are based on Statistics New Zealand's prototype Longitudinal Business Database (LBD), which draws together administrative and survey data on all economically significant enterprises (firms) in the New Zealand economy (Fabling 2009). Firm performance measures (labour and multi-factor productivity, the capital-labour ratio, and total employment) are derived from Inland Revenue Department and Annual Enterprise Survey data,[18] while merchandise export activity is identified from data collected by the New Zealand Customs Service.[19] A full list of the variables used, their definitions and summary statistics by export status can be found in Appendix A.

We restrict attention to firms that ever have an employing manufacturing plant,[20] and to those firm-year observations for which we have a full set of the components necessary to calculate lagged outcome variables (employment, value-added, capital services), since these are clearly important variables in the matching process (see figure 2). As firm investments may also predict entry (eg, gearing up in anticipation of increased output), we also restrict the population to those firms where we can measure lagged changes in capital and labour inputs. These constraints yield an unbalanced panel of 87,270 observations, consisting of 25,977 firms over the six years 2001-2006.

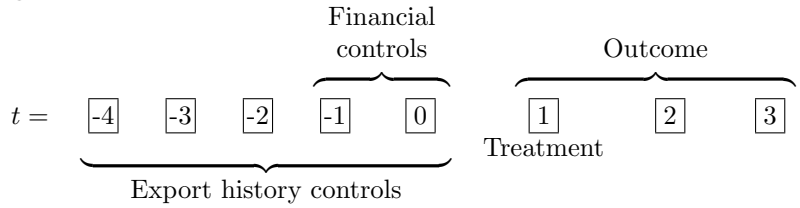
We implement propensity score matching based on lagged firm characteristics and compare difference-in-difference outcomes between entrants and non-entrants up to two years after market entry. In order to implement this approach we track firms over a five year window (figure 3): performance levels in the *"control year"* ($t = 0$) together with changes from the year prior ($t = -1$) provide the *ex ante* variables 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 years including and following treatment ($t \in 1, 2, 3$) are when we compare growth (relative to $t = 0$) in outcomes

[18] Following the method of Fabling and Grimes (2009) and dropping observations in the top and bottom one percent of capital-labour ratio and labour productivity distribution to remove implausible values.

[19] Matched to firms using tax identifiers and address information.

[20] In a small number of cases, observed exports are reallocated from non-manufacturing to manufacturing enterprises within the same parent-subsidiary group. The methodology and rationale for this allocation is documented in Fabling and Sanderson (2010). 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.

Figure 3
Timeline



between the treated and matched controls.[21] We additionally make use of export histories calculated over $-4 \leq t \leq 0$ to identify the appropriate population and to create additional matching variables, extending our data requirements up to a possible eight years.

We use the export history data to separate the population of firms into those with and without prior export experience. We then consider two alternative forms of entry – *"first-time"* entry into exporting and *"incumbent"* entry into new markets by firms with export experience. The latter definition is novel and allows an additional test of the LBE hypothesis.[22] Having already observed a firm exporting we can be more confident that they have the desire and ability to enter further export markets, reducing the chance that unobserved differences between treatment and control groups will bias the results. For example, the move into exporting can represent a significant increase in risk for New Zealand firms and many business owners may have no interest in making that leap, even if the firm's observable characteristics suggest an ability to export.[23] If performance gains from *first-time* exporting are caused by market expansion, or by learning driven by contact with new competitors, suppliers or consumers, we should naturally expect that entry into *additional* offshore markets should also lead to LBE.

[21] Statistics New Zealand's enterprise identifiers can be broken by changes in legal structure. We repair these breaks, for non-grouped firms, by using the permanent plant-level identifiers based on employee-tracking methods. We treat two firm ids as relating to the same business if one id ceases employing in a month, the other id starts employing in the next month and all employing plants move from the first to second id.

[22] This test bears some similarities with Park et al (forthcoming) who focus on exogenous changes in exports for incumbent exporters, as opposed to looking specifically at effects associated with entry into new geographic markets.

[23] According to the 2007 Business Operations Survey over half of manufacturing firms with 6+ employees do not earn any overseas income. Of those firms, 21 percent cite prohibitive costs or barriers to exporting, 54 percent note that the New Zealand market is sufficient, while 59 percent state that their requirement for physical proximity to customers prevents them from entering offshore markets (Statistics New Zealand 2008).

Table 1
Share of firms exporting by lagged export status

Years exported $-4 \leq t \leq 0$	Number of firms	Exporting at t=1 No	Exporting at t=1 Yes	Proportion (Yes/(Yes+No))
0	69,993	0.786	0.016	0.020
1	4,230	0.034	0.015	0.306
2	2,346	0.013	0.014	0.528
3	1,839	0.007	0.014	0.666
4	1,815	0.005	0.016	0.770
5	7,047	0.003	0.078	0.962

Table 2
Share of exporters and entrants by entry type and year

Year	Number of exporters	Proportion exporting $-4 \leq t \leq 0$	Entry rate First-time	Entry rate Incumbent
2001	2,475	0.178	0.017	0.347
2002	2,646	0.185	0.021	0.332
2003	2,790	0.190	0.023	0.347
2004	2,994	0.203	0.024	0.352
2005	3,093	0.210	0.020	0.345
2006	3,282	0.219	0.017	0.318
Total	17,280	0.198	0.020	0.340

Tables 1 and 2 demonstrate the importance of export history to identifying firms that may enter new markets. Table 1 tabulates export status in the treatment year with a count of prior years spent exporting. Firms with some experience are an order of magnitude more likely to export at $t = 1$ than those with no export history. Further, participation rates at $t = 1$ rise rapidly with prior participation. Table 2 summarises rates of entry into new export markets (ie, treatment rates). First-time entry is a rare event, with approximately two percent of firms entering in any given year. In contrast, incumbent entry is not rare with around a third of exporters adding one or more export destinations in a year. In fact, incumbent exporters, which make up 18 to 22 percent of all employing manufacturers (column 1), create the majority of new market entry events. Fabling and Sanderson (2010) show that a large proportion of aggregate trade growth comes from firms adding products or countries, implying that the dynamics of incumbent exporter expansion is important from a macroeconomic perspective.

To test whether relatively more sophisticated export markets yield greater

Table 3
Entry types

	Entry type	Population	Treatment
(1)	First-time (Any)	Firms that haven't exported in the past five years	Entry into exporting
(2)	First-time (High)	As above, but not firms only entering low-income countries	Entry into a high-income country
(3)	Incumbent (Any)	Firms with prior export experience	Entry into a new country
(4)	Incumbent (High)	As above, but not firms only entering low-income countries	Entry into a new high-income country

In high-income market entry models, we drop firms that enter a low-income country from the potential control group so as to consistently compare entrants to non-entrants across models.

opportunities for learning, we distinguish between entry into any market and high-income markets only, where these are defined as countries having GDP per capita greater than USD17,000.[24] 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.[25]

Table 3 summarises the four resulting populations and treatment variables. In each case we calculate the difference-in-difference estimate of the treatment effect for a matched sample of firms. Matching is based on predicted probabilities from a probit regression of the treatment variable on lagged firm performance variables, location,[26] and industry-year dummies. Lagged financial performance characteristics are included in all specifications and follow the outcome variables we are interested in: MFP; log total employ-

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

[25] In subsequent robustness testing, we apply a definition of high-income based on GDP per capita and OECD membership.

[26] 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. See Maré (2008) and Fabling et al (2009a) for New Zealand evidence of agglomeration and localised export learning effects respectively. Location dummies that never have statistically significant coefficients are pooled and constitute the reference group.

ment; and the capital-labour ratio (labour productivity is dropped because of its high correlation with the MFP variables).[27] Lagged performance variables are entered into the probit as a set of twenty quantile dummies to allow for potential non-linearity in their effect.[28] Pre-entry changes in capital and labour inputs are also included as are a foreign ownership dummy, a dummy for working-proprietor only firms, and a set of dummies capturing the number of years the firm has been in operation.

In specifications (3) and (4) we also add export history variables (defined in Appendix A). The estimated probability of treatment (or propensity score) is then used to match each treated firm to untreated firms within the same industry, using radius matching with replacement and a caliper of 0.001.[29] We then pool observations across all industries to compare the change in outcome between the treated and control groups. Standard errors are calculated by bootstrapping across both the first stage propensity score estimation and the second stage estimation of the treatment effect. The bootstrapped sample is drawn independently across four groups based on treatment status and availability of future outcome variables to maintain approximately the same matched sample size across repetitions.[30]

4 Results

4.1 Matching models

Table 4 reports the critical propensity score (probit) models on which the matching relies. 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 causal effects. The dependent variables (treatments) of the four models relate to first-time or incumbent entry into either any market or only high-income markets (table 3).

[27] Balancing tests performed on the excluded labour productivity variable confirm that the inclusion of MFP is sufficient to balance lagged labour productivity.

[28] Quantiles boundaries are recalculated for each population since the distributions of non-exporters and exporters are quite different (figure 1).

[29] Since all years are pooled, we exclude matches of treated firms to themselves in other years ("self-matches").

[30] The probit model is estimated across all firms having the lagged performance variables. Variation in the bootstrap sample size still arise due to the possible selection of treated firms for which no control lies within the caliper.

Table 4
Marginal effects probit – export market entry

	First-time		Incumbent	
	Any	**High**	**Any**	**High**
	(1)	(2)	(3)	(4)
Δtotemp	0.010***	0.007**	0.192***	0.178***
	[0.003]	[0.003]	[0.034]	[0.030]
δ(totemp$_{t=-1}=0$)	0.001	0.001	-0.137***	-0.103***
	[0.003]	[0.003]	[0.030]	[0.021]
Δklratio	-0.001	0.000	0.043*	0.035*
	[0.002]	[0.002]	[0.022]	[0.020]
wp_only	-0.006***	-0.004***	0.004	-0.012
	[0.002]	[0.001]	[0.021]	[0.016]
fdi	0.007	0.009*	-0.007	-0.014
	[0.005]	[0.005]	[0.014]	[0.012]
first_activity ($t=-4$)	-0.003	-0.003	-0.200***	-0.162***
	[0.002]	[0.002]	[0.043]	[0.042]
first_activity ($t=-3$)	0.000	-0.001	-0.131***	-0.085***
	[0.002]	[0.002]	[0.033]	[0.025]
first_activity ($t=-2$)	0.000	-0.001	-0.117***	-0.082***
	[0.002]	[0.002]	[0.035]	[0.026]
first_activity ($t=-1$)	-0.001	-0.001	-0.077**	-0.056*
	[0.002]	[0.002]	[0.038]	[0.030]
δ(exports$_{t=0}>0$)			0.076***	0.057***
			[0.012]	[0.010]
lcountry_incum			0.067***	0.044***
			[0.008]	[0.007]
lcountry_entry			0.131***	0.112***
			[0.008]	[0.007]
lcountry_exit			-0.023**	-0.027***
			[0.009]	[0.009]
δ(country_incum $=0$)			-0.053***	-0.036***
			[0.012]	[0.011]
δ(country_entry $=0$)			-0.080***	-0.045***
			[0.012]	[0.011]
δ(country_exit $=0$)			-0.055***	-0.043***
			[0.011]	[0.010]
Δexports			0.033***	0.040***
			[0.010]	[0.009]
oz_export_share			-0.013	0.019
			[0.016]	[0.015]
δ(oz_exports >0)			-0.055***	-0.072***
			[0.015]	[0.015]
non_oz_hi_export_share			0.037**	0.061***
			[0.017]	[0.015]
δ(non_oz_hi_exports >0)			0.034***	0.034***
			[0.013]	[0.011]
lavg_exports_per_emp			0.008***	0.007***
			[0.003]	[0.002]
MFP, klratio, ltotemp		+ (see Appendix B)		
N	69,990	69,651	17,280	14,868
Pseudo R^2	0.084	0.078	0.210	0.194
Treatment rate	0.020	0.016	0.340	0.233

Robust (clustered on firm) standard errors in brackets (significance at * 10%; ** 5%; *** 1%). Regressions include (unreported) region and industry-year dummies. Coefficients for lagged performance quantile dummies are reported in Appendix B.

For presentation purposes, coefficients related to lagged performance quantile dummies appear in Appendix B. Those coefficients confirm the picture conveyed by figure 2 that larger, more productive and more capital intensive firms are more likely to self-select into exporting. Not only is this finding supported for first-time entry, but prior performance (or scale at least) is also important for selection into new markets by incumbent exporters.

Returning to table 4, we see that pre-entry employment growth (Δtotemp) also predicts entry for both first-time and incumbent exporters consistent with likely some "gearing up" in production capacity pre-entry. Capital deepening (Δklratio) is also a precursor to market expansion for incumbent, but not first-time, entrants – a distinction we return to when discussing the investment dynamics subsequent to entry (section 4.2).

Working-proprietor only firms are less likely to enter into exporting and so are hardly present in the incumbent population. In contrast, foreign-owned firms are more likely to enter high-income markets for the first time, even after conditioning out higher average productivity levels. Finally, for incumbents, firms that are younger are consistently more likely to enter into exporting or new markets. For example, an incumbent exporter that is first observed with sales at $t = -4$ (first_activity($t = -4$)) is 20 percent less likely to enter a new export market than a firm that has just started up ($t = 0$ being the omitted category). Consistent with that picture, firms transitioning from non-employing at $t = -1$ to employing at $t = 0$ are around six to seven percent more likely to enter a new market than firms with static employment.[31]

Not only does subsetting on incumbent exporters raise the theoretical plausibility of controlling for self-selection, the explanatory power of the model (pseudo R^2) roughly doubles due to the population change and inclusion of export experience variables (columns (3) and (4)). Export-related variables are chosen to capture the dynamics of the firm's trade history – with the hope that matching on them increases the likelihood of selecting controls that have similar *treatment* histories, so that estimated effects of new market entry are not spuriously based on, say, lagged first-time entry effects.[32] In particular, we control for the change in the number of countries that a firm is trading to over the last five years, decomposing that into (log) counts of entering,

[31] Being the sum of the Δtotemp and $\delta(\text{totemp}_{t=-1} = 0)$ coefficients since Δtotemp$= 1$ for entering employers.

[32] Unfortunately, we do not have sufficient data to match exactly on lagged treatment variables as these would require more than five years of consistently measured export history data, given our definition of treatment.

exiting and incumbent countries,[33] and the (normalised) change in export value over the period. Firms with larger portfolios of destinations (lcountry_incum), those that have been adding destinations over the recent past (lcountry_entry), those that are growing their export value (Δexports), and those experiencing a continuous exporting spell (ie, $\delta(\text{exports}_{t=0} > 0)$) are more likely to enter a new country. Firms exiting countries (lcountry_exit) are less likely to enter.

Additionally, since Australia is a geographically, culturally and institutionally close market we control for the fact that firms that send a large proportion of their exports there may not be well equipped or inclined to export elsewhere. Similarly, we control for other high-income country export shares on the grounds that these markets may selectively favour more "export-able" firms, and for the possibility that more export-intensive firms are more likely to add new destinations. Consistent with these expectations, firms that trade to Australia ($\delta(\text{oz\_exports}_{t=0} > 0)$) are six to seven percent less likely to enter new markets while firms already exporting to other high-income countries are more likely to enter and this likelihood rises with the share of lagged exports going to high-income countries. Finally, firms with higher export sales per employee are also more likely to enter new markets.

4.2 Causal estimates

Having, at least partially, explained treatment using lagged firm performance and trade history variables, we now examine the causal effects of exporting on performance. Tables 5 and 6 presents difference-in-difference results for the weighted matched populations.

Beginning with the standard methodology of comparing first-time entrants with matched non-exporters, table 5 reveals a positive causal relationship between exporting and three of the four performance variables. New exporters exhibit a growth premium of around 3-4 percent in labour productivity, 4-5 percent in the capital-labour ratio, and 7-12 percent in employment relative to non-exporters, but are not significantly different from non-exporters with respect to multifactor productivity. Consistent with a zero effect on MFP, labour productivity results are completely explained by higher post-entry capital-labour ratios. Growth in the capital-labour ratio appears to be immediate, one-off and persistent, while employment gains continue over

[33] Dummies are included and set to one where the relevant count of countries is zero, while the continuous (logged) variable is set to zero in these cases.

time though with most growth occurring in the year of entry (reported coefficients are cumulative over time). However, since the employment differential between entrants and matched non-entrants grows and the capital-labour ratio differential stays approximately constant, net capital investment growth rates must also be (roughly two percent) higher for entrants after entry. Thus first-time entrants make large investments in the year of entry, with smaller ongoing investment in inputs over subsequent years and no obvious learning effects.

Results for first-time high-income market entry events are also reported in table 5. Perhaps because around 76 percent of first-time entry involves a high-income destination market, we cannot identify any significant difference between high-income and any country entry. However, the fact that point estimates of significant coefficients are always higher for high-income entrants is suggestive of the potential importance of destination country development levels in subsequent outcomes.

By comparison, input growth dynamics are weaker for entry into any new destination country by incumbent exporters (table 6). These results may well reflect timing issues – recalling that gearing up in both employment and capital is a strong predictor of incumbent entry (table 4). Under this interpretation, firms either learn about the need to scale up when entering new markets and do so in advance of entry, or have more certainty of likely success from future entry and so commit earlier to investment decisions. There is weak evidence that employment continues to expand post-entry (coefficients rise from 2.8 percent in the year of entry to 4.5 percent two years after entry), even after controlling for pre-entry growth trajectories.

Labour productivity growth estimates are similar between first-time and incumbent (any country) entrants, with point estimates for incumbents of around 3-5 percent. In the case of incumbents though, we cannot invoke a differential capital investment rate to explain higher productivity growth, since we match on pre-entry levels and growth rates of the capital-labour ratio and do not see significant post-entry differences in the capital-labour ratio. Where incumbent entry really differs from first-time entry is in the identified multifactor productivity effects, suggestive of true learning-by-exporting. However, when we restrict our attention to high-income country entry events, productivity effects disappear while employment results remain similar. The absence of consistent estimates of the MFP results for a subset of firms that, at least on some theoretical grounds, should be *more* likely to benefit from LBE casts doubt on the underlying source of the productivity gains.

Table 5
Causal effect of first-time entry into exporting

	Any country			High-income country		
	$t=1$	$t=2$	$t=3$	$t=1$	$t=2$	$t=3$
	(1)	(2)	(3)	(4)	(5)	(6)
MFP	0.015	0.001	0.002	0.031	0.000	-0.012
	[0.016]	[0.020]	[0.023]	[0.020]	[0.024]	[0.028]
klratio	0.037**	0.048*	0.045*	0.042**	0.057**	0.052*
	[0.017]	[0.024]	[0.024]	[0.017]	[0.023]	[0.030]
LP	0.040**	0.033*	0.037	0.059***	0.037	0.030
	[0.017]	[0.020]	[0.025]	[0.050]	[0.024]	[0.030]
ltotemp	0.073***	0.091***	0.116***	0.078***	0.098***	0.125***
	[0.012]	[0.019]	[0.024]	[0.015]	[0.022]	[0.027]
N	55,104	41,100	29,826	54,816	40,872	29,664
Treatment rate	0.022	0.023	0.024	0.017	0.018	0.019
Proportion dropped						
Treated	0.018	0.022	0.025	0.013	0.025	0.032
Control	0.062	0.082	0.138	0.081	0.100	0.172

Difference-in-difference (DID) estimator, from $t=0$ to outcome year, applied to matched sample. Radius matching (caliper 0.001, with replacement) with observations pooled across years and matched within two-digit industry (precluding self-matches). Bootstrapped standard errors in brackets (significance at * 10%; ** 5%; *** 1%). Bootstrapping encompasses both probit and DID stages (100 repetitions) and is stratified on treatment and the existence of future MFP to maintain approximately constant (weighted) population size (N) across estimates. The table also reports the treatment rate (average proportion of firms entering), and the proportion of treated (control) firms dropped because there is no control (treated) firm within the caliper distance. All balancing tests (equivalence of weighted means of matching variables across treated and controls) passed at the 5% level (one-sided test).

Table 6
Causal effect of incumbent entry into new export markets

	Any country			High-income country		
	$t=1$	$t=2$	$t=3$	$t=1$	$t=2$	$t=3$
	(1)	(2)	(3)	(4)	(5)	(6)
MFP	0.026**	0.039**	0.024	-0.009	-0.010	-0.011
	[0.013]	[0.018]	[0.026]	[0.019]	[0.027]	[0.031]
klratio	-0.014	0.015	0.023	-0.013	0.024	0.034
	[0.014]	[0.017]	[0.025]	[0.018]	[0.024]	[0.032]
LP	0.026*	0.048***	0.041*	-0.008	0.003	0.005
	[0.013]	[0.018]	[0.025]	[0.020]	[0.028]	[0.030]
ltotemp	0.028***	0.032**	0.045**	0.036***	0.034**	0.021
	[0.008]	[0.015]	[0.020]	[0.010]	[0.016]	[0.026]
N	14,670	11,262	8,325	12,522	9,564	7,074
Treatment rate	0.354	0.363	0.364	0.243	0.250	0.252
Proportion dropped						
Treated	0.322	0.373	0.452	0.294	0.358	0.412
Control	0.305	0.371	0.482	0.378	0.477	0.529

See table 5 for notes. All balancing tests passed at the 5% level (one-sided test) except in the case of the first_activity ($t=-4$) for specifications (1) and (3), and δ(lcountry_exit = 0) for specification (6).

Table 7
Causal effect of first-time entry excluding subsequently treated

	Any country		
	$t = 1$	$t = 2$	$t = 3$
	(1)	(2)	(3)
MFP	0.015	-0.008	-0.001
	[0.016]	[0.020]	[0.034]
klratio	0.037**	0.037	-0.009
	[0.017]	[0.024]	[0.040]
LP	0.040**	0.014	0.009
	[0.017]	[0.020]	[0.034]
ltotemp	0.073***	0.065***	0.067**
	[0.012]	[0.021]	[0.029]
N	55,104	36,324	23,397
Treatment rate	0.022	0.019	0.017
Proportion dropped			
Treated	0.018	0.026	0.022
Control	0.062	0.147	0.276

See table 5 for notes. Population excludes firms treated in years after $t = 1$ and up to the and including the outcome year. All balancing tests passed at the 5% level (one-sided test) except in the case of the Auckland Regional Council dummy for specification (3).

Taking the first-time and incumbent employment and investment dynamics together it seems possible that the subsequent growth dynamics for first-time entrants are driven by expansion into additional export markets (ie, subsequent incumbent entry). To test this idea we repeat the analysis for first-time entry, but excluding from the population firms which are treated (or re-treated) in years up to and including the year the effect is measured (table 7).[34]

The exclusion of future-treated firms has the effect of removing the apparent ongoing employment growth effect from the first-entry event, consistent with our earlier interpretation that ongoing growth in employment may be driven by subsequent entry events. Taken at face value, these coefficients would imply a one-off scale benefit from first-time entry, rather than the dynamic gains implied by the estimates in table 5. However, using future export decisions to determine the population is fraught with causality concerns. An

[34] Obviously this restriction places no constraint on the $t = 1$ results and these are merely repeated for convenience. Only results for any entry event are reported for brevity, but the patterns are consistent for high entry events also.

alternative interpretation of the rising employment growth effect from first-time entrants is that the first entry event raises firm scale and this, coupled with the recent experience in exporting (as evidenced by table 4), *causes* firms to enter additional markets. In this sense, we should attribute the ongoing employment growth to the initial decision to enter into exporting. We return to the issue of serial (endogenous) treatment in the conclusions.

4.3 Robustness

Another possible explanation for the apparent rise in employment growth coefficients may be changes in the composition of the sample at different times because of attrition. To check this we perform two (unreported) robustness checks on the any country specifications. Firstly, since employment data has greater coverage than value-added data we reestimate causal effects solely for employment. Additionally, we reestimate the model for all three time periods for the subset of firms that have outcome data in every period. In both cases, results continue to show increasing employment gains over time for first-time entrants. While other previously significant point estimates remain positive in the common sample test, only the $t = 0$ capital-labour ratio results for first-time entrants, and the ($t = 2$) MFP and ($t = 2, 3$) labour productivity results for incumbents, are now significantly different from zero.

Declining significance for some coefficients partly reflects the effect of sample size on standard errors, but may also suggest the possibility that the central estimates are biased by selective attrition from the sample. If export entry raises performance and firms exit based on competitiveness (eg, there is a market-specific productivity threshold below which it is unprofitable to remain in operation), then coefficients may be biased downwards. In the common sample test above then, we lose good (in a *match* sense) control firms who could be included in $t = 2$, say, with poor growth outcomes but subsequently exit in $t = 3$ for performance reasons.

To test whether attrition bias is potentially an issue, we match treated firms to controls ignoring the availability of future employment data and then test whether the matched control group are more likely to have future employment data missing (ie, to exit).[35] Across all three time periods ($t = 1, 2, 3$), and for both first-time and incumbent entry, control firms are more likely to exit (significant at the ten percent level), suggesting that entering exporting and

[35] We use employment here as the measure because it is based on comprehensive mandatory PAYE tax filings, and so is not subject to idiosyncratic filing patterns.

expanding markets reduce the probability of exit. As a consequence, tables 5 and 6 may represent underestimates of the true causal effects of entry.

Focussing on the $t = 2$ (any country) estimates as generally representative, we also try excluding firms with two or less employment to test whether employment effects are driven by the small number of working-proprietor only firms entering into exporting. When low employment firms are excluded, estimated employment gains decline from 9.1 percent for first-time entrants to 5.8 percent (still significant at the one percent level). Incumbent entry employment results are almost unchanged, reflecting the fact that very few incumbent exporters are small. The labour productivity and capital-labour ratio coefficients becomes insignificant (at the ten percent level) for first-time entrants to any country, while productivity effects for incumbent entry remain significant and consistent with main estimates.

Other robustness tests performed on all the $t = 2$ results include: changing the high-income criteria to require the country to also be an OECD member (thus excluding most oil-rich nations, tax havens, etc); and dropping from the population firms that have low historical export intensities (the bottom quartile of lavg_export_per_emp) or low shares of differentiated goods exports.[36] These tests failed to highlight particular weakness in the results. Specifically, only two changes occur in the significance of coefficients across these three tests.[37]

5 Conclusion

Visual investigation (figure 2) and probit estimates (table 4 and Appendix B) both support the expectation that better New Zealand firms self-select into exporting. Given such self-selection, any credible attempt to estimate the effect of entry on firm performance must adequately account for preexisting differences between entrants and non-entrants. We do this by matching entering (treated) firms to non-entering (control) firms whose lagged characteristics indicate they were similarly likely to enter. We then bootstrap difference-in-difference estimates of outcomes across treated and control groups.

Concern about the adequacy of the matching model for first-time entrants,

[36] Less than ten percent of exports in differentiated products, as defined by Rauch (1999).

[37] With the modified definition of high-income countries the labour productivity effect becomes significant for first-time entrants (the point estimate increases to 4.1 percent). For the differentiated goods subsample, the incumbent (high-income) entrant employment coefficient becomes insignificant at the 10 percent level.

and consideration of the theoretical bases for believing treatment effects might exist, lead us to subset on incumbent exporters and consider the effect on these firms of the decision to enter a new market for the first time. Our results suggest that propensity score matching can be significantly improved in this manner since we can plausibly argue that unobservable characteristics that would determine firms' ability to export are controlled for by having already observed these firms exporting (ie, exporters are matched to exporters), and by the fact that exporters have more available matching variables, in particular their detailed export histories. Inclusion of these variables is critical to our ability to assert that measured causal effects are due to the current treatment and not simply hangovers from some earlier (perhaps initial) entry into exporting.

For new exporters we find that entry is associated with strong employment growth coupled with ongoing capital investment and, hence, raised capital-labour ratios. As a consequence, first-time entrants have "permanently" higher measured labour productivity than they would otherwise have had. These effects are economically material – the year after entry, employment is 9.1 percent higher and labour productivity 3.3 percent higher. For incumbent exporter entry, causal effects are also observed on employment, though in a more modest 3-5 percent range. This finding holds despite the fact that incumbents gear up both employment and capital investment prior to entry (table 4) – factors that are accounted for in the matching method.

Together, these investment dynamics raise questions as to whether the identified "first-time" entry effect should be interpreted as solely attributable to that event or also to subsequent export entry decisions. Firms already in an export market are more likely to expand into new ones (Fabling et al 2009a) and firms with prior experience are more likely to survive when they enter markets (Fabling and Sanderson 2010). Improvements to econometric techniques for analysing multiple (endogenous) treatment events are necessary before the relative impact of sequential entries can be fully unravelled.[38]

Employment effects are robust to population variations including dropping firms largely producing undifferentiated goods or with relatively low exports per employee, to changes in the population based on data availability, and to

[38] As far as we are aware, Lechner and coauthors are the only researchers to have applied sequential causal matching models to economic questions (eg, Lechner 2009, Lechner and Wiehler 2010). Using such models in the current paper would be difficult since their methodology relies on observing the entire sequence of treatments – a requirement that would yield a very small population of multiply-treated firms given our choice of treatment variable and the data available.

an alternative definition of high-income market entry. Only in the case where very small firms (with two or less employment) are dropped are employment coefficients materially lower for first-time entrants, though still significantly different from zero at the one percent level.

We find tentative evidence to suggest that exporting to high-income countries has a stronger effect on firm performance, with point estimates for this subsample generally higher than for the full population of entry events for first-time entrants (though never statistically significantly higher). Evidence for multifactor productivity learning-by-exporting effects – as opposed to labour productivity increases driven by capital deepening – are restricted to incumbent entry into new markets. However, when we subset on high-income entry events, these effects disappear, raising questions about the source of the estimated productivity gains. True learning effects should, if anything, be stronger for entry into markets from which more can be learned.

Even in the absence of compelling MFP effects, our results suggest that export market entry yields welfare gains through resource reallocation. Capital-intensity differences between exporters and non-exporters are striking – being in the top quartile of the capital-labour ratio is associated with a roughly two percent higher probability of entry into exporting (Appendix B), and this capital-intensity gap widens post-entry. Understanding differences in capital intensity and usage may represent a key area for deeper insight into why some firms can export and others can't or won't. Expansion into new export markets draws employment into firms that have a clear (labour) productivity advantage – an advantage that is not lost as employment expands. Further, our lack of findings for conclusive LBE-derived productivity gains do not in themselves imply no "learning" has occurred. Expansion itself – growing employment by more than ten percent – may require learning in terms of management systems, quality control, etc. Understanding how firms adapt their business operations during this period of rapid growth should be a research priority.

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Appendix A: Summary statistics and data definitions

Table 8
Matching variable summary statistics by export status

	Non-exporter $(-4 \leq t \leq 0)$		Exporter $(-4 \leq t \leq 0)$	
	Mean	St. dev.	Mean	St. dev.
MFP	-0.017	0.768	0.082	0.596
klratio	-0.078	0.968	0.381	0.803
LP	-0.085	0.859	0.457	0.697
ltotemp	-0.171	0.998	1.386	1.540
Δtotemp	0.142	0.353	0.050	0.231
Δklratio	0.017	0.230	0.024	0.196
$\delta(\text{totemp}_{t=-1} = 0)$	0.119		0.038	
wp_only	0.468		0.102	
fdi	0.006		0.114	
first_activity$(t = -4)$	0.693		0.907	
first_activity$(t = -3)$	0.068		0.034	
first_activity$(t = -2)$	0.072		0.028	
first_activity$(t = -1)$	0.076		0.020	
lcountry_incum			0.566	0.914
lcountry_entry			0.483	0.726
lcountry_exit			0.299	0.606
Δexports			0.209	0.618
oz_export_share			0.502	0.418
non_oz_hi_export_share			0.273	0.368
lavg_exports_per_emp			8.066	2.415
$\delta(\text{exports}_{t=0} > 0)$			0.752	
δ(country_incum = 0)			0.381	
δ(country_entry = 0)			0.304	
δ(country_exit = 0)			0.526	
δ(oz_exports > 0)			0.774	
δ(non_oz_hi_exports > 0)			0.604	

Performance variables follow Fabling and Grimes (2009). Top and bottom 1% of labour productivity and capital-labour ratio dropped. Changes in exports defined across $t \in \{-4, -3\}$ and $t \in \{-1, 0\}$. Data sources: L (LEED); Y and K (AES and IR10); FDI (LBF and IR4); trade (Customs); sales (BAI).

ltotemp	Log total employment ($\ln L$) where L is working proprietors plus average monthly employees
klratio	Capital-labour ratio ($\ln K - \ln L$) where K is capital services
LP	Labour productivity ($\ln Y - \ln L$) where Y is value-added
MFP	Multi-factor productivity, ϵ, from OLS regression: $\ln Y = \alpha \ln L + \beta \ln K + c + \epsilon$ with industry-specific α, β, c
wp_only	Dummy=1 if working proprietors only
FDI	Dummy=1 if firm is foreign-owned (LBF foreign ownership $\geq$25% or IR4 foreign control)
first_activity($t = x$)	Dummy=1 if x is the earliest year where sales observed in the prior five years
δ(totemp$_{t=-1} = 0$)	Dummy=1 if firm is non-employing at $t = -1$
lcountry_X	Log count of countries entered, exited or incumbent in (= 0 if none)
δ(country_X) = 0	Dummy=1 if count of countries equals zero
ΔX	Normalised change in X, ie $\Delta X/\Sigma X$
oz_export_share	Export share to Australia
δ(oz_exports > 0)	Dummy=1 if oz_export_share> 0
non_oz_hi_export_share	Export share to other high-income
δ(non_oz_hi_exports > 0)	Dummy=1 if nonoz_hi_export_share> 0
lavg_exports_per_emp	Log of average exports per employee
exporter($t = 0$)	Dummy=1 if firm exported in $t = 0$

Appendix B: Quantile dummies for selection equations

Table 9
Export market entry - MFP quantile dummies

	(1)	(2)	(3)	(4)
Quantile 2	0.001	0.000	-0.006	-0.021
	[0.003]	[0.003]	[0.024]	[0.020]
Quantile 3	0.000	-0.001	-0.011	-0.013
	[0.003]	[0.002]	[0.024]	[0.020]
Quantile 4	0.004	0.001	-0.032	-0.039**
	[0.004]	[0.003]	[0.023]	[0.019]
Quantile 5	0.001	0.000	0.015	0.006
	[0.003]	[0.003]	[0.025]	[0.022]
Quantile 6	-0.001	-0.002	-0.034	-0.025
	[0.003]	[0.002]	[0.024]	[0.020]
Quantile 7	-0.002	-0.003	-0.016	-0.005
	[0.003]	[0.002]	[0.024]	[0.021]
Quantile 8	-0.002	-0.003	0.016	-0.001
	[0.003]	[0.002]	[0.025]	[0.021]
Quantile 9	-0.001	-0.002	-0.023	-0.033*
	[0.003]	[0.002]	[0.024]	[0.019]
Quantile 10	0.000	-0.003	-0.021	-0.017
	[0.003]	[0.002]	[0.024]	[0.021]
Quantile 11	0.004	0.000	0.012	0.006
	[0.003]	[0.002]	[0.025]	[0.022]
Quantile 12	0.004	0.001	0.005	-0.008
	[0.003]	[0.003]	[0.025]	[0.022]
Quantile 13	-0.002	-0.003	0.011	-0.010
	[0.003]	[0.002]	[0.025]	[0.021]
Quantile 14	0.000	-0.002	0.010	0.008
	[0.003]	[0.002]	[0.025]	[0.022]
Quantile 15	0.004	0.001	0.021	0.006
	[0.004]	[0.003]	[0.025]	[0.022]
Quantile 16	-0.001	-0.001	0.034	0.008
	[0.003]	[0.002]	[0.025]	[0.022]
Quantile 17	0.006	0.003	0.042*	0.019
	[0.004]	[0.003]	[0.025]	[0.022]
Quantile 18	0.002	0.001	0.045*	0.009
	[0.003]	[0.003]	[0.025]	[0.021]
Quantile 19	0.006	0.003	0.016	-0.002
	[0.004]	[0.003]	[0.025]	[0.021]
Quantile 20	0.013***	0.009**	0.038	0.003
	[0.005]	[0.004]	[0.026]	[0.022]

Table 4 lagged MFP quantile dummies.

Table 10
Export market entry - klratio quantile dummies

	(1)	(2)	(3)	(4)
Quantile 2	-0.002	-0.001	0.004	0.001
	[0.003]	[0.003]	[0.025]	[0.023]
Quantile 3	0.003	0.003	0.039	0.006
	[0.004]	[0.004]	[0.026]	[0.022]
Quantile 4	0.003	0.003	0.028	0.027
	[0.004]	[0.003]	[0.026]	[0.024]
Quantile 5	0.005	0.003	0.043	0.015
	[0.004]	[0.003]	[0.026]	[0.023]
Quantile 6	0.008*	0.006	0.063**	0.035
	[0.005]	[0.004]	[0.027]	[0.024]
Quantile 7	0.008*	0.007	0.025	0.006
	[0.004]	[0.004]	[0.026]	[0.022]
Quantile 8	0.011**	0.009**	0.023	0.016
	[0.005]	[0.004]	[0.026]	[0.023]
Quantile 9	0.008*	0.007*	0.043	-0.008
	[0.004]	[0.004]	[0.027]	[0.022]
Quantile 10	0.010**	0.010**	0.020	0.008
	[0.005]	[0.004]	[0.026]	[0.023]
Quantile 11	0.012**	0.011**	0.032	-0.002
	[0.005]	[0.005]	[0.026]	[0.023]
Quantile 12	0.014***	0.008**	0.056**	0.028
	[0.005]	[0.004]	[0.027]	[0.024]
Quantile 13	0.013**	0.011**	0.057**	0.014
	[0.005]	[0.005]	[0.027]	[0.023]
Quantile 14	0.017***	0.013***	0.019	-0.012
	[0.006]	[0.005]	[0.026]	[0.022]
Quantile 15	0.018***	0.016***	0.034	0.022
	[0.006]	[0.005]	[0.026]	[0.024]
Quantile 16	0.021***	0.018***	0.065**	0.026
	[0.006]	[0.006]	[0.027]	[0.024]
Quantile 17	0.013**	0.011**	0.000	-0.002
	[0.005]	[0.005]	[0.026]	[0.022]
Quantile 18	0.018***	0.015***	0.032	0.002
	[0.006]	[0.005]	[0.026]	[0.022]
Quantile 19	0.017***	0.015***	0.060**	0.017
	[0.006]	[0.005]	[0.027]	[0.023]
Quantile 20	0.029***	0.023***	0.051*	0.032
	[0.007]	[0.007]	[0.027]	[0.024]

Table 4 lagged klratio quantile dummies.

Table 11
Export market entry - ltotemp quantile dummies

	(1)	(2)	(3)	(4)
Quantile 2	0.014**	0.011**	0.020	0.015
	[0.006]	[0.005]	[0.029]	[0.025]
Quantile 3	0.008	0.010*	0.019	0.019
	[0.006]	[0.006]	[0.028]	[0.025]
Quantile 4	0.014**	0.012**	0.045	0.032
	[0.006]	[0.006]	[0.031]	[0.027]
Quantile 5	0.013**	0.011**	0.106***	0.046
	[0.006]	[0.005]	[0.034]	[0.029]
Quantile 6	0.012**	0.009*	0.076**	0.054*
	[0.006]	[0.005]	[0.033]	[0.031]
Quantile 7	0.020***	0.015**	0.094***	0.042
	[0.007]	[0.006]	[0.035]	[0.030]
Quantile 8	0.016**	0.017***	0.107***	0.081**
	[0.006]	[0.006]	[0.034]	[0.032]
Quantile 9	0.020***	0.020***	0.129***	0.078**
	[0.007]	[0.007]	[0.035]	[0.032]
Quantile 10	0.024***	0.023***	0.105***	0.068**
	[0.008]	[0.007]	[0.035]	[0.032]
Quantile 11	0.029***	0.019***	0.155***	0.058*
	[0.008]	[0.007]	[0.035]	[0.031]
Quantile 12	0.032***	0.028***	0.145***	0.109***
	[0.009]	[0.008]	[0.035]	[0.034]
Quantile 13	0.028***	0.022***	0.170***	0.082**
	[0.008]	[0.007]	[0.036]	[0.032]
Quantile 14	0.029***	0.025***	0.166***	0.115***
	[0.008]	[0.008]	[0.035]	[0.035]
Quantile 15	0.038***	0.031***	0.184***	0.128***
	[0.010]	[0.009]	[0.036]	[0.035]
Quantile 16	0.045***	0.035***	0.233***	0.128***
	[0.011]	[0.009]	[0.035]	[0.034]
Quantile 17	0.048***	0.038***	0.225***	0.141***
	[0.011]	[0.010]	[0.036]	[0.036]
Quantile 18	0.063***	0.045***	0.265***	0.171***
	[0.013]	[0.011]	[0.035]	[0.037]
Quantile 19	0.077***	0.063***	0.236***	0.187***
	[0.014]	[0.013]	[0.036]	[0.037]
Quantile 20	0.112***	0.092***	0.297***	0.236***
	[0.018]	[0.017]	[0.041]	[0.041]

Table 4 lagged ltotemp quantile dummies.