Department for Business & Trade

Analysis

The impact of export promotion activities on firm outcomes

An econometric evaluation prepared for the Department for Business & Trade

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frontier economics

This is a report of research carried out by Frontier Economics, on behalf of the Department for Business & Trade.

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Executive Summary

Context

Frontier Economics was commissioned by the Department for Business & Trade (DBT) to assess the economic impact of DBT's export promotion activities on the firms which receive them.

Our study covers a range of export promotion activities provided to firms in the UK by DBT including: providing advice, information and support to firms looking to export through the Exporting is GREAT service; leveraging overseas networks to create opportunities for British exporters; and finding overseas buyers for UK firms which are looking to export. These activities are intended to encourage new firms to export and current exporters to export more. In the longer term, the aim is to increase the output and success of UK firms by increasing the export intensity of their business models where possible.

We examine how receipt of DBT support provided between 2014 and 2016 impacted firm-level economic outcomes up to three years after the support was received. The five key firm-level economic outcomes examined are survival, goods exports, employment, turnover and productivity (using turnover per employee as a proxy).

Previous research, systematically reviewed in our “Lessons Learned” report (Frontier Economics, 2019), identified some evidence that receipt of export promotion services is associated with positive impacts in terms of goods exports, employment, turnover and productivity (Mion & Muuls, 2015; Rincón-Aznar et al., 2015). Our report builds on these previous studies by using more up-to-date and comprehensive data. This allows us to examine variation in the impact of DBT support over time, as well as variation by firm size and by the number of interactions between DBT and supported firms.

Data

The dataset we use is created from administrative sources, most importantly the Inter-departmental Business Register (IDBR), Her Majesty's Revenue and Customs (HMRC) overseas trade statistics and survey data provided by DBT. The resulting dataset is a yearly panel that contains records of the key firm-level outcomes listed above, interactions with DBT and potential drivers of DBT support for around 1.5 million firms between 2014 and 2016, along with earlier data that helps us test whether the necessary assumptions for our analysis are valid. Approximately 1.5% of the firms in our dataset were supported by DBT in some way.

Within the timeline of this project and given the data available on the economic outcomes of interest, we were able to estimate the impact of DBT support up to three years later for survival, goods export status and goods export value, up to two years later for employment and up to three years later for turnover and turnover per employee. Table 1 summarises the outcome data available.

Table 1. Outcome data available for estimating the impact of DBT support, by support year and outcome

Support year	Employment	Turnover and turnover per employee	Survival, export value and export status
2014	t+1, t+2, t+3	t+1, t+2, t+3	t+1, t+2, t+3
2015	t+1, t+2	t+1, t+2	t+1, t+2
2016	t+1	t+1	t+1

Methodology

Our dataset tells us how firms which received DBT export promotion support performed in subsequent years in terms of our outcomes (**survival, goods exports, employment, turnover and productivity**). We do not, however, know what those outcomes would have been in the absence of support from DBT. We therefore need an econometric model to establish the counterfactual (i.e., what outcomes would have occurred for each firm without export promotion support) and allow us to estimate the impact of DBT export promotion support. This estimation is complicated by the fact that supported firms may differ systematically from the wider population of UK firms. For example, a firm that seeks DBT support may be more likely than the average UK firm to already be an exporter and be more likely to operate in a trade-intensive industry such as manufacturing.

To account for these differences between supported and unsupported firms and to estimate the impact of DBT export promotion support (the "treatment effect"), we take a combined propensity score matching and difference-in-differences (PSM-DiD) approach. Our approach can be summarised in three main steps:

- First, we model the likelihood that a firm with a given set of characteristics will receive export promotion support from DBT (its "propensity score").
- Second, based on the propensity score, we match "treated firms" (i.e., those receiving DBT support), to "control" firms. Control firms are unsupported firms that look very similar to supported firms in terms of recent growth in key outcomes, industry, location, age and other observable characteristics.
- Third, we transform the outcomes of interest into difference form, by subtracting the baseline pre-treatment value for each firm,[1][2] and estimate the average difference in outcomes between the matched treatment and control groups, which gives us a measure of the impact of DBT export promotion support on our outcomes.

[1] For survival, there is no baseline difference as all firms “survive” before they are treated.
[2] By subtracting the baseline pre-treatment values, we ensure that our results are not driven by pre-existing differences in outcomes. Although the matching process should ensure that the treatment and control firms have similar baseline pre-treatment outcomes, it is possible that differences remain because the matching is based on the overall propensity score, which is also affected by other covariates (such as industry and location). We therefore want to net these off to get an estimate of the impact.

- This PSM-DiD approach is in line with previous scholarship in the area and is discussed at length in our Lessons Learned report (Frontier Economics, 2019).

Along with average effects across all supported firms, we also present our estimates separately for small firms (fewer than 50 employees) and medium and large firms (50 or more employees). This is to reflect the potential for (i) variation in the different drivers of treatment probability by firm size; and (ii) variation in the impact of DBT support by firm size. In addition, our analysis is carried out separately for outcomes one, two and three years after export promotion services are provided, allowing the analysis to give an indication of both the short- and medium-term impact of DBT support. In addition, our analysis is carried out separately for outcomes one, two and three years after export promotion services are provided, allowing the analysis to give an indication of both the short- and medium-term impact of DBT support.

Results for the impact of DBT support

Table 2 summarises the estimated impact of DBT support on outcomes one, two and three years after treatment. These results are representative of the impact of DBT support for the support profile of the average supported firm (i.e., the results cover the mix of different profiles of export promotion support received by firms in practice).[3]

Overall, our results suggest that DBT support has a statistically significantly positive impact on the survival, probability of exporting goods and employment of supported firms. The impacts of support on turnover and value of export are also statistically significant if analysed in a logarithmic specification. A logarithmic specification uses the change in the log of the outcome as the dependent variable, allowing impacts to be measured in proportional terms so that, for example, treatment gives an 8% uplift in export value. Given the range of firm sizes, modelling treatment effects with a proportional increase rather than using "linear" specification assumes that the same uplift in absolute terms is applied, regardless of the starting size. This is borne out by the logarithmic effects, which show positive impacts that increase over time, are statistically significant and are of broadly similar magnitude when comparing across different outcomes. By contrast, there is much less congruence in the linear results for employment, turnover and export value.[4]

[3] We consider impacts for more specific profiles of DBT support below.
[4] It should be noted that the linear £ outcomes have considerable volatility over time and are likely to be more prone to the effects of outliers.

Table 2. Impact of DBT support for all treated firms, all firm sizes, all treatment years

Outcomes	T+1	T+2	T+3
Survival	2.2%*	3.4%*	4.4%*
Export status	7.2%*	8.0%*	7.7%*
Export value (£000s)	175	369	784
Employment	5.7*	10*	5.3*
Turnover (£000s)	4,197	1,717	-6,013
Log export value	0.083*	0.087*	0.108*
Log employment	0.049*	0.062*	0.064*
Log turnover	0.038*	0.058*	0.067*

Note: * indicates a statistically significant difference in the matched average treatment effect at the 95% confidence level.

It is worth noting that the impact of DBT support on outcomes two and three years later results from a combination of the initial treatment episode (at year t) and, for a majority of firms (around 60% of our sample), repeated support in following years.

Survival

As Table 3 shows, we find consistent evidence of positive, statistically significant survival impacts as a result of receiving support from DBT for both small firms and large firms, with slightly larger impacts estimated for small firms.

Table 3. Effect of DBT support on firm survival, by firm size, all treatment years

	T+1	T+2	T+3
All firms	2.2%*	3.4%*	4.4%*
Small firms	2.5%*	3.8%*	4.8%*
Medium and large firms	0.7%*	1.6%*	2.8%*

Note: * indicates any year with a statistically significant difference in the matched average treatment effect at the 95% level or higher. For results by firm size, results are deemed statistically significant if they are statistically significant for at least one of the separately estimated years. For the "all firms" results, statistical significance is based on a weighted average of the results by firm size.

Survival effects grow over time. For small firms, survival effects associated with DBT support rise from 2.2 percentage points one year after treatment to 3.4 percentage points after three years and 4.4 percentage points after three years. For medium and large firms, survival effects associated with DBT support rise from 0.7 percentage points one year after treatment to 1.6 percentage points after two years and 2.8 percentage points after three years.

The presence of a survival effect makes the interpretation of other estimates more challenging. Firms supported by DBT are more likely than control firms to be active one year, two years and three years from treatment. This makes it difficult to understand to what extent the effect of DBT support on other outcomes (e.g., employment) at t+1 (where t is the treatment year) is due to impacts on firms that would have survived anyway (regardless of support) or due to effects on which firms make it to t+1. However, the estimated survival effects are relatively small, especially

if compared to baseline survival rates. Therefore, the selection into survival is likely to have a relatively small influence on our estimated impact of DBT support on other outcomes.

Goods exports

We find that DBT support is associated with a higher likelihood of exporting goods. This effect is statistically significant for all firm sizes and broadly stable over time. When measured in a logarithmic specification, the effect on value of exports is positive and significant, slightly increasing over time. Overall, the results are driven by small firms, which consistently see bigger and more significant impacts than medium and large firms.

Table 4. Effect of DBT support on goods exports, by firm size, all treatment years

Size band	Outcome	t+1	t+2	t+3
All firms	Export status	7.2%*	8.0%*	7.7%*
Small firms	Export status	7.6%*	8.3%*	8.0%*
Medium and large firms	Export status	5.8%*	6.7%*	6.1%
All firms	Export value (£000s)	175	369	784
Small firms	Export value (£000s)	121	214	329
Medium and large firms	Export value (£000s)	402	1,010	2,718
All firms	Log export value	0.083*	0.087*	0.108*
Small firms	Log export value	0.093*	0.111*	0.115*
Medium and large firms	Log export value	0.06	0.037	0.094

Note: * indicates any year with a statistically significant difference in the matched average treatment effect at the 95% level or higher. For results by firm size, results are deemed statistically significant if they are statistically significant for at least one of the separately estimated years. For the "all firms" results, statistical significance is based on a weighted average of the results by firm size.

The effect of treatment on export status is larger for small firms both in absolute terms (the size of the treatment effect) and relative to the baseline probability of exporting. Specifically, DBT support is associated with a 7.6 percentage point increase in the proportion of exporters among the small treated firms, from a baseline of around 30% of exporters; among large firms, treatment is associated with a 5.8 percentage point increase, from a baseline of around 70% of exporters. This is in line with international evidence on the impact of export promotion (e.g., Broocks & Van Biesebroeck, 2017; Munch & Schaur, 2018).

These effect sizes suggest that export promotion services provided in 2014 and 2015 may have led around 2,900 additional small firms and around 600 additional medium and large firms to start exporting two years after first treatment.

Since our data focuses on goods exports (and there is no equivalent record of services exports at the firm level), this analysis may underestimate the impact of DBT support on total exports (including goods and services).

Employment and turnover

As Table 5 shows, our results suggest that DBT support is associated with an increase in firm size through employee count. The effect of support on employment is primarily driven by an impact on small firms, an effect that is roughly constant over time. The effect for medium and large firms is rather unstable, non-significant one and three years after support but significant two years after support. The results for log turnover and log employment are also positive, significant and driven by small firms, with results for medium and large firms positive, unstable and insignificant.[5] The results for turnover are not statistically significant.

Table 5. Effect of DBT support on firm employment and turnover, by firm size, all treatment years

Outcome	Size band	T+1	T+2	T+3
Employment	All firms	5.7*	10*	5.3*
Employment	Small firms	0.6*	0.8*	0.9*
Employment	Medium and large firms	26.7	47.8*	23.3
Turnover (£000s)	All firms	4,197	1,717	-6,013
Turnover (£000s)	Small firms	-215	-2944	32
Turnover (£000s)	Medium and large firms	22,848	20,819	-31,176
Log employment	All firms	0.049*	0.062*	0.064*
Log employment	Small firms	0.055*	0.069*	0.074*
Log employment	Medium and large firms	0.025*	0.035*	0.027
Log turnover	All firms	0.038*	0.058*	0.067*
Log turnover	Small firms	0.042*	0.061*	0.074*
Log turnover	Medium and large firms	0.02	0.048*	0.041

Note: * indicates any year with a statistically significant difference in the matched average treatment effect. For results by firm size, results are deemed statistically significant if they are statistically significant for at least one of the separately estimated years. For the "all firms" results, statistical significance is based on a weighted average of the results by firm size.

- Similar to Rincón-Aznar et al. (2015), we find some evidence of positive effects of treatment on the employment of larger firms. Small treated firms would have grown by 0.6 fewer employees on average after one year without treatment, while medium and large treated firms would have grown by 26.7 fewer employees after one year, around one-third of their actual growth.

[5] Note that variation in persistence of impacts over time is also affected by the changing sample composition, with t+1 estimated from 2014, 2015 and 2016 cohorts, but t+3 estimated only from the 2014 cohort.

Treatment duration
As noted above, the results in Table 1 to Table 5 combine results for firms that received DBT support in a single year (e.g., 2014 only) and multiple years (e.g., 2014 and 2015).

As an extension, we considered how these impacts break down if splitting by treatment duration. Overall, we might expect that a longer treatment duration is associated with larger impacts from DBT support. There are a number of reasons for believing this:

- For a firm to be receiving support in a later year, by definition, it must have survived and, absent any effects, this will be correlated with higher growth than if it had not survived.
- Firms seeking more substantial support in the first place may have more ambitious growth plans than those that do not, or they may subsequently seek further support following on from successful experience of exporting from the first round. In other words, renewed support may follow ongoing export success.
- Absent any selection or survival effects, DBT support may bring direct benefits.

There may also be differences in the composition of the groups, which could also affect performance. In fact, when comparing small firms in the 2014 cohort, we find the two groups have similar sector and region profiles. However, points of difference are that those receiving treatment in multiple years are:

- larger (12 employees vs 9);
- more productive (turnover per employee 17% higher);
- more likely to already export (44% vs 29%); and
- more innovative (twice as likely to have innovation support and significantly more likely to file patents).

Overall, treatment in multiple years is associated with larger effects that grow over time, across all the outcomes exhibiting positive impacts (probability of exporting and employment). For example, after two years, the impact on exporter status for the multiple-treatment years group relative to one year only is 8 percentage points higher for small firms and 3 percentage points higher for medium/large firms. Treatment in multiple years is associated with survival rates between 1 and 6 percentage points higher than if treated in just one year.

Treatment intensity within a year
The results in Table 1 to Table 5 also combine results for firms that received only one instance of support from DBT within a year with firms that received multiple

instances of support from DBT within a year. Our data suggests that the latter is more common, with each firm receiving an average of around three service deliveries per year.

A more detailed breakdown of these results suggests that more intensive treatment is associated with larger impacts from DBT support on export status. Among small firms, receiving multiple instances of support rather than a single instance is linked with an increase in the probability of exporting in the following year of 2 to 5 percentage points, depending on treatment year – a sizeable effect compared to the impact of single treatment (5.2 percentage points for treatment in 2014 and 6.4 percentage points in 2015 respectively). However, the results for other outcomes are not statistically significant.

As in the case of treatment duration, the results from this comparison should be interpreted with caution. It is possible that different types of firms select into our single- and multiple-treatment groups.

Geographic variation in results

We also explored whether the results vary at a geographic level. This involved aggregating one-year impacts over the three treatment years in scope and dividing the UK into five aggregate regions, and exploring these separately. We continued to distinguish small firms from medium and large firms.

This analysis found little variation in impacts by region. While there were some differences in results, this should be expected in any exercise involving repeated cuts of the data. It was not obvious that any region outperformed others across multiple metrics.

Robustness of the results

We performed a number of checks to test the robustness of our results and found that:

- Our results are robust to removing enterprises whose exports were estimated by apportioning. This suggests that our data-linking process did not directly lead to biased results.
- Comparing the estimated impact of DBT support across treatment years (2014, 2015, 2016) suggests that for survival and probability of exporting the estimated impacts are similar in each year, whereas in some instances employment, value of exports and productivity estimates vary substantially from year to year.[6] This may reflect variations in the size and composition of the treated sample from year to year resulting from the limitations of treatment data. It should also be noted that these are continuous variables that can potentially take extreme values and are thus more prone to outliers than binary variables such as survival or exporter status.
- Testing the sensitivity of results to the exclusion of the top 5% of businesses by turnover suggests that excluding the largest firms has an impact on our

[6] When this was tested, turnover results were estimated based on only one year of treatment data, so this robustness check was not possible for turnover.

employment results for medium and large firms. In particular, the estimated impact of DBT support on the employment of medium and large supported firms falls once the largest 5% of firms are excluded from the analysis.

- Our results are robust to using an ordinary least squares (OLS) estimation methodology as opposed to our matching methodology.
- While we matched as closely as possible on observable characteristics of the firms, as in any econometric approach, there may be factors that differ between treated and control firms that cannot be measured in the available data. This could, for example, include expectations of future survival or export potential (not captured by past firm-level growth rates) leading firms to seek DBT support for innovation, and this could partly account for some of the results.

Options for future research

Our work gives some initial evidence that when private firms receive export promotion support, their economic outcomes in terms of survival, employment and likelihood of exporting goods improve.

Future research could examine potential impacts of DBT services not addressed by this analysis due to methodological and data restrictions. For example:

- Alternative definitions of “treatment” could be analysed – while our analysis considers the impact of all DBT export promotion activities in aggregate, we did not investigate the impacts of specific DBT services (e.g., comparing the impact of events to webinars). Research projects separate from this study are helping to address this gap.
- In addition, replicating the analysis in future years when more outcome data is available would allow for a fuller assessment of the trajectory of impacts to be made. While this research measures impacts on firm-level outcomes up to three years after initial receipt of support, DBT support is likely to also have impacts beyond three years (and even our analysis of impacts three years ahead is restricted by the data available).

Introduction

This report was commissioned by the Department for Business & Trade (DBT) to assess the economic impact of its export promotion activities on the firms which receive them. The report builds directly on our “Lessons Learned” report, which systematically reviews previous analyses of export promotion and the methodological approaches used (Frontier Economics, 2019).

The analysis focuses on a range of services provided to firms in the UK that directly support their efforts to export goods and services. These policies include:

- Providing advice, information and support to firms looking to export through the Exporting is GREAT service;
- Leveraging overseas networks to create opportunities for British exporters; and
- Finding overseas buyers for UK firms which are looking to export.

These services are intended to encourage new firms to export and current exporters to export more. In the longer term, the aim is to increase the output and success of UK firms by increasing the export intensity of their business models where possible.

The project builds directly on two existing econometric studies previously commissioned by UK Trade and Investment (UKTI):

- Mion & Muuls (2015) evaluated the impact of UKTI services on the intensive margin (volume of exports per firm) and extensive margin (number of exporters, number of countries, number of products) of exports. They found positive impacts on exports, mainly through the extensive margin (expansion of existing exporters into new countries) but no statistically significant differences by policy or firm type.
- Rincón-Aznar et al. (2015) of the National Institute for Economic and Social Research (NIESR) analysed the impact of a wide range of UKTI services on employment growth, turnover growth and asset growth. They identified positive effects on turnover and labour productivity, while the employment effects depend on the specification chosen.

As discussed in our "Lessons Learned" report and a previously published literature review, evidence from these previous studies and others suggests a net positive effect of export promotion support on exports, as well as further impacts on employment, turnover and productivity.

The econometric analysis included in this report is intended to provide robust estimates of the impact of DBT’s export promotion services on key firm-level outcomes, in particular:

- survival;
- goods exports;

- employment; and
- turnover.[7]

Our analysis improves on the existing literature primarily by building and using a more complete set of firm-level data than was previously available. Past research on trade promotion in the UK (Rincon-Anzar et al., 2015; Mion & Muuls 2015) relied on the Financial Analysis Made Easy (FAME) dataset of UK companies, a private database of firms based on public Companies House records. Due to the data sources FAME is built on, it under-represents small firms and contains no information on exports. By linking the Inter-departmental Business Register (IDBR) with data from Her Majesty's Revenue and Customs (HMRC) overseas trade statistics, we can analyse a much wider selection of firms, especially smaller ones, and test for much more pertinent outcomes. Using this dataset, we aim to make the following improvements on and contributions to the literature by:

- Using data that includes a greater proportion of micro, small and medium enterprises compared to previous studies reliant on datasets which over-represent large firms;
- Using more up-to-date data than the most recent UK studies of export promotion, which were published in 2015;
- Measuring the impacts of export promotion up to three years after treatment;
- Including information on additional potential drivers of selection into treatment (e.g., previous receipt of support for innovation); and
- Filling gaps identified as part of analysis on value for money (VfM) generally, and specifically the possibility of variation in the impact of DBT support by firm size and by the number of interactions between DBT and the supported firm.

Taken together, this amounts to a substantial step forward in understanding the impact of export promotion services on firms, and it is a useful input to DBT's VfM evaluation process.

A more complete understanding of how this analysis fits into the existing literature is available from the Lessons Learned report (Frontier Economics, 2019).

Structure of the report

The report is structured as follows and presents:

- The data sources used in the analysis, including a brief overview of the data on services provided by DBT, the process of data linking and the characteristics of firms appearing in the dataset;

[7] For effects of support on outcomes one year after support (t+1) was received, we also investigated the impact on turnover per employee as a proxy for firm productivity. However, this outcome variable was discarded when looking at the impact of support two and three years after support. This is due to a) no effects being found at t+1, and b) concerns that the exact timing of employment and turnover variables may not always be the same, so that the ratio of the two measures might be a misleading proxy in some cases.

- The methodology used to estimate the treatment effects of export promotion, the propensity score specification and the matching algorithm used in our combined propensity score matching and difference-in-differences (PSM-DiD) approach;
- The results, including estimates of one-, two- and three-year impacts for each of the outcomes of interest; and
- Conclusions from the analysis.

Data

The aim of this analysis is to estimate impacts of export promotion services on firm-level outcomes. To achieve that, we constructed a dataset at the firm level using a range of relevant sources. To evaluate the impact of DBT's export promotion services, we needed data on:

- Which firms have used these services ("treatment data");
- The outcomes of interest, including both exports and other business outcomes ("outcome data"); and
- The characteristics of treated and non-treated firms ("control data").

Data in this form is not readily available and this required us to construct it from a number of different sources, listed below. As the sources include confidential data, in particular overseas trade statistics provided by HMRC, we accessed and analysed the data in a secure research setting, the HMRC Datalab.

- Treatment data. This is sourced from DBT management information. It reports the name and postcode for firms using DBT services between 2014 and 2016.
- Outcome data (exports). This includes HMRC overseas trade statistics and administrative and survey data on goods exported over a minimum value threshold. The data covers the years 2012 to 2017.
- Outcome data (firm survival, employment, turnover). This is sourced from the L-IDBR, a version of the Inter-departmental Business Register which includes additional quality checks and covers 99% of UK businesses. Data is available for 2012 to 2017 for employment and 2012 to 2016 for turnover. To address the data gap in relation to turnover, the 2018 IDBR, which carries data turnover relating to 2017, is added.
- Control data. This is sourced from the IDBR and includes information on firm location, industry, ownership structure, birth date and legal status. The controls run from 2012 to 2016.
- Control data (past support for innovation). This comes from a list of firms that received support for innovation from Innovate UK, covering the period from 2012 to 2015.
- Control data (past intellectual property activity). We used data from the Intellectual Property Office (IPO) which identifies firms that have applied for and have been granted patents in Great Britain and Europe. The data covers 2012 to 2015.

Each of the data sources listed above is collected and maintained for other statistical and administrative purposes, so the process of transforming and combining them into a single analysis-ready dataset formed a substantial part of our work.

In this section, we discuss in turn:

- The sources of data used in our analysis;
- How the separate sources were linked; and
- The resulting analytical dataset, including summary statistics and potential sources of bias involved in the linking process.

Further detail on the data cleaning and linking process is available in Annex D.

Data sources

In this section we give a high-level overview of the data sources used in this study.

Inter-departmental Business Register (IDBR)

The IDBR is an Office for National Statistics (ONS) dataset of UK businesses, based on value added tax (VAT) and pay as you earn (PAYE) returns. The IDBR gives a comprehensive overview of UK firms, with information on turnover, employment, legal status, sector (Standard Industrial Classification (SIC)) and location. The IDBR has excellent coverage, as it includes all VAT- or PAYE-registered companies, representing the vast majority of UK economic activity.[8]

The IDBR is collected in quarterly snapshots, with its comprehensive coverage over time making it suitable for longitudinal analysis such as difference-in-differences analysis. However, it should be noted that there may be lags in updating the IDBR, giving potential concerns around the timeliness of some of the data. For this reason, we use the Longitudinal IDBR (L-IDBR), a variant of the IDBR with more precise information on the timing of employment and turnover information and indicators on the quality of the data collected. This is intended to give more reliable information on the timing of impacts.

HMRC overseas trade statistics

HMRC collects information on the export of goods by businesses in the UK. The type of information depends on whether goods are exported to countries within or outside the European Union (EU):

- For exports to non-EU countries, information is mostly drawn from customs declarations for each "consignment", an individually reported batch of categorised goods being exported.
- For exports to EU Member States, information on goods exported is gathered from a survey (Intrastat) and VAT data. VAT-registered businesses report the value of imports and exports made with EU countries on their VAT return, and businesses above certain thresholds are required to make full reports on arrivals and dispatches through Intrastat.[9]

[8] Many sole traders will not appear in the IDBR as they are not VAT- or PAYE-registered. While they represent a significant proportion of businesses, their smaller size means they only represent a small proportion of economic activity. For example, Business Population Estimates suggest that only 2.5% of UK private sector turnover is for unregistered firms with no employees.

[9] Note that the reporting thresholds for Intrastat changed in 2009, which would give the impression of a large reduction in the volume of traders at that time.

It should be noted that the export value provided to HMRC is on an FOB (free-on-board) delivery basis, which means the cost of exported goods to the overseas buyer, stripping out insurance, transport and cargo fees. As a result, FOB values may not accurately reflect the corresponding turnover value. Other sources of potential measurement error include exports being made in different currencies, and the possibility that exports are transactions across international borders within a multi-national firm rather than an open-market exchange.

It is important to note that the export data only covers physical movement of goods and does *not* include trade in services. This is a significant limitation, as the DBT export promotion activities are intended to cover both goods and services, but the latter is missing in the data.[10] Our analysis will therefore underestimate the impact of export promotion services on exports, although it should remain an unbiased estimate of the effect of those services on goods exports specifically.

Data on DBT support

We use a dataset built by the market research company Kantar Public to identify firms receiving export promotion support from DBT, based on internal DBT management information files. This is the most comprehensive source of information on which firms were supported by DBT from 2014 to 2016. However, there are some gaps in coverage, with some services better recorded than others. As a result, it is not possible to assess exactly what proportion of total service deliveries are recorded. Importantly, enterprises are identified by a Companies House Reference Number (CRN) in only around 20% to 30% of cases. This makes linking to L-IDBR and export data more challenging, relying on “fuzzy matching” by ONS staff in cases where the CRN is unavailable.

Other sources of control data

The propensity to innovate has been linked to the propensity to export and therefore failing to control for differences between supported and unsupported firms in their propensity to innovate could lead to biased estimates of the impact of DBT support. In line with previous research (Mion & Muuls 2015; Rincón-Aznar et al., 2015), we use data on patent grants and applications provided by the IPO. We also build on existing research by identifying projects funded by Innovate UK, including all collaborative research and development (R&D), feasibility, smart and innovation voucher grants, and Knowledge Transfer Partnerships.[11] This allows us to ensure that estimated impacts are not driven by the receipt of these other forms of support, and provides a further proxy for firms' propensity to innovate.

The data-linking process

In this section, we outline the linking processes we undertook for each source dataset. We discuss the impacts on the data at each step, giving an indication of the data lost. Full summary tables of the data at every step are available in Annex A.

[10] Services exports are significant, accounting for a total of £292 billion in 2020, according to Pink Book estimates. https://www.ons.gov.uk/economy/nationalaccounts/balanceofpayments/datasets/3tradeinservicesthepinkbook2016

[11] Available at https://www.gov.uk/government/publications/innovate-uk-funded-projects

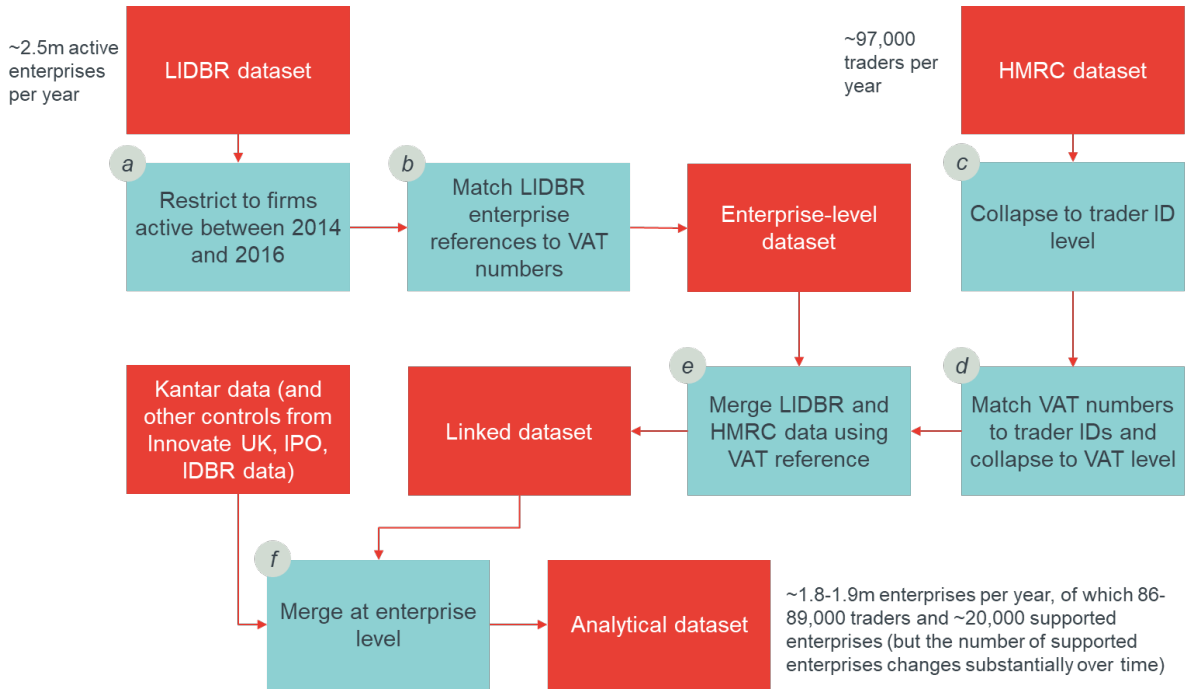
The aim of the data-linking process was to produce a yearly panel of firms with outcome, control and treatment data for each. In our case, we had the ONS definition of “enterprise” as the reference list of firms to which we matched all other characteristics and observations. Many of our other datasets were not at the enterprise level, and so had to be aggregated or linked to that level to produce the final analytical dataset.

The steps in the data-linking process were as follows:

1. Restrict the L-IDBR dataset to firms active between 2014 and 2016. The only treatment records we had available were for those years, meaning that the active firms in those years represent the full treated and untreated sample.
2. Match the restricted L-IDBR dataset to VAT identifiers using an ONS-provided lookup. This was important for matching the L-IDBR data to the HMRC exports data.
3. Collapse the HMRC goods export data to the trader level, getting the total value of exports for each trader as a result.
4. Match the trader-level HMRC export data to VAT identifiers using an ONS-provided lookup. Once matched, we collapsed again to the VAT level, as some trader identifiers shared a VAT reference (but not vice versa).
5. Merge together the L-IDBR and HMRC export data using the VAT identifiers. At this stage, we removed the firms and traders that could not be linked to VAT identifiers. This may be misleading as there will be firms without a VAT identifier which did not in fact export any goods, but there is no way to distinguish them from those which did.
6. Merge in treatment and control data at the enterprise level, specifically the Kantar data on treatment, Innovate UK data, IPO patent filings and additional IDBR data, all of which are discussed in the data section above.

Figure 1 presents a diagram showing this process.

Figure 1. Overview of data-linking process



At the end of the process, we had an analytical dataset from which we could see the records of treatment, outcomes and controls necessary to undertake our analysis.

We summarise the process in greater detail in Annex A - Further detail on data sources and construction of the dataset and, in the following section, we discuss whether and how the linking process might bias our estimates.

Description of sample of firms in analytical dataset

The full analytical dataset is a yearly panel of firms treated in the years 2014 to 2016, amounting to over 4 million observations, of which 1.5% were supported by DBT in some way (see Table 6). The sample amounts to roughly 60% of the total business population of the UK.[12]

Table 6. Sample size and proportion of supported firms, 2014-2016

Coverage measure	2014	2015	2016	Total
Sample size (N)	1,406,316	1,455,057	1,496,846	4,358,219
% treated	1.7%	1.7%	1.1%	1.5%

The treated and untreated firms in our sample differ substantially by both outcome and control characteristics. As Table 7 shows, supported firms tend to be larger, more turnover intensive and more likely to export, to have higher export value and to

[12] The dataset includes treatment firms placed in their first year of treatment, so that they will appear only in their cohort baseline year. They are compared with control firms which are active in that year and which do not receive treatment at any point. Thus, the control group will be repeated over the three years, insofar as the population of firms remains the same.

be older. They are also more likely to have received support from Innovate UK and be more innovative (as indicated by patent filings).[13] We report additional characteristics of supported and unsupported firms in Annex B, showing that supported firms are somewhat more likely than unsupported firms to be based in London or in the West Midlands.

Table 7. Characteristics of supported and unsupported firms, 2014-2016

	Unsupported	Supported
N	4,294,331	63,888
Survival	92.6%	96.9%
Employees	11.6	190.3
Turnover (£000s)	1,753	42,122
Turnover per employee	151	221
Exporting	3.8%	42.8%
Export value (£000s per exporter)	5,206	9,665
Age	14.9	16.8
Foreign ownership	N/A (disclosure) – disclosure control	N/A (disclosure)
Innovate support	0.1%	3.9%
Filed patents (GB)	0.1%	2.3%
Filed patents (non-GB)	0.04%	1.4%

Note: to preserve confidentiality of the underlying data, the figures above do not include the full sample in the following cases: survival figures for supported firms are only based on small firms.

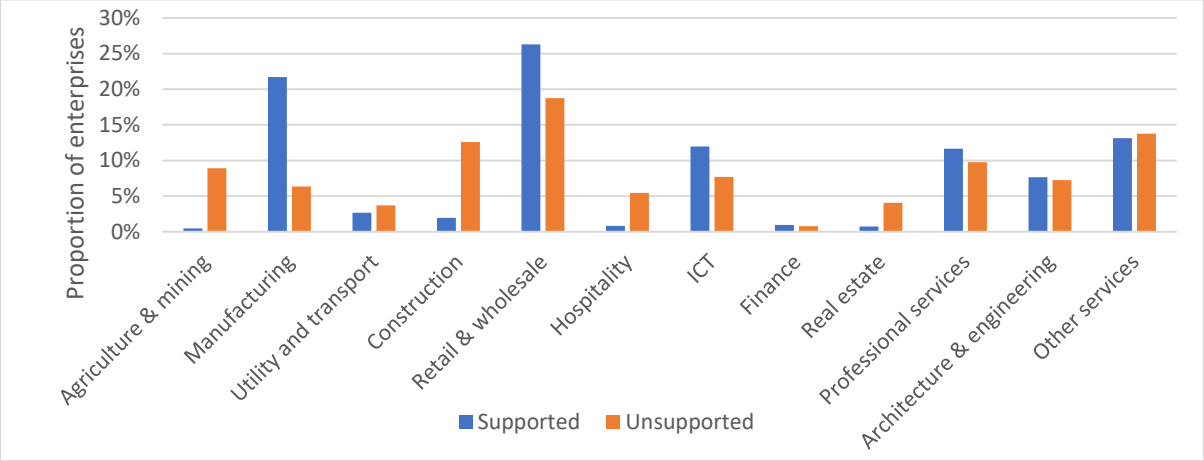
Supported firms were also growing at a faster rate than unsupported firms, prior to treatment. For example, firms treated in 2014 grew on average by 0.34 employees between 2012 and 2013, compared to 0.1 employees among the unsupported firms. The difference in growth rates between the two groups is statistically significant. As discussed in the methodology section that follows, it is crucial that the PSM procedure gives a matched control group with similar pre-treatment trends as the treatment group. This means focusing on firms within the unsupported group that have faster growth and are more like the treatment group.

Figure 2 reports the distribution of supported and unsupported firms by industry in 2014.[14] Supported firms are noticeably more likely to be in manufacturing, retail and wholesale, and information and communication compared to unsupported firms. This distribution is broadly consistent across treatment years.

[13] Note that this table relates to the full sample of firms that appear in the data. However, some firms will drop out of the analysis if there is missing data for at least one variable needed to calculate the propensity scores. This “attrition” is fairly minor; the full sample of 63,888 treated firms in the dataset falls to 60,082 observations with propensity scores. The corresponding statistics for the estimation sample, together with cuts by size band, are provided in Annex B.

[14] This industry breakdown relates to the estimation sample rather than the full sample.

Figure 2. Distribution of supported and unsupported firms by industry, 2014



Much of the difference in outcomes between treated and untreated firms is likely to be due to selection, which will confound our estimates of the impact of DBT's services. For example, it might be easier for larger firms to seek the support of DBT. Firms that are already exporting will find it more worthwhile to receive support from DBT. Removing these confounding factors from our estimates is the focus of our methodology section.

Limitations of dataset

The analysis suffers from a number of limitations, including potential sources of bias, and general limitations associated with the sample. These are discussed in turn.

In terms of bias, there are several aspects of obtaining and linking the data that may be problematic. While overall we consider these to be a low risk, given how our final dataset compares with the underlying data sources, it is worth setting them out in further detail. Specifically, there are three types of bias that we may be concerned about:

1. Attenuation bias – the estimated effect of treatment being smaller as a result of treatment not being recorded accurately;
2. Services in the data not being representative of the full extent of DBT services; and
3. Potentially biased estimates from selection of the treated/non-treated firms in the sample.

Attenuation bias may result from a lack of accurate information on which firms have been treated and on when treatment has taken place. We know that the sample of treated firms recorded in the DBT support data in 2016 is substantially lower than for 2015, and therefore that estimates based on the 2016 treatment year may suffer from attenuation bias.

Even in previous years (2014 and 2015), our engagement with DBT suggests that not all export promotion services are recorded in centralised databases, and that coverage may not be 100% for services that are recorded. The DBT support data likely includes good coverage of missions, Tradeshow Access Programme and ; a

partial coverage of events, post significant assists, services provided by sector teams and Defence and Security Organisation (DSO); and limited coverage of digital products and partnerships. This suggests that our estimates may include some attenuation bias as a result of treated firms being included in control groups because their treatment was not recorded. This is likely to lead to estimates of impact that are somewhat smaller than the true effect of the services included in the DBT support data.

Selection of firms in our analytical dataset may lead to a bias of the estimated effect of export promotion if: 1) the treated and/or untreated in the analytical dataset differ from the raw data in a particular way and 2) that difference is important for the outcome (survival, employment, turnover, exports).

Existing evidence (outside the UK) shows that the impact of export promotion tends to be larger for smaller firms (Broocks & Van Biesebroeck, 2017; Munch & Schaur, 2015; Volpe and Carballo, 2010). Moreover, firms that have been more intensively treated (i.e., used services more often/a greater number of services) may also experience greater benefits from public support.

Therefore, we focus on checking whether enterprises in our analytical dataset are:

- Smaller or larger than enterprises in the underlying data sources; and/or
- More or less likely to be intensively treated than firms in the original DBT support files.

In principle, selection of the analytical dataset compared to underlying sources along other dimensions (e.g., industry) may also introduce bias in the estimated impacts. However, based on our comprehensive review of existing evidence on the impact of export promotion, we assess this to be a low risk.

The linked IDBR-HMRC data we use for this project improves substantially on previous analysis (which used FAME data) in terms of coverage of smaller businesses. Only the smallest traders (below the VAT threshold) and traders in VAT-exempt sectors are excluded from our dataset.

The first of these two selection effects may cause a bias in the estimates of impact, but one that can be accounted for; the second is unlikely to cause bias (no evidence that the effect of export promotion is particularly strong or weak in VAT-exempt sectors).

There are some more general limitations of the dataset in terms of the variables included. For example:

- Measurement error, such as timeliness of outcomes in IDBR and gaps in goods export data. This may reduce the size of estimated coefficients.
- Non-inclusion of services export data. Some of the treated firms will be services exporters and these impacts will not be measured, which will understate the true impacts.

Finally, an overarching challenge is whether the control group contains sufficiently close comparators to the treatment group. In the case of large firms, the pool of potential comparators is much thinner. Estimated effects relate only to those firms that have similar firms in the control group. For some firms this will not be the case, so it is difficult to infer what the effect on these firms is.

Methodology

In this section, we discuss our overall approach to understanding the impact of export promotion on firm outcomes using the dataset we constructed.

Econometric approach

The goal of our analysis is to know the impact of export promotion on firms that receive it.

In the dataset we constructed, we know the outcomes for each of these firms (their turnover, employment, exports and survival) but we do not know what those outcomes would have been in the absence of support from DBT. We therefore need an econometric model to establish the counterfactual, i.e., what would have occurred for each firm without export promotion support.

In addition to knowing the outcomes for supported firms, we also know the outcomes for firms that did not receive support from DBT. A straightforward approach would be to treat these observations as "controls", implying that the average difference in outcomes between supported and unsupported firms is the impact we are seeking to estimate. This approach is flawed, however, as firms do not receive support from DBT at random, and the factors that determine their choice to seek support may also determine changes in the outcomes of interest. These statistical issues are known in the literature as "selection bias".

To avoid the problem of selection bias, we use a propensity score matching (PSM) approach. PSM is a common method for establishing treatment effects (see Rosenbaum & Rubin, 1983) and has been the primary method used in the literature on trade promotion in the UK (see the accompanying Lessons Learned report (Frontier Economics, 2019)).

We start by modelling the likelihood that each firm will receive support from DBT, conditional on its observable characteristics. The predicted probability of support produced by this model is the "propensity score", a value for each firm between 0 and 1 reflecting its probability of treatment. This is regardless of whether it actually receives the treatment or not.

Each treated firm is then matched to one or more untreated firms based on their propensity score. This process creates a "control group" of firms which were similarly likely to receive treatment and can therefore provide appropriate counterfactual outcomes. The average difference between the matched treatment and control groups is then the average effect of treatment on the treated (ATT), an estimate of the impact of export promotion services.

In addition to controlling for the propensity for receiving support, we also transform the outcomes of interest into difference form, by subtracting the baseline pre-treatment value for each firm. Since the propensity score is estimated on a range of variables (such as region, age and innovative support) unrelated to size, treated firms may be matched to firms of a different size or export level. Differencing out the baseline outcome makes the treatment and control more directly comparable. It is

also less demanding to control for everything that might influence the change in outcomes rather than everything that might influence the level of outcomes. This is known as a difference-in-differences (DiD) approach.

Assumptions required

There are three important assumptions required for the causal inferences of our econometric approach to be valid. These set out the circumstances in which PSM methodology will give valid results. In other words, we need to establish that these assumptions hold in order to be confident in drawing causal inferences.

Firstly, we require the "conditional independence assumption" (CIA). This is the underlying principle of the PSM process, in that it assumes there is no difference in expected baseline outcomes between treated and untreated firms, conditional on a set of observed characteristics. In this context, the observed characteristics are what we use to estimate the propensity score, conditional on which the treated and untreated groups have identical expected outcomes, apart from their treatment.

Testing whether the CIA holds would require showing the absence (or presence) of any differences between treated and untreated firms conditional on their observed characteristics. This is not possible, as potential further unobserved characteristics are, by definition, unavailable to us. We can nonetheless test the "success" of the matching procedure in removing observable differences between treated and untreated firms.

Secondly, the PSM methodology requires "common support" regarding propensity scores. This assumes that there is an overlap between treated and untreated firms in terms of propensity scores, which is what allows us to match treated and untreated firms at all. This can be checked by plotting the distribution of propensity scores among the treated and untreated firms.

Thirdly, we assume "common trends" in the treated and untreated groups. This is required to make conclusions from the DiD form of our estimation. DiD estimates require that, in the absence of treatment, the outcome of interest (e.g., exports) would have followed the same trend among the treated and among the control firms. Similarly to the CIA, this assumption cannot be tested because we cannot observe what would have happened to the treated firms in the absence of treatment. However, we can check whether treated and matched control firms were following common trends prior to treatment. If this were the case, this would give us greater confidence that the common trends assumption might hold.

Measurement of treatment

Our PSM-DiD approach to estimating the impact of DBT support requires a clear, binary measure of "treatment" that we can use to estimate propensity scores and treatment effects. In reality, DBT offers a range of services and support to different parts of a firm, often repeatedly or over extended periods. Whichever measure of treatment we use will inevitably be a simplification of that reality and reflect only the average impact of a range of treatments.

The main source for treatment data is the DBT business support dataset. For the purposes of simplicity, transparency and tractability, we chose the simplest measure of treatment available: whether the firm appears in the DBT business support dataset

at all. In other words, we mark a firm as treated if any recorded support by DBT can be linked back to it, regardless of the nature of that treatment.

While a more complex measure of treatment might improve the granularity of the treatment effects estimated, it is limited by the availability of the data.

One goal of our analysis is to understand how the impact of export promotion services changes with the intensity of the services provided. This is challenging due to the difficulty of measuring intensiveness in a reliable and consistent way in the available data. As such, a full model of the relationship between intensity and impact is not feasible. Nevertheless, in line with the way in which DBT models the VfM of its services, we define “intensity” as the number of times DBT has interacted with a firm using its services, and we are able to indicate whether receiving more than one instance of support (e.g., participating in one inward or outward mission) is linked with a greater impact on any of the outcomes of interest.

To answer this question, we estimate treatment effects separately for two groups of treated firms: firms that received one instance of treatment in a given year ("single-treatment" firms); and firms that received more than one instance of treatment ("multiple-treatment" firms). Both groups are compared to the same control group – firms that did not receive any treatment in the year. The results from this comparison should be interpreted with caution. It is possible that different types of firms select into our single- and multiple-treatment groups. For example, some firms that are not current exporters may be more "export ready" and may start exporting with one instance of treatment. Others may be less export ready and require several instances of treatment before they can start exporting. In this case, the estimated effect of multiple treatment may underestimate the potential impact of providing single-treated firms with additional treatment instances. However, it is also possible that multiple-treatment firms are the ones that most want to export (or to export more). In that case, the estimated effect of multiple treatment may be an over-estimate. Because export readiness and desire to export can at best be proxied in the data, it is not possible to ascertain which of the two effects above may be at play. Nonetheless, comparing the estimated effect of single and multiple treatment gives us some indication of whether and how much multiple treatment may lead to greater effects than single-treatment instances.

Measurement of impact

This analysis seeks to estimate the impact of export promotion on firm survival, employment, turnover, turnover to employment ratio (as a proxy for productivity) and goods exports. As discussed above, we take a DiD approach, which leaves undefined the exact time period over which we allow impact to occur.

Changes to export decisions can be a slow process, and the transition to different practices could have long-lasting and gradual effects. For this reason, we would ideally be agnostic about the length of time over which impacts occur, and let it emerge from our estimates. Furthermore, we would ideally observe each firm continuously, and see the pace at which impacts occur. Unfortunately, we are restricted in this by our data, which is observed at yearly intervals for a limited period after the intervention occurs.

To best account for the timing of impacts, outcomes and treatments, we estimate the impact of treatment on the change in outcomes between the year before treatment t-1 and one, two and three years after treatment(t+1, t+2, t+3).[15] This DiD between treated and control is interpreted as the causal effect of export promotion services. This strategy is applied to the impact of DBT support on export value, employment, turnover and the turnover to employment ratio. We also estimate the impact of support on the probability of exporting (defined as reporting exports for a value greater than zero) and on the probability of supported firms remaining active. A DiD strategy is not appropriate for estimating impacts on survival and export status, which are binary outcomes. Therefore, for these outcomes, in for example the one-year impact case, we estimate the impact of DBT support on the outcome at t+1, rather than on the change in the outcome between t-1 and t+1.

Heterogeneity of impact

One key purpose of this report is to investigate the different effects that export promotion can have on different types of firms, and on small firms in particular. To do so, we ran our analysis separately for two separate size groupings:

- Small firms (49 or fewer employees); and
- Medium and large firms (50 or more employees).

Moreover, as discussed above, we test the impact of treatment changes when we restrict the sample of treated firms to:

- “Single-treated” firms (only one instance of treatment recorded in the DBT support data); or
- “Multiple-treatment” firms (two or more instances of treatment recorded in the DBT support data).

The propensity score model

Our method for removing the risk of selection bias is to compare firms with a similar likelihood of receiving export promotion support from DBT. To do so, we predict the probability of receiving treatment for each firm, using a range of control variables. We assume that, conditional on this probability, there is no expected bias in the estimated treatment effect.

Choice of variables

The selection of variables on which we estimate the propensity of treatment reflects the data available and is informed by the precedent set by other UK studies of export promotion. The motivation for using the IDBR dataset is based on its comprehensive coverage and genuine longitudinal structure. The controls were selected in order to exploit the broad range of variables included, but to do so in a parsimonious manner. The IDBR controls are as follows:

[15] Analysing the DiD of t+1 relative to t-1 ensures we are accurately interpreting the treatment as happening at some point in year t. If we only compared t to t +1, we might accidentally include the effect of treatment in our baseline.

- Turnover at t-1, the year prior to treatment, both as a continuous and as a categorical group variable;
- Employment at t-1, both as a continuous and as a categorical group variable;
- Age, both as a continuous and as a categorical group variable;
- Foreign ownership, as a dummy variable for either immediate or ultimate foreign ownership;
- Legal status, as a categorical variable for being a company, a sole proprietorship, a partnership or a public body;
- Industry, partially aggregated up from SIC 2007 letter codes; and
- Geography in the form of Government Office Regions (for England) or countries (for the rest of the UK).[16]

Additionally, we can use the goods export data as a control for the likelihood of receiving export promotion. Specifically, we use aggregate export value (either for EU, non-EU or combined) and export status (either for EU, non-EU or combined), again all measured at t-1.

There is evidence that past innovation is linked with a greater likelihood of exporting and of receiving export promotion support (Breinlich et al., 2012). To control for this, we merged data on patent filings from the UK IPO with records of past support by Innovate UK. The resulting variables used are whether a firm has ever filed for a patent in the UK, whether it has filed for a patent outside the UK, and whether it received support from Innovate UK in that period.

Propensity score specification

The question we are attempting to answer with the propensity score is "what is the likelihood of receiving support from DBT?". Since the outcome is binary, and we are predicting the probabilities (between 0 and 1), it is appropriate to use a logit model of treatment.

The propensity score for each firm is the predicted probability of treatment estimated through this logit model. Choosing between estimations of the propensity score when the true drivers of treatment are unknown is a difficult balancing act. The desired outcome is that each treated firm is matched to a credible set of controls, which accurate propensity scores will allow. A more predictive model of treatment can reduce the number of controls with similar propensity scores to treated firms. However, a more predictive model may also improve the quality of matches and produce more precise estimates of the treatment effect.

Overall, we aimed to balance these pressures by maintaining the simplicity of our propensity score model specification, while preferring models that produce more

[16] We investigated the use of other geographic codes, including Nomenclature of Territorial Units for Statistics (NUTS) areas and Local Enterprise Partnership (LEP) areas. However, using groups of around 40 dummy variables was not feasible given our sample size of treated firms and the number of other control variables that we want to include in our propensity score model. Using NUTS2 or LEP areas would lead to a significant loss of observations due to collinearity between different variables.

accurate “classification” of treated and untreated firms. For the purposes of assessing our models, classification in this context means the proportion of treated firms which have a propensity score higher than the overall treated rate (and vice versa for control firms). A classification rate of 50% would mean a model of no value, where a classification rate of 100% would mean a model of no overlap.

In determining the appropriate way to model the propensity of treatment, we chose to estimate each model separately for each year of treatment data available, even though we keep the specification constant across years. We allow the coefficients to vary by year through separate estimation as we know that the representativeness of our data in 2015 and 2016 is different to that in 2014. We keep the specification constant as the limited choice of variables we have remains the same and the drivers of the likelihood of treatment ought to be consistent across time, an assumption backed up by the consistency of coefficients in our estimated models.

A further division of estimates we use is to estimate the propensity scores for small firms (fewer than 50 employees) and medium and large firms (50 or more employees) separately. This reflects the potential for different drivers of treatment probability from DBT’s perspective. It is also intended to align with our later choice to stratify our matching procedure by firm size to better understand variation in impact by firm size.

In the process of coming to our final propensity score model, we tested a number of different specifications of varying levels of complexity. The first model (M1) used controls for size (including both continuous variables and size group dummies), age, foreign ownership, industry code, geography, legal status, Innovate UK support and exporter status. Further models tested include interactions between continuous size and group dummy variables, splits between EU and non-EU exports, and further interactions for export value between continuous and group dummy variables. As these additional variables gave only very minor improvement in model performance, (in terms of goodness-of-fit and correctly classifying observations), we opted for the simpler M1 model.[17]

Interpreting the likelihood of treatment

Our chosen propensity score models produce a range of probabilities of treatment for both treated and untreated firms, as we would expect given our common support assumption. This, along with the estimated coefficients of our propensity score model, provides valuable information on the likelihood of a firm receiving support from DBT.[18] Certain types of firms are more likely to receive support from DBT. This reveals a number of interesting insights:

- Larger firms are more likely to receive support from DBT. This is true even within the size bands in which we separately estimated propensity scores: bigger firms, both in terms of turnover and employee count, are more likely to be supported.

[17] More detail on performance of the different first-stage models is provided in Annex C, with Table 43setting out the full selection of variables used.
[18] Refer to Table 44 in Annex C for model coefficients.

- Firms that already export are more likely to receive support from DBT, but no such relationship exists between the value of exports and propensity.
- Foreign-owned firms are substantially less likely to receive support from DBT when controlling for the other variables in the propensity score model, even though foreign-owned firms are disproportionately represented among recipients of DBT support. This is because they have other characteristics that make support more likely, such as being larger and more likely to export.
- There is substantial variation in support levels across industry and geography, with these categories often having similar impacts to size. For example, sectors such as manufacturing, professional services and ICT are around two-thirds more likely to receive support than agricultural firms with the same mix of other characteristics.

An important result of our propensity score specification is that the common support assumption does not hold in all of these cases, even though it does in the vast majority. As will become clear from our matching outputs, some treated firms will have no control firms with similar propensity scores.

The matching algorithm

The second stage of the PSM approach is the matching of treated firms to control firms based on propensity scores. There are a wide range of ways in which this matching may occur through the choice of different algorithms. The goal of the matching process is to find credible counterfactual observations for each of the treated firms, so the success of a matching process depends on the extent to which:

- Treated firms are successfully matched to controls; and
- The treated firms and the matched controls are as similar as possible in terms of their observable characteristics.

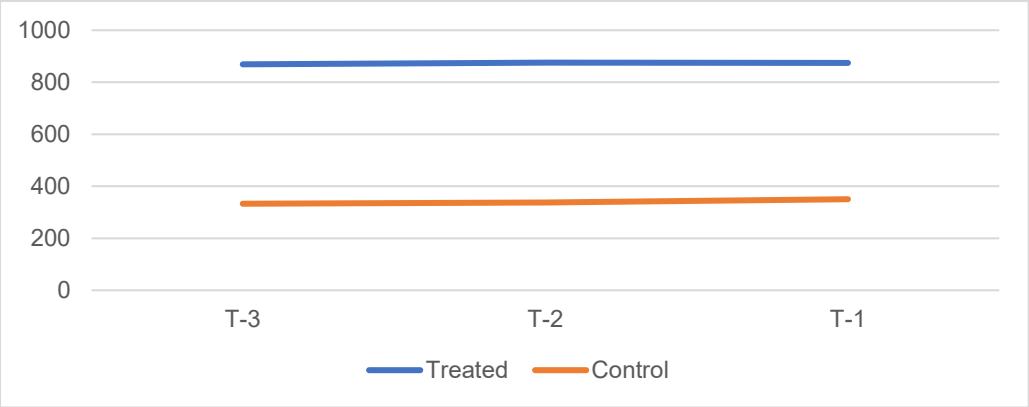
Our overall approach is to use a “nearest neighbour” (NN) matching procedure in which each treated unit is matched to its n closest control group observations in terms of propensity score. To reduce the possibility of “bad matches” a calliper is added, requiring that a treated firm can only be matched to a control firm with a sufficiently similar propensity score. A smaller calliper means a higher quality of matches at the expense of some unmatched firms, as there may be treated observations without nearby comparable controls. We also stratify the matching by firm size, meaning that a small firm will only be matched to other small firms and medium/large firms will only be matched to other medium/large firms.

A tighter calliper and larger n will tend to result in treatment and matched controls that are more similar, but will result in more observations being unmatched. We tested various combinations in terms of the number n of NNs used and the size of calliper. We concluded that the trade-off was best met using a calliper of 0.0001 and using only one NN.[19]

[19] Further discussion of this approach is provided in Annex C, with detailed comparison of performance of the algorithms at Table 48.

A key consideration is that the PSM results in the treatment and matched controls having similar growth trajectories in the period preceding treatment. If this is the case, it is reasonable to attribute any subsequent divergence to the programme impact. However, if the two groups are already evolving differently, this is more difficult. Figure 3 shows that, once the matching procedure has taken place, there are no clear differences in pre-treatment trends between treated and matched control firms.

Figure 3. Pre-treatment trends, employment, treatment year 2015



Measuring the impact of DBT support two and three years after treatment

As noted above, this report extends the previous literature by considering treatment effects over multiple years. A challenge in this analysis is that many firms treated in given year t also use export promotion services in years t+1 and t+2.

For firms treated at both t and t+1, it is very difficult to disentangle the impact of treatment in t on the outcome in t+2 from the impact of treatment in t+1. A similar, and obviously more acute, issue arises when considering impacts to t+3 as firms may have multiple combinations of support received in years t, t+1 and t+2.

For firms treated at t, but not t+1, we can identify the long-term effect of this treatment. However, it must be noted that firms that are not treated repeatedly ("T-only") are different in some respects from those that receive repeated treatment ("multi-T"). This is shown in Table 8 for a selection of variables.[20] One of the more apparent differences is that those which are treated in multiple years already have considerably higher propensity to export, prior to receiving support. There is also higher representation among manufacturing firms. Other variables such as location or most other sectors have very little difference between the two groups.

[20] A more detailed breakdown is provided in Table 42, Annex B.

Table 8. Comparison of key characteristics, by treatment duration

Variable	Small T- only	Small multi-T	Medium/ large T-only	Medium/ large multi-T
Turnover (t-1), (£000s)	1,906	2,936	137,432	116,855
Employees (t-1)	9	12	453	568
Export dummy at t-1	26%	44%	55%	75%
Export value (t-1), (£000s)	1,093	415	10,299	19,009
Firm age	14	15	27	29
Dummy for foreign-owned enterprises	4%	4%	22%	23%
Manufacturing dummy	15%	21%	27%	40%

Therefore, the "T-only" estimate is specific to the type of firm that is likely not to receive several treatments over time. Estimates on the T-only group may be either an under- or over-estimate of how much "multi-T" companies have benefitted from treatment at T:

- They may be an over-estimate if "multi-T" firms are those that require more support.
- They may be an under-estimate if "multi-T" firms are more interested in/willing to/able to expand their exports and therefore access more of DBT's support.

Logarithmic dependent variables

We should note that measuring turnover, exports value and employment in linear terms may give considerable variation in these outcomes, which may make effects difficult to detect. We pursue an alternative strategy using a logarithmic (log) dependent variable to mitigate this.

The linear approach assumes that the impact on turnover in £ terms would be the same regardless of whether a firm has a turnover of £100k or £10 million. This may not be appropriate, as a £1 million uplift would be very large for a small firm but fairly small for a large firm. By contrast, the log dependent variable approach has the property of impacts being measured in proportional rather than absolute terms. This might model the impact as a 10% uplift across firms of all sizes. This allows the impact in absolute terms to be bigger for larger firms but the same in percentage terms.[21] This may well be more appropriate given the range of firm sizes observed in

[21] A log dependent variable approach is similar to using percentages as the dependent variable and, for small changes, they are approximately equal. For larger changes, logarithms are preferable to percentages, as they are symmetric between increases and decreases. For example, a logarithm treats halving and doubling as being the same magnitude of change, whereas in percentage terms the doubling (+100%) is twice as large a change as the halving (-50%). Note that strictly speaking, the formula for deriving exact percentage changes from a log variable is given by exp(beta)-1. For example, a coefficient of 0.07 entails a percentage increase of exp(0.07)-1 = 1.073-1 = 7.3%.

the data. This also has the advantage that the error variance is also more likely to be constant.

Note that logarithms are only defined for positive values, so the results for log export value focus only on firms that were already exporting prior to treatment. It therefore breaks the export effect into 1) entry into exporting (via the export dummy variable), and 2) growth among established exporters.

Robustness checks

To make sure our results are robust to the methodological choices we made, we produce alternative estimates.

Firstly, we test how sensitive our results are to the exclusion of specific observations, as follows:

- By testing the sensitivity of results to the exclusion of the largest businesses. We do this by removing the top 5% of businesses by turnover and re-running the matching. This is consistent with past work on the topic by NIESR.
- By testing the effect of the data-linking processes by excluding those firms which had exports apportioned due to sharing a VAT reference. This will give an indication as to whether our choices in this area have biased results.

A separate check of our results is to re-estimate the treatment effects using standard ordinary least squares (OLS) regression with the inputs to the propensity score as controls. We also repeat this exercise on a trimmed sample of firms based on propensity scores to ensure comparability with our matching estimates. The interpretation of coefficients for this method is more straightforward and allows a clearer sense of the way in which the PSM process removes selection bias. The full coefficients for these estimates are compared to our matching estimates and discussed in the results section where relevant.

Limitations with methodology

A fundamental limitation of the approach used is that selection into treatment may be correlated with unobservable factors that affect subsequent performance. For example, a firm may recently have undertaken an innovation and be looking to market this and may opt to engage with export promotion services. Suppose there are also firms that have not undertaken similar innovation, have no desire to serve additional markets and do not seek any export promotion support. Comparing the two groups, “selection” into using the services may be driving the results above and beyond any causal impact of the programme. This would bias upwards the estimated impact.[22]

The Maryland Scale is a system for classifying the robustness of evaluation approaches.[23] Level 5, the most robust, covers randomised controlled trials, where selection into treatment is entirely at random, so there is no link between treatment and any firm-level characteristics, observed or not. The next level of robustness,

[22] Note that the direction of “bias” is not necessarily upwards. A counterexample would be if firms anticipating success felt that export promotion services were unnecessary, while weaker firms seek support, which would cause a downward bias.

[23] A useful overview is available at https://whatworksgrowth.org/resources/the-scientific-maryland-scale/

level 4, relates to “quasi-experimental” approaches. While not explicitly randomised, these methods exploit arbitrary variation, such as discontinuity in eligibility criteria; whether a firm is one side or another of an arbitrary threshold has little bearing on outcomes and is virtually random.

PSM and DiD are at a lower level of robustness, level 3. This is because assignment into treatment and control may be correlated with various characteristics, observed or unobserved, and these in turn may be correlated with post-treatment outcomes. Thus there is scope for selection into treatment to be confounded with the effects of treatment. In the context of policy evaluation, it is difficult to achieve more robust methodologies than level 3, unless randomness or arbitrary variation is purposefully built into the policy design; this may raise important equity or ethical considerations or place the evaluation objectives at odds with the desired benefits of the policy.

Within Maryland level 3, PSM goes as far as possible to mitigate the effects of differences between treatment and control groups by focusing on comparator firms that have a similar propensity for treatment and are alike in terms of the characteristics that drive treatment. This involves using balancing tests and stratifying by size band to achieve this as far as possible. Crucially, the parallel trends assumption establishes that the matched control group evolves similarly to the treatment group in the period preceding the intervention. In the hypothetical selection bias example presented above, we might expect some pre-treatment difference in growth trajectories between the groups, which would cause the parallel trends assumption to fail.

Clearly, the process to implement PSM involves “fine-tuning” for the tests to work as well as possible. Particularly in relation to medium and large firms, not every test will be met in every case. This reflects data limitations (refer to the discussion of limitations in the previous section on dataset), as not every firm will have a comparator that is sufficiently similar.

Results

Success of the matching exercise

The PSM approach we employ controls for observable differences between treated and non-treated firms by only using data on firms that have a sufficiently close comparator. The more demanding the definition of "sufficiently close", the greater the proportion of firms that will be dropped from the analysis, potentially giving a trade-off between internal and external validity of the analysis. However, we see that the matching algorithm results in only a fairly small loss of treated firms from the sample, as shown in Table 9.

Table 9. Sample size of treated firms before and after matching

Size band		2014 treatment	2015 treatment	2016 treatment
Small	Before	19,428	19,454	12,986
Small	After	17,779	17,661	12,137
Medium/large	Before	4,298	4,614	3,108
Medium/large	After	3,973	4,290	2,914

The charts below show the proportion of medium and large treated firms by turnover category and by industry, comparing before and after matching. Before matching, there are some clear compositional differences with supported firms larger in turnover terms and significantly more geared towards manufacturing. Post matching, the differences are more limited, again suggesting that the sample of firms included in our results is not significantly different from the overall sample of firms receiving export promotion support. This gives us confidence that the results presented in the next section are representative of the effect of the mix of DBT services included in our support data.

Figure 4. Distribution of treated medium and large firms by turnover group, 2014

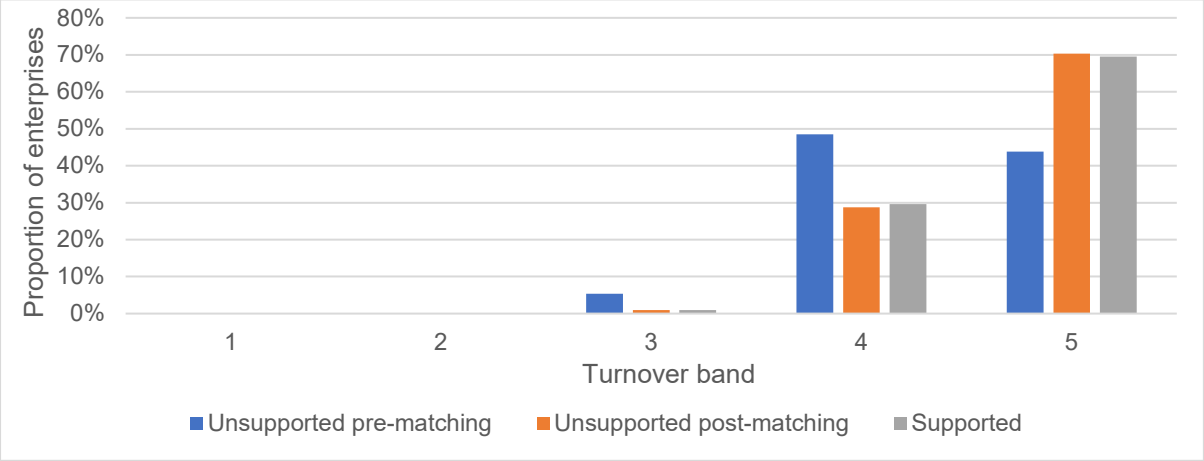
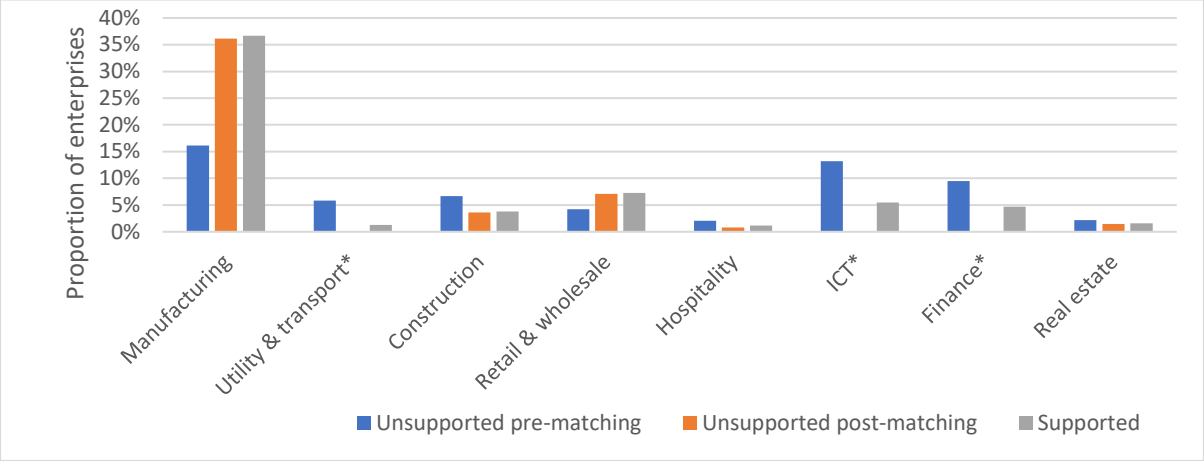


Figure 5. Distribution of treated medium and large firms by industry, 2014



Note: *Asterisks indicate sectors that have had data suppressed and do not indicate zero values. Some sectors have been suppressed entirely.

The impact of DBT support one year after treatment

We find that DBT support is associated with:

- A statistically significant increase in survival. Small firms see an uplift of 2.5 percentage points, and medium/large firms of 0.7 percentage points.
- A statistically significant increase in the rate of exporting. The uplift for small firms is in the order of 7.6 percentage points and for medium/large firms it is 5.8 percentage points.
- An increase in employment of 0.6 employees for small firms and 26.7 employees for medium/large firms. Both these effects are significant at the 10% level.
- Positive impacts on turnover, employment and export growth for small firms when estimated in a logarithmic specification. Treated firms grow between 4% and 9% relative to the counterfactual. The effects are statistically significant for small firms. For large firms, the effect is only significant in the case of log employment.

Average results across treatment years

Table 10 below summarises the main results of our PSM-DiD approach for the impact of DBT support one year after treatment. The key results are in the rightmost column, which shows estimated treatment effects ("matched" columns) for small and medium/large firms, for all the outcomes of interest.[24] To clarify, this shows the differences in outcomes between the treatment group and the control group derived using PSM. We also report simple mean differences between treated and non-treated firms ("unmatched" columns). Moving from "unmatched" to "matched" shows how the differences between groups change when we move to a more relevant counterfactual. The results in the table are obtained by averaging across the effects estimated separately for each treatment year, which are reported in Table 11 and

[24] Since they are binary state variables, survival and export status are simply difference in means, rather than difference-in-difference.

Table 12. The star levels show the statistical significance of the effects. For example, DBT support is associated with an increase of 7.6 percentage points in the proportion of exporters among the treated, and this effect is marked with ** because this effect is statistically significant at the 99% level of confidence in the treatment years used in our analysis (2014, 2015, 2016).[25]

[25] Note that statistical significance levels are based on averaging standard errors from the underlying year-specific effects. The “correct” method would be to compute bootstrapped standard errors; however, the computation power needed for this in such a large sample would be prohibitive.

Table 10. Average treatment effects for one year after treatment, all treatment years[26]

Size band	Outcome	Unmatched	Matched
Small	Survival	4.3%***	2.5%***
Small	Export status	34.0%	7.6%***
Small	Turnover (£000s)	91	-215
Small	Employment	1.3***	0.6***
Small	Turnover per employee (£000s)*	-3.1	-14.8
Small	Export value (£000s)	N/A (disclosure)	121
Small	Log turnover	0.05***	0.04***
Small	Log employment	0.06***	0.05***
Small	Log export value	N/A (disclosure)	0.09***
Medium/large	Survival	1.3%***	0.7%***
Medium/large	Export status	43.9%	5.8%***
Medium/large	Turnover (£000s)	9,406	22,848
Medium/large	Employment	21.4**	26.7
Medium/large	Turnover per employee (£000s)+	9.0	11.9
Medium/large	Export value (£000s)	N/A (disclosure)	£402
Medium/large	Log turnover	0	0.02
Medium/large	Log employment	0.04***	0.03**
Medium/large	Log export value	N/A (disclosure)	0.06
All firms	Survival	3.7%***	2.2%***
All firms	Export status	35.9%	7.2%***
All firms	Turnover (£000s)	1,874	4,197
All firms	Employment	5.2***	5.7***
All firms	Turnover per employee (£000s)+	-0.7	-9.8
All firms	Export value (£000s)	N/A (disclosure)	175
All firms	Log turnover	0.04***	0.04***
All firms	Log employment	0.06***	0.05***
All firms	Log export value	N/A (disclosure)	0.08**

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. +Export status and value, survival, employment and turnover are estimated on 2014, 2015 and 2016 observations. Turnover per employee is based on 2014 observations only. “Unmatched” differences for export value and export status exclude 2016 for disclosure reasons.

[26] Differences in survival rates in the unmatched samples cannot be reported for disclosure control reasons. Because only a small proportion of firms in certain groups in our analysis do not survive, reporting both the baseline survival rates and the effects of DBT support on survival would result in reporting some figures based on small samples of enterprises (below HMRC's threshold for enterprises).

The effect of treatment on export status is larger for small firms. This is the case both in absolute terms (the size of the treatment effect reported above) and relative to the baseline probability of exporting. Treatment is associated with a 7.6 percentage point increase in the proportion of exporters among the small treated firms, from a baseline of around 30% of exporters; among large firms, treatment is associated with a 5.8 percentage point increase, from a baseline of around 70% of exporters. This is in line with international evidence on the impact of export promotion (e.g., Broocks & Van Biesebroeck, 2017; Munch & Schaur, 2018). We also see that the effect on employment, turnover and export value in proportional terms (using log specification) is greater for small firms.

The presence of a survival effect makes the interpretation of other estimates more challenging. Firms supported by DBT are more likely than control firms to be active one year from treatment. This makes it difficult to understand to what extent the effect of DBT support on other outcomes (e.g., employment) at t1 (where t is the treatment year) is due to impacts on firms that would have survived anyway (regardless of support) or due to effects on which firms make it to t+1. However, the estimated survival effects are relatively small, especially if compared to baseline survival rates.[27] Therefore the selection into survival is likely to have a relatively small influence on our estimated impact of DBT support on other outcomes.

Results by treatment year

Table 11, Table 12 and Table 13 report our estimates by treatment year, for small firms, medium/large firms and all firms respectively. The tables show rows for each outcome variable in turn, with separate rows for the "average treatment effect for the treated" (ATT) and the t-statistic for statistical significance. The results are generally consistent across treatment years, apart from employment effects among medium/large firms, which are significantly larger for the 2015 treatment year (51 additional employees) compared to 2014 (20 additional employees).

[27] Baseline survival rates cannot be reported for disclosure control reasons.

Table 11. Average treatment effects by treatment year for one year after treatment, small firms

Outcome	Statistic	2014 treatment	2015 treatment	2016 treatment
Survival	Estimated ATT	2.5%***	2.4%***	2.6%***
Survival	T-statistic	10.6	8.4	7.2
Export status	Estimated ATT	7.8%***	7.5%***	7.4%***
Export status	T-statistic	10.93	10.89	8.82
Turnover (£000s)	Estimated ATT	-84.1	22.8	-744.7
Turnover (£000s)	T-statistic	-0.5	0.2	-0.2
Employment	Estimated ATT	0.8***	0.4	0.7***
Employment	T-statistic	5.7	1.4	3.5
Turnover per employee (£000s)	Estimated ATT	-14.8*	N/A	N/A
Turnover per employee (£000s)	T-statistic	-1.92	N/A	N/A
Export value (£000s)	Estimated ATT	373	-15	-47
Export value (£000s)	T-statistic	1.42	-0.32	-0.34
Log turnover	Estimated ATT	0.06***	0.03***	0.03***
Log turnover	T-statistic	5.72	3.57	2.11
Log employment	Estimated ATT	0.06***	0.05***	0.05***
Log employment	T-statistic	9.12	8.47	5.69
Log export value	Estimated ATT	0.11***	0.07***	0.10***
Log export value	T-statistic	3.42	2.22	2.35

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

Table 12. Average treatment effects by treatment year for one year after treatment, medium/large firms

Outcome	Statistic	2014 treatment	2015 treatment	2016 treatment
Survival	Estimated ATT	0.7%***	0.9%***	0.5%**
Survival	T-statistic	3.0	3.1	2.0
Export status	Estimated ATT	5.4%***	6.5%***	5.6%***
Export status	T-statistic	3.86	4.37	3.70
Turnover (£000s)	Estimated ATT	14,986.4**	15,783.4**	42,893.9
Turnover (£000s)	T-statistic	1.9	2.4	1.1
Employment	Estimated ATT	20.5	50.7**	-0.4
Employment	T-statistic	1.4	2.1	-0.1
Turnover per employee (£000s)	Estimated ATT	11.9	N/A	N/A
Turnover per employee (£000s)	T-statistic	0.23	N/A	N/A
Export value (£000s)	Estimated ATT	890	228	-1.9
Export value (£000s)	T-statistic	0.12	0.08	-0.002
Log turnover	Estimated ATT	0.01	0.03*	0.02
Log turnover	T-statistic	0.71	1.86	1.17
Log employment	Estimated ATT	0.02**	0.02*	0.03*
Log employment	T-statistic	2.15	1.87	1.93
Log export value	Estimated ATT	0.00	0.08*	0.12**
Log export value	T-statistic	0.09	1.68	2.40

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

Table 13. Average treatment effects by treatment year for one year after treatment, all firms

Outcome	Statistic	2014 treatment	2015 treatment	2016 treatment
Survival	Estimated ATT	2.1%***	2.1%***	2.2%***
Survival	T-statistic	9.25	7.38	6.18
Export status	Estimated ATT	7.3%***	7.3%***	7.0%***
Export status	T-statistic	9.64	9.62	7.83
Turnover (£000s)	Estimated ATT	2,606	3,160	7,869
Turnover (£000s)	T-statistic	-0.11	0.62	0.06
Employment	Estimated ATT	4.5***	10.4	0.5***
Employment	T-statistic	4.86	1.55	2.79
Turnover per employee (£000s)	Estimated ATT	-9.53	N/A	N/A
Turnover per employee (£000s)	T-statistic	-1.50	N/A	N/A
Export value (£000s)	Estimated ATT	469	34	-38
Export value (£000s)	T-statistic	1.18	-0.24	-0.28
Log turnover	Estimated ATT	0.05***	0.03***	0.03*
Log turnover	T-statistic	4.82	3.23	1.93
Log employment	Estimated ATT	0.05***	0.05***	0.05***
Log employment	T-statistic	7.76	7.09	4.93
Log export value	Estimated ATT	0.07**	0.07**	0.11**
Log export value	T-statistic	2.37	2.04	2.37

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

The impact of multiple treatment

We are interested in knowing whether receiving more than one treatment episode in a year leads to any difference in the impact of treatment. To test this, we divide the matched treated firms into "single-treatment" and "multiple-treatment" groups. We can then compute the estimated effect of DBT support separately for these two

groups. The estimated treatment effect is calculated by comparing both the single-treatment and the multiple-treatment firms with similar untreated firms.

For most outcomes, the estimated impact of DBT support is greater among multiple-treated firms compared to single-treated firms. As reported in Table 14, among small firms, receiving multiple-treatment episodes rather than a single treatment is linked with an increase in the probability of exporting in the following year of between 2 and 5 percentage points, depending on the cohort year. The uplift due to moving from single to multiple treatment is sizeable compared to the impact of single treatment on its own (ranging between 5 and 6 percentage points). These findings on the direction and statistical significance of the effect of multiple treatment are confirmed by alternative estimation methods (unweighted and weighted OLS regressions) employed as part of our robustness checks, which are described in detail in the next section of this report.[28]

[28] Table 16 and Table 17 include standard errors from t-tests of the difference between the multiple-treated and single-treated impact estimates. Using this approach, only differences in the effect of treatment on export status are statistically significant at the 95% level or above. However, note that testing the statistical significance of the difference between the single-treatment and multiple-treatment impacts is not straightforward, as both estimates are generated through a PSM procedure, with associated issues around the estimation of unbiased standard errors (as described earlier in this section and in footnote 24).

Table 14. The impact of multiple treatments on non-export outcomes one year after treatment, small firms

Outcome	Statistic	2014 treatment	2015 treatment
Survival	Multiple-treated effect	2.0%	1.6%
Survival	Single-treated effect	1.6%	1.5%
Survival	Difference	0.3%	0.03%
Survival	T-stat for difference	0.85	0.07
Turnover (£000s)	Multiple-treated effect	-159.9	N/A
Turnover (£000s)	Single-treated effect	-15.7	N/A
Turnover (£000s)	Difference	-144.3	N/A
Turnover	T-stat for difference	-0.67	N/A
Employment	Multiple-treated effect	1.02	0.24
Employment	Single-treated effect	0.62	0.56
Employment	Difference	0.40*	-0.32
Employment	T-stat for difference	1.89	-0.34
Turnover per employee (£000s)	Multiple-treated effect	-18.17	N/A
Turnover per employee (£000s)	Single-treated effect	-11.73	N/A
Turnover per employee (£000s)	Difference	-6.44	N/A
Turnover per employee	T-stat for difference	-0.41	N/A

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed from t-tests of the difference between the multiple-treated and single-treated effects.

Table 15. The impact of multiple treatments on export outcomes one year after treatment, small firms

Outcome	Statistic	2014 treatment	2015 treatment	2016 treatment
Export status	Multiple-treated effect	10.6%	8.4%	N/A – (disclosure)
Export status	Single-treated effect	5.2%	6.4%	N/A – (disclosure)
Export status	Difference	5.4%***	1.9%***	3.1%***
Export status	T-stat for difference	7.97	2.82	3.69
Export value (£000s)	Multiple-treated effect	671.2	-15.8	-158.9
Export value (£000s)	Single-treated effect	105.4	-13.5	50.9
Export value (£000s)	Difference	565.8	-2.4	-209.8
Export value	T-stat for difference	0.98	-0.04	-1.21

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed from t-tests of the difference between the multiple-treated and single-treated effects.

Estimates of the effect of multiple treatment of medium and large firms follow a similar pattern compared to the estimates on small firms: the only outcome affected by the number of treatment episodes is export status, as shown in Table 16 and Table 17. To prevent possible disclosure of confidential information at firm level, we cannot report the effects of treatment on export status specific to single-treated and multiple-treated firms. However, the analysis shows that the average effect of treatment on export status for medium and large firms is 5% to 6% depending on treatment year, as reported in Table 12 above. Therefore, as in the case of small firms, the estimated difference between single- and multiple-treated firms in the impact of treatment on export status is relatively large in magnitude, at around 4 percentage points for firms treated in 2014 and 2016. However, this difference is not statistically significant for firms treated in 2015.

Table 16. The impact of multiple treatments on non-export outcomes one year after treatment, medium and large firms

Outcome	Statistic	2014 treatment	2015 treatment
Turnover (£000s)	Multiple-treated effect	17,952	N/A
Turnover (£000s)	Single-treated effect	10,565	N/A
Turnover (£000s)	Difference	7,387	N/A
Turnover	T-stat for difference	0.60	N/A
Employment	Multiple-treated effect	29.70	64.74
Employment	Single-treated effect	6.56	24.95
Employment	Difference	23.14	39.79
Employment	T-stat for difference	1.07	1.15
Turnover per employee (£000s)	Multiple-treated effect	-5.29	N/A
Turnover per employee (£000s)	Single-treated effect	37.49	N/A
Turnover per employee (£000s)	Difference	-42.78	N/A
Turnover per employee	T-stat for difference	-0.38	N/A

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed from t-tests of the difference between the multiple-treated and single-treated effects.

Table 17. The impact of multiple treatments on export outcomes one year after treatment, medium and large firms

Outcome	Statistic	2014 treatment	2015 treatment	2016 treatment
Export status	Multiple-treated effect	N/A (disclosure)	N/A (disclosure)	N/A (disclosure)
Export status	Single-treated effect	N/A (disclosure)	N/A (disclosure)	N/A (disclosure)
Export status	Difference	4.5%***	2.4%	4.3%**
Export status	T-stat for difference	2.86	1.55	2.35
Export value (£000s)	Multiple-treated effect	975.5	128.9	948.8
Export value (£000s)	Single-treated effect	758.8	410.1	-1,120.2
Export value (£000s)	Difference	216.7	-281.2	2,069.0
Export value	T-stat for difference	0.04	-0.11	0.85

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed from t-tests of the difference between the multiple-treated and single-treated effects. Variation in impact on survival not reported due to disclosure control: because only a small proportion of treated firms do not survive, reporting the estimated effect would disclose information on small samples (below HMRC's threshold).

Robustness checks

We implemented a number of robustness checks to determine whether results are driven by particular aspects of the estimation method, the dataset or its construction. We find that the results presented so far are robust to changes in the specification of the estimation method and to the exclusion of firms whose exports have been apportioned across several enterprise references. However, we see that excluding the largest firms changes the results somewhat, noticeably reducing the estimated impact of support on the employment of medium and large supported firms. Our main results on employment are therefore driven at least in part by the effect of treatment on a minority of relatively large firms. These results are presented and discussed in further detail in Annex D, Table 50 to Table 52.

The impact of DBT support two and three years after treatment

The data available at the time of research means that outcomes two years after treatment can be analysed for the 2014 and 2015 cohorts. For three years after treatment, only the 2014 cohort can be used.

In general, the effects of DBT support at t+1 shown in the previous section of this report persist over time, but with some variation by outcome and firm type. Looking at t+1, t+2 and t+3 outcomes in the round, we find that DBT support is associated with:

- Higher survival rates, which are statistically significant and gradually increase over time. The effect for small firms grows from 2.5 percentage points after one year to 3.8 percentage points after two years, to 4.4 percentage points after three

years. For medium/ large firms, the effects are 0.7 percentage points, 1.6 percentage points and 2.8 percentage points respectively.

- There is a less obvious pattern in terms of propensity to export. The effect on small firms is 7.6 percentage points after one year, rising to 11 percentage points after two years. However, for large firms, the effect falls from 5.8 percentage points after one year to 3.3 percentage points after two years.
- For employment, the effects gradually increase for small firms, rising from 0.6 employees after one year to 0.9 employees after three years. For medium and large firms, there is more of a mixed picture. While impacts rise from 26.7 additional employees after one year to 47.8 after two years, they subsequently fall back to 23.3 employees. Only the impact after two years is statistically significant.
- The effects on turnover and value of exports continue to be statistically insignificant at t+2 and t+3 when measured using a linear specification. This is consistent with the t+1 findings shown in the previous section of this report.
- When measured using a logarithmic specification, the effect for employment, turnover and export value are positive and, in most cases, are statistically significant. Depending on the outcome, small firms grow by 6% to 11% relative to the counterfactual, whereas for large firms the range is 2% to 5%.

These results are summarised in Table 18.

Table 18. Average treatment effects, two and three years after treatment

Size band	Outcome	T+2	T+3
Small	Export status	8.3%*** (11.92)	8%*** (11.4)
Small	Export value (£000s)	214 (0.81)	329 (1.26)
Small	Log export value	0.11*** (3.05)	0.11*** (2.74)
Small	Survival	3.8%*** (10)	4.8%*** (10.91)
Small	Employment	0.8** (2.5)	0.9** (2.46)
Small	Turnover (£000s)	-2,944 (-0.8)	32 (0.03)
Small	Log employment	0.07*** (8.67)	0.07*** (8.01)
Small	Log turnover	0.06*** (5.22)	0.07*** (5.61)
Medium/large	Export status	6.7%*** (4.76)	6.1%*** (4.37)
Medium/large	Export value (£000s)	1,010 (0.08)	2,718 (0.57)
Medium/large	Log export value	0.04 (0.81)	0.09 (1.55)
Medium/large	Survival	1.6%*** (4.1)	2.8%*** (5.38)
Medium/large	Employment	47.8** (2.2)	23.3 (1.24)
Medium/large	Turnover (£000s)	20819 (0.9)	-31176 (-0.81)
Medium/large	Log employment	0.03** (2.16)	0.03 (1.54)
Medium/large	Log turnover	0.05** (2.42)	0.04* (1.67)
All firms	Export status	8%*** (10.57)	7.7%*** (10.12)
All firms	Export value (£000s)	369 (0.67)	784 (1.13)
All firms	Log export value	0.09** (2.28)	0.11** (2.34)
All firms	Survival	3.4%*** (8.89)	4.4%*** (9.9)
All firms	Employment	10** (2.46)	5.3** (2.22)2.
All firms	Turnover (£000s)	1717 (-0.43)	-6013 (-0.14)
All firms	Log employment	0.06*** (7.28)	0.06*** (6.67)
All firms	Log turnover	0.06*** (4.66)	0.07*** (4.83)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

Table 19 shows the difference between treatment and control groups two years after the treatment year. The results are split out for the small and medium/large groups and by outcome. The “unmatched” column shows the raw difference between treatment and control groups, while the “matched” column shows the difference between groups once the PSM procedure has been implemented and the control group constituted accordingly, which is the appropriate measure for assessing the causal effects of treatment. The results encompass the 2014 and 2015 cohorts, looking at outcomes for 2016 and 2017 respectively.[29] In general, the differences are smaller when assessed on a matched basis. This indicates that some of the raw difference is due to compositional differences between the groups, which is removed by matching.

[29] When looking at the impact of support two and three years after support, we did not investigate effects on turnover per employee. This is due to a) no effects being found at t+1, and b) concerns that the exact timing of employment and turnover variables may not always be the same, so that the ratio of the two measures might be a misleading proxy in some cases.

Table 19. Average treatment effects for two years after treatment, all treatment years

Size band	Outcome	Unmatched	Matched
Small	Survival	7.2%***	3.8%***
Small	Export status	n/a (disclosure)	8.3%***
Small	Export value (£000s)	135	214
Small	Employment	1.8***	0.8**
Small	Turnover (£000s)	102	-2,944
Small	Log export value	0.13**	0.11***
Small	Log employment	0.07***	0.07***
Small	Log turnover	0.06***	0.06***
Medium/large	Survival	2.5%***	1.6%***
Medium/large	Export status	n/a (disclosure)	6.7%***
Medium/large	Export value (£000s)	2,229*	1,010
Medium/large	Employment	37.9***	47.8**
Medium/large	Turnover (£000s)	14,527	20,819
Medium/large	Log export value	0.04	0.04
Medium/large	Log employment	0.06***	0.03**
Medium/large	Log turnover	0.003	0.05**
All firms	Survival	6.3%***	3.4%***
All firms	Export status	n/a (disclosure)	8%***
All firms	Export value (£000s)	544	369
All firms	Employment	8.9***	10**
All firms	Turnover (£000s)	2,932	1,717
All firms	Log export value	0.05***	0.06***
All firms	Log employment	0.06***	0.06***
All firms	Log turnover	0.1***	0.09**

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

Table 20 breaks down the results further by cohort year, also separately reporting the t-statistic for statistical significance. Overall, the separate cohorts show very similar results to each other, with positive effects on survival and exporter status of a similar magnitude across years. For small firms, logarithmic results are also similar across years, although there is some difference for larger firms.

Table 20. Average treatment effects by treatment year for two years after treatment

Size band	Outcome	Coefficient (2014)	T-statistic (2014)	Coefficient (2015)	T-statistic (2015)
Small	Survival	3.5%	10.14	4.0%	9.89
Small	Export status	8.5%	12.1	8.1%	11.74
Small	Export value (£000s)	431	1.53	3	0.11
Small	Employment	1.1	4.12	0.5	0.93
Small	Turnover (£000s)	-87	-0.50	-5,826	-1.00
Small	Log export value	0.12	3.20	0.11	2.90
Small	Log employment	0.07	9.24	0.07	8.11
Small	Log turnover	0.07	6.03	0.05	4.40
Medium/large	Survival	1.4%	3.92	1.8%	4.17
Medium/large	Export status	6.1%	4.52	7.2%	4.97
Medium/large	Export value (£000s)	2,219	0.23	-99	-0.05
Medium/large	Employment	48.9	1.86	46.8	2.50
Medium/large	Turnover (£000s)	6,489	0.78	34,089	0.97
Medium/large	Log export value	0.06	1.29	0.02	0.37
Medium/large	Log employment	0.02	1.53	0.05	2.73
Medium/large	Log turnover	0.03	1.69	0.07	3.09
All firms	Survival	3.16%	9.00	3.56%	8.77
All firms	Export status	8.1%	10.72	7.9%	10.42
All firms	Export value (£000s)	772	1.28	-18	0.08
All firms	Employment	10.2	3.69	9.9	1.25
All firms	Turnover (£000s)	1,159	-0.26	2,270	-0.60
All firms	Log export value	0.10	2.59	0.08	2.05
All firms	Log employment	0.06	7.69	0.06	6.98
All firms	Log turnover	0.06	5.21	0.06	4.14

Note: standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

We also provide results for three years after treatment. These are shown in Table 21. Note that as these are drawn from one single cohort, there is no need to break results down further on this basis. For small firms these show continued growth with survival and employment effects both slightly bigger than at two years after (4.8 vs 3.8 percentage points and 0.9 vs 0.8 employees). In the case of medium and large firms, there is less obvious growth over time in effects. While the survival effect grows from 1.6 percentage points after two years to 2.8 percentage points after three years, the employment effect falls from a significant effect (47.8 additional employees after two years) to a non-significant effect (23.3 employees) after three years. This is similar to the one-year impact.

Table 21. Average treatment effects for three years after treatment, 2016 cohort

Size band	Outcome	Unmatched	Matched
Small	Export status	32%***	8%***
Small	Export value (£000s)	218	329
Small	Log export value	0.17***	0.11***
Small	Survival	9.6%***	0.048***
Small	Employment	2.1***	0.95**
Small	Turnover (£000s)	218	32
Small	Log employment	0.07***	0.07***
Small	Log turnover	0.07***	0.07***
Medium/large	Export status	41%***	6%***
Medium/large	Export value (£000s)	1,484	2,718
Medium/large	Log export value	0.03	0.09
Medium/large	Survival	0.036***	0.028***
Medium/large	Employment	11.8	23.3
Medium/large	Turnover (£000s)	16,963	-31,176
Medium/large	Log employment	0.08***	0.03
Medium/large	Log turnover	-0.01	0.04*
All firms	Export status	33.5%***	7.7%***
All firms	Export value (£000s)	460	784
All firms	Log export value	0.12***	0.11**
All firms	Survival	12%***	7%***
All firms	Employment	5.3***	10.2**
All firms	Turnover (£000s)	7,111	-8,622
All firms	Log employment	0.07***	0.06***
All firms	Log turnover	0.05***	0.07***

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

In interpreting these results, it is worth noting that the effects shown above may arise not only from treatment in the first year t, but also from firms receiving treatment in subsequent years, which is the case for around 60% overall.

Variation in results by treatment duration

We next consider how the results accrue over time and how this varies according to the treatment duration. First, we look at export-related outcomes two years after first treatment. In each case the "multi-T" impacts are substantially larger than when treatment is in one year only, apart from 2015 for medium and large firms, where the effects are of a similar size. There is little obvious pattern in relation to export value, where the results are largely non-significant. However, measured in logarithmic terms, the results point to statistically significant export increases in the region of 10% for small firms. The log export results for large firms are positive, and slightly larger than for log employment or log turnover (see Table 22); however, they are statistically non-significant.

Table 22. Export treatment effects, by treatment duration and time elapsed since treatment

Size band	Treatment duration	Outcome	T+1	T+2	T+3
Small	T-only	Export status	4.1%*** (5.68)	4.3%*** (6.32)	2.9%*** (4.57)
Small	Multi-T	Export status	10.2%*** (13.93)	11.4%*** (16.29)	12.6%*** (17.57)
Medium/ large	T-only	Export status	4.5%** (2.1)	3.5%* (1.74)	1.4% (0.66)
Medium/ large	Multi-T	Export status	6.4%*** (4.75)	8.1%*** (6.07)	8.3%*** (6.09)
Small	T-only	Export value (£000s)	238 (0.02)	354 (0.33)	527 (0.77)
Small	Multi-T	Export value (£000s)	35 (0.54)	111 (1.16)	165* (1.67)
Medium/ large	T-only	Export value (£000s)	890 (-0.04)	2,706 (-0.11)	5,922 (0.52)
Medium/ large	Multi-T	Export value (£000s)	217 (0.12)	284 (0.16)	1,286 (0.59)
Small	T-only	Log export value	0.01 (0.09)	0.01 (0.17)	0.05 (0.77)
Small	Multi-T	Log export value	0.13*** (3.73)	0.15*** (4.22)	0.14*** (3.6)
Medium/ large	T-only	Log export value	-0.01 (-0.11)	-0.02 (-0.19)	0.1 (1.05)
Medium/ large	Multi-T	Log export value	0.08* (1.72)	0.05 (1.12)	0.09* (1.7)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

Table 23 considers survival, employment and turnover outcomes, and splits the results for firms treated in one year only (T-only) from those treated in multiple years (Multi-T). The successive columns show the effect of treatment on the outcome one year, two years and three years later.

From this, the main insight is that multi-year contact invariably leads to larger impacts than when a firm is treated in one year only.[30] We can also look at how impacts accrue over time. The smaller "T-only" impacts show little variation over time, suggesting that the impact in these cases is fixed. However, the multi-T duration interventions show a clearly increasing positive trajectory, with survival and employment impacts all growing over time. Interpreting the survival result is intuitive since, at the very least, firms receiving support in subsequent years will need to be active at the point of receiving further support.

The logarithmic results show positive effects on turnover for both small firms and for medium/large treated in multiple years (the T-only effects for medium/large are non-significant). These show a similar pattern to the survival and employment impacts, which are bigger for small firms. The log turnover and employment impacts are broadly in line with each other, indicating a consistent effect on growth across these variables, so that the changes are broadly neutral with respect to turnover per employee.

Table 23. Non-export treatment effects, by treatment duration and time elapsed since treatment, small firms

Duration	Outcome	T+1	T+2	T+3
T-only	Survival	1.3%*** (3.19)	1.8%*** (3.45)	1.2%** (2.16)
Multi-T	Survival	3.4%*** (13.29)	5.3*** (15.13)	8%*** (18.8)
T-only	Employment	0.67*** (3.73)	0.55** (2.19)	0.63*** (2.91)
Multi-T	Employment	0.59*** (3.39)	0.99*** (2.77)	1.21** (2.09)
T-only	Turnover (£000s)	-99 (0.49)	-176 (0.92)	-100 (0.82)
Multi-T	Turnover (£000s)	-299 (0.05)	-4982 (0.63)	141 (0.72)
T-only	Log employment	0.04*** (5.13)	0.04*** (4.52)	0.02* (1.79)
Multi-T	Log employment	0.06*** (10.03)	0.09*** (11.63)	0.12*** (13.08)
T-only	Log turnover	0.03*** (2.8)	0.03** (2.43)	0.01 (0.72)
Multi-T	Log turnover	0.05*** (4.8)	0.08*** (7.26)	0.13*** (9.62)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

[30] Note that the distinction explored here between treatment in multiple years vs treatment in one year only is not the same as the distinction between intensive and "normal" treatment explored earlier in the report, even though there is some overlap between the two distinctions.

Table 24. Average treatment effects, by treatment duration and time elapsed since treatment, medium/large firms

Duration	Outcome	T+1	T+2	T+3
T-only	Survival	0.0% (0.21)	0.8% (1.21)	1.1% (1.26)
Multi-T	Survival	1.0%*** (3.73)	2.0%*** (5.29)	3.6%*** (7.29)
T-only	Employment	-3.7 (0.2)	2.2 (0.06)	-13.4 (0.62)
Multi-T	Employment	38.2* (1.86)	67.3*** (3.1)	40** (2.08)
T-only	Turnover (£000s)	4,742 (0.63)	-8,489 (0.62)	-16,747 (1.22)
Multi-T	Turnover (£000s)	29,536** (2.34)	33,407 (1.52)	-37,721 (0.62)
T-only	Log employment	0.03* (1.73)	0.03 (1.24)	-0.01 (-0.22)
Multi-T	Log employment	0.02** (2.08)	0.04** (2.55)	0.04** (2.35)
T-only	Log turnover	0 (0.17)	0 (0)	-0.02 (-0.56)
Multi-T	Log turnover	0.03* (1.7)	0.07*** (3.45)	0.07*** (2.67)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

Robustness checks – OLS estimation

We undertook systematic robustness checks using an OLS DiD regression on a trimmed sample. The "trimming" involves calculating the upper and lower quartiles of the propensity score range for the relevant part of the treatment group and retaining just the observations of the treatment and control groups that lie in the middle quartiles. This means that we compare treatment and control units that have a broadly similar propensity score allowing for a like-for-like comparison. In particular, the analysis removes treated units with a very high propensity and control units with a very low propensity, which are unlikely to provide a suitable basis for comparison.[31]

The results are broadly similar to those estimated using direct PSM. Among small firms, we continue to see positive effects on survival and employment. The effects are larger for those receiving treatment in multiple years and these grow over time. For medium/large firms, the effects are discernible only for those with treatment in multiple years. There are no discernible effects on turnover. The OLS results are presented in Annex D, Table 53 to Table 56.

[31] Results on the full sample, rather than the trimmed sample, are provided in Table 55 and Table 56, Annex D.

Estimating regional variation in the impact of export promotion

A separate issue we explore is whether there is any regional variation of impact. In a scoping phase, we determined that sufficient sample sizes would be generated by analysing outcomes one year after treatment, pooling across the 2014, 2015 and 2016 cohorts, and looking at five different regions. The regions are:

- London
- South of England (South East + East of England + South West)
- North of England (North East + North West + Yorkshire)
- Midlands (East Midlands + West Midlands)
- Scotland + Wales + Northern Ireland

Table 25 shows the export-related outcomes, with columns for the different regions. Visual comparison of the scores confirms that there is relatively little regional variation in the effects. While it is likely that variation between regions may be tested and found to be significant, it is not obvious that any systematic or intelligible pattern in terms of regional variation would emerge. We see that export status impacts for firms in the North are somewhat larger (9% and 11% for small and medium/large firms respectively compared with 7.6% and 5.8% at national level). This is not robust to checks shown in following tables within this section of the report.

Table 25. Regional breakdown of export-related outcomes one year after treatment

Size band	Outcome	South	London	Midlands	North	Scotland/ Wales/NI
Small	Exporter status	9%*** (14.79)	5.7%*** (8.45)	10.4%*** (11.89)	9%*** (10.44)	7.4%*** (5.38)
Small	Export value (£000s)	2.5 (0.05)	-62.4 (-0.83)	80.5 (1.57)	38.3 (1.57)	54.5 (0.75)
Small	Log export value	0.16*** (5.94)	0.04 (0.83)	0.07* (1.91)	0.1** (2.56)	-0.11 (-1.57)
Medium/ large	Exporter status	5%*** (3.41)	6.8%*** (3.32)	6.1%*** (3.03)	10.7%*** (5.77)	11.4%*** (3.72)
Medium/ large	Export value (£000s)	-1,400.2 (-0.76)	2,144.9 (0.32)	806.8 (1.1)	319 (0.43)	1,640.6** (2.00)
Medium/ large	Log export value	0.02 (0.51)	0 (0.05)	-0.01 (-0.15)	0.06 (1.23)	0.11 (1.32)
All firms	Exporter status	8.3%*** (12.92)	5.9%*** (7.65)	9.7%*** (10.34)	9.4%*** (9.5)	8.2%*** (5.03)
All firms	Export value (£000s)	-240 (-0.09)	309 (-0.64)	219 (1.48)	97 (1.33)	418 (1.04)
All firms	Log export value	0.123*** (4.42)	0.03 (0.61)	0.046 (1.26)	0.085** (2.1)	-0.029 (-0.48)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

Employment and turnover outcomes also show little by way of regional variation. We see:

- Largely significant impacts on employment, ranging from 0.6 to 0.9 employees for small firms, while for larger firms there is more dispersion in the range of impacts;
- Little effect on turnover, with positive effects at the 10% significance level in some cases and negative or insignificant effects in others;
- Positive effects on log employment, ranging from 4-6% for small firms and 2-3% for medium and large firms; and
- Positive effects on log turnover, around 2-5% for small firms and generally statistically significant.

Table 26. Regional breakdown of non-export-related outcomes one year after treatment

Size	Outcome	South	London	Midlands	North	Scotland/ Wales/NI
Small	Employment	0.6*** (7.64)	0.9*** (5.5)	0.7** (2.21)	0.8*** (3.72)	0.6 (1.13)
Small	Turnover (£000s)	-33.7 (-0.36)	-24.3 (-0.09)	522.5 (0.99)	113.8* (1.7)	-78.4 (-0.83)
Small	Log employment	0.06*** (12.05)	0.06*** (8.76)	0.04*** (6.1)	0.05*** (7.35)	0.06*** (4.93)
Small	Log turnover	0.04*** (5.93)	0.05*** (4.48)	0.02*** (2.62)	0.05*** (5.29)	0.04** (2.55)
Medium/ large	Employment	27.4*** (2.72)	41.1* (1.77)	48.9** (2.26)	16.3 (1.06)	12.3 (1.5)
Medium/ large	Turnover (£000s)	5,283.8* (1.95)	5,7578.2 (1.12)	2,621.4 (0.64)	5,665.8* (1.86)	6,375.3 (0.62)
Medium/ large	Log employment	0.03*** (2.75)	0.03* (1.9)	0.04** (2.41)	0.03** (2.17)	0.02 (1.09)
Medium/ large	Log turnover	0.02* (1.67)	0.05** (2.2)	-0.01 (-0.61)	0.04** (2.52)	0.04 (1.44)
All firms	Employment	5.288*** (6.79)	7.696*** (4.87)	9.874** (2.22)	4.083*** (3.16)	3.269 (1.22)
All firms	Turnover (£000s)	876 (0.03)	9,489 (0.11)	918 (0.93)	1,273* (1.73)	1,366 (-0.51)
All firms	Log employment	0.052*** (10.37)	0.058*** (7.53)	0.039*** (5.37)	0.045*** (6.21)	0.052*** (4.02)
All firms	Log turnover	0.04*** (5.2)	0.051*** (4.11)	0.018** (2.01)	0.049*** (4.71)	0.044** (2.3)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively. Standard errors computed as in Abadie and Imbens (2006) using the Stata psmatch2 command.

As a robustness check, we also repeated the procedure of OLS DiD regression on a trimmed sample removing the top and bottom quartiles. This is shown in Annex D, Table 57 and Table 58. Overall, these are similar to the preceding PSM results, with no obvious pattern in regional variation. On this basis there is little support for the view that there is any obvious systematic regional variation in impacts. In other words, we can be fairly confident that the general impacts observed are not specific to any one geography and might be expected to occur in any location.

Conclusions

The analysis described in this report shows that export promotion services provided by DBT are linked with supported firms growing in employment, being more likely to export, and increasing the value of their exports and their turnover in percentage terms. These results are generally robust to changes in the specification of our econometric estimation strategy, but there is some variation across firm types, years and outcomes:

- The positive effect of DBT support on export value is mainly driven by small firms; the effect on export value of medium and large firms is positive but not statistically significant.
- The estimated impact of support on the employment of medium and large firms is driven in part by effects on the largest firms in the sample (largest 5% in baseline employment distribution).
- Estimates of the impact of support on the probability of exporting are consistent across treatment years (2014, 2015, 2016), while other estimates can vary substantially from year to year. This is likely to reflect variations in the size and composition of the treated sample.
- The impact of support on export value and turnover is statistically significant in logarithmic specifications (when we estimate the effect of support on percentage changes in export value) but not in linear specifications. This could reflect difficulties in estimating statistically significant effects on these two variables as a result of their greater volatility compared to employment and export status.

The estimated impact of support on the probability of exporting is sizeable. Looking at impacts two years after treatment, the increase in likelihood is 8.3 percentage points among small firms and 6.7 percentage points among medium and large firms.[32] These effects on probability of export are statistically significant. Modelled in logarithmic terms, we also see a positive significant impact on export value for small firms of 11.9 percentage points, and a 4.0 percentage points effect for medium and large firms, although this effect is not statistically significant. So overall, we find that DBT support results in more firms exporting and in existing exporters exporting more. Combining these effects, we can see overall value uplifts of 20.2% for small firms and 10.7% for medium and large firms.[33]

[32] These effect sizes suggest export promotion services provided in 2014 and 2015 may have led around 2,900 additional small firms and around 600 additional medium and large firms to start exporting (in the sense of not exporting at the baseline year but to be exporting two years after first treatment). These figures are obtained by multiplying the estimated effect of support on the probability of exporting by the sample size of supported firms included in the estimation (around 36,000 small firms and 9,000 medium and large firms). This sample is a subset of the total number of firms supported by DBT in 2014 (there are around 40,000 firms with a CRN in total included in the DBT support data in those years, and this is itself a subset of all firms receiving DBT export promotion services, given the data limitations described earlier in this report).

[33] The overall uplift for medium and large firms is 6.7% if omitting the statistically non-significant effect on log export value.

In terms of heterogeneity of impacts, we consistently see that impacts are more significant and larger for small firms than for medium or large firms. The impacts are also considerably larger, and growing over time, for firms that interacted with DBT over multiple years than for firms that had support in only one year. In general, the effects of DBT support at t+1 persist over time, but with some variation by outcome and firm type.

In terms of geographic variation, there is little obvious difference in the responses of firms in different regions. As might be expected when running results for many different cuts of the sample, a range of estimates will be observed, but these do not point to any systematic regional effects. In other words, the region in which a firm is located is unlikely to have much bearing on the impact of the intervention.

In terms of understanding limitations, much of this relates to how DBT support and exports are measured:

- A key limitation is that the management information does not always identify recipients of business support. This means there will be cases where supported firms are not identified in the data and instead appear in the control group. If they also see a positive impact from support, this will bias the estimated effects downwards. Likewise, there may be other forms of business support provided by other organisations; if these are correlated (positively or potentially negatively), this would be confounded with the impacts attributed to DBT.

- There are concerns around the quality of export data, which is collated from different sources with different reporting thresholds. In particular, the export data includes goods but not services, so the results will omit any direct impact via services exports. The business data also suffers limitations, such as timeliness in the IDBR. The outcome measures in it are also fairly crude in that turnover is not necessarily a strong signal of profitability or productivity, and employment is measured only on a headcount basis. There may also be difficulties in generalising findings to the wider business population. At one end, the dataset does not cover sole traders well, so it is difficult to infer what impacts might be felt for that segment. And, looking at large firms, there are cases where there are no sufficiently similar comparators, making it difficult to measure impacts in these cases.

- A more fundamental limitation arising from the observational nature of the study is that treatment is non-random. This is mitigated as far as possible using the PSM approach, which makes treatment and control groups similar in terms of observable characteristics. However, it is possible that unobservable factors may affect the decision to seek DBT support and be correlated with business prospects. To overcome this limitation would require an experimental or quasi-experimental policy design, which may raise other difficulties.

Opportunities for further research could involve investigating the effects of specific DBT services (e.g., comparing events to webinars), or exploring the intensity of support in a “dose-response” framework. As more data becomes available it will be

possible to explore more granular hypotheses, although difficulties posed by atypical economic shocks such as Covid-19 would need to be considered.

Annex A - Further detail on data sources and construction of the dataset

Description of data sources

This section outlines in detail the data sources used in the study.

Inter-departmental Business Register (IDBR)
The IDBR is an Office for National Statistics (ONS) dataset of UK businesses used for a wide range of statistical purposes across government, as well as for research purposes.

The main administrative sources for this data are value added tax (VAT) and pay as you earn (PAYE) information provided to the ONS by Her Majesty's Revenue & Customs (HMRC). The administrative data is organised within hierarchical statistical units with the following definitions:

- An individual site (e.g., a store or office) is a local unit;
- An enterprise is the smallest combination of legal units with a certain degree of autonomy within an enterprise group; and
- An enterprise group is a group of legal units under common ownership.

For reasons discussed in the data-linking subsection below, the focus of our analysis is at the enterprise level. However, we do use information on ownership from the enterprise group level to estimate the likelihood of treatment.

The data in the IDBR is intended to be a comprehensive list of all the firms in the UK, with the following characteristics recorded for each:

- Turnover to the nearest £1,000 from VAT returns;
- Number of employees from PAYE records;
- Legal status (e.g., whether it is a sole proprietor or a partnership);
- Standard Industrial Classification (SIC) according to ONS definitions; and
- Geography (where the business is registered).

Since the data sources are large administrative records from taxes that businesses are legally obliged to pay, the coverage of the IDBR is extensive. However, the sources in question do not cover all firms. According to separate estimates by BEIS,[34] just 45% of businesses in the UK were registered for VAT or PAYE in 2018. Since we are interested in the impacts of DBT activities on employment, turnover, survival and exports, it is not a major concern if we are restricted by the data to looking only at firms with employees as they also will tend to account for most of the

[34] https://www.gov.uk/government/collections/business-population-estimates

turnover and exports. Nonetheless, many firms in VAT-exempt sectors[35] will have positive turnover and employment but no reference for VAT number, which does create issues discussed in the section on data linking.

As we are pursuing a difference-in-difference (DiD) approach, we chose to use the L-IDBR (the Longitudinal IDBR). Compared to the IDBR, the L-IDBR includes more precise information on the timing of employment and turnover information and includes indicators of the quality of turnover and employment collected.

The full IDBR is recorded every quarter and contains additional information on ownership, birth date and geography, which can be merged back into the L-IDBR where needed.

The L-IDBR is made using yearly extracts of the IDBR, meaning that our full dataset of UK firms is a yearly panel of all VAT- and/or PAYE-registered firms in the UK, which we use as a reference for matching other characteristics. The L-IDBR is available from 2003 to 2016 (as of the time of analysis), which means we can analyse pre-treatment trends largely unrestricted, and can track the relevant outcomes (turnover, employment and survival) for any interaction with DBT in 2015 or earlier.

HMRC overseas trade statistics

HMRC collects information on the export of goods by businesses in the UK. The main sources of this information depend on whether goods are exported to countries within or outside the European Union (EU):

- For exports to non-EU countries, information is mostly provided by traders in customs declarations for each "consignment", an individually reported batch of categorised goods being exported. Each of these consignments is given a monetary value, a category and an identifier for the trader who is exporting it.
- For exports to EU Member States, information on goods exported is gathered from a survey (Intrastat) and VAT data. Businesses that have trade with EU Member States above minimum thresholds (described below) report data to HMRC monthly. The data specifies, among other characteristics, what commodity is being exported and what partner country the commodity is being exported to.

Our analysis investigates the effect of support from DBT on supported enterprises. Therefore, we aggregate and/or convert information in HMRC overseas trade statistics to the enterprise level.

Reporting thresholds

Statistical data on goods exported outside the EU is generally covered by the customs administration associated with each shipment. By contrast, goods exported to other countries inside the EU are not necessarily captured by customs administration due to the Customs Union between Member States.

[35] A range of goods and services are VAT exempt, the most important of which is food for human consumption.

All VAT-registered businesses are required to report the total value of both imports and exports made with EU countries on their VAT return, and businesses above certain thresholds are required to make full reports on arrivals and dispatches through Intrastat. According to the HMRC Overseas Trade in Goods Statistics Methodology document, the thresholds are as follows.

Businesses whose annual value of arrivals and/or dispatches exceeds a given exemption threshold are required to provide an Intrastat declaration each month, showing full details of their arrivals (imports) and dispatches (exports) during that month. For the calendar years 2010 to 2013, these thresholds were set at £600,000 for arrivals and £250,000 for dispatches. For 2014, the arrivals threshold increased to £1,200,000, and the arrivals threshold for 2015 was £1,500,000.

These detailed Intrastat declarations are required to cover at least 93 per cent of the value of trade for arrivals, and at least 97 per cent of the value of trade for dispatches. Previously, in 2009, a single capture rate of 97 per cent was in place for both arrivals and dispatches. This change resulted in the number of Intrastat traders falling from around 33,000 in 2009 to around 27,500 from 2010 onwards

This is an important consideration for measuring firm-level exports to the EU, which may appear to change substantially across the sample period, but only in response to specific reporting rules imposed by HMRC rather than meaningful statistical variation.

Statistical values

For the purposes of our research, the outcome of interest is the turnover and profit to firms that result from exports, and how it changes in response to DBT support. Since our observation of export values comes from HMRC overseas trade statistics, it is important to understand how consignments are valued, and how this relates to firm outcomes.

The valuation of exports as provided by HMRC is done on an FOB (free-on-board) delivery basis. In simple terms, this means the cost of the exported goods to the overseas buyer, not including the costs of shipping from the point of export. The export valuation is therefore intended to include the costs of shipping to the point of export (including insurance, transport, etc.) and any export-point fees (such as docking fees or loading fees). For this reason, the statistical value recorded may not accurately reflect the turnover and profit recorded by the exporting firm, depending on the terms of its agreement with the purchaser.

The valuing of goods for export is not always straightforward. For example:

- The agreement to purchase goods may be made in a foreign currency, in which case the price must be converted to GBP by the reporting firm using official exchange rates.
- The exporting agreement may not be an open-market exchange, reflecting instead the movement of goods across international borders by a multi-national firm. In this case, the market value of the goods may not yet even be established at the time of reporting, leading to further difficulties.

A range of well-established rules are in place to deal with these statistical difficulties. However, they introduce errors into the measurement of the value of interest, like the examples listed above, that should be kept in mind.

Export of services

Many instances of exports by UK firms will not be captured by the HMRC consignment data since they do not involve any physical movement of goods. This is a significant shortcoming in our measurement, since the export promotion activities undertaken by DBT are not restricted to goods, and services exports accounted for a total of £162.1 billion according to the most recent estimates.[36]

There is no equivalent record of services exports at the firm level, meaning there is no alternative source of data to rectify this – the ONS estimate referenced above is based on a survey. This means that our analysis will underestimate the impact of export promotion services on exports, although it should remain an unbiased estimate of the effect of those services on goods exports specifically.

Data on DBT support

To identify which firms have received export promotion support provided by DBT, we rely on a dataset built by the market research company Kantar Public, which assembles information from several hundred files provided by DBT and filters out any cases of clearly inaccurate or inconsistent information. This dataset is the most comprehensive source of information on which firms were supported by DBT between the 2014 and 2016 calendar years. However, its coverage of DBT services is not perfect. For example, services provided by International Trade Advisors, Tradeshow Access Programme services and missions are better recorded than other services. As a result, the estimates provided by this report reflect the mix of services covered in the DBT support dataset. This is closely, but not completely, aligned to the mix of services provided by DBT, meaning that services whose delivery is better recorded will be somewhat over-represented. The lack of complete information for some services means it is not possible to assess exactly what proportion of total service deliveries (SDs) are recorded.

Table 27 below reports the number of unique SDs, unique enterprises and enterprises identified by a Companies House Reference Number (CRN) included in the DBT support data. Each enterprise received an average of around three SDs. Enterprises are identified by a CRN in only 20% to 30% of cases. The lack of a CRN is important because it makes it more challenging to find the enterprises included in the DBT support data in the other data sources used for our analysis (chiefly the HMRC overseas trade statistics and the L-IDBR).

The decrease in SDs and unique enterprises receiving SDs between 2015 and 2016 does not reflect a decrease in DBT's activities but rather a decline in the coverage of the DBT support data.

36 https://www.ons.gov.uk/businessindustryandtrade/internationaltrade/bulletins/internationaltradeinservices/2017

Table 27. Sample size in DBT support data

Year	Service deliveries	Unique enterprises	Unique enterprises identified by Companies House Reference Number
2014	228,939	73,713	18,419
2015	241,628	66,994	21,451
2016	157,275	42,760	19,749

Table 28 below reports the number of SDs associated with each service category as described in the DBT support data. However, the information in the dataset regarding which services were provided to enterprises is available in around only two-thirds of records. Therefore, the true distribution of services in the DBT support data may differ significantly from what is reported below.[37][38]

Table 28. Export promotion services included in DBT support data

Service category	Number of service deliveries
Events	180,335
Export Communication Review Scheme (ECR)	1,352
Export Market Research Scheme (EMRS)	1,260
Gateway to Global Growth	2,881
Inward Missions	3,610
Market Visit Support (MVS)	2,486
OBN Chargeable Services	110
Open to Export Assist (OtE)	53
Outward Missions	8,842
Passport to Export (including PtE R&D)	8,713
Significant Assistance (PIMS) and Significant Assistance Development Aid	168,709
Tradeshow Access Programme (TAP)	17,265

To link the DBT support data with HMRC and L-IDBR data, we relied on data-linking services provided by the HMRC Datalab and by the ONS. Where a CRN was included, the support data was linked to anonymised identifiers by the HMRC Datalab team. Where a CRN was not included, firms were “fuzzy matched” to an enterprise reference number (the identifier included in L-IDBR) by ONS staff, where possible, and the data was provided to HMRC for uploading to the Datalab.

Other sources of control data

We use data from two distinct sources to proxy for firms' propensity to innovate. The propensity to innovate has been linked to the propensity to export and therefore failing to control for differences between supported and unsupported firms in their propensity to innovate could lead to biased estimates of the impact of DBT support. In line with previous research (Rincón-Aznar et al., 2015; Mion & Muuls 2015), we

[37] Due to the gaps in the recording service line, the number of service deliveries is lower in Table 28 than in Table 27.

[38] A more detailed version of this table, cut by firm size and years of interaction, is provided in Annex B.

use data on patent grants and applications provided by the Intellectual Property Office (IPO). We also build on existing research by adding data on projects funded by Innovate UK, including all collaborative research and development (R&D), feasibility, smart and innovation voucher grants, and Knowledge Transfer Partnerships.[39] This allows us to ensure that estimated impacts are not driven by the receipt of these other forms of support, and provides a further proxy for firms' propensity to innovate.

Data-linking process

To construct the analytical dataset, we perform the following key operations:

1. HMRC overseas trade statistics
    - Aggregating from consignment level to trader level (for trade with non-EU partners)
    - Matching from trader identifier to VAT reference
    - Trader VAT ID to enterprise reference
    - Allocating exports to enterprises where necessary
2. L-IDBR data[40]
    - Restriction to active firms between 2014 and 2016
    - Linking to VAT ID
3. DBT support data
    - Matching undertaken by ONS
    - Aggregating from observation (“episode”) level to enterprise level
    - Linking of DBT support data to L-IDBR data based on enterprise reference numbers
4. IPO and Innovate UK data

HMRC overseas trade statistics
The “raw” HMRC customs declarations data includes information on individual "consignments", individually reported batches of categorised goods being exported. Consignments can be attributed to the organisation that is exporting the goods, identified by a trader ID. Table 29 shows the total number of consignments, their value and the number of traders making the consignments. The number of consignments and traders included in the non-EU data is larger than in the EU data. This is because the minimum size over which trade must be reported is lower for non-EU exports than for EU exports.

[39] Available at https://www.gov.uk/government/publications/innovate-uk-funded-projects
[40] An additional step for the longer-term analysis is to supplement the L-IDBR with the IDBR, thus extending the analysis to cover outcomes in 2017.

Table 29. Volume of trade in goods reflected in HMRC overseas trade statistics, 2014-2016

Partner	Year	Consignments	Value (£bn)	Traders
EU	2014	8,888,375	146.7	22,031
EU	2015	10,649,929	133.7	22,265
EU	2016	10,839,178	143.6	22,227
Non-EU	2014	4,902,760	163.7	75,332
Non-EU	2015	4,947,545	171.3	75,237
Non-EU	2016	5,418,713	163.1	77,087

The first step in linking exports data to other enterprise data is to link trader IDs to VAT IDs. A VAT ID is not available for 50 to 90 EU traders and 5,000-6,000 non-EU traders depending on year. This is likely due to traders whose size is below the minimum thresholds for VAT registration or which operate in VAT-exempt sectors. Table 30 shows the total volume of trade and number of traders matched to VAT IDs. Table 31 shows the corresponding figures that are matched into the L-IDBR data.

Table 30. Volume and value of trade in goods matched to VAT IDs reflected in HMRC overseas trade statistics data, 2014-2016

Partner	Year	Consignments	Value (£bn)	Traders
EU	2014	7,872,474	132.8	21,973
EU	2015	8,229,818	122.9	22,198
EU	2016	8,646,986	134.9	22,137
Non-EU	2014	4,649,525	152.8	69,641
Non-EU	2015	4,695,889	160.6	69,318
Non-EU	2016	5,138,308	152.3	70,435

Table 31. Volume and value of trade in goods matched to L-IDBR data, 2014-2016

Partner	Year	Consignments	Value (£bn)	Traders
EU	2014	7,485,659	109.8	20,869
EU	2015	7,818,970	102.3	21,233
EU	2016	8,183,625	111.8	21,166
Non-EU	2014	4,448,339	145.7	66,798
Non-EU	2015	4,496,575	151.6	67,442
Non-EU	2016	4,837,333	143.8	68,711

L-IDBR data

Table 32 reports the total number of enterprises included in L-IDBR for which information on employment and turnover is available. The number of enterprises is broadly in line with the total reported in ONS statistical bulletins, at around 2.5 million

active enterprises in each year.[41] Turnover data is only available for almost all businesses up to 2015. This is due to the longer lags with which turnover data is included in L-IDBR (up to two years for many enterprises) compared to employment data (in most cases, updated with a one-year lag).

Table 32. Economic activity reflected in L-IDBR data, 2014-2016

Year	Number of enterprises non-missing employment	Total employment	Number of enterprises with non-missing turnover	Total turnover (£bn)
2014	2,345,597	28,647,401	2,194,952	4,372
2015	2,542,916	29,412,742	2,368,541	4,602
2016	2,662,196	30,001,379	1,109,432	504

Around 500,000 to 600,000 enterprises per year cannot be matched to a VAT ID. This is due to enterprises that are included in L-IDBR as they run a PAYE scheme but are not required to pay VAT, either because their turnover is below minimum thresholds or because they operate in VAT-exempt sectors. Employment, turnover and number of enterprises matches to VAT IDs is shown in Table 33.

Table 33. Economic activity included in L-IDBR, enterprises matched to VAT IDs only, 2014-2016

Year	Number of enterprises with non-missing employment	Total employment	Number of enterprises with non-missing turnover	Total turnover (£bn)
2014	1,839,275	23,284,056	1,689,889	4,052
2015	1,912,972	23,991,803	1,748,250	4,278
2016	1,985,971	24,587,685	458,161	204

Further detail on data-linking issues

EU traders in our analytical dataset are somewhat larger than in the HMRC data; however, differences between the two datasets on non-EU trade are smaller (and run in the opposite direction). The larger traders not included in our EU export sample are likely large firms that are VAT registered in countries other than the UK. As we are matching to enterprises using VAT references, large multinationals without UK VAT references are likely to be larger on average.

[41] See for example Figure 1 in https://www.ons.gov.uk/businessindustryandtrade/business/activitysizeandlocation/bulletins/ukbusinessactivitysizeandlocation/2019, which reports the number of UK enterprises as: 2,360,355 in 2014; 2,449,416 in 2015 and 2,554,510 in 2016.

For non-EU exports, our sample is largely representative of the underlying HMRC data, except for a divergence in 2017 figures.

Table 34. Comparing EU trade volumes and values in analytical dataset to “raw” HMRC data

Dataset	Year	Mean consignments per trader	Analytical dataset	Analytical dataset
Analytical	2013	338	5,512,877	710,489
Analytical	2014	359	5,259,882	731,198
Analytical	2015	368	4,817,417	680,800
Analytical	2016	387	5,281,012	748,281
Analytical	2017	400	5,880,153	812,961
HMRC	2013	454	6,787,299	709,434
HMRC	2014	403	6,658,874	742,200
HMRC	2015	478	6,004,394	693,338
HMRC	2016	488	6,460,499	760,876
HMRC	2017	500	7,044,790	811,145

Table 35. Comparing non-EU trade volumes and values in analytical dataset to “raw” HMRC data

Dataset	Year	Mean consignments per trader	Analytical dataset	Analytical dataset
Analytical	2013	66	2,715,815	31,900
Analytical	2014	67	2,181,219	30,000
Analytical	2015	67	2,247,950	29,575
Analytical	2016	70	2,092,378	29,178
Analytical	2017	76	2,465,125	35,855
HMRC	2013	64	2,656,698	29,147
HMRC	2014	65	2,172,562	27,728
HMRC	2015	66	2,277,035	27,222
HMRC	2016	70	2,116,149	26,582
HMRC	2017	70	2,351,633	29,084

Firms in our final analytical dataset tend to be substantially larger than the raw data from which they are drawn. This is due to two things: the likelihood of survival (from pre 2014 to post 2014) and the need for VAT registration. The gap is at its widest in 2011, as this is when the effect of the 2014-2016 activity filter is largest.

In the years 2014-2016, the effect of the cleaning process is that analytical dataset firms are roughly one employee larger on average

Table 36. Comparing enterprises in analytical dataset to “raw” L-IDBR data, employment, and turnover per employee

Dataset	Year	Mean employment	Turnover/employee
Analytical	2011	15.2	185,398
Analytical	2012	14.6	169,364
Analytical	2013	13.6	192,276
Analytical	2014	12.7	174,055
Analytical	2015	12.5	178,310
Analytical	2016	12.4	8,311
HMRC	2011	12.5	156,248
HMRC	2012	12.6	144,795
HMRC	2013	12.3	168,430
HMRC	2014	12.2	152,617
HMRC	2015	11.6	156,474
HMRC	2016	11.3	16,809

Firms in our analytical dataset also experience higher turnover per employee than the L-IDBR raw data, again a mixture of survival and the VAT threshold. As with employment, firms in our final analytical dataset have higher turnover than the L-IDBR raw data, with the biggest difference occurring in 2011. This is shown in Table 37.

Table 37. Comparing enterprises in analytical dataset to “raw” L-IDBR data, turnover

Dataset	Year	Mean turnover	Median turnover
Analytical	2011	2,924	138
Analytical	2012	2,550	133
Analytical	2013	2,706	129
Analytical	2014	2,398	127
Analytical	2015	2,447	125
Analytical	2016	446	90
HMRC	2011	2,152	121
HMRC	2012	2,001	120
HMRC	2013	2,257	121
HMRC	2014	1,992	124
HMRC	2015	1,943	124
HMRC	2016	455	100

The substantial difference in median turnover in early years disappears in 2014-2016, suggesting that while the median turnover of a firm that survives multiple years is higher, the difference in mean turnover after that point is driven entirely by exclusion of extremely small firms.

The difference between the DBT support data without matching entrefs and those with matching entrefs is negligible in terms of their number of appearances in the dataset

Allocation of exports for complex businesses

The issue: many enterprises can share the same VAT number

Goods export data are recorded at the level of a "consignment", a record of exporting a single commodity in a given month. Each of these consignments is listed with a "trader ID", corresponding to a "VAT unit or branch of a VAT unit that is exporting goods". Data on DBT support, employment, turnover, industry, location and other characteristics is recorded at the level of "enterprises".

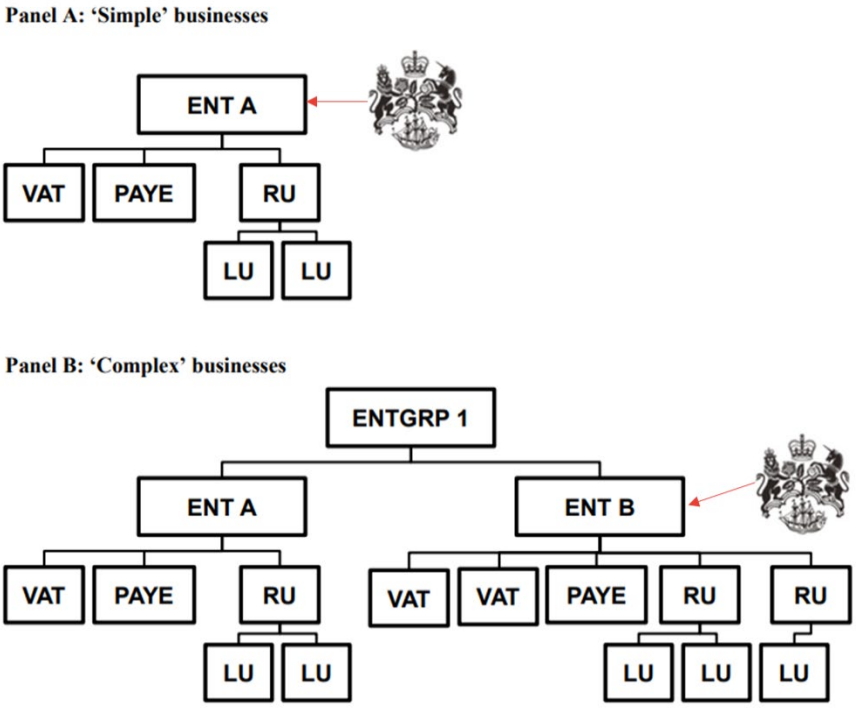
Trader IDs can be matched to the enterprises they belong to through an intermediate step – matching both trader IDs and enterprise IDs (entref) to VAT IDs. Panel A in Figure 6 shows an example of "simple" business in the IDBR, for which one VAT reference matches directly to one enterprise reference, which in turn may be supported by DBT. For over 99% of entrefs, this straightforward structure applies: they are linked to only one VAT ID.

The remaining <1% of enterprises have a more complex structure, such as the one show in panel B of Figure 6. In this example, enterprise B contains several VAT reference numbers, which means that more than one trader ID will match to the enterprise. For our purposes, this is a straightforward problem to solve, as we assign to enterprise B all of the exports that are registered with either VAT number.[42] The more difficult problem is the 0.4-0.5% of enterprises (again depending on year) for which their matched VAT ID is linked to other enterprises. These cases account for very few enterprises but for a larger proportion of trade in goods (over 15%). This reflects even more complex business structures than that found in panel B, where enterprises with a common owner may share a VAT ID but report as separate enterprises under the ONS definition.

Where a VAT ID is linked to several enterprises (for "complex businesses"), we need to allocate the trade recorded against the VAT ID across the enterprises connected with that VAT ID. This is important since different methodologies may dilute or exaggerate the impact of DBT support on business exports. However, the process of allocating exports from VAT IDs to enterprises for complex businesses could be very time-consuming due to the variety of within-group corporate structures and the lack of information available to identify those structures. For example, VAT ID 1 could be linked to entrefs A and B, which are both equally export intensive, while VAT ID 2 could be linked to entrefs C and D, which correspond respectively to the UK-focused and export-focused parts of the business. The data includes very limited information that could allow us to distinguish the case of example ID 1 from that of example ID 2. Because of this complexity, our approach needed to trade off resources devoted to this allocation versus the rest of our analysis.

[42] This occurs for around 0.3-0.9% of enterprises, depending on year.

Figure 6. The structure of simple and complex business in the IDBR



Source: adapted from Figure 2 in Trade and productivity in the UK: New findings. ESCoE Discussion Paper 2018-09.

Our approach

Since we are focused on the impact of export promotion activities on business outcomes, our proposed approach is to use a simple and transparent employment-weighted method of apportioning exports to enterprises where a VAT ID is associated with more than one enterprise. This follows the employment apportionment method used by Wales et al. (2018). There are three key reasons to use this approach:

- Wales et al. (2018) show that this approach provides similar results to more complex methods.
- Among simple methods, apportionment can be based on employment or turnover. However, L-IDBR data does not include information on 2016 turnover for a large majority of businesses.
- Moreover, our conversations with the authors of the Wales et al. (2018) paper suggest that turnover information at enterprise level is generally less reliable than employment information.

For future studies, it would be worth exploring further the advantages and disadvantages of apportioning based on employment or based on turnover. One advantage of turnover-based apportioning is that, for simple businesses (where we can easily compare exports, turnover and employment), turnover and exports are more closely correlated (correlation coefficient of 30-40% depending on year) compared to employment and exports (correlation coefficient of 10-15% depending

on year). However, for this project, turnover-based apportioning would not be feasible for 2016, where turnover is missing from L-IDBR for a large majority of businesses.

For apportionment scenarios when none or all the recipient enterprises have been supported by DBT, the potential for impact on our final estimates is negligible. Where apportionment occurs between enterprises where one enterprise has received DBT support and another has not, we can note these instances and test their influence on our estimates of DBT support impact.

Potential future work to improve the apportionment method

The potential additional steps described below could focus on cases where the employment-based allocation results in very high or very low export/employment ratios. “Very high” (low) could be defined as in the top 5% (bottom 5%) of the export/employment ratio distribution among simple businesses. These steps were tested in Wales et al. (2018) in the context of apportioning exports to reporting units, and they would involve:

- Aggregating to the enterprise group level; or
- Weighting apportionment by product and industry.

Some of the complex cases where the simple allocation rule produces unsatisfactory results may include enterprises that belong to the same enterprise group. Aggregating exports to the group level may remove the allocation issue if all the relevant entrefs belong to the same group. For example, if VAT ID A is associated with entrefs 1 and 2, and entrefs 1 and 2 form group Z, the allocation becomes a 1:1 from VAT ID A to enterprise group Z. This is related to the first stage of the approach used in Wales et al. (2018). Aggregating up to enterprise group level poses some issues from an evaluation perspective. If DBT support generates (potential) outcomes at the enterprise level, measuring exports at the enterprise group level could dilute estimates of impact. In general, we think that the enterprise is the best level of analysis, as it is the more granular level compared to enterprise group. However, in these complex cases where measuring exports at enterprise level is problematic, aggregating comes at a lower cost – because the more granular, enterprise-level information is likely to be imprecise.

A different approach would involve assigning a greater proportion of exports to industries that are more likely to export the relevant good. For example, suppose apples worth £100 are recorded against VAT ID A, which is linked to enterprises 1 and 2. The industry enterprise 1 operates in, according to IDBR, is agriculture, while enterprise 2’s industry is manufacturing. Suppose the data shows that, overall, 100% of apple exports come from agriculture enterprises, and 0% come from manufacturing enterprises. In this case, we would allocate 100% of the apple exports recorded against VAT ID A to enterprise 1. In practice, few cases will be this clear-cut. This is a time-consuming approach that first requires the creation of weighting matrices as in the second-stage approaches used in Wales et al. (2018).

Annex B – Further detail on characteristics of the sample

Table 38. DBT export promotion services – number of services delivered, cut by firm size and interaction in different years (repeat/non-repeat)

Service category	Small	Medium/ large	Size missing	Single years	Multiple years
Events	35,453	32,666	112,216	64,598	115,737
Export Communication Review Scheme (ECR)	770	138	444	190	1,162
Export Market Research Scheme (EMRS)	602	170	488	158	1,102
Gateway to Global Growth	1,516	418	947	529	2,352
Inward Missions	742	1,133	1,735	562	3,048
Market Visit Support (MVS)	1,251	250	985	354	2,132
OBN Chargeable Services	51	31	28	8	102
Open to Export Assist (OtE)	17	10	26	12	41
Outward Missions	2,113	2,310	4,419	1,540	7,302
Passport to Export (including PtE R&D)	3,951	385	4,377	1,103	7,610
Significant Assistance (PIMS) and Significant Assistance Development Aid	52,654	43,704	72,351	26,367	142,342
Tradeshow Access Programme (TAP)	7,872	2,578	6,815	3,051	14,214

Table 39. Regional distribution of supported and unsupported firms, 2014-2016

	Unsupported	Supported
North East	2.6%	2.1%
North West	9.1%	9.3%
Yorkshire and the Humber	6.9%	7.2%
East Midlands	6.7%	7.6%
West Midlands	7.9%	9.6%
East of England	10.2%	10.1%
London	16.3%	21.4%
South East	15.9%	16.7%
South West	9.5%	7.8%
Scotland	7.3%	4.6%
Wales	4.3%	2.2%
Northern Ireland	3.4%	1.3%
Total sample size (number of enterprises)	4,294,331	63,888

Table 40. Characteristics of supported and unsupported firms in estimation sample, 2014-2016

	Unsupported – small	Unsupported – medium/large	Supported – small	Supported – medium/ large
N	3,817,656	91,779	48,096	11,986
Survival	93.9%	98.6%	97.2%	99.5%
Employees	4.0	351.1	11.0	519.7
Turnover (£000s)	779	62,690	2,726	112,877
Turnover per employee	194	179	250	220
Exporting	3.5%	28.2%	36.1%	69.8%
Export value (£000s per exporter)	4,339	13,503	1,566	18,914
Age	16.0	24.7	14.8	28.9
Foreign ownership	0.1%	1.5%	n/a (disclosure)	n/a (disclosure)
Innovate support	0.1%	1.5%	1.8%	3.4%
Filed patents (GB)	0.0%	1.1%	0.8%	5.9%
Filed patents (non-GB)	0.0%	0.6%	0.0%	3.0%
Manufacturing	6.1%	15.9%	18.8%	37.7%
Retail & wholesale	18.7%	14.8%	27.8%	17.8%
Information & communication	8.2%	4.4%	13.0%	7.1%
Professional services & head offices	10.4%	0.0%	13.0%	4.3%
Other services	13.4%	0.0%	12.7%	n/a (disclosure)

Note: these summary statistics relate to firms for which propensity scores have been calculated. Where a covariate is missing, the observation falls out of the estimation sample.

Table 41. Characteristics of large exporters (£250k+ total annual export value), 2015

Employment group	Median EU export value	Median non-EU export value	Average EU export value	Average non-EU export value	Number of enterprises
1-4 employees	£483,828	£37,750	£3,973,532	£2,562,666	74,352
5-9 employees	£449,011	£27,550	£1,879,870	£777,534	76,911
10-49 employees	£578,452	£46,735	£1,852,277	£2,000,905	130,623
50-249 employees	£1,367,970	£174,524	£4,901,354	£3,090,542	28,525
250+ employees	£4,620,888	£233,984	£31,486,502	£29,081,281	7,559

Table 42. Comparison of characteristics of firms treated in one year only and treated in multiple years, small firms in 2014 cohort

Variable	Small T-only	Small multi-T	Medium/ large T-only	Medium/ large multi-T
I1_employment_group_1	48%	36%	0%	0%
I1_employment_group_2	37%	42%	0%	0%
I1_employment_group_3	15%	23%	0%	0%
I1_employment_group_4	0%	0%	44%	41%
I1_employment_group_5	0%	0%	32%	31%
I1_employment_group_6	0%	0%	24%	28%
I1_turnover_group_1	20%	13%	0%	0%
I1_turnover_group_2	18%	12%	0%	0%
I1_turnover_group_3	29%	27%	0%	0%
I1_turnover_group_4	31%	43%	34%	30%
I1_turnover_group_5	3%	5%	62%	70%
Turnover (t-1), £000	1,906	2,936	137,432	116,855
Employees (t-1)	9	12	453	568
Export dummy at t-1	26%	44%	55%	75%
Export value (t-1), £000	1,093	415	10,299	19,009
Firm age	14	15	27	29
age_group_1	21%	19%	0%	2%
age_group_2	24%	24%	7%	5%
age_group_3	31%	29%	20%	16%
age_group_4	24%	27%	69%	78%
Dummy for foreign-owned enterprises	4%	4%	22%	23%
Legal status 1	98%	98%	94%	95%
Legal status 2	0%	0%	0%	0%
Legal status 3	0%	0%	0%	0%
Legal status 4	0%	0%	0%	0%
Legal status 5	1%	2%	4%	4%
Legal status 6	0%	0%	0%	0%
Industry dummy for agriculture & mining	1%	1%	0%	0%
Manufacturing	15%	21%	27%	40%
Utilities	1%	0%	0%	0%
Construction	2%	1%	3%	2%
Retail & wholesale	30%	28%	20%	17%
Transport & storage	2%	2%	3%	3%
Accommodation & food services	1%	0%	3%	0%
Information & communication	14%	12%	8%	7%
Finance	1%	0%	0%	2%
Real estate	1%	0%	0%	0%
Professional services	9%	8%	0%	2%
Head offices and management consultancy	4%	5%	0%	4%
Architecture & engineering	8%	8%	4%	7%

Administrative & support Services	7%	7%	8%	5%
Public administration & defence	0%	0%	0%	0%
Education, health & social care	2%	2%	7%	4%
Arts & entertainment	2%	1%	0%	1%
Other services	3%	3%	0%	1%
Dummy for receipt of support from Innovate UK (last three years from t-1)	1%	2%	0%	4%
Dummy for patents filed in the GB (as of t-1)	0%	1%	0%	8%
Dummy for patents filed outside GB (as of t-1)	0%	0%	0%	4%
Dummy for North East	2%	2%	3%	2%
Dummy for North West	9%	9%	9%	11%
Dummy for Yorkshire and the Humber	6%	7%	8%	9%
Dummy for East Midlands	7%	8%	7%	8%
Dummy for West Midlands	10%	10%	10%	10%
Dummy for East of England	11%	11%	9%	9%
Dummy for London	22%	21%	21%	19%
Dummy for South East	17%	17%	14%	16%
Dummy for South West	9%	8%	8%	7%
Dummy for Scotland	4%	4%	7%	7%
Dummy for Wales	2%	2%	3%	2%
Dummy for Northern Ireland	1%	1%	0%	2%

Annex C – Propensity score matching approach

This section provides further detail on the methodology and diagnostic tests relating to propensity score matching (PSM).

Variables included in model
The first model, M1, has the simplest treatment of size and export value. We measure size with one continuous variable each for employee count and turnover, grouping variables for both. Export intensity is included simply as a dummy variable for whether a firm has exported or not and a continuous variable for total export value. The second model, M2, interacts the continuous employee and turnover variables with each of their grouping variables, so the marginal impact of size varies by size to approximate potential non-linearities. In addition, the measurement of export status and export value is split into EU and non-EU exports, to reflect their different reporting thresholds. Finally, M3 conducts the same exercise with EU and non-EU exports that M2 does with size variables, interacting the continuous measures with grouping variables to approximate non-linear effects.

The summary classification rates are listed in Table 43 for each model in each year. From this we determine that the additional complexity, non-linearity and detail of the M2 and M3 models do not lead to substantial and worthwhile improvements on the more parsimonious M1 model. We therefore proceed in the PSM process using the propensity scores estimated by M1, although we undertook robustness checks using alternative propensity scores to ensure our results are not unique to propensity score model selection.

Table 43. Comparison of propensity score model specifications

	M1	M2	M3
Size	✓	✓	✓
Age	✓	✓	✓
Foreign ownership	✓	✓	✓
Industry	✓	✓	✓
Geography	✓	✓	✓
Legal status	✓	✓	✓
Innovate support	✓	✓	✓
Exporter status	✓	✓	✓
Export destination		✓	✓
Size interactions		✓	✓
Export interactions			✓
Pseudo R2	0.21	0.21	0.21
Mean propensity score of treated	10.5%	11.0%	11.0%
Classification rate of treated	79.7%	79.9%	80.5%
Classification rate of untreated	72.6%	73.8%	71.8%

Note: pseudo R2, mean propensity score of treated, classification rates are simple averages across six models, one for each treatment year (2014, 2015, 2016) and firm group in terms of employment (small and medium/large).

The coefficients for the first-stage propensity score model are shown in Table 44. A positive coefficient shows that a firm with that characteristic is more likely to be treated, other things being equal.

Table 44. Coefficients for first-stage propensity score model, using M1 controls

Variable	Small 2014	Small 2015	Small 2016	Medium/ large 2014	Medium/ large 2015	Medium/ large 2016
Turnover band 1	base	base	base	base	base	base
Turnover band 2	0.01 (0.9)	-0.002 ** (-0.23)	-0.032 ** (-2.6)	0.129 (0.7)	0.099 ** (0.43)	-0.27 ** (-1.2)
Turnover band 3	0.195 *** (17.33)	0.197 *** (17.29)	0.164 *** (12.53)	-0.043 (-0.29)	0.08 ** (0.43)	-0.433 ** (-2.52)
Turnover band 4	0.34 *** (24.63)	0.353 *** (25.65)	0.316 *** (19.96)	0.24 * (1.82)	0.417 ** (2.5)	0.018 ** (0.13)
Turnover band 5	0.4 *** (14.81)	0.448 *** (17.56)	0.354 *** (11.92)	0.425 *** (3.22)	0.588 *** (3.53)	0.137 ** (1)
Employment band 1	base	base	base	omitted	omitted	omitted
Employment band 2	0.12 *** (10.73)	0.105 *** (9.38)	0.119 *** (9.08)	omitted	omitted	omitted
Employment band 3	0.105 *** (3.85)	0.078 *** (2.86)	0.037 ** (1.19)	omitted	omitted	omitted
Employment band 4	omitted	omitted	omitted	base	base	base
Employment band 5	omitted	omitted	omitted	0.041 * (1.68)	0.026 ** (1.1)	-0.029 ** (-1.06)

Employment band 6	omitted	omitted	omitted	0.139 *** (4.74)	0.159 *** (5.57)	0.126 *** (3.98)
Turnover	0 *** (-866.36)	0 ** (-0.62)	0 ** (0.88)	0 ** (2.58)	0 ** (1.27)	0 ** (0.46)
Employees	0.011 *** (12.2)	0.011 *** (12.39)	0.012 *** (11.76)	0 *** (4.15)	0 *** (4.82)	0 *** (4.9)
Export status	0.774 *** (80.85)	0.804 *** (84.22)	0.791 *** (72.96)	0.711 *** (29.14)	0.741 *** (31.38)	0.759 *** (28.71)
Export value	0 (-0.72)	0 ** (-1.76)	0 ** (-1.55)	0 (-0.56)	0 ** (-0.26)	0 ** (0.46)
Age band 1	base	base	base	base	base	base
Age band 2	0.03 *** (2.84)	0.036 *** (3.5)	-0.011 ** (-0.98)	0.082 (1.2)	0.121 ** (1.77)	0.129 ** (1.57)
Age band 3	0.078 *** (5.76)	0.075 *** (5.57)	0.006 ** (0.39)	0.099 (1.56)	0.134 ** (2.11)	0.159 ** (2.09)
Age band 4	0.073 *** (2.85)	0.101 *** (3.97)	0.026 ** (0.9)	0.14 * (1.92)	0.137 ** (1.89)	0.193 ** (2.27)
Age (years)	-0.01 *** (-10.9)	-0.011*** (-12.08)	-0.01 *** (-9.55)	0.006 *** (4.18)	0.006 *** (4.42)	0.006 *** (3.95)
Foreign-owned	-0.134 *** (-6.33)	-0.077*** (-3.77)	-0.135*** (-5.43)	-0.265 *** (-10.22)	-0.252*** (-9.98)	-0.26 *** (-9.22)
Agriculture & mining	base	base	base	base	base	base
Manufacturing	0.497 *** (12.87)	0.512 *** (13.11)	0.474 *** (10.84)	0.266 *** (3)	0.334 *** (3.92)	0.283 *** (2.91)
Utilities	0.32 *** (5.57)	0.308 *** (5.43)	0.055 ** (0.79)	0.341 *** (2.73)	0.316 *** (2.63)	0.104 ** (0.74)
Construction	-0.299 *** (-7.12)	-0.258*** (-6.12)	-0.293*** (-6.17)	-0.193 * (-1.93)	-0.149 ** (-1.55)	-0.177 ** (-1.6)
Retail & wholesale	0.39 *** (10.28)	0.379 *** (9.83)	0.257 *** (5.94)	-0.024 (-0.27)	-0.029 ** (-0.33)	-0.05 ** (-0.5)
Transport & storage	0.03 (0.68)	0.004 ** (0.09)	-0.106 ** (-2.04)	-0.145 (-1.46)	-0.117 ** (-1.22)	-0.142 ** (-1.29)
Accommodation & food services	-0.388 *** (-7.82)	-0.37 *** (-7.41)	-0.457*** (-7.86)	-0.334 *** (-3.28)	-0.455*** (-4.56)	-0.473*** (-4.04)
Information & communication	0.533 *** (13.75)	0.522 *** (13.32)	0.446 *** (10.13)	0.403 *** (4.24)	0.359 *** (3.93)	0.381 *** (3.67)
Finance	0.08 (1.43)	0.132 ** (2.44)	0.088 ** (1.42)	0.209 ** (1.98)	0.114 ** (1.11)	0.158 ** (1.36)
Real estate	-0.163 *** (-3.39)	-0.208*** (-4.16)	-0.329*** (-5.47)	-0.218 * (-1.74)	0 ** (0)	-0.108 ** (-0.78)
Professional services	0.511 *** (13.02)	0.509 *** (12.79)	0.414 *** (9.27)	0.398 *** (3.48)	0.286 ** (2.57)	0.457 *** (3.8)
Head offices and management consultancy	0.406 *** (10.06)	0.424 *** (10.4)	0.353 *** (7.71)	0.648 *** (6.39)	0.686 *** (7.09)	0.764 *** (7.07)
Architecture & engineering	0.46 *** (11.72)	0.471 *** (11.87)	0.395 *** (8.86)	0.283 *** (2.97)	0.302 *** (3.31)	0.271 ** (2.6)
Administrative & support services	0.338 *** (8.56)	0.342 *** (8.54)	0.215 *** (4.76)	-0.088 (-0.93)	-0.118 ** (-1.3)	-0.132 ** (-1.27)
Public administration & defence	-0.202 (-0.47)	base	base	-0.92 *** (-3.09)	-0.718 ** (-2.4)	-0.735 ** (-1.9)
Education, health & social care	0.524 *** (11.97)	0.518 *** (11.75)	0.437 *** (8.83)	0.079 (0.78)	-0.028 ** (-0.29)	0.023 ** (0.2)
Arts & entertainment	0.233 *** (5.2)	0.275 *** (6.11)	0.145 *** (2.8)	-0.156 (-1.43)	-0.12 ** (-1.15)	-0.078 ** (-0.66)
Other services	0.329 *** (7.82)	0.279 *** (6.45)	0.214 *** (4.36)	0.044 (0.38)	-0.012 ** (-0.11)	-0.049 ** (-0.38)
North East	base	base	base	base	base	base

North West	0.053 ** (1.99)	-0.024 ** (-0.95)	0.022 ** (0.77)	0.198 *** (2.94)	0.302 *** (4.49)	0.228 *** (3.13)
Yorkshire	0.086 *** (3.16)	0 ** (0.01)	0.027 ** (0.92)	0.222 *** (3.21)	0.312 *** (4.55)	0.152 ** (2.02)
East Midlands	0.096 *** (3.55)	0.077 *** (2.99)	0.054 ** (1.83)	0.22 *** (3.14)	0.321 *** (4.62)	0.189 ** (2.5)
West Midlands	0.168 *** (6.38)	0.078 *** (3.08)	0.043 ** (1.5)	0.303 *** (4.46)	0.368 *** (5.43)	0.186 ** (2.5)
East of England	0.095 *** (3.63)	0.019 ** (0.75)	-0.009 ** (-0.32)	0.207 *** (3.04)	0.272 *** (4.01)	0.135 ** (1.82)
London	0.179 *** (7.14)	0.084 *** (3.54)	0.078 *** (2.87)	0.367 *** (5.65)	0.398 *** (6.11)	0.309 *** (4.38)
South East	0.088 *** (3.49)	0.007 ** (0.3)	0.016 ** (0.6)	0.262 *** (4)	0.328 *** (5.02)	0.208 *** (2.92)
South West	0.097 *** (3.65)	0.022 ** (0.85)	-0.03 ** (-1.01)	0.246 *** (3.52)	0.281 *** (4.03)	0.153 ** (2)
Scotland	-0.1 *** (-2.63)	-0.113*** (-3.12)	-0.212*** (-4.82)	-0.096 (-1.04)	-0.103 ** (-1.11)	-0.066 ** (-0.67)
Wales	-0.054 * (-1.88)	-0.109*** (-3.97)	-0.134*** (-4.25)	0.184 *** (2.6)	0.231 *** (3.28)	0.074 ** (0.94)
Northern Ireland	-0.048 (-1.48)	-0.072 ** (-2.32)	-0.156*** (-4.21)	-0.043 (-0.51)	0.149 ** (1.85)	-0.058 ** (-0.63)
Legal status 1	base	base	base	base	base	base
Legal status 2	-1.106 *** (-32.15)	-1.156*** (-28.92)	-1.206*** (-20.14)	base	base	base
Legal status 3	-1.055 *** (-26.83)	-1.16 *** (-23.51)	-1.169*** (-16.68)	-0.965 *** (-7.14)	-1.065*** (-6.89)	-0.975*** (-5.29)
Legal status 4	-0.298 (-1.26)	-0.218 ** (-1.02)	-0.064 ** (-0.28)	-0.371 *** (-4.36)	-0.567*** (-5.85)	-0.595*** (-5.03)
Legal status 5	0.019 (0.72)	0.007 ** (0.26)	0.032 ** (0.97)	-0.157 *** (-2.81)	-0.037 ** (-0.69)	-0.005 ** (-0.07)
Legal status 6	1.095 *** (29.25)	1.1 *** (33.41)	0.988 *** (28.96)	0.395 *** (7.58)	0.398 *** (8.01)	0.426 *** (8.35)
Constant	-2.744 *** (-60.29)	-2.696*** (-59.55)	-2.732*** (-53.57)	-2.399 *** (-13.47)	-2.674*** (-13.22)	-2.412*** (-12.67)
N	1,256,866	1,290,078	1,322,636	32,464	35,410	36,033
Psuedo R-squared	0.211	0.213	0.205	0.157	0.172	0.170
Mean propensity score of untreated	0.013	0.013	0.009	0.115	0.111	0.076
Mean propensity score of treated	0.078	0.079	0.058	0.244	0.252	0.187
Treatment rate	0.014	0.014	0.009	0.132	0.130	0.086
Treated classification rate	0.815	0.794	0.798	0.764	0.770	0.784
Untreated classification rate	0.709	0.736	0.737	0.691	0.706	0.702

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

The first-stage model produces propensity scores which show the likelihood of firms to be treated. For the approach to allow for comparators to be drawn, we need the propensity score distributions for treatment and control groups to overlap. This is verified by looking at the distribution of propensity scores.

Figure 7. Propensity score density, small firms, 2014 treatment year

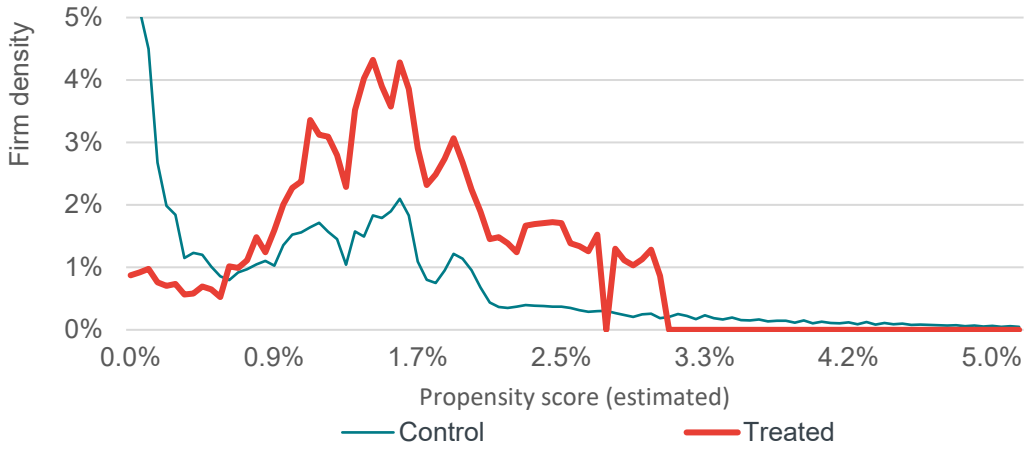
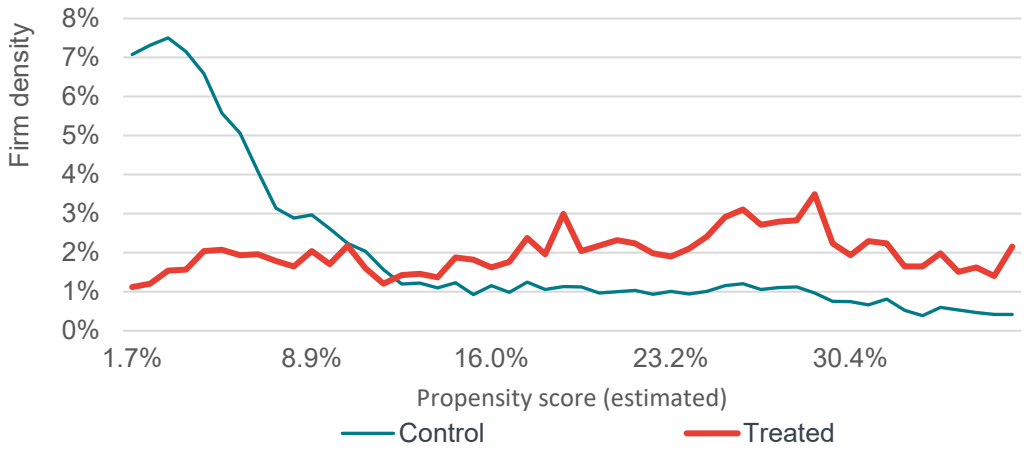


Figure 8. Propensity score density, medium and large firms, 2014 treatment year



Choice of matching algorithm

While various matching algorithms are used in the PSM literature, the simplest and most commonly used is “nearest neighbour” (NN) matching, in which the treated observations are matched to the n closest control observations by propensity score. For example, matching to five NNs would mean that the counterfactual for each treated observation is the average of the untreated five firms closest to it in terms of propensity score.

Other algorithms, such as radius matching or kernel matching, have the advantage of using all sufficiently similar control observations for each treated firm, rather than taking only the very closest few as NN matching does. However, these methods are more computationally intensive, a constraint that can be problematic especially in the

presence of large sample sizes. In addition, it increases the risk of "bad" matches that are further away in propensity score than the NNs. We tried to test radius matching but the computation time required was so much greater than with NN that it would have been infeasible.

The possibility of "bad matches" is less important within the NN framework, as only a few control observations are needed per treated firm. However, it is still possible for a treated firm to be matched to a control with a very different propensity score, especially in sparsely populated parts of the distribution. To manage this, we add a "calliper" within which matching is allowed to occur. For a calliper of 0.01, a treated firm is only able to match to control firms with a propensity score within 0.01 of the treated firm's propensity score. A smaller calliper means a higher quality of matches at the expense of some unmatched firms, as there may be treated observations without nearby comparable controls.

To further manage the risk of inappropriate matches, we choose strata across which no matches are allowed to occur, such as size or region. The PSM process by design matches firms with different characteristics to one another, but we may never want to match certain types of firms. From our understanding of the literature and DBT's processes for offering support, there is no specific characteristic with enough importance to force a match within a stratum except based on size. Regarding size, we know that international evidence shows that smaller firms benefit more from support, so this is something we would seek to investigate. Stratifying our matches (and our propensity score estimation) by size means we have robust, separate estimates of DBT support impact by the size of the firm.

We tested a range of approaches with these parameters to ensure that we are using the most appropriate matching procedure, and that our final impact estimates are robust to our choice of matching process.

In choosing between matching results, we use two metrics that reflect the criteria of success mentioned above:

- The number of treated firms left unmatched; and
- The number of observable differences removed – to do this we conduct balancing tests for all the variables included in our propensity score modelling specifications, and test whether the two means (of the matched treated and control groups) are different at the 5% confidence level.

Our chosen matching algorithm uses one NN matching within a calliper of 0.0001. In the process of selecting our final matching algorithm, we tested a wide range of approaches, but we present a selection of approaches below to indicate our motivation. The first column shows the effect of widening our calliper, while the final column shows the impact of increasing the number of NNs. All results are shown for 2014 only but the comparisons between results are consistent with other years.

Table 45. Performance of matching algorithms, all treatment years

Test	Calliper = 0.001; 1 nearest neighbour	Calliper= 0.0001; 1 nearest neighbour	Calliper = 0.0001; 3 nearest neighbours	Calliper = 0.0001; 5 nearest neighbours	Calliper = 0.0001; 5 nearest neighbours
Proportion of treated matched	99.7%	99.0%	99.0%	99.0%	99.7%
Proportion of "passed" balancing tests	80.4%	82.1%	83.9%	85.7%	83.9%
Proportion of treated matched	98.6%	93.0%	93.0%	93.0%	98.6%
Proportion of "passed" balancing tests	92.7%	92.7%	94.5%	96.4%	96.4%

The yearly results of the core matching algorithm are shown in Table 46.

Table 46. Performance of preferred matching algorithm, by treatment year

Size band	Test	2014 treatment	2015 treatment	2016 treatment
Small	Matching	99.7%	99.8%	99.9%
Small	Balancing	19.6%	14.5%	20.0%
Medium/large	Matching	98.6%	98.7%	98.6%
Medium/large	Balancing	7.3%	7.3%	5.5%

Non-matched treated firms are very slightly larger than the matched firms in terms of employment. For example, considering small firms treated in 2015, the matched treated have 10.76 employees on average compared to 10.81 in the pre-matching group. There is a more substantial difference between matched and non-matched treated firms in terms of export value, particularly among medium and large firms. For example, for firms treated in 2014, the average export value in 2013 among all treated firms is £26.5 million, compared to £16.3 million in the matched treated group.

Success of the matching algorithm

Table 47. Balancing tests from matching, small firms, treatment year 2014

Control	Sample	Treated	Controls	Difference	S.E.	T-stat (abs)
Export dummy at t-1	Pre-match	0.358	0.035	0.323	0.0014	225.02
.	Matched	.	.	-0.004	0.0051	0.75
Export dummy at t-2	Pre-match	0.335	0.033	0.302	0.0014	215.21
.	Matched	0.33	0.295	0.035	0.0049	7.2
l1_employment_group_1	Pre-match	0.41	0.759	-0.349	0.0032	108.32
.	Matched	.	.	0.011	0.0052	2.13

l1_employment_group_2	Pre-match	0.395	0.202	0.193	0.003	63.85
.	Matched	0.396	0.409	-0.013	0.0052	2.43
l1_employment_group_3	Pre-match	0.195	0.04	0.156	0.0015	104.11
.	Matched	.	.	0.002	0.0042	0.38
l1_turnover_group_1	Pre-match	0.16	0.385	-0.224	0.0036	61.57
.	Matched	.	.	0.006	0.0039	1.55
l1_turnover_group_2	Pre-match	0.149	0.286	-0.137	0.0034	40.39
.	Matched	.	.	0.007	0.0038	1.95
l1_turnover_group_3	Pre-match	0.276	0.227	0.049	0.0032	15.56
.	Matched	0.277	0.28	-0.003	0.0048	0.57
l1_turnover_group_4	Pre-match	0.374	0.096	0.278	0.0022	124.26
.	Matched	0.372	0.381	-0.01	0.0051	1.87
l1_turnover_group_5	Pre-match	0.04	0.006	0.034	0.0006	56
.	Matched	.	.	-0.00101	0.0021	0.49
Turnover (t-1), £000	Pre-match	2,476	770	1,706	399.88	4.27
.	Matched	2,447	2,609	-162	190.35	0.85
Employees (t-1)	Pre-match	10.97	3.96	7.01	0.0504	139.11
.	Matched	10.81	10.84	-0.033	0.1232	0.27
Export value (t-1), £000	Pre-match	744.1	222.9	521.2	347	1.5
.	Matched	736.5	936.6	-190.1	494338	0.38
Firm age	Pre-match	14.76	15.96	-1.19	0.0879	13.57
.	Matched	14.75	14.81	-0.0601	0.1118	0.54
age_group_1	Pre-match	0.203	0.201	0.002	0.003	0.61
.	Matched	.	.	0	0.0043	0.04
age_group_2	Pre-match	0.242	0.238	0.004	0.0032	1.19
.	Matched	.	.	0.002	0.0045	0.5
age_group_3	Pre-match	0.301	0.263	0.038	0.0033	11.47
.	Matched	0.3	0.3	0	0.0049	0.06
age_group_4	Pre-match	0.254	0.298	-0.044	0.0034	12.71
.	Matched	0.254	0.256	-0.002	0.0046	0.46
Dummy for foreign-owned enterprises	Pre-match	0.037	0.009	0.028	0.0007	38.12
.	Matched	.	.	-0.004	0.0021	2.15
Legal status 1	Pre-match	0.977	0.622	0.355	0.0036	97.99
.	Matched	.	.	0.002	0.0016	1.01
Legal status 2	Pre-match	0.004	0.218	-0.214	0.0031	69.43
.	Matched	.	.	0	0.0007	0.59
Legal status 3	Pre-match	0.003	0.14	-0.137	0.0026	52.79
.	Matched	.	.	-0.001	0.0006	1.3
Legal status 4	Pre-match	.	.	.	.	.
.	Matched	.	.	.	.	.
Legal status 5	Pre-match	0.016	0.02	-0.004	0.001	3.64
.	Matched	.	.	0	0.0013	0.3
Legal status 6	Pre-match	.	.	.	.	.
.	Matched	.	.	.	.	.
Dummies for industry codes	.	.	.	.	.	.
Agriculture & mining	Pre-match	0.006	0.092	-0.087	0.0022	40.11
.	Matched	0.006	0.006	-0.001	0.0008	1.03
Manufacturing	Pre-match	0.182	0.062	0.12	0.0018	65.74
.	Matched	0.18	0.182	-0.002	0.0041	0.44
Utilities	Pre-match	0.006	0.004	0.002	0.0005	4.51
.	Matched	0.006	0.007	-0.001	0.0008	0.87
Construction	Pre-match	0.018	0.129	-0.11	0.0025	44.16
.	Matched	0.018	0.017	0.001	0.0014	1.01
Retail & wholesale	Pre-match	0.284	0.19	0.094	0.003	31.8

.	Matched	0.285	0.32	-0.035	0.0049	7.21
Transport & storage	Pre-match	0.016	0.033	-0.017	0.0013	12.52
.	Matched	0.017	0.015	0.002	0.0013	1.5
Accommodation & food services	Pre-match	0.006	0.054	-0.048	0.0017	28.46
.	Matched	0.006	0.007	-0.001	0.0008	0.87
Information & communication	Pre-match	0.13	0.079	0.051	0.002	25.23
.	Matched	.	.	0.022	0.0034	6.55
Finance	Pre-match	0.005	0.007	-0.002	0.0006	3.52
.	Matched	0.005	0.004	0.001	0.0007	1.65
Real estate	Pre-match	0.007	0.041	-0.034	0.0015	23.03
.	Matched	0.007	0.007	0	0.0009	0.38
Professional services	Pre-match	0.081	0.061	0.02	0.0018	11.01
.	Matched	.	.	0.004	0.0029	1.53
Head offices and management consultancy	Pre-match	0.048	0.039	0.009	0.0015	5.83
.	Matched	.	.	0.008	0.0021	3.93
Architecture & engineering	Pre-match	0.08	0.073	0.007	0.002	3.51
.	Matched	.	.	0.001	0.0029	0.49
Administrative & support services	Pre-match	0.067	0.062	0.005	0.0018	2.69
.	Matched	.	.	-0.002	0.0027	0.57
Public administration & defence	Pre-match	.	.	.	.	.
.	Matched	.	.	.	.	.
education, health & social care	Pre-match	0.022	0.014	0.008	0.0009	8.6
.	Matched	.	.	0.001	0.0015	0.7
Arts & entertainment	Pre-match	0.015	0.028	-0.013	0.0012	10.47
.	Matched	0.015	0.017	-0.002	0.0013	1.28
Other services	Pre-match	0.028	0.032	-0.004	0.0013	2.87
.	Matched	0.028	0.028	0	0.0018	0.1
Dummy for receipt of support from Innovate UK (last three years from t-1)	Pre-match	0.022	0.001	0.021	0.0002	86.03
.	Matched	0.013	0.01	0.004	0.0011	3.26
Dummy for patents filed in the GB (as of t-1)	Pre-match	0.012	0	0.011	0.0002	63.43
.	Matched	.	.	0.007	0.0009	8.2
Dummy for patents filed outside GB (as of t-1)	Pre-match	.	.	.	.	.
.	Matched	.	.	.	.	.
Dummy for North East	Pre-match	0.018	0.025	-0.007	0.0012	5.79
.	Matched	.	.	0	0.0014	0.16
Dummy for North West	Pre-match	0.088	0.091	-0.002	0.0022	1.12
.	Matched	.	.	0.001	0.003	0.26
Dummy for Yorkshire and the Humber	Pre-match	0.068	0.07	-0.001	0.0019	0.58
.	Matched	.	.	-0.001	0.0027	0.54
Dummy for East Midlands	Pre-match	0.072	0.068	0.004	0.0019	2.26
.	Matched	.	.	0.002	0.0027	0.6
Dummy for West Midlands	Pre-match	0.099	0.08	0.019	0.002	9.56
.	Matched	.	.	-0.001	0.0032	0.27
Dummy for East of England	Pre-match	0.107	0.103	0.004	0.0023	1.79
.	Matched	.	.	0	0.0033	0.1
Dummy for London	Pre-match	0.217	0.152	0.064	0.0027	23.68

.	Matched	.	.	-0.008	0.0044	1.74
Dummy for South East	Pre-match	0.172	0.159	0.013	0.0027	4.74
.	Matched	0.171	0.165	0.007	0.004	1.7
Dummy for South West	Pre-match	0.083	0.098	-0.015	0.0022	6.75
.	Matched	.	.	0.001	0.0029	0.29
Dummy for Scotland	Pre-match	0.042	0.074	-0.032	0.002	16.26
.	Matched	.	.	0.005	0.0021	2.26
Dummy for Wales	Pre-match	0.021	0.045	-0.023	0.0015	15.04
.	Matched	.	.	-0.002	0.0016	1.18
Dummy for Northern Ireland	Pre-match	0.012	0.036	-0.025	0.0014	17.58

Note: pre-match sample size: 17,966 treated and 1,237,901 untreated firms. Matched sample size: 17,779 treated and 16,700 untreated firms. "." Figures in the data indicate suppressions for statistical disclosure control

Testing the econometric specification

Table 48. Robustness of estimates to matching algorithm, 2014

Firm size	Matching algorithm	Treatment effect
Small	N(1), calliper 0.001	7.7%
Small	N(1), calliper 0.0001	7.8%
Small	N(3), calliper 0.0001	7.8%
Small	N(5), calliper 0.0001	7.8%
Medium/ large	N(1), calliper 0.001	5.3%
Medium/large	N(1), calliper 0.0001	6.5%
Medium/large	N(3), calliper 0.0001	5.7%
Medium/large	N(5), calliper 0.0001	5.9%

Annex D – Further econometric robustness tests

Results one year after treatment

Alternative specification of estimation method
Table 49 reports the results from regressions on the trimmed sample to our main results.[43] Estimates on the small firm sample are very similar to our main results reported in Table 16 and Table 17: we estimate a positive and statistically significant effect of DBT support on survival, export status and employment, while the effect on turnover and export value is not statistically significant. The magnitude of the survival, export status and employment effects is also in line with our main results and, like in our main results, this magnitude is consistent across treatment years. In 2014, our OLS estimates of the impact of DBT support on survival, export status and employment are 3.9 percentage points, 6 percentage points and 0.6 employees respectively, compared to 1.8 percentage points, 7.8 percentage points and 0.8 employees in our main results.

Table 49. Estimates of impact from regressions on trimmed sample

Size band	Outcome	2014 treatment	2015 treatment
Small	Survival	3.9%***	3.0%***
Small	Export status	6.0%***	6.1%***
Small	Turnover (£000s)	183.4	626.5
Small	Employment	0.6***	0.7***
Small	Export value (£000s)	130.1	1.7
Medium/large	Survival	1.1%***	0.8%**
Medium/large	Export status	22.1%***	23%***
Medium/large	Turnover (£000s)	15,745***	-5,060
Medium/large	Employment	22.9*	27.9***
Medium/large	Export value (£000s)	795.3	250.6

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

Removing apportioned export data
As described in earlier sections of this report, matching HMRC data on exports to information from L-IDBR and other datasets involved linking VAT ID-level information to the enterprise reference level. In most cases, this link is straightforward, but in other cases one VAT ID corresponds to several enterprise reference numbers. Where this happens, one export figure (recorded in HMRC data at VAT ID level) must be apportioned across several enterprises. This introduces possible error in the measurement of export information, which might influence our estimates of the

[43] "Trimming" refers to removing observations whose propensity score is in the upper or lower quartile, thus focusing the OLS comparison on firms that have a medium propensity for treatment. This avoids a common difficulty with standard DiD regression analysis, where the treatment and control groups can have very different treatment propensities, and thus have important differences in terms of observable characteristics.

impact of support on exports. Table 50 shows that excluding apportioned exports has a small effect on our estimates. Excluding apportioned exports, we estimate that DBT support in 2014, 2015 and 2016 leads to an increase in the probability of small firms exporting of 7.6, 7.5 and 7.3 percentage points respectively, compared to 7.8, 7.5 and 7.4 in the full sample (as reported in Table 11). The results are close to those for the full sample for medium and large firms as well. As reported in Table 12, estimated effects of treatment on export status for medium and large firms treated in 2014, 2015 and 2016 are 5.4, 6.5 and 5.6 percentage points respectively.

Table 50. Impact of treatment on export outcomes one year after treatment, removing apportioned exports, 2014-2016

Size band	Outcome	2014 treatment	2015 treatment	2016 treatment
Small	Export status	7.6%***	7.5%***	7.3%***
Small	Export value (£000s)	366.6	-3.1	37.5
Medium/large	Export status	6.5%***	6.2%***	5.8%***
Medium/large	Export value (£000s)	1,780.6	461.5	783.5

Note: *, **, *** indicate statistical signi**ficance at the 90%, 95%, 99% level respectively.**

Table 51. Impact of treatment on turnover and employment, removing apportioned exports

Size band	Outcome	2014 treatment	2015 treatment
Small	Employment	0.75**	0.28
Small	Turnover (£000s)	-87.3	N/A
Medium/large	Employment	8.39	28.85
Medium/large	Turnover (£000s)	4,784.1	N/A

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

Removing the largest firms

Removing the top 5% of the firm size distribution in terms of employment (measured the year before DBT support was received) reduces the estimated impact of support on employment. The estimated impact is not statistically significant in 2014 or 2015, as reported in Table 52, compared to an estimated impact of 20.5 (significant at 90% level) and 50.7 (significant at 95% level) employees.

Table 52. Impact of treatment on medium and large firms one year after treatment, removing 5% largest firms

Outcome	2014 treatment	2015 treatment
Employees	3.16	14.60
Turnover (£000s)	7,944.0	-14.1
Export value (£000s)	1,734.5	-1,026.7
Export status	6.4%***	7.2%***

Note: *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

Results two and three years after treatment

OLS regression results
The OLS regression results for small firms up to three years after treatment are shown in Table 53. These are very similar to the results obtained by PSM.

Table 53. Average treatment effects by treatment duration and time elapsed since treatment, estimated using OLS on trimmed sample – small firms

Duration	Outcome	T+1	T+2	T+3
T-only	Survival	1.1%*** (3.17)	1.6%*** (3.58)	1.5%*** (2.81)
Multi-T	Survival	2.7%*** (16.77)	4.9%*** (19.77)	7.3%*** (24.38)
T-only	Employment	0.6*** (2.58)	0.6** (2.11)	0.8** (2.25)
Multi-T	Employment	0.7*** (3.07)	1.2*** (3.77)	1.4*** (6.39)
T-only	Turnover (£000s)	-102 (-0.39)	-132 (-0.54)	-975 (-1.08)
Multi-T	Turnover (£000s)	31 (0.23)	69 (0.5)	-742 (-1.23)
T-only	Export status	4.7%*** (9.98)	12.3%*** (24.82)	4.1%*** (9.48)
Multi-T	Export status	11.2%*** (22.64)	4.8%*** (10.51)	14.2%*** (26.89)
T-only	Export value (£000s)	-71 (-1.19)	-153** (-2.06)	-176** (-2.11)
Multi-T	Export value (£000s)	-91 (-1.47)	-108 (-1.59)	-252*** (-2.63)
T-only	Log export value	0.05 (0.45)	0.16*** (4.22)	0.04 (0.26)
Multi-T	Log export value	0.12*** (3.29)	0.11 (0.81)	0.21*** (5)
T-only	Log turnover	0.035*** (2.89)	0.043*** (3.24)	0.045*** (3.14)
Multi-T	Log turnover	0.067*** (7.46)	0.103*** (9.91)	0.157*** (12.73)
T-only	Log employment	0.052*** (6.21)	0.038*** (3.99)	0.042*** (4.24)
Multi-T	Log employment	0.065*** (9.57)	0.086 *** (10.98)	0.128*** (13.86)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

The results for medium and large firms are shown in Table 54. Again, these show considerably stronger results for those with treatment in multiple years. The results are broadly similar to those estimated using PSM.

Table 54. Average treatment effects by treatment duration and time elapsed since treatment, estimated using OLS on trimmed sample – medium and large firms

Duration	Outcome	T+1	T+2	T+3
T-only	Survival	-0.1% (-0.18)	0.1% (0.12)	0.1% (0.12)
Multi-T	Survival	0.9%*** (4.67)	1.7%*** (5.52)	2.7%*** (7.18)
T-only	Employment	-2.1 (-0.18)	0.1 (0)	1.3 (0.07)
Multi-T	Employment	27.5* (1.76)	35.8* (1.62)	10.3 (0.67)
T-only	Turnover (£000s)	28,221 (1)	39,364 (0.98)	81,391 (1)
Multi-T	Turnover (£000s)	2,443 (0.28)	-5,073 (-0.4)	4,825 (0.36)
T-only	Export status	4.4%*** (3.15)	8.4%*** (10.76)	2.3% (1.6)
Multi-T	Export status	7.4%*** (9.77)	4.4%*** (3.18)	8.9%*** (10.74)
T-only	Export value (£000s)	-87 (-0.54)	3,429 (1.31)	-2,233*** (-2.77)
Multi-T	Export value (£000s)	2,657 (1.29)	-489 (-0.5)	4,687 (1.44)
T-only	Log export value	-0.07 (-0.98)	0.03 (0.86)	-0.05 (-0.54)
Multi-T	Log export value	0.04 (1.19)	-0.04 (-0.57)	0.03 (0.79)
T-only	Log turnover	0.007 (0.35)	-0.006 (-0.19)	-0.051 (-1.35)
Multi-T	Log turnover	0.021* (1.65)	0.052*** (3.27)	0.062*** (3.22)
T-only	Log employment	0.018 (1.18)	0.027 (1.48)	-0.022 (-0.71)
Multi-T	Log employment	0.029*** (3.09)	0.041*** (3.44)	0.075*** (5.36)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

We also show results for the full sample rather than the trimmed sample. These are in Table 55 and Table 56.

Table 55. Average treatment effects by treatment duration and time elapsed since treatment, estimated using OLS on full sample – small firms

Duration	Outcome	T+1	T+2	T+3
T-only	Survival	3.3%*** (26.13)	5.7%*** (29.11)	8.4%*** (35.24)
Multi-T	Survival	1.2%*** (4.82)	1.5%*** (4.67)	1.4%*** (3.75)
T-only	Employment	1*** (5.69)	1.5*** (6.76)	1.9*** (7.87)
Multi-T	Employment	0.6*** (3.31)	0.6*** (3.08)	0.5*** (2.77)
T-only	Turnover (£000s)	45 (0.36)	-514 (-0.74)	-307 (-0.65)
Multi-T	Turnover (£000s)	-91 (-0.52)	-441 (-1.02)	-387 (-1.05)
T-only	Export status	11.3%*** (34.09)	12.5%*** (37.38)	14.2%*** (39.83)
Multi-T	Export status	5.1%*** (14.66)	5.1%*** (14.69)	4.1%*** (12.45)
T-only	Export value (£000s)	-118** (-2.16)	-168*** (-2.77)	-187** (-2.5)
Multi-T	Export value (£000s)	-110** (-2.43)	-139*** (-2.64)	-165*** (-2.66)
T-only	Log export value	0.11*** (4.57)	0.13*** (5.22)	0.16*** (5.62)
Multi-T	Log export value	0.06 (1.57)	0.03 (0.76)	0.06 (1.41)
T-only	Log employment	0.07*** (15.78)	0.1*** (17.79)	0.13*** (20.4)
Multi-T	Log employment	0.05*** (8.43)	0.04*** (6.9)	0.03*** (3.99)
T-only	Log turnover	0.06*** (8.6)	0.09*** (11.75)	0.14*** (14.41)
Multi-T	Log turnover	0.04*** (5.05)	0.05*** (5.2)	0.04*** (3.58)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

Table 56. Average treatment effects by treatment duration and time elapsed since treatment, estimated using OLS on full sample – medium and large firms

Duration	Outcome	T+1	T+2	T+3
T-only	Survival	1%*** (7.82)	2%*** (9.92)	3.1%*** (12.26)
Multi-T	Survival	0% (0.1)	0.1% (0.2)	0.2% (0.32)
T-only	Employment	37.3** (2.39)	57.6*** (3.01)	49.6*** (2.7)
Multi-T	Employment	5.5 (0.44)	30.2 (1.51)	14 (0.77)
T-only	Turnover (£000s)	23,940 (1.19)	37,032 (1.46)	52,471 (1.29)
Multi-T	Turnover (£000s)	8,221 (0.93)	10,021 (0.79)	-6,193 (-0.24)
T-only	Export status	8.4%*** (13.99)	9.5%*** (15.74)	10.3%*** (16.17)
Multi-T	Export status	4.4%*** (4.7)	3.7%*** (4.03)	2.6%*** (2.65)
T-only	Export value (£000s)	1,821 (1.04)	3,812* (1.69)	6,015** (2.05)
Multi-T	Export value (£000s)	-804 (-0.79)	-1,135 (-0.84)	-2,402 (-1.17)
T-only	Log export value	0.05 (1.57)	0.05* (1.65)	0.04 (1.09)
Multi-T	Log export value	-0.04 (-0.73)	-0.01 (-0.16)	-0.01 (-0.09)
T-only	Log employment	0.03*** (4.06)	0.04*** (4.02)	0.06*** (5.17)
Multi-T	Log employment	0.03** (2.41)	0.03** (2.39)	0.01 (0.59)
T-only	Log turnover	0.03** (2.44)	0.06*** (4.77)	0.07*** (4.52)
Multi-T	Log turnover	0.01 (0.73)	0 (0.14)	-0.02 (-0.6)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

Regional results – OLS with trimmed sample

As with the regional PSM results, the regional OLS results show consistent positive effects on exporter status, but little obvious (or reliable) effects on export value. In terms of other outcomes, we can see positive effects on employment for small firms, statistically insignificant impacts for medium/large firms and little obvious impact on

turnover. Meanwhile, log employment and turnover generally show positive impacts of a similar magnitude to those estimated using PSM.

Table 57. Regional breakdown of export-related outcomes one year after treatment, estimated using OLS regression on trimmed sample

Size	Outcome	South	London	Midlands	North	Scotland/ Wales/NI
Small	Exporter status	9.3%*** (25.45)	7.9%*** (10.34)	9%*** (17.45)	4.2%*** (11.25)	9.3%*** (17.15)
Small	Export value (£000s)	-49 (-1.4)	29 (0.68)	14 (0.27)	-203** (-2.2)	-43* (-1.76)
Small	Log export value	0.131*** (4.22)	-0.005 (-0.06)	0.101** (2.27)	0.026 (0.27)	0.057 (1.38)
Medium/ large	Exporter status	6.6%*** (8.92)	5.1%*** (3.83)	7.6%*** (9.18)	5.7%*** (4.83)	6.5%*** (7.2)
Medium/ large	Export value (£000s)	202 (0.18)	649 (0.8)	732 (1.16)	1,480 (0.73)	-37 (-0.08)
Medium/ large	Log export value	0.008 (0.22)	-0.02 (-0.29)	-0.008 (-0.2)	-0.054 (-0.79)	-0.016 (-0.39)
All firms	Exporter status	9.2%*** (24.96)	7.8%*** (10.14)	9%*** (17.21)	4.3%*** (11.06)	9.2%*** (16.85)
All firms	Export value (£000s)	-41 (-1.35)	48 (0.68)	36 (0.3)	-151** (-2.11)	-43* (-1.71)
All firms	Log export value	0.103*** (3.31)	-0.008 (-0.11)	0.076* (1.71)	0.008 (0.03)	0.041 (0.98)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

Table 58. Regional breakdown or non-export-related outcomes one year after treatment, estimated using OLS regression on trimmed sample

Size	Outcome	South	London	Midlands	North	Scotland/ Wales/NI
Small	Employment	0.5*** (4.94)	1.1 *** (2.7)	1.2*** (3.3)	0.6*** (4.57)	0.7* (1.69)
Small	Turnover (£000s)	0.1 (1.34)	0.4 (0.86)	0.1 (1.6)	-0.7 (-0.62)	-0.1 (-1.1)
Small	Log employment	0.059*** (11.89)	0.057*** (4.28)	0.068*** (9.3)	0.071*** (9.58)	0.054*** (8.2)
Small	Log turnover	0.054*** (7.63)	0.051*** (2.81)	0.065*** (6.83)	0.063*** (5.5)	0.039*** (4.55)
Medium/ large	Employment	11.7 (1.32)	9.3 (1.26)	27.9 (1.5)	18.4 (1.61)	37.2 (1.13)
Medium/ large	Turnover (£000s)	2.2 (0.6)	4.1 (0.44)	11.3*** (2.63)	73.5 (0.76)	-3.6 (-1.13)
Medium/ large	Log employment	0.034*** (3.83)	0.015 (0.72)	0.022** (2.18)	0.032** (2.4)	0.027** (2.21)
Medium/ large	Log turnover	0.037*** (2.96)	0.011 (0.56)	0.032** (2.53)	0.016 (0.78)	0.015 (1.03)

Note: T-statistics in parentheses. *, **, *** indicate statistical significance at the 90%, 95%, 99% level respectively.

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