# Does subsidising business advice improve firm performance? Evidence from a large RCT

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**Abstract**
We evaluate the impact of the UK's Growth Vouchers Programme, which offered subsidised business advice to 15,207 randomly selected small and medium size enterprises. Using administrative and survey data, we show that the programme increased turnover by 8.2% but only in the short-term and potentially at the expense of non-supported firms. We find that subsidised advice appears to improve firms' capabilities and practices in a way that is consistent with the increase in turnover. We also demonstrate that propensity score matching introduces a sizeable upward bias to estimated effects on turnover and employment and that this bias grows over time.

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# 1. Introduction

Productivity in the UK has been stagnant since 2008 and is often characterised by a distribution with a 'long tail' of low-productivity firms (De Loecker, Obermeier & Van Reenen, 2022). As a result, greater attention has been put on improving firms' performance, reflected in high demand for consulting and business support programmes.[1] Public support for business advice can be justified if advice improves firm performance and if market failures, including information asymmetries or externalities, cause firms to under-invest in advice. The UK's Growth Vouchers Programme (GVP) was a large-scale randomised controlled trial set-up to assess the effects on firm performance from tackling three main barriers that firms face when accessing advice: the perception that strategic advice is not needed, uncertainty about where to find good advice and the cost of purchasing advice (BIS, 2014).

To do this, GVP offered diagnostic support and subsided the cost of accessing advice from a directory of advisors, for small businesses in the UK between January 2014 and March 2015. Firms were eligible if they had been trading for at least one year, had less than 50 employees and had not paid for business advice in the previous three years. The programme offered online or personal diagnostic support to all applicants and subsidised the costs of purchasing advice for a group of randomly selected firms. Treated firms were offered a voucher, that would cover half of the cost of advice purchased, up to £2,000. Of 28,158 firms registering online, 20,283 firms completed the application and 75% of these firms were randomised to treatment, 25% to a control group.

The large sample size and experimental set up allows us to estimate the causal effect of receiving and using a voucher and the protocol envisaged exploring heterogeneity by type of intervention.[2] We assess impacts using administrative data from the Inter-Departmental Business Register (IDBR), the most comprehensive source of data on firms in the UK, covering around 2.7 million firms.[3] We complement this with information from baseline and endline survey data collected 12 and 24 months after firms received diagnostic support.

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[1] Consulting services are expected to grow around 13% in 2022 according to a survey from the Management Consultancies Association (https://www.mca.org.uk/press-releases/growth-for-uk-consulting). Around 40% of SMEs in the UK used some type of formal business assistance between 2008 and 2011, and nearly half of them relied on a publicly funded programme (North et al., 2011).

[2] https://www.gov.uk/government/publications/growth-vouchers-programme-trial-protocol.

[3] The IDBR data available to us corresponds to the first quarter of every year between 2012 and 2019.

The IDBR allows us to consider the impact of the programme on turnover and employment. Firms that used their voucher saw an 8.2% increase in turnover after 12-months, but this effect did not persist at 24-months. The programme had no effect on employment. We find similar results using survey data on firm performance collected by GVP. The survey also provides evidence on capabilities and practices adopted, and we argue that these are consistent with the finding of a positive effect on turnover, mainly due to the development of new products, rather than improved access to the UK or other markets. One concern is the extent to which the effect on turnover represents displacement since between 69% and 76% of firms that used the voucher are likely to serve local markets.[4]

In line with the trial protocol, we explore heterogeneity by type of diagnostic received and theme of advice selected. Caution is needed here given the diagnostic was a small component of the programme and the theme was selected by the firm. We find no significant differences in effects on turnover and employment between types of diagnostic. For theme of advice, the only significant results is among firms that received business advice on sales & marketing, whose turnover increased by 18.8% and 19.8% after 12 and 24 months, respectively. Results are inconclusive when undertaking heterogeneity analysis by firm size and age.

We contribute to the literature using randomised controlled trials (RCTs) to assess the impact of business support. Much of the existing evidence involves small trials in developing countries focused on the impact of providing training and consulting. Our research fills an important gap by looking at subsidising business advice in a developed country in a large trial (15,207 randomly selected firms). Overall, the literature suggests there is heterogeneity in the effectiveness of training and consulting projects. Moreover, even though some studies find positive effects on practices adopted, managerial capabilities and other intermediate outcomes, such as increased business knowledge, the impact on profit or revenue often turns out to be statistically insignificant (Mano et al., 2012; Valdivia, 2015; Bruhn & Zia, 2013; Karlan & Valdivia, 2011; Karlan, Knight and Udry, 2015; Bardasi et al., 2021). McKenzie and Woodruff (2014) provided a review and

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[4] We assign firms to tradable and non-tradable sectors based on their SIC 4 digit code (see appendix section A.5). From the group of firms that received and used the voucher, 46.4% are in medium-tradable, 23.0% in non-tradable services, and 6.6% in other sectors, which would be considered as non-tradable by some definitions.

highlight several key issues including small sample sizes with a group of heterogeneous firms and a focus on short time horizons (usually outcomes within a year).

The number of experiments in developed countries has so far been limited. Benus et al., (1994) evaluate two RCTs in the United States offering entrepreneurial training, business support and financial assistance. They find that these programmes increased the likelihood of establishing a new business after nearly 2 years. Also in the United States, project Growing America through Entrepreneurship (GATE) was implemented as a randomised controlled trial providing an assessment of business needs, classroom training, counselling, and assistance in applying for financing to 4,197 individuals. GATE's official project evaluation from Benus et al. (2010) finds that participants were more likely to start a business, although businesses started by the treated group did not experience higher growth and profitability. Fairlie, Karlan and Zinman (2015) further analyse project GATE and find no effect on business sales, earnings, employment, or firm survival in the long-term. Contrary to this, Georgiadis and Pitelis (2016) report positive effects on firm performance of randomly assigned training for employees and managers in a sample of 1,325 small and medium-sized enterprises in the UK. A small number of RCTs in developed countries look at training for start-ups. See, for example Nagel, Huber, Van Praag & Goslinga (2019) using mandatory tax training in the Netherlands provided to a randomly selected group of first-time entrepreneurs or Camuffo et al. (2020) testing the effect of teaching a scientific approach to business performance on 116 Italian start-ups.

We also contribute to the literature by comparing our estimates from the randomised controlled trial to those obtained from propensity score matching (PSM) and estimate the magnitude of the selection bias in this context. To the best of our knowledge, this is the first study that provides additional evidence on the magnitude of the bias introduced by PSM in the context of firm-level interventions. LaLonde (1986) was one of the first to undertake a similar exercise for National Supported Work (NSW), a labour training programme. Dehejia and Wahba (1999, 2002) build on LaLonde (1986) using the kind of PSM techniques we use below. See also Smith and Todd (2001, 2005). Heckman et al. (1996, and 1998) and Heckman et al. (1997) do something similar for another training programme (the US National Job Training Partnership Act). Michalopoulos, Bloom and Hill (2004) compare experimental estimates with PSM results from welfare-to-work programmes in the United States. Such exercises are increasingly common in the education literature. For example, Agodini and Dynarski (2004) compare PSM and

experimental estimates for the School Dropout Demonstration Assistance programme in the United States; Wilde and Hollister (2007) compare experimental results from the Tennessee's Student Teacher Achievement Ratio Project (STAR Project) with PSM estimates. Diaz and Handa (2006) conclude that PSM results are only similar to experimental estimates for some outcomes. Weidmann and Miratrix (2021) examine 14 studies for a set of small school-level interventions across primary schools in England. They find no evidence of substantial selection bias due to unobserved characteristics, in part due to a large overlap between treatment and control groups in terms of observed covariates.

Our research provides additional evidence on the magnitude of the bias introduced by PSM in the context of firm-level interventions. Even when using comprehensive administrative data and achieving good matching properties, our PSM results suggest positive and persistent impacts of the programme on both turnover and employment in every period, contrary to the results from exploiting the RCT. However, if we exclude the selection effect and isolate the main aspect of the programme, the subsidised advice component, the results indicate small and temporary effects, consistent with our experimental estimates. This suggests that the bias introduced from unobservables is not only large, but increases over time. Overall, these results highlight the importance of evaluating the impact of business support using a robust methodology, ideally a randomised controlled trial.

The remainder of this paper is structured as follows: In the next section, we describe the Growth Vouchers Programme. Section 3 presents the experimental set up and identification strategy, while Section 4 discusses data sources. Section 5 contains our results, examining the impact of the programme on firm performance, exploring channels through which firms improved their outcomes, and looking at heterogeneous treatment effects. Section 6 compares our experimental results to those obtained from propensity score matching approach and provides evidence on the bias introduced when not considering unobservable characteristics. Finally, Section 7 concludes.

## 2. The Growth Vouchers Programme

The UK's Growth Vouchers Programme (GVP) was designed and implemented by the Department for Business, Innovation and Skills (BIS) and the Behavioural Insights Team

(BIT) in 2014 and 2015.[5] GVP was set up as a randomised controlled trial and aimed to encourage firms to take-up business advice and to help them grow.[6] Firms were eligible if they had been trading for at least one year, had less than 50 employees and had not paid for advice in the previous three years.[7] BIS and its delivery partners promoted the programme using a range of marketing activities, including email campaigns, telemarketing and social media.[8]

The programme offered online or personal diagnostic support to all applicants and subsidised the costs of purchasing advice for a group of randomly selected firms. Randomisation occurred at two points and determined whether the firm was offered an online or personal diagnostic and whether the firm received a voucher to subsidise advice. The next section describes this randomisation in detail. The remainder of this section describes each step of the programme.

Both diagnostics aimed to understand the needs and preferences of firms for receiving advice in one of five areas, rather than providing a comprehensive diagnostic and imposing a recommended theme of advice.[9] GVP was structured around five areas or 'themes' – sales & marketing, raising finance, leadership & management, workforce development and digital technologies.[10] The online diagnostic involved a short questionnaire covering the five themes of advice. For the personal diagnostic, firms were allocated to regional delivery partners who had to offer a diagnostic shortly after application.[11] Firms were offered the option of undertaking the diagnostic face-to-face, or via telephone or Skype.[12]

The personal diagnostic involved a greater time commitment than the online diagnostic (which took around 10-15 minutes). This may explain why completion rates differed by the type of diagnostic – 62% for personal, 83% for online. Firms receiving the personal

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[5] BIS is now the Department for Business, Energy and Industrial Strategy (BEIS).
[6] See page 4 of the trial protocol https://www.gov.uk/government/publications/growth-vouchers-programme-trial-protocol.
[7] Self-reported eligibility was checked by delivery partners. See page 9 of the trial protocol.
[8] BIS commissioned delivery partners across the UK to recruit firms onto the programme through marketing activities, check eligibility of applicants, carry out diagnostic assessments and assess claims for funding.
[9] BIS and BIT were concerned about the depth of the firm diagnostics, and worried that imposing a theme of advice recommended by delivery partners could discourage voucher take-up.
[10] This is the ordering of themes used in the online questionnaire.
[11] Delivery partners had to carry out the diagnostics within two weeks of a firm applying, but this rule was later relaxed. BIS gave delivery partners a broad remit in the design of personal diagnostic assessments.
[12] From the 9,488 firms allocated to a personal diagnostic, 42% received it face-to-face, while 58% over the phone or via Skype. Differences in resources and capacity meant variation across delivery partners in the style, length, and content of assessments. Most firms receiving a personal diagnostic reported that the conversation with the advisor helped clarify areas for improvement (BIS, 2014).

diagnostic discussed their needs and preferred theme of advice with the delivery partner. For the online diagnostic, the theme of advice was presented to the firm upon completion of the self-assessed online diagnostic, based on their responses to questions in each area.

Firms then selected a theme of advice, and some were randomly offered a voucher, capped at £2,000, that would cover up to 50% of the cost of purchasing advice. All firms were then referred to an online directory of advisors – developed and run by Enterprise Nation, a business support network. Although voucher randomisation was conditional on the theme of advice suggested by the diagnostic, firms were not restricted to accessing that kind of advice. Firms did not have to use the voucher to pay for the service. Those that were not allocated a voucher had to pay the full cost of any advice received.

The online directory contained details of thousands of consultants in each of the five areas of advice, listed by the type of support they offered. Consultants were selected for the directory based on their experience, qualifications and membership of professional bodies (BIS, 2014). They provided bespoke advice which generally involved assistance over weeks or months. The consultant usually reviewed information about the structure and performance of the firm, before discussing improvements, targets and how to monitor progress. The intensity of the support varied and often involved working closely with the consultant to develop a business plan or a marketing strategy or explore financing alternatives.

## 3. Experimental set up and identification

The trial protocol allowed for firms to be randomised at two points, determining the type of diagnostic and whether the firm received a voucher (see Figure 1). The protocol determined that 25% of eligible firms were to be allocated to the online diagnostic and 75% to the personal diagnostic. The diagnostic assessment resulted in firms choosing a theme of advice. Within each theme, firms were to be randomly allocated to a treatment or control group with 75% of firms allocated to the treatment group and 25% to the control group.[13]

The treatment group received a voucher that could be used to cover 50% of the cost of business advice purchased from an online directory of consultants. Although firms could

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[13] A stratified randomised design by business sector was not feasible due to the lack of accurate information by sector (BIS, 2014).

spend as much on advice as they wanted, the value of the voucher was capped at £2,000. Overall, the mean value claimed was £1,714, against an average total purchase of £3,713 for advice.[14] The control group only received the diagnostic (online or personal, depending on the initial randomisation) and a referral to the same online directory.

Figure 1. Timeline of the programme
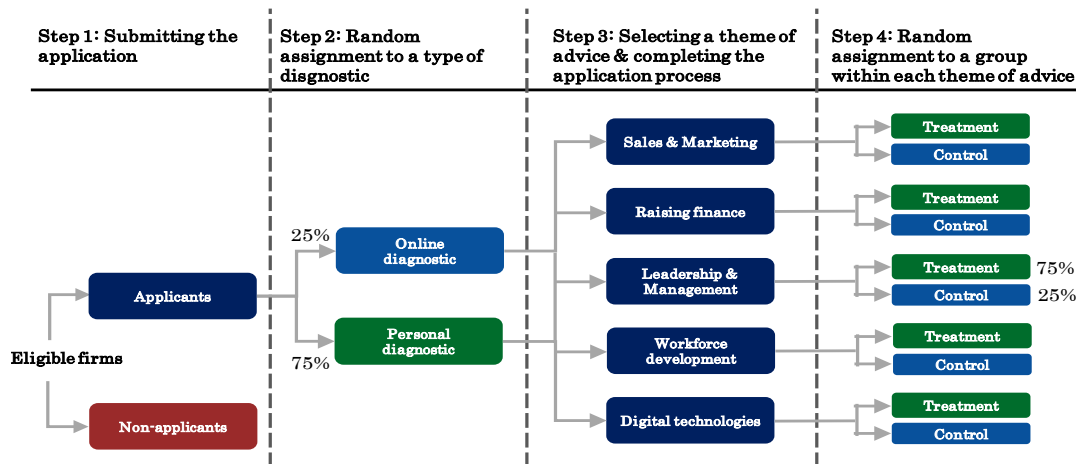


Note: The diagram shows the journey of firms from application to the allocation of vouchers. Source: Based on GVP's full trial protocol, available at https://www.gov.uk/government/publications/growth-vouchers-programme-trial-protocol.

As with any voluntary programme, unobservables might drive self-selection in terms of who applies and completes the application, which has some implications for external validity. However, if the programme had been implemented as planned, then conditional on completing the application, the two-step randomisation would have allowed us to estimate the effect of type of diagnostic and of subsidising the cost of purchasing business advice (via a voucher) between treated and control firms. In practice, several complications arise due to changes in implementation and issues with compliance.

First, the random allocation of firms to each type of diagnostic broke down in the last month (March 2015) when many firms applied in response to a large marketing campaign, initiated because of concerns over enrolment. Around 31% of the total number of applications were submitted in this month, surpassing the capacity to deliver personal diagnostics. As a result, the allocation ratio between personal and online diagnostics was not followed, and a larger share of applicants received an online diagnostic.

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[14] One third of firms that received and used the voucher claimed less than £2,000. For this group, the mean total cost of advice received was £2,489. The remaining 67% of firms claimed the maximum value of £2,000 and paid an average of £4,311 in total.

Second, around 33% of those receiving an online diagnostic picked a theme of advice different to that suggested by the outcome of the online questionnaire. While the headline statistic suggest self-selection was only an issue for the online diagnostic, it may also be an issue for the personal diagnostic as the theme was selected by the firm in discussion with the advisor. The potential self-selection into a type of advice limits our ability to draw conclusions on the causal impact of GVP by theme of advice. While the protocol was not followed in terms of the random allocation to diagnostic, random allocation of the vouchers, conditional on selected theme of advice, was implemented as planned. This is illustrated in Figure 2 which presents the breakdown of firms that selected each theme, as well as the allocation to treatment and control within each of them.

A third issue with the implementation of the programme was the low take up of the GVP voucher. Only 33% of firms that were randomly allocated a voucher used it.[15] Comparing voucher recipients to non-recipients provides an estimate of the intention-to-treat effects (see Figure 3 for a description of the different groups of firms). The low take-up means that this may not be particularly informative in terms of the impact of subsidised advice on firm performance. For this reason, we also estimate the effect of treatment on the treated using an instrumental variables (IV) approach which exploits the variation in take up of the voucher induced by the random allocation of the voucher.

Figure 2. Voucher allocation by theme of advice
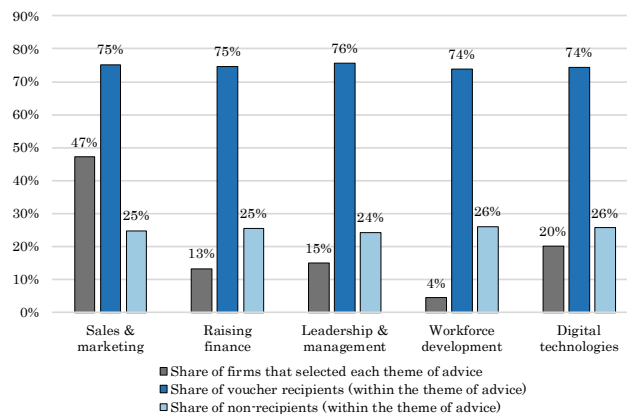


Note: The grey bars show the distribution of firms among the five themes of advice. The blue bars depict the allocation of firms to treatment and control group within each theme of advice. Source: Author calculation using baseline information.

To account for the breakdown of the random assignment to diagnostic, our preferred specifications include the full set of interactions between i) the type of diagnostic received;

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[15] Firms that used the voucher were more likely to be able to afford the cost of business advice, since at baseline they have a higher turnover and more employees compared to non-compliers.

ii) the theme of advice selected; and iii) a dummy indicating if the firm applied in the last month. We also include a set of basic controls which correspond to month of application and cohort of firms to improve precession by accounting for potential differences in diagnostic and treatment delivery across cohorts and over time. For more details on the identification strategies, see appendix section A.4.

Figure 3. Group of applicants, recipients and compliers
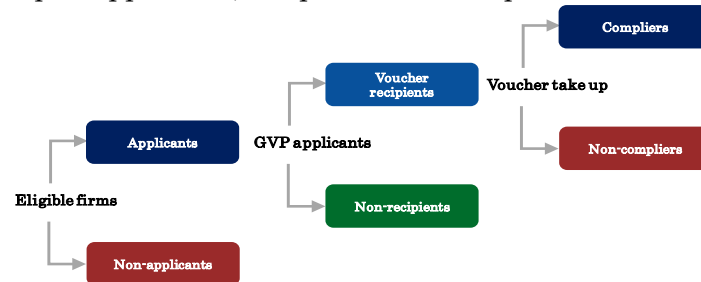


Note: The diagram shows the split of firms depending on whether they were eligible, applied to the programme and used the voucher. Source: Author calculation using baseline information.

# 4. Data

Our analysis uses secondary data from the Inter-Departmental Business Register (IDBR) and survey data collected by the GVP programme. IDBR provides data on employment and turnover and can be used to calculate survival rates. IDBR is a live register held by HM Revenue and Customs and constructed via Pay As You Earn (PAYE) and Value Added Tax (VAT) records. It covers all firms except those with no employees or that are below the VAT threshold. Firms on the IDBR account for almost 99 percent of UK economic activity (ONS, 2019).

The version provided to us by the Department for Business, Energy and Industrial Strategy (BEIS) represents a point in time snapshot for the first quarter of every year from 2012 to 2019. Our analysis considers the sample of applicants from the GVP programme that we can identify in the IDBR – we manage to match around 67% of firms.[16] Secondary data has a reporting lag of 1 year for employment, and 2 years for turnover; thus, we can only estimate effects for 2016-2018 on employment, 2016-2017 on turnover. Firm outcomes are not updated for every firm in every year, so we use the group of firms with updated data on each outcome in each year.[17]

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[16] The unique code of applicant firms provided to us does not allow us to identify some of them in the IDBR.
[17] This does not alter the balance of firms allocated to treatment and control groups. For all outcomes and periods, the proportion of treated firms ranges between 74.4% for those with updated data on employment from the 12-month IDBR and 75.0% among firms with updated data on turnover from the 12-month IDBR.

That is, we do not impose a sample restriction to consider a balanced panel. The data appendix (section A.1) has further details on variable definitions, cleaning of the data and how we deal with the small number of firms that appear in the GVP programme data even though the IDBR data suggest they are ineligible.

Survey data is available at baseline, and 12 and 24 months after diagnostic. Follow up-interviews of treated and control firms covering firm performance, expectations, capabilities, and practices were done 12 and 24-months after firms had completed the diagnostic.[18] Performance was self-reported by firms and includes turnover, number of employees and establishments, among other outcomes. The sampling strategy followed a census approach for the baseline and 12-month survey data, while the 24-month survey contacted every participant who responded to the 12-month survey. Baseline information was collected for 28,158 firms via an online questionnaire during the application process. Of these, 20,283 completed the diagnostic and application process and 7,743 did not. 5,623 firms completed the 12-month survey.[19] From these, 1,982 firms completed the 24-month survey (the rest either were not reached or did not complete the survey). Since the application period ran from January 2014 to March 2015, interviews were conducted in two cohorts to collect information approximately 12 and 24 months after receiving the diagnostic, regardless of the month of application. Remember, that all firms receive a diagnostic, while only some firms receive a voucher to cover for business advice and that the latter is the treatment of interest.

Gaps between the date of diagnostic and receiving the advice, and the timing of both relative to IDBR and survey data introduce variation in timing of the outcome data relative to treatment, as summarised in Table 1.[20] For example, the 12-month survey reports data that, on average, comes only 7.9 months after the voucher was claimed, while the gap is 9.7 months for IDBR. In what follows we discuss results at 12, 24 and 36 months

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[18] Eight capabilities covered: managing people; developing a business plan; accessing the UK market; accessing other markets; developing new products; accessing external finance; recruiting new employees; using IT to grow. Nine practices covered having a: business plan; marketing plan; marketing budget; corporate website; workforce strategy; recruitment budget; training budget; cash flow forecast; regular financial review.

[19] A consulting firm conducted the telephone surveys using a Computer Assisted Telephone Interviewing system. The questionnaires included skip patterns, dynamic checks and constraints to ensure the quality of information collected. Interviewers were randomly allocated to businesses and were supervised during data collection.

[20] 42 firms were allocated and used the voucher, but at the time of the 12-month survey had not received the business support. Hence, these firms are considered as non-participants in estimates after 12 months and as participants after 24 months. Similarly, 3 participants claimed the voucher after the 1st quarter of 2016, which corresponds to the date of IDBR 2016. So, they are also considered as non-participants in estimates after 12 months and as participants in subsequent periods.

while acknowledging that this is a simplification and abstracts from variation seen in both surveys. More details on how we consider the timing of the treatment in relation to the data are in the data appendix (A.1).

Table 1. Timing of outcome data relative to claim date of the voucher

Number of months between claim date of the voucher and date of the data used for analysis

| Dataset | Mean | Median | s.d. | Min. | Max. |
|---|---|---|---|---|---|
| 12-month survey | 7.9 | 7.7 | 2.3 | 3.1 | 14.0 |
| 24-month survey | 19.6 | 19.4 | 2.4 | 3.8 | 25.8 |
| 12-month IDBR | 9.7 | 8.9 | 3.2 | 0.3 | 20.5 |
| 24-month IDBR | 21.7 | 20.9 | 3.2 | 12.3 | 32.5 |
| 36-month IDBR | 33.7 | 32.9 | 3.2 | 24.3 | 44.5 |

Note: For the 12/24-months survey the estimates use the mid-point of the period when interviews were conducted for each cohort of firms. For secondary sources, the summary statistics are calculated for firms that claimed the voucher using the mid-point of the period from the IDBR.

Given these issues with timing for the analysis of survey outcomes, we include dummies that capture the month of application and the survey-cohort of firms as our set of basic control variables. Note that, similarly to the analysis of administrative data, we also include the full set of interactions between the type of diagnostic received; the theme of advice selected; and a dummy indicating if the firm applied in the last month to account for the breakdown of the random assignment to diagnostic.

# 5. Results

In 2015, the average firm that completed the GVP application process had been operating for over 9 years, employed 7.3 people and had an annual turnover of around £773,000 (Table A.1). For this group of firms, the median growth in turnover a year prior to the programme (between 2014 and 2015) was 7.4%. Table A.2 to Table A.4 report balancing checks between treatment and control groups using GVP baseline data for the three samples for which we have IDBR, 12 and 24-month survey data, respectively. Each row reports results from a regression of the baseline characteristic on a dummy variable indicating treatment status, our basic set of controls and the full set of interactions, as detailed above.

For the sample of firms that are matched to the IDBR, there are statistically significant differences between treatment and control for three out of 47 variables in the baseline data. There is one statistically significant difference at baseline for the sample of firms in

the 12 month-survey and four for the sample in the 24-month survey. Eight significant differences from a total of 141 balancing tests is in line with what would be expected to find by chance at 5% levels of significance and the magnitude of these differences is relatively small. Controlling for baseline characteristics does not change our results.

## 5.1    Effect of the programme on firm performance

The IDBR provides the most reliable source of data for firm performance – it is subject to more cross-checking than the self-reported survey data and although the matching rate from GVP to IDBR is around 67%, there is no reason to think that this is related to participation in GVP. Table 2 reports estimates from ordinary least squares (OLS) and IV turnover and employment from the 12, 24 and 36-month IDBR.[21] First-stage results for the IV are in Table A.5. The coefficient for voucher allocation is positive and statistically significant in all the samples considered.[22] As expected, the first-stage estimates indicate that the allocation of vouchers induced firms to use the voucher given to them.

For both OLS and IV, column 1 in Table 2 presents results from a specification with no controls. Column 2 controls for type of diagnostic and theme of advice as well as for our basic set of controls, while column 3, our preferred specification, includes all the interactions between type of diagnostic, theme of advice and the last month of application, and the basic set of controls.[23] All estimates on turnover and employment are conditional on firm survival.[24] As discussed above the OLS results provide estimates of the intention-to-treat (ITT) and the IV results provide estimates of the average treatment effect on the treated (ATT).

Effects on employment are negative, small in magnitude and insignificant in all periods. For both outcomes, the IV coefficient is about three times as large as the OLS coefficient after 12 and 24 months, but they are both zero in the 36-month IDBR. Despite

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[21] As discussed in section 3, the 12-month IDBR reports outcomes somewhere between 9.7 and 13.1 months after treatment. Similarly, on average the 24-month IDBR reports outcomes between 21.7 and 25.1 months after treatment, and the 36-month IDBR reports outcomes between 33.7 and 37.1.

[22] A larger coefficient in the first stage from survey data relative to the IDBR reflects a larger proportion of compliers captured in the former sample compared to the latter. In the same way, the 24-month survey sample includes a larger share of compliers relative to the 12-month survey. The split of firms in survey data is 76% treated and 24% control in the 12-month survey, and 80% treated and 20% control in the 24-month survey.

[23] From the overall group of participants, we can identify 13,444 firms in the IDBR. However, only 9,452 firms have updated data on turnover from the 12-month IDBR and 8,855 from the 24-month IDBR. Similarly, for employment only 7,326 firms have updated data from the 12-month IDBR; 6,606 from the 24-month IDBR; and 6,084 from the 36-month IDBR.

[24] While survival is not an outcome considered in the trial protocol, we present ITT and IV estimates for survival in Table A.6 as turnover and employment data is conditional on firm survival. The results suggest GVP had no impact on the survival of firms in any period.

voucher take-up of only 33%, the OLS estimate from receiving a voucher indicates a positive effect on turnover of 3% after 12 months. The IV estimate for firms that received and used the voucher is 8.2%. This corresponds to an increase of £73,120, given the average turnover of compliers in 2015. The positive effect on turnover is short-lived – the OLS and IV coefficients at 24 months are smaller (0.9% and 2.4% respectively) and statistically insignificant.

Table 2. OLS and IV estimates for firms in the IDBR

|  | OLS (1) | OLS (2) | OLS (3) | IV (1) | IV (2) | IV (3) |
|---|---|---|---|---|---|---|
| *Turnover after 12 months* | | | | | | |
| Allocation of voucher | 0.030* (0.014) | 0.030* (0.014) | 0.030* (0.014) | N/A | N/A | N/A |
| Take up of the voucher | N/A | N/A | N/A | 0.083* (0.039) | 0.082* (0.039) | 0.082* (0.039) |
| Obs. | 9,452 | 9,452 | 9,452 | 9,452 | 9,452 | 9,452 |
| *Turnover after 24 months* | | | | | | |
| Allocation of voucher | 0.010 (0.022) | 0.009 (0.022) | 0.009 (0.022) | N/A | N/A | N/A |
| Take up of the voucher | N/A | N/A | N/A | 0.027 (0.061) | 0.025 (0.061) | 0.024 (0.061) |
| Obs. | 8,855 | 8,855 | 8,855 | 8,855 | 8,855 | 8,855 |
| *Employment after 12 months* | | | | | | |
| Allocation of voucher | -0.008 (0.007) | -0.009 (0.007) | -0.009 (0.007) | N/A | N/A | N/A |
| Take up of the voucher | N/A | N/A | N/A | -0.025 (0.020) | -0.025 (0.020) | -0.026 (0.020) |
| Obs. | 7,326 | 7,326 | 7,326 | 7,326 | 7,326 | 7,326 |
| *Employment after 24 months* | | | | | | |
| Allocation of voucher | -0.007 (0.011) | -0.007 (0.011) | -0.008 (0.011) | N/A | N/A | N/A |
| Take up of the voucher | N/A | N/A | N/A | -0.021 (0.032) | -0.021 (0.031) | -0.022 (0.032) |
| Obs. | 6,606 | 6,606 | 6,606 | 6,606 | 6,606 | 6,606 |
| *Employment after 36 months* | | | | | | |
| Allocation of voucher | 0.000 (0.015) | 0.001 (0.015) | -0.000 (0.015) | N/A | N/A | N/A |
| Take up of the voucher | N/A | N/A | N/A | 0.000 (0.043) | 0.002 (0.043) | -0.000 (0.043) |
| Obs. | 6,084 | 6,084 | 6,084 | 6,084 | 6,084 | 6,084 |
| Basic controls | No | Yes | Yes | No | Yes | Yes |
| Type of diagnostic & theme of advice | No | Yes | N/A | No | Yes | N/A |
| Full set of interactions | No | No | Yes | No | No | Yes |

Note: OLS and IV regressions for percentage change since baseline. Robust standard errors in parentheses; ** p<0.01, * p<0.05. Basic controls include month of application and cohort. The full set of interactions corresponds to all combinations between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. The result on turnover after 12 months is significant at a 7% level when applying the Romano-Wolf multiple hypothesis correction for turnover and employment with 1,000 bootstrap repetitions. N/A: not applicable.

The OLS and IV estimates for turnover and employment are robust to several sensitivity checks. First, we restrict the analysis to the group of firms with updated data in all three periods, thus considering the same group of firms after 12, 24 and 36 months

(Table A.7). The positive IV effect on turnover after 12 months is slightly smaller (7.1%) and significance drops (to around 6%). The effects for turnover at 24 months and for employment are also unchanged. Second, we exclude firms that applied in the last month when the random allocation between type of diagnostic was not followed (Table A.8). The results are similar across all variables and time periods – the IV effect on turnover is 10.6% after 12 months. Third, we exclude firms that did not select the recommended theme of advice (Table A.9). We also find similar effects for all variables and periods, including a positive IV coefficient for turnover of 10.2% after 12 months. Finally, we apply the Romano-Wolf multiple hypothesis correction (Romano and Wolf, 2005a & 2005b) using a step-down procedure for our two outcomes with 1,000 bootstrap repetitions to derive standard errors. Our IV result on turnover after 12 months remains significant, albeit at a 7% level.

The 12-month survey identifies 99 always-takers – firms that were not allocated a voucher but purchased business advice from the GVP online directory. We consider these control firms as participants in IV estimates in all the periods as an additional robustness check (Table A.10).[25] The IV effect is now 8.9% on turnover after 12 months, with all other results continuing to be insignificant. Finally, the survey data identifies firms that received any other business advice paid at a commercial rate, or free or subsidised advice as part of other government schemes. These firms received support similar to GVP, but are not always-takers since that is defined in relation to the allocation of a GVP voucher. Thus, we expand the definition of participation into the programme by considering all firms that either i) paid for advice from the GVP online directory; or ii) received any other type of advice (Table A.11).[26] Using this definition, the IV estimate is significant and around the same magnitude (9.1%) for turnover after 12 months, with all other results continuing to be insignificant.

## 5.2    Self-reported outcomes, expectations, capabilities, and practices

Estimates for self-reported outcomes – the number of establishments, choice to begin exporting, increase in turnover, employment and growth expectations – from the 12- and

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[25] The 24-month questionnaire did not include the question on whether firms had purchased advice from the GVP online directory or not. Thus, we cannot identify control firms that paid for GVP advice from the 24-month survey.

[26] The survey data allows us to identify firms that received any other type of business advice. Those captured from the 12-month survey are considered as participants in the 12, 24 and 36-month IDBR estimates, while those identified from the 24-month survey are considered as participants only in the 24 and 36-IDBR estimates.

24-month surveys relative to baseline provide supporting evidence for a short-term effect on turnover relative to baseline (Table 3 and Table 4).[27] There is no effect on number of establishments, employment or beginning to export. Effects on turnover are consistent with IDBR – positive and significant at 12-months, smaller and insignificant at 24 months.[28] Note that the coefficients are not directly comparable to those from IDBR as the survey asks whether turnover increased, decreased or stayed the same, while IDBR reports values. The effect on growth expectations is positive in both periods, although we do not know whether these changes in sentiment are a direct effect of the advice, or an indirect effect resulting from the increase in turnover at 12-months.

Table 3. OLS and IV estimates on self-reported outcomes for firms in the 12-month survey

|  | OLS (1) | IV (2) | OLS (3) | IV (4) |
|---|---|---|---|---|
|  | Number of establishments | | Begin exporting | |
| Allocation of voucher | -0.009 (0.007) | N/A | 0.003 (0.011) | N/A |
| Take up of the voucher | N/A | -0.020 (0.015) | N/A | 0.008 (0.024) |
| Obs. | 3,614 | 3,614 | 5,579 | 5,579 |
|  | Increase in turnover | | Employment | |
| Allocation of voucher | 0.040** (0.016) | N/A | -0.125 (0.113) | N/A |
| Take up of the voucher | N/A | 0.092** (0.036) | N/A | -0.257 (0.231) |
| Obs. | 5,499 | 5,499 | 3,608 | 3,6,08 |
|  | Growth expectations | | | |
| Allocation of voucher | 0.101* (0.040) | N/A | | |
| Take up of the voucher | N/A | 0.206* (0.083) | | |
| Obs. | 3,573 | 3,573 | | |

Note: OLS and IV estimates from our preferred specification, which controls for cohort and month of application and includes the full set of interactions between type of diagnostic, theme of advice and dummy for last month of application. N/A: not applicable. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$.

Table 5 and Table 6 report results for a range of business capabilities and practices from the 12- and 24- month surveys. While one could consider these independently of the results on firm performance, we focus on the extent to which they are consistent with the impacts on turnover identified in Table 2 to Table 4. Capabilities three to five – which include accessing UK and other markets and developing new products – appear the most

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[27] As discussed in section 3, on average these survey results are for data a couple of months before the corresponding IDBR data.
[28] We do not consider a sample restriction for the sub-set of firms that are included in both IDRB and survey data as this results in a small sample size for both IDBR and survey estimates.

directly relevant for explaining the increase in turnover, so it is reassuring that the only significant increase in capabilities is in this sub-group. The results suggest that development of new products, rather than improved access to the UK or other markets could explain the increase in turnover after 12 months.

Table 4. OLS and IV estimates on self-reported outcomes for firms in the 24-month survey

|  | OLS (1) | IV (2) | OLS (3) | IV (4) |
|---|---|---|---|---|
|  | *Number of establishments* | | *Begin exporting* | |
| Allocation of voucher | -0.006 (0.032) | N/A | -0.011 (0.024) | N/A |
| Take up of the voucher | N/A | -0.011 (0.063) | N/A | -0.022 (0.046) |
| Obs. | 1,959 | 1,959 | 1,965 | 1,965 |
|  | *Increase in turnover* | | *Employment* | |
| Allocation of voucher | 0.019 (0.028) | N/A | -0.237 (0.251) | N/A |
| Take up of the voucher | N/A | 0.037 (0.054) | N/A | -0.464 (0.493) |
| Obs. | 1,944 | 1,944 | 1,953 | 1,953 |
|  | *Growth expectations* | | | |
| Allocation of voucher | 0.144** (0.057) | N/A | | |
| Take up of the voucher | N/A | 0.282** (0.112) | | |
| Obs. | 1,930 | 1,930 | | |

Note: OLS and IV estimates from our preferred specification, which controls for cohort and month of application and includes the full set of interactions between type of diagnostic, theme of advice and dummy for last month of application. N/A: not applicable. Robust standard errors in parentheses; ** $p < 0.01$, * $p < 0.05$.

For practices, the strongest effect is for improvements in the share of theme-specific practices adopted. Although this is not surprising – the theme of advice and the business advice purchased were selected by firms and tailored to their needs – it is consistent with the effects on turnover as sales & marketing was the most popular theme of advice. Similarly, the only significant effect on specific practices is for having a marketing plan – again consistent with the effect on turnover.

The impact on capabilities remains the same after 24 months: improvements in developing new products, as well as a positive and statistically significant effect on theme-specific practices adopted. Firms receiving and using vouchers also move on from having a marketing plan (after 12 months) to having a marketing budget (after 24 months). The results suggest firms tended to maintain their improvements in terms of capabilities and practices adopted after 24 months, but that does not appear to translate in to sustained turnover growth. In summary, improvements in capabilities and adoption of practices

could help firms with a one-off increase in turnover to a new higher level (after 12 months), but maintaining these changes is not enough for sustaining growth, which would be reflected as an increase in turnover to an even higher level after 24 months.

Table 5. OLS and IV estimates on capabilities and practices for firms in the 12-month survey

| | OLS (1) | IV (2) | OLS (3) | IV (4) | OLS (5) | IV (6) |
|---|---|---|---|---|---|---|
| | *Capability 1: Managing people* | | *Capability 2: Developing a business plan* | | *Capability 3: Accessing the UK market* | |
| Allocation of voucher | 0.043 (0.031) | N/A | 0.040 (0.031) | N/A | 0.006 (0.034) | N/A |
| Take up of the voucher | N/A | 0.087 (0.062) | N/A | 0.081 (0.063) | N/A | 0.013 (0.070) |
| Obs. | 3,353 | 3,353 | 3,562 | 3,562 | 2,945 | 2,945 |
| | *Capability 4: Accessing other markets* | | *Capability 5: Developing new products* | | *Capability 6: Accessing external finance* | |
| Allocation of voucher | 0.017 (0.043) | N/A | 0.070* (0.033) | N/A | 0.024 (0.038) | N/A |
| Take up of the voucher | N/A | 0.037 (0.094) | N/A | 0.142* (0.067) | N/A | 0.049 (0.080) |
| Obs. | 1,853 | 1,853 | 3,285 | 3,285 | 2,442 | 2,442 |
| | *Capability 7: Recruiting new employees* | | *Capability 8: Using IT to grow* | | *Share of theme-specific practices adopted* | |
| Allocation of voucher | 0.000 (0.034) | N/A | 0.030 (0.031) | N/A | 0.378** (0.064) | N/A |
| Take up of the voucher | N/A | 0.000 (0.069) | N/A | 0.061 (0.064) | N/A | 0.871** (0.147) |
| Obs. | 3,085 | 3,085 | 3,528 | 3,528 | 5,579 | 5,579 |
| | *Practice 1: Having a business plan* | | *Practice 2: Having a marketing plan* | | *Practice 3: Having a marketing budget* | |
| Allocation of voucher | 0.003 (0.019) | N/A | 0.047* (0.020) | N/A | 0.011 (0.020) | N/A |
| Take up of the voucher | N/A | 0.007 (0.040) | N/A | 0.096* (0.041) | N/A | 0.023 (0.042) |
| Obs. | 3,615 | 3,615 | 3,615 | 3,615 | 3,615 | 3,615 |
| | *Practice 4: Having a corporate website* | | *Practice 5: Having a workforce strategy* | | *Practice 6: Having a recruitment budget* | |
| Allocation of voucher | 0.014 (0.012) | N/A | 0.024 (0.018) | N/A | -0.014 (0.013) | N/A |
| Take up of the voucher | N/A | 0.029 (0.025) | N/A | 0.048 (0.037) | N/A | -0.029 (0.027) |
| Obs. | 3,615 | 3,615 | 3,615 | 3,615 | 3,615 | 3,615 |
| | *Practice 7: Having a training budget* | | *Practice 8: Having a cash flow forecast* | | *Practice 9: Having a regular financial review* | |
| Allocation of voucher | 0.007 (0.019) | N/A | -0.004 (0.018) | N/A | 0.018 (0.015) | N/A |
| Take up of the voucher | N/A | 0.015 (0.039) | N/A | -0.009 (0.038) | N/A | 0.036 (0.031) |
| Obs. | 3,615 | 3,615 | 3,615 | 3,615 | 3,615 | 3,615 |

Note: OLS and IV estimates from our preferred specification, which controls for cohort and month of application and includes the full set of interactions between type of diagnostic, theme of advice and dummy for last month of application. N/A: not applicable. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$.

Table 6. OLS and IV estimates on capabilities and practices for firms in the 24-month survey

| | OLS (1) | IV (2) | OLS (3) | IV (4) | OLS (5) | IV (6) |
|---|---|---|---|---|---|---|
| | *Capability 1: Managing people* | | *Capability 2: Developing a business plan* | | *Capability 3: Accessing the UK market* | |
| Allocation of voucher | -0.001 (0.043) | N/A | 0.026 (0.043) | N/A | 0.031 (0.048) | N/A |
| Take up of the voucher | N/A | -0.002 (0.084) | N/A | 0.051 (0.084) | N/A | 0.061 (0.096) |
| Obs. | 1,813 | 1,813 | 1,938 | 1,938 | 1,608 | 1,608 |
| | *Capability 4: Accessing other markets* | | *Capability 5: Developing new products* | | *Capability 6: Accessing external finance* | |
| Allocation of voucher | 0.111 (0.072) | N/A | 0.111* (0.046) | N/A | 0.030 (0.053) | N/A |
| Take up of the voucher | N/A | 0.243 (0.157) | N/A | 0.217* (0.090) | N/A | 0.059 (0.106) |
| Obs. | 858 | 858 | 1,758 | 1,758 | 1,316 | 1,316 |
| | *Capability 7: Recruiting new employees* | | *Capability 8: Using IT to grow* | | *Share of theme-specific practices adopted* | |
| Allocation of voucher | -0.002 (0.049) | N/A | 0.042 (0.044) | N/A | 0.274** (0.092) | N/A |
| Take up of the voucher | N/A | -0.004 (0.095) | N/A | 0.081 (0.086) | N/A | 0.536** (0.178) |
| Obs. | 1,614 | 1,614 | 1,926 | 1,926 | 1,965 | 1,965 |
| | *Practice 1: Having a business plan* | | *Practice 2: Having a marketing plan* | | *Practice 3: Having a marketing budget* | |
| Allocation of voucher | 0.017 (0.028) | N/A | 0.024 (0.029) | N/A | 0.062* (0.028) | N/A |
| Take up of the voucher | N/A | 0.033 (0.055) | N/A | 0.046 (0.056) | N/A | 0.120* (0.056) |
| Obs. | 1,952 | 1,952 | 1,955 | 1,955 | 1,958 | 1,958 |
| | *Practice 4: Having a corporate website* | | *Practice 5: Having a workforce strategy* | | *Practice 6: Having a recruitment budget* | |
| Allocation of voucher | 0.030 (0.018) | N/A | 0.046 (0.026) | N/A | 0.001 (0.020) | N/A |
| Take up of the voucher | N/A | 0.058 (0.036) | N/A | 0.089 (0.051) | N/A | 0.001 (0.038) |
| Obs. | 1,948 | 1,948 | 1,951 | 1,951 | 1,959 | 1,959 |
| | *Practice 7: Having a training budget* | | *Practice 8: Having a cash flow forecast* | | *Practice 9: Having a regular financial review* | |
| Allocation of voucher | 0.031 (0.027) | N/A | -0.007 (0.026) | N/A | 0.037 (0.021) | N/A |
| Take up of the voucher | N/A | 0.060 (0.054) | N/A | -0.013 (0.051) | N/A | 0.073 (0.040) |
| Obs. | 1,959 | 1,959 | 1,954 | 1,954 | 1,955 | 1,955 |

Note: OLS and IV estimates from our preferred specification, which controls for cohort and month of application and includes the full set of interactions between type of diagnostic, theme of advice and dummy for last month of application. N/A: not applicable. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$.

## 5.3 Extension: heterogeneity by type of diagnostic, theme of advice and firm characteristic

One of the objectives set out in the trial protocol of the programme was to compare the impact of each type of diagnostic (personal or online). To assess the effect of the GVP by diagnostic type, we estimate the ITT and ATT coefficients restricting the sample to the sub-set of firms that received each diagnostic.[29] The implementation of GVP complicates the comparison of the effects by type of diagnostic. First, as discussed above, firms applying in the last month were not randomly allocated to a type of diagnostic. Second, dropout rates differ by diagnostic. Of the 7,743 firms that received diagnostic support but did not complete the application process, 74% were from the personal track and only 26% received an online diagnostic. However, from the total of 20,283 firms that completed the diagnostic and application process, those who received a personal diagnostic and were allocated a voucher, ended up having a higher voucher take up (39% vs. 27% from the online diagnostic track). One possible explanation for these differences is that arranging a face-to-face meeting or a phone call with a delivery partner for the diagnostic was more costly, compared to simply completing an online questionnaire. Given these differences, our estimates for the effect of GVP by type of diagnostic are likely to have different external validity across these two groups. This imbalance urges caution when interpreting these results.

Table A.12 contains the results on firm performance from IDBR. The overall impact of the GVP on turnover after 12 months (8.2%) is no longer significant in any of the groups, although the point estimates are similar in magnitude (8.0% and 8.3% for personal and online diagnostic respectively). This is explained by larger standard errors, which are driven by a smaller sample size in each case. The programme had no effect on employment for either group.[30] For employment, coefficients are negative for personal diagnostic and positive for online diagnostic in all periods. None of these differences are large in comparison to the standard errors.

GVP offered five themes of business advice: sales & marketing, raising finance, leadership & management, workforce development and digital technologies. The trial protocol complicates the interpretation of estimates by theme of advice as the theme is

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[29] The results are similar when considering the whole sample and including an interaction term between diagnostic type and voucher allocation (for ITT estimates) or voucher take up (for IV estimates).

[30] We obtain the same set of results when excluding firms that enrolled in the last month of the application period (the random allocation between type of diagnostic was not followed in this month).

self-selected. For those receiving the personal diagnostic this self-selection occurs because the firm selects the theme when discussing their needs and preferences with the advisor. For firms using the online diagnostic, it occurs because the outcome of the tool is based on their responses to questions in each area. Further, firms could select a theme of advice different to the outcome of the online questionnaire.

With this caveat in mind and given the effects on turnover reported so far, the most interesting estimates by theme are for firms receiving advice on sales & marketing (Table A.13). As before, GVP had no significant impact on employment for this theme of advice but effects on turnover are larger and persistent – the IV estimates indicate turnover increased by 18.8% and 19.8% after 12 and 24 months, respectively.[31] The increase in turnover corresponds to £133,065 and £140,323, respectively, given the average turnover of compliers in 2015. GVP had no impact on turnover for firms which selected any other theme of advice.[32] This suggests the overall GVP impact on turnover after 12 months is driven by firms which selected sales & marketing. While the effect on turnover among those receiving advice on sales & marketing is similar after 24 months, point estimates are smaller for all the other themes of advice, which ultimately results in an overall insignificant effect on turnover after 24 months. There are no significant effects on firm performance for firms that selected raising finance, leadership & management, workforce development or digital technologies.

We also expand the analysis to explore heterogeneity by firm size and age, even though this was not covered in the trial protocol (see section A.3). We do not find clear patterns on the impact of GVP along either of these dimensions. In summary, we find no evidence that the impact of GVP on firm performance differs according to the type of diagnostic received or the characteristic of the firm. Receiving advice on sales & marketing led to a larger and longer lasting effect on turnover, while there was no effect on turnover for firms on other themes.

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[31] The results are similar when not restricting the sample to the sub-set of firms that selected each theme of advice, but considering the whole sample and including interaction terms for each theme of advice and voucher allocation (for ITT estimates) or voucher take up (for IV estimates).

[32] This finding is in line with Anderson, Chandy and Zia (2018) who analyse a randomised controlled trial in South Africa and conclude that marketing training was more successful at increasing turnover than training on finance.

# 6. Comparing RCT to PSM estimates

In addition to providing evidence on the impact of subsidising business advice, a secondary objective for GVP was to provide evidence on the feasibility of experimental designs as a means to improve evaluations of policy effectiveness (BIS, 2016). This section provides such evidence by comparing the RCT-ATT estimates to those obtained from propensity score matching (PSM) – a methodology widely used in policy evaluation as an alternative to trials. Our secondary data has good coverage of both applicants and non-applicants. The randomisation in GVP helps to control for the fact that firms selected into treatment based on unobservable characteristics that may also affect firm performance. PSM can control for selection on observable but not unobservable characteristics and so comparing RCT to PSM illustrates the impact of selection on unobservables.

To implement PSM, we pool firms from all themes of advice, and estimate the ATT by comparing firms that used the voucher (i.e., compliers) to a control group of non-applicant firms matched from the IDBR.[33] The set of non-applicants used for matching is restricted to those in IDBR which meet the eligibility criteria of the programme and are from the same industries as GVP applicants.[34] The propensity score comes from estimating a probit model for the probability a firm receives and uses a voucher based on a rich set of covariates.[35] Each treated firm is matched to three nearest neighbours based on the propensity score restricted to the common support region and the resulting control group used to estimate the ATT. For the assignment equation, we use the probit specification which minimises the number of differences between treated and control groups at baseline after controlling for the propensity score.[36] The PSM estimates use between 1,182 and 1,444 GVP compliers – as with the RCT the number of observations varies by year depending on the number of firms with updated IDBR data for a given outcome and year.[37]

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[33] PSM estimates exclude non-compliers and non-recipients as they received diagnostic support.

[34] GVP was open to participants from all industries, but firms did not apply from some sectors. We limit the PSM donor pool to the industries of GVP applicants. This corresponds to 550 4-digit SIC 2007 industries from a total of 724 in IDBR.

[35] These include age, age squared, turnover in 2015 and 2014, employment in 2014 and 2015, as well as dummy variables for region, industry and an interaction for London and the biggest sectors; professional, scientific & technological activities, and wholesale & retail.

[36] Table A.16 and Table A.17 contain five different specifications for the assignment equation of PSM, for turnover and employment, respectively. In each case, the last row (5) corresponds to the model used to obtain the ATT estimate. As shown in these tables, the model considered is the specification which results in the minimum number of differences between treated and control groups at baseline after controlling for the propensity score.

[37] As described above, some participants claimed the voucher after the 1st quarter of 2016, which corresponds to the date of IDBR 2016. These are considered as non-participants and excluded in PSM estimates after 12 months, but considered as participants and included in PSM estimates for subsequent periods.

A total sample of more than 394,000 non-applicant firms are used for the matching process. The estimation was conducted with bootstrap (non-parametric with replacement, using 1,000 repetitions) to derive standard errors for the propensity score and the average treatment effect on the treated. Section A.4 provides details.

For each outcome and period, Figure A.1 plots the distribution of the propensity score for GVP compliers and the matched control group. The quality of the match is good and after controlling for the propensity score there are only a few statistically significant differences between treatment and control groups on firm characteristics at baseline (one and two for turnover after 12 and 24 months respectively, and one and two for employment after 24 and 36 months respectively; see Table A.14 and Table A.15).

Table 7 contains ATT estimates from PSM. For all outcomes and periods, the PSM estimates are positive and significant, and increase over time. As can be seen from Table 8, there are substantial differences between the RCT-IV estimates of ATT (column 1) and the PSM estimates (column 2). The RCT results suggest no effect on employment, in contrast to the positive and increasing effects found using PSM. The turnover estimates are reasonably close at 12-months but deviate markedly at 24-months – increasing in the PSM, while converging to zero in the RCT.

Table 7. PSM estimates for turnover and employment

|  | ATT | No. of GVP compliers | Obs. |
|---|---|---|---|
| Turnover after 12 months | 0.063** (0.020) | 1,363 | 452,742 |
| Turnover after 24 months | 0.125** (0.032) | 1,301 | 416,292 |
| Employment after 12 months | 0.031** (0.008) | 1,376 | 480,069 |
| Employment after 24 months | 0.077** (0.012) | 1,276 | 432,989 |
| Employment after 36 months | 0.100** (0.018) | 1,182 | 394,644 |

Note: PSM estimates for percentage changes since baseline. Bootstrapped standard errors in parentheses; ** p<0.01, * p<0.05.

The PSM estimates in column (2) capture the joint effect of i) selection bias; ii) receiving diagnostic support; iii) subsidised business advice. We can exploit the fact that GVP vouchers were randomly allocated to provide an estimate of the joint effect of (i) and (ii).[38] These effects are reported in column 3 of Table 8 and are estimated by using PSM to

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[38] We cannot isolate the effect of selection since all GVP applicants received diagnostic support.

compare GVP applicants who received the diagnostic but not the voucher against a control group of firms constructed using eligible non-applicants.[39] These estimates can be used to isolate the effect of subsidised business advice by taking the difference between column 2 and column 3, as reported in column 4. Comparing to column (1) we see that these PSM estimates of the ATT now match those reported in column 1 in terms of magnitude and significance.

In summary, PSM provides highly misleading results for turnover and employment. Excluding the effect of diagnostic and selection using the randomisation in GVP – which would, of course, not be available in a non-trial setting – PSM estimates are similar to IV. Despite the availability of high-quality secondary data on a range of observables, unobservable characteristics that influence the selection of firms into treatment introduce a large upward bias to PSM estimates, which increases over time.[40]

Table 8. Comparison of IV and PSM estimates

| | ATT | | PSM effect of GVP selection and diagnostic | PSM effect of GVP subsidised advice (2-3) |
|---|---|---|---|---|
| | IV | PSM | | |
| | (1) | (2) | (3) | (4) |
| Turnover after 12 months | 0.082* (0.039) | 0.063** (0.020) | -0.004 (0.020) | 0.067** (0.015) |
| Turnover after 24 months | 0.024 (0.061) | 0.125** (0.032) | 0.088** (0.032) | 0.037 (0.023) |
| Employment after 12 months | -0.026 (0.020) | 0.031** (0.008) | 0.032** (0.009) | -0.001 (0.007) |
| Employment after 24 months | -0.022 (0.032) | 0.077** (0.012) | 0.066** (0.014) | 0.011 (0.011) |
| Employment after 36 months | -0.000 (0.043) | 0.100** (0.018) | 0.095** (0.018) | 0.005 (0.015) |

Note: IV and PSM estimates for percentage changes since baseline. Robust standard errors in parentheses for column 1 and bootstrapped standard errors in parentheses for columns 2 and 3. Standard errors of column 4 obtained from a Wald test for the difference between the coefficient of dummy variable for compliers and the coefficient of dummy variable for non-recipients, both derived from an OLS regression for each outcome of interest, controlling for the propensity score and with robust standard errors; ** $p<0.01$, * $p<0.05$.

# 7. Conclusions

GVP was set up as a randomised controlled trial and aimed to encourage firms to take-up business advice and to help them grow. The programme increased turnover, but not

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[39] These PSM estimates exclude compliers and non-compliers.
[40] Michalopoulos, Bloom and Hill (2004) also conclude that the bias is smaller in the short-term, when comparing experimental vs. PSM estimates.

employment, among treated firms. The impact on turnover was temporary – turnover increased 8.2% but only after the first 12 months with no significant effect at 24 months. Changes in self-reported outcomes, capabilities and practices are all broadly consistent with these headline findings. Survey estimates suggest that development of new products, rather than improved access to the UK or other markets could explain growth in turnover after 12 months.

Our results on firm performance, capabilities, and practices are consistent with other evaluations.[41] That is: i) positive effects for some outcomes, not others; ii) results that are somewhat better for turnover than employment; iii) effects that tend to be temporary (see, for example, Nagel, Huber, Van Praag & Goslinga, 2019); and iv) some improvements in capabilities and practices that are not reflected on firm performance.[42]

While it is tempting to speculate why the effect is temporary – perhaps GVP failed to address barriers to financing physical capital (Kosters & Obschonka, 2011; Rogers & Helmers, 2008; Schoonjans et. al. 2013) or improve managerial capital (Bertrand and Schoar, 2003; Bennedsen et al. 2007; Bloom and van Reenen, 2010); or, more generally, because the advice on longer term changes was of poor quality or insufficient, or not implemented by firms, GVP provides no additional evidence to distinguish between these competing hypotheses.[43]

Nevertheless, the impact on turnover is large, and while these gains in turnover are also large relative to costs, one concern with this effect is the extent to which it represents displacement.[44] Somewhere between 69% and 76% of those that received and used the voucher are firms in non-tradable sectors and medium tradable services – i.e., they are likely to serve local markets. Consistent with this, only 25% of firms are exporting at baseline and GVP has no effect on starting exporting (both statistics come from self-

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[41] For a systematic review of evaluations from OECD countries, see http://www.whatworksgrowth.org/policy-reviews/business-advice/.
[42] Lambrecht & Pirnay (2005) find no effect on performance from subsidised private external consultancy in Belgium, but improvements on management practices and capabilities. Bardasi et al. (2021) and Karlan, Knight & Udry (2015) find that business advice leads to the adoption of new and better business practices (in Tanzania and Ghana respectively) but find no effect on revenues or profits. Iacovone, Maloney & Mckenzie (2019) compare two types of business support in Colombia; both improve management practices, yet only one of them leads to a positive effect on employment.
[43] The quality and intensity of the business advice, and the extent to which firms followed it, was not monitored during the trial.
[44] Although these effects are large, other quasi-experimental evaluations find even larger effects. For example, Manaresi et al. (2022) evaluating an export voucher subsidy for SMEs in Italy find larger impacts on multiple outcomes and periods.

reported outcomes in survey data).[45] This suggests that a significant proportion of the increased sales at GVP supported firms could be at the expense of other non-supported firms, although the data available do not allow us to quantify the magnitude of this substitution effect.

One objective of GVP, being implemented as a randomised controlled trial, was to improve the robustness of evaluations and provide evidence in favour of experimental designs for assessing policy effectiveness – these results are consistent with that objective. Despite the availability of comprehensive secondary data and achieving good matching, PSM provides highly misleading results for turnover and employment. Comparing results from PSM to those from the RCT, we show that the bias introduced from unobservables is not only large, but it increases over time.

The main rationale for governments subsidising business advice is that many firms underinvest in business support due to market failures, and that firms would perform better by improving their management and production processes. The programme had a positive and large effect on turnover, albeit potentially at the expense of other non-supported local firms. The subsidised advice appears to have improved firms' capabilities and practices in a way that was consistent with the increase in turnover. However, the effect on turnover did not persist beyond 12 months. Further evidence is needed to better identify how the kind of business advice provided through GVP, or similar programmes, can generate persistent effects on firm performance. Our finding that results from PSM can be highly misleading suggests that additional randomised controlled trials will play a key part in developing this evidence.

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[45] Non-tradable corresponds to the public sector, construction, and the least tradable services, following Faggio & Overman (2014) and Jensen & Kletzer (2006). See appendix section A.5.

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# Appendix

## A.1 Data

*Variable construction and processing*

- Employment: IDBR includes number of employees and employment - where the latter is constructed by adding the number of proprietors to the former, which for many firms correspond to imputed data. We use number of employees for the analysis, to limit the measurement error that can arise from this imputed data.

- Winsorising: For employment and turnover, we obtained the annual percentage change and winsorise the top 1% of the distribution across the whole sample, including non-applicants. For employment, we also scaled the values by 1, to avoid having firms with zero employees.

- Survival: IDBR includes the death year of the firm, which we use to construct a dummy variable equal to one whenever the firms is alive and zero otherwise.

*Sample restrictions*

- We excluded 25 firms with repeated unique identifiers.

- Through IDBR we identified a small sub-set of firms that were not eligible to participate in the programme but that ultimately applied and were included (less than 0.8% of completed applications). For these, we introduced two set of rules applied to our analysis from both IDBR and survey data; i) we excluded firms that started trading in 2015 or later, those that stopped operating in 2012 or before and firms with more than 60 employees in 2014; and ii) we included firms that had between 50 and 60 employees in 2014 and those from regions in the UK beyond England, which were not originally covered by GVP.[46]

*Timing of survey and secondary data*

 For the survey data, the timing of interviews was relative to the date of diagnostic. In contrast, IDBR provides us with a 'point-in-time' snapshot regardless of the date of diagnostic. Using the same survey-cohorts for the analysis with IDBR data would

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[46] According to the IDBR, 71 firms started trading in 2015 or later (63 were allocated a voucher); 29 stopped operating in 2012 or before (20 were allocated a voucher), 33 had more than 60 employees in 2014 (24 were allocated a voucher), 15 had between 50 and 60 employees in 2014 (12 were allocated a voucher), and 13 were registered in a region not originally covered by GVP (12 were allocated a voucher).

introduce up to 12 months variation in the length of time that had passed post-diagnostic and post-treatment. Instead, for the analysis using the IDBR we create our own cohorts based on the date of diagnostic relative to the date of the IDBR. We use the date of diagnostic rather than the voucher claim date as the latter is only available for a sub-set of firms.

To do this, we annualize the IDBR data using cut-offs 6 months before and after the midpoint of IDBR in each year. As the midpoint of IDBR is the 15th of February, this gives us a year-end of 16th August. To construct the cohorts, we assign firms to this annualised data using the diagnostic date plus 103 days (median delay diagnostic to voucher claim) against these periods to define the cohort of firms. The first cohort is composed of firms whose diagnostic plus 103 days falls between the 10th of May 2014 (the first diagnostic we observe) and 16th August 2014, the second cohort is composed of firms for whom this date is between the 17th August 2014 and the 5th of August 2015 (the last diagnostic we observe).[47] For the analysis of IDBR outcomes we consider the month of application and IDBR-cohort of firms as our set of basic control variables.

We use IDBR 2014 for the baseline for the first cohort, and IDBR 2015 for the second cohort. This raises the possibility that some firms in cohort 2 will have received advice just before baseline. At most, firms that receive their diagnostics in mid-August and spend their voucher immediately will receive advice 6 months before baseline. Yet, for the average firm, having the diagnostic in mid-August implies receiving advice only 2.5 months before baseline. We do not worry about this since even 6 months is a relatively short period for introducing changes within the business and improving firm performance. Besides, the reporting of IDBR has some lags and considering the following IDBR year for these firms allows us to reduce data gaps and increases our sample size. The final three rows of Table 1 show the results of constructing IDBR cohorts in this way. For the first cohort, IDBR data for 2015 provides outcomes on average 9.7 months after the voucher was claimed. Among firms that received and used the voucher, a median of 103 days passed between the date at which they received their diagnostic and when they claimed the voucher. This means that on average, the 12-month IDBR reports outcomes somewhere between 9.7 and 13.1 months after treatment. Similarly, on average the 24-

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[47] The resultant allocation of firms to cohorts corresponds to 1,345 assigned to the 2015 cohort (74.5% allocated a voucher and 25.5% from the control group), while 12,099 firms to the 2016 cohort (75.0% allocated a voucher and 25.0% from the control group). In addition, the cohort was imputed for 13 firms with missing information for the diagnostic date; these were allocated to the 2016 cohort.

month IDBR reports outcomes between 21.7 and 25.1 months after treatment, and the 36-month IDBR reports outcomes between 33.7 and 37.1 months after.

To illustrate this, the first two rows of Table 1 report statistics for the timing of the survey relative to the date at which the firms claimed their voucher. Firms claimed the voucher after receiving the business advice, so this date marks the end of their participation in the programme.[48] We do not know how quickly firms claimed the voucher following the receipt of advice. We only know that the treatment occurred sometime between the date of the diagnostic and the date at which the voucher was claimed. The rule, not always enforced, was that vouchers had to be claimed within 90 days from the date of diagnostic. The first row of Table 1 shows that the 12-month survey reports data that, on average, comes only 7.9 months after the voucher was claimed. Not all firms used the voucher, but for those that did around 103 days passed between the date at which they received their diagnostic and when they claimed the voucher.[49] This means that on average, the 12-month survey data reports outcomes somewhere between 7.9 and 11.3 months after treatment. Similarly, on average the 24-month survey reports outcomes between 19.6 and 23 months after treatment. There is considerable variation around these means in both surveys.

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[48] In order to claim the voucher, businesses had to provide details of the advice received and evidence that an invoice was paid.
[49] The median number of days between diagnostic and claim date for firms that received and used the voucher correspond to 103, with an average of 106 days.

# A.2 Additional results, tables, and figures

Table A.1. Summary statistics at baseline for GVP firms in IDBR

| Firm characteristic in 2015 | Obs. | Mean | s.d. | Min. | Max. |
|---|---|---|---|---|---|
| Age (no. of years) | 13,385 | 9.4 | 9.2 | 1 | 42 |
| Turnover (thousands of annual £) | 10,200 | 772.5 | 1,490.0 | 1 | 38,531 |
| Turnover % growth (2014 to 2015) | 9,148 | 47.5 | 920.0 | -99.9 | 69,500 |
| Employment (no. of employees) | 8,381 | 7.3 | 8.7 | 1 | 301 |
| Exporting firm (share of firms) | 13,444 | 0.25 | 0.4 | 0 | 1 |
| Location | | | | | |
| East of England | 13,444 | 0.08 | 0.28 | 0 | 1 |
| East of Midlands | 13,444 | 0.06 | 0.24 | 0 | 1 |
| Greater London | 13,444 | 0.18 | 0.39 | 0 | 1 |
| North (East & West) | 13,444 | 0.16 | 0.36 | 0 | 1 |
| South East | 13,444 | 0.09 | 0.29 | 0 | 1 |
| South West | 13,444 | 0.11 | 0.31 | 0 | 1 |
| West Midlands | 13,444 | 0.14 | 0.34 | 0 | 1 |
| Yorkshire & Humberside | 13,444 | 0.10 | 0.30 | 0 | 1 |
| Other | 13,444 | 0.08 | 0.27 | 0 | 1 |
| Sector (SIC 2007) | | | | | |
| Agriculture, forestry & fishing | 13,389 | 0.01 | 0.11 | 0 | 1 |
| Manufacturing | 13,389 | 0.09 | 0.28 | 0 | 1 |
| Construction | 13,389 | 0.07 | 0.26 | 0 | 1 |
| Wholesale & retail trade | 13,389 | 0.18 | 0.39 | 0 | 1 |
| Transportation & Storage | 13,389 | 0.02 | 0.13 | 0 | 1 |
| Accommodation & food services | 13,389 | 0.03 | 0.18 | 0 | 1 |
| Information & communication | 13,389 | 0.12 | 0.32 | 0 | 1 |
| Real estate activities | 13,389 | 0.02 | 0.13 | 0 | 1 |
| Professional, scientific & technical | 13,389 | 0.23 | 0.42 | 0 | 1 |
| Administrative & support service | 13,389 | 0.10 | 0.30 | 0 | 1 |
| Education | 13,389 | 0.03 | 0.17 | 0 | 1 |
| Other service activities | 13,389 | 0.11 | 0.31 | 0 | 1 |
| Activities of households as employers | 13,389 | 0.01 | 0.01 | 0 | 1 |

Note: Age, turnover and employment obtained from IDBR. Exporting status, location and sector obtained from baseline information.

Table A.2. Balance of treatment and control groups at baseline for firms in IDBR

| | With covariates | Obs. | | With covariates | Obs. |
|---|---|---|---|---|---|
| Age (no. of years) | -0.242 (0.186) | 13,385 | Sector | | |
| Turnover (1,000 £) | 1.464 (31.561) | 10,200 | Aerospace | -0.002 (0.001) | 13,444 |
| No. of employees | -0.568** (0.214) | 8,381 | Agriculture | -0.001 (0.002) | 13,444 |
| Exporting firm (share) | 0.012 (0.009) | 13,444 | Automotive | 0.001 (0.003) | 13,444 |
| Turnover growth (%) | -16.989 (23.918) | 9,148 | Business services | 0.017** (0.006) | 13,444 |
| Growth expectations | -0.002 (0.011) | 13,369 | Clothing/footwear | -0.001 (0.003) | 13,444 |
| Location | | | Communications | -0.004 (0.003) | 13,444 |
| East of England | 0.005 (0.005) | 13,444 | Construction | 0.003 (0.006) | 13,444 |
| East of Midlands | -0.003 (0.005) | 13,444 | Creative media | 0.002 (0.005) | 13,444 |
| Greater London | -0.001 (0.008) | 13,444 | Education & training | 0.004 (0.004) | 13,444 |
| North (East & West) | 0.012 (0.007) | 13,444 | Electronics | 0.003 (0.003) | 13,444 |
| South East | -0.006 (0.006) | 13,444 | Environment | 0.001 (0.002) | 13,444 |
| South West | 0.006 (0.006) | 13,444 | Food & drink | 0.006 (0.004) | 13,444 |
| West Midlands | -0.009 (0.007) | 13,444 | Financial services | -0.000 (0.001) | 13,444 |
| Yorkshire & Humberside | -0.009 (0.006) | 13,444 | Fire & security | -0.004 (0.005) | 13,444 |
| Other | 0.004 (0.005) | 13,444 | Jewellery/tableware | 0.001 (0.002) | 13,444 |
| Capabilities | | | Healthcare | -0.007 (0.005) | 13,444 |
| 1: Managing people | 0.000 (0.017) | 13,197 | Household goods | -0.000 (0.003) | 13,444 |
| 2: Developing a business plan | 0.002 (0.019) | 13,364 | Leisure & tourism | -0.001 (0.003) | 13,444 |
| 3: Accessing the UK market | 0.011 (0.020) | 12,708 | Mechanical/electric | 0.002 (0.002) | 13,444 |
| 4: Accessing other markets | 0.005 (0.027) | 10,684 | Oil & gas | 0.000 (0.001) | 13,444 |
| 5: Developing new products | 0.012 (0.020) | 13,151 | Ports & logistics | -0.000 (0.001) | 13,444 |
| 6: Accessing external finance | 0.032 (0.024) | 12,087 | Computer services | 0.002 (0.005) | 13,444 |
| 7: Recruiting new employees | 0.000 (0.020) | 12,912 | Sports & leisure | 0.001 (0.003) | 13,444 |
| 8: Using IT to grow | -0.013 (0.024) | 13,336 | Others | -0.024** (0.009) | 13,444 |

Note: Each row corresponds to an OLS regression of voucher allocation on a firm characteristic. The covariates correspond to month of application, cohort and the full set of interactions between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$.

Table A.3. Balance of treatment and control groups at baseline for firms interviewed in the 12-month survey

| | With covariates | Obs. | | With covariates | Obs. |
|---|---|---|---|---|---|
| Age (no. of years) | 0.172 (0.132) | 5,579 | Sector | | |
| Self-reported turnover (£) | 40,543.6 (25,458.0) | 5,579 | Aerospace | -0.001 (0.002) | 5,579 |
| Self-reported no. of employees | 0.010 (0.244) | 5,579 | Agriculture | -0.003 (0.004) | 5,579 |
| Exporting firm (share) | 0.006 (0.014) | 5,579 | Automotive | -0.006 (0.005) | 5,579 |
| Having a business plan | -0.025 (0.016) | 5,579 | Business services | 0.013 (0.008) | 5,579 |
| Growth expectations | -0.030 (0.018) | 5,550 | Clothing/footwear | -0.003 (0.005) | 5,579 |
| Location | | | Communications | 0.005 (0.004) | 5,579 |
| East of England | -0.007 (0.009) | 5,579 | Construction | 0.007 (0.008) | 5,579 |
| East of Midlands | -0.001 (0.008) | 5,579 | Creative media | -0.004 (0.009) | 5,579 |
| Greater London | -0.022 (0.013) | 5,579 | Education & training | -0.007 (0.009) | 5,579 |
| North (East & West) | 0.009 (0.011) | 5,579 | Electronics | 0.003 (0.005) | 5,579 |
| South East | 0.007 (0.009) | 5,579 | Environment | -0.001 (0.004) | 5,579 |
| South West | 0.010 (0.010) | 5,579 | Food & drink | 0.007 (0.006) | 5,579 |
| West Midlands | 0.007 (0.011) | 5,579 | Financial services | 0.001 (0.002) | 5,579 |
| Yorkshire & Humberside | -0.012 (0.010) | 5,579 | Fire & security | -0.015* (0.008) | 5,579 |
| Other | 0.009 (0.008) | 5,579 | Jewellery/tableware | 0.004 (0.003) | 5,579 |
| Capabilities | | | Healthcare | -0.011 (0.007) | 5,579 |
| 1: Managing people | 0.045 (0.027) | 5,423 | Household goods | -0.001 (0.004) | 5,579 |
| 2: Developing a business plan | 0.039 (0.030) | 5,552 | Leisure & tourism | 0.005 (0.004) | 5,579 |
| 3: Accessing the UK market | 0.033 (0.032) | 5,249 | Mechanical/electric | 0.004 (0.003) | 5,579 |
| 4: Accessing other markets | 0.057 (0.042) | 4,410 | Oil & gas | -0.001 (0.002) | 5,579 |
| 5: Developing new products | 0.052 (0.032) | 5,449 | Ports & logistics | -0.001 (0.001) | 5,579 |
| 6: Accessing external finance | 0.034 (0.038) | 4,939 | Computer services | -0.000 (0.007) | 5,579 |
| 7: Recruiting new employees | 0.046 (0.032) | 5,246 | Sports & leisure | -0.006 (0.005) | 5,579 |
| 8: Using IT to grow | 0.033 (0.038) | 5,535 | Others | 0.002 (0.013) | 5,579 |

Note: Each row corresponds to an OLS regression of voucher allocation on a firm characteristic. The covariates correspond to month of application, cohort and the full set of interactions between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. Robust standard errors in parentheses; ** p<0.01, * p<0.05.

Table A.4. Balance of treatment and control groups at baseline for firms interviewed in the 24-month survey

| | With covariates | Obs. | | With covariates | Obs. |
|---|---|---|---|---|---|
| Age (no. of years) | 0.414 (0.239) | 1,965 | Sector | | |
| Self-reported turnover (£) | 72,970.8 (43,148.1) | 1,965 | Aerospace | 0.002 (0.003) | 1,965 |
| Self-reported no. of employees | 0.581 (0.417) | 1,965 | Agriculture | -0.004 (0.007) | 1,965 |
| Exporting firm (share) | 0.012 (0.026) | 1,965 | Automotive | -0.006 (0.009) | 1,965 |
| Having a business plan | -0.061* (0.028) | 1,965 | Business services | 0.042** (0.014) | 1,965 |
| Growth expectations | -0.019 (0.031) | 1,956 | Clothing/footwear | 0.006 (0.008) | 1,965 |
| Location | | | Communications | -0.004 (0.008) | 1,965 |
| East of England | -0.034 (0.018) | 1,965 | Construction | 0.027* (0.012) | 1,965 |
| East of Midlands | 0.002 (0.014) | 1,965 | Creative media | -0.009 (0.017) | 1,965 |
| Greater London | 0.021 (0.021) | 1,965 | Education & training | -0.014 (0.016) | 1,965 |
| North (East & West) | -0.009 (0.020) | 1,965 | Electronics | 0.015* (0.007) | 1,965 |
| South East | 0.003 (0.017) | 1,965 | Environment | 0.005 (0.005) | 1,965 |
| South West | 0.003 (0.018) | 1,965 | Food & drink | 0.003 (0.012) | 1,965 |
| West Midlands | 0.002 (0.020) | 1,965 | Financial services | 0.002 (0.003) | 1,965 |
| Yorkshire & Humberside | -0.005 (0.016) | 1,965 | Fire & security | -0.019 (0.013) | 1,965 |
| Other | 0.018 (0.014) | 1,965 | Jewellery/tableware | 0.003 (0.006) | 1,965 |
| Capabilities | | | Healthcare | -0.013 (0.013) | 1,965 |
| 1: Managing people | 0.039 (0.047) | 1,905 | Household goods | 0.000 (0.008) | 1,965 |
| 2: Developing a business plan | 0.049 (0.053) | 1,954 | Leisure & tourism | 0.008 (0.007) | 1,965 |
| 3: Accessing the UK market | 0.020 (0.057) | 1,852 | Mechanical/electric | 0.001 (0.008) | 1,965 |
| 4: Accessing other markets | 0.082 (0.077) | 1,555 | Oil & gas | -0.005 (0.005) | 1,965 |
| 5: Developing new products | 0.081 (0.058) | 1,924 | Ports & logistics | -0.001 (0.003) | 1,965 |
| 6: Accessing external finance | -0.025 (0.071) | 1,722 | Computer services | -0.009 (0.013) | 1,965 |
| 7: Recruiting new employees | 0.027 (0.057) | 1,835 | Sports & leisure | -0.011 (0.009) | 1,965 |
| 8: Using IT to grow | 0.041 (0.067) | 1,951 | Others | -0.017 (0.024) | 1,965 |

Note: Each row corresponds to an OLS regression of voucher allocation on a firm characteristic. The covariates correspond to month of application, cohort and the full set of interactions between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. Robust standard errors in parentheses; ** p<0.01, * p<0.05.

Table A.5. First stage of IV estimation for firms in survey data and IDBR

| | IDBR | | 12-month survey | | 24-month survey | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (1) | (2) | (1) | (2) |
| Allocation of voucher | 0.344** (0.005) | 0.344** (0.004) | 0.438** (0.008) | 0.434** (0.008) | 0.514** (0.013) | 0.512** (0.014) |
| Basic controls & Full set of interactions | No | Yes | No | Yes | No | Yes |
| Obs. | 13,444 | 13,444 | 5,579 | 5,579 | 1,965 | 1,965 |

Note: OLS regression for use of the voucher on allocation of voucher. Basic controls include month of application and cohort. The full set of interactions corresponds to all combinations between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. Robust standard errors in parentheses. ** p<0.01, * p<0.05.

Table A.6. OLS and IV estimates for survival

| | OLS (1) | OLS (2) | OLS (3) | IV (1) | IV (2) | IV (3) |
|---|---|---|---|---|---|---|
| | *Survival after 12 months* | | | | | |
| Allocation of voucher | -0.003 (0.006) | -0.003 (0.006) | -0.003 (0.006) | N/A | N/A | N/A |
| Take up of the voucher | N/A | N/A | N/A | -0.010 (0.017) | -0.008 (0.017) | -0.009 (0.017) |
| Obs. | 13,274 | 13,274 | 13,274 | 13,274 | 13,274 | 13,274 |
| | *Survival after 24 months* | | | | | |
| Allocation of voucher | -0.008 (0.007) | -0.008 (0.007) | -0.008 (0.007) | N/A | N/A | N/A |
| Take up of the voucher | N/A | N/A | N/A | -0.023 (0.020) | -0.022 (0.020) | -0.023 (0.020) |
| Obs. | 13,274 | 13,274 | 13,274 | 13,274 | 13,274 | 13,274 |
| | *Survival after 36 months* | | | | | |
| Allocation of voucher | -0.001 (0.008) | -0.001 (0.008) | -0.001 (0.008) | N/A | N/A | N/A |
| Take up of the voucher | N/A | N/A | N/A | -0.003 (0.023) | -0.002 (0.023) | -0.002 (0.023) |
| Obs. | 13,274 | 13,274 | 13,274 | 13,274 | 13,274 | 13,274 |
| Basic controls | No | Yes | Yes | No | Yes | Yes |
| Type of diagnostic & theme of advice | No | Yes | N/A | No | Yes | N/A |
| Full set of interactions | No | No | Yes | No | No | Yes |

Note: OLS and IV regressions for survival status since baseline. Robust standard errors in parentheses; ** p<0.01, * p<0.05. Basic controls include month of application and cohort. The full set of interactions corresponds to all combinations between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. N/A: not applicable.

Table A.7. Robustness check 1: Using the same sample of firms across time for each outcome

| | OLS (1) | OLS (2) | IV (1) | IV (2) |
|---|---|---|---|---|
| *Turnover after 12 months* | | | | |
| Allocation of voucher | 0.026 (0.014) | 0.026 (0.014) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.072 (0.039) | 0.071 (0.039) |
| Obs. | 8,827 | 8,827 | 8,827 | 8,827 |
| *Turnover after 24 months* | | | | |
| Allocation of voucher | 0.012 (0.022) | 0.011 (0.022) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.032 (0.061) | 0.029 (0.061) |
| Obs. | 8,827 | 8,827 | 8,827 | 8,827 |
| *Employment after 12 months* | | | | |
| Allocation of voucher | -0.007 (0.007) | -0.008 (0.007) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.020 (0.021) | -0.022 (0.021) |
| Obs. | 5,859 | 5,859 | 5,859 | 5,859 |
| *Employment after 24 months* | | | | |
| Allocation of voucher | -0.005 (0.012) | -0.005 (0.012) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.014 (0.033) | -0.015 (0.033) |
| Obs. | 5,859 | 5,859 | 5,859 | 5,859 |
| *Employment after 36 months* | | | | |
| Allocation of voucher | 0.002 (0.015) | 0.001 (0.015) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.005 (0.043) | 0.004 (0.044) |
| Obs. | 5,859 | 5,859 | 5,859 | 5,859 |
| Basic controls & full set of interactions | No | Yes | No | Yes |

Note: OLS and IV regressions for percentage change since baseline. Basic controls include month of application and cohort. The full set of interactions corresponds to all combinations between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. N/A: not applicable. Robust standard errors in parentheses; ** p<0.01, * p<0.05.

Table A.8. Robustness check 2: Excluding firms that enrolled in the last month of the application period

| | OLS (1) | OLS (2) | IV (1) | IV (2) |
|---|---|---|---|---|
| | *Turnover after 12 months* | | | |
| Allocation of voucher | 0.040* (0.017) | 0.039* (0.017) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.108* (0.047) | 0.106* (0.047) |
| Obs. | 6,442 | 6,442 | 6,442 | 6,442 |
| | *Turnover after 24 months* | | | |
| Allocation of voucher | 0.019 (0.028) | 0.017 (0.028) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.051 (0.074) | 0.046 (0.074) |
| Obs. | 6,040 | 6,040 | 6,040 | 6,040 |
| | *Employment after 12 months* | | | |
| Allocation of voucher | -0.013 (0.008) | -0.013 (0.008) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.036 (0.024) | -0.037 (0.024) |
| Obs. | 5,020 | 5,020 | 5,020 | 5,020 |
| | *Employment after 24 months* | | | |
| Allocation of voucher | -0.012 (0.013) | -0.012 (0.013) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.034 (0.038) | -0.034 (0.038) |
| Obs. | 4,550 | 4,550 | 4,550 | 4,550 |
| | *Employment after 36 months* | | | |
| Allocation of voucher | 0.002 (0.018) | 0.002 (0.018) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.006 (0.050) | 0.007 (0.051) |
| Obs. | 4,175 | 4,175 | 4,175 | 4,175 |
| Basic controls & full set of interactions | No | Yes | No | Yes |

Note: OLS and IV regressions for percentage change since baseline. Basic controls include month of application and cohort. The interaction term corresponds to a double interaction between type of diagnostic and theme of advice. N/A: not applicable. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$.

Table A.9. Robustness check 3: Excluding firms that did not select the suggested theme of advice

| | OLS (1) | OLS (2) | IV (1) | IV (2) |
|---|---|---|---|---|
| | *Turnover after 12 months* | | | |
| Allocation of voucher | 0.038* (0.016) | 0.039* (0.016) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.098* (0.041) | 0.102* (0.041) |
| Obs. | 7,825 | 7,825 | 7,825 | 7,825 |
| | *Turnover after 24 months* | | | |
| Allocation of voucher | 0.017 (0.025) | 0.018 (0.025) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.045 (0.064) | 0.047 (0.064) |
| Obs. | 7,339 | 7,339 | 7,339 | 7,339 |
| | *Employment after 12 months* | | | |
| Allocation of voucher | -0.009 (0.008) | -0.009 (0.008) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.024 (0.021) | -0.025 (0.021) |
| Obs. | 6,065 | 6,065 | 6,065 | 6,065 |
| | *Employment after 24 months* | | | |
| Allocation of voucher | -0.013 (0.012) | -0.013 (0.012) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.036 (0.033) | -0.036 (0.033) |
| Obs. | 5,485 | 5,485 | 5,485 | 5,485 |
| | *Employment after 36 months* | | | |
| Allocation of voucher | -0.006 (0.016) | -0.006 (0.016) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.017 (0.045) | -0.017 (0.044) |
| Obs. | 5,066 | 5,066 | 5,066 | 5,066 |
| Basic controls & full set of interactions | No | Yes | No | Yes |

Note: OLS and IV regressions for percentage change since baseline. Basic controls include month of application and cohort. The full set of interactions corresponds to all combinations between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. N/A: not applicable. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$.

Table A.10. Robustness check 4: Treating non-recipients that paid for GVP advice as participants

| | OLS (1) | OLS (2) | IV (1) | IV (2) |
|---|---|---|---|---|
| | *Turnover after 12 months* | | | |
| Allocation of voucher | 0.030* (0.014) | 0.030* (0.014) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.090* (0.043) | 0.089* (0.043) |
| Obs. | 9,452 | 9,452 | 9,452 | 9,452 |
| | *Turnover after 24 months* | | | |
| Allocation of voucher | 0.010 (0.022) | 0.009 (0.022) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.029 (0.066) | 0.026 (0.066) |
| Obs. | 8,855 | 8,855 | 8,855 | 8,855 |
| | *Employment after 12 months* | | | |
| Allocation of voucher | -0.008 (0.007) | -0.009 (0.007) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.027 (0.022) | -0.028 (0.022) |
| Obs. | 7,326 | 7,326 | 7,326 | 7,326 |
| | *Employment after 24 months* | | | |
| Allocation of voucher | -0.007 (0.011) | -0.008 (0.011) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.023 (0.034) | -0.024 (0.034) |
| Obs. | 6,606 | 6,606 | 6,606 | 6,606 |
| | *Employment after 36 months* | | | |
| Allocation of voucher | 0.000 (0.015) | -0.000 (0.015) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.000 (0.046) | -0.001 (0.046) |
| Obs. | 6,084 | 6,084 | 6,084 | 6,084 |
| Basic controls & full set of interactions | No | Yes | No | Yes |

Note: OLS and IV regressions for percentage change since baseline. Basic controls include month of application and cohort. The full set of interactions corresponds to all combinations between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. N/A: not applicable. Robust standard errors in parentheses; ** p<0.01, * p<0.05.

Table A.11. Robustness check 5: Extending the definition of treatment to include firms that paid for any type of non-GVP advice

| | OLS (1) | OLS (2) | IV (1) | IV (2) |
|---|---|---|---|---|
| | *Turnover after 12 months* | | | |
| Allocation of voucher | 0.030* (0.014) | 0.030* (0.014) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.092* (0.044) | 0.091* (0.044) |
| Obs. | 9,452 | 9,452 | 9,452 | 9,452 |
| | *Turnover after 24 months* | | | |
| Allocation of voucher | 0.010 (0.022) | 0.009 (0.022) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.031 (0.069) | 0.028 (0.069) |
| Obs. | 8,855 | 8,855 | 8,855 | 8,855 |
| | *Employment after 12 months* | | | |
| Allocation of voucher | -0.008 (0.007) | -0.009 (0.007) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.028 (0.023) | -0.029 (0.023) |
| Obs. | 7,326 | 7,326 | 7,326 | 7,326 |
| | *Employment after 24 months* | | | |
| Allocation of voucher | -0.007 (0.011) | -0.008 (0.011) | N/A | N/A |
| Take up of the voucher | N/A | N/A | -0.025 (0.036) | -0.025 (0.036) |
| Obs. | 6,606 | 6,606 | 6,606 | 6,606 |
| | *Employment after 36 months* | | | |
| Allocation of voucher | 0.000 (0.015) | -0.000 (0.015) | N/A | N/A |
| Take up of the voucher | N/A | N/A | 0.000 (0.049) | -0.001 (0.049) |
| Obs. | 6,084 | 6,084 | 6,084 | 6,084 |
| Basic controls & full set of interactions | No | Yes | No | Yes |

Note: OLS and IV regressions for percentage change since baseline. Basic controls include month of application and cohort. The full set of interactions corresponds to all combinations between type of diagnostic, theme of advice and a dummy indicating if the firm applied in last month. N/A: not applicable. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$.

Table A.12. OLS and IV estimates by type of diagnostic for firms in the IDBR

| | Personal | | Online | |
|---|---|---|---|---|
| | OLS | IV | OLS | IV |
| | (1) | (2) | (1) | (2) |
| *Turnover after 12 months* | | | | |
| Allocation of voucher | 0.033 (0.021) | N/A | 0.025 (0.020) | N/A |
| Take up of the voucher | N/A | 0.080 (0.050) | N/A | 0.083 (0.064) |
| Obs. | 4,759 | 4,759 | 4,693 | 4,693 |
| *Turnover after 24 months* | | | | |
| Allocation of voucher | 0.000 (0.032) | N/A | 0.012 (0.030) | N/A |
| Take up of the voucher | N/A | 0.000 (0.077) | N/A | 0.039 (0.097) |
| Obs. | 4,453 | 4,453 | 4,402 | 4,402 |
| *Employment after 12 months* | | | | |
| Allocation of voucher | -0.018 (0.010) | N/A | 0.000 (0.010) | N/A |
| Take up of the voucher | N/A | -0.046 (0.025) | N/A | 0.001 (0.034) |
| Obs. | 3,702 | 3,702 | 3,624 | 3,624 |
| *Employment after 24 months* | | | | |
| Allocation of voucher | -0.024 (0.016) | N/A | 0.011 (0.015) | N/A |
| Take up of the voucher | N/A | -0.061 (0.039) | N/A | 0.038 (0.052) |
| Obs. | 3,360 | 3,360 | 3,246 | 3,246 |
| *Employment after 36 months* | | | | |
| Allocation of voucher | -0.006 (0.022) | N/A | 0.009 (0.021) | N/A |
| Take up of the voucher | N/A | -0.016 (0.054) | N/A | 0.030 (0.070) |
| Obs. | 3,094 | 3,094 | 2,990 | 2,990 |

Note: OLS and IV regressions for percentage change since baseline using our preferred specification, which controls for cohort and month of application and includes the full set of interactions between type of diagnostic, theme of advice and dummy for last month of application. N/A: not applicable. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$.

Table A.13. OLS and IV estimates for firms in the IDBR by theme of advice

| | Sales & marketing | | Raising finance | | Leadership & management | | Workforce development | | Digital technologies | |
|---|---|---|---|---|---|---|---|---|---|---|
| | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | (1) | (2) | (1) | (2) | (1) | (2) | (1) | (2) | (1) | (2) |
| *Turnover after 12 months* | | | | | | | | | | |
| Allocation of voucher | 0.068** (0.021) | N/A | -0.052 (0.049) | N/A | 0.024 (0.031) | N/A | -0.005 (0.050) | N/A | 0.005 (0.028) | N/A |
| Take up of the voucher | N/A | 0.188** (0.059) | N/A | -0.132 (0.125) | N/A | 0.074 (0.093) | N/A | -0.029 (0.286) | N/A | 0.013 (0.071) |
| Obs. | 4,354 | 4,354 | 1,220 | 1,220 | 1,388 | 1,388 | 495 | 495 | 1,995 | 1,995 |
| *Turnover after 24 months* | | | | | | | | | | |
| Allocation of voucher | 0.073* (0.032) | N/A | -0.155 (0.084) | N/A | 0.003 (0.044) | N/A | -0.007 (0.087) | N/A | -0.023 (0.046) | N/A |
| Take up of the voucher | N/A | 0.198* (0.089) | N/A | -0.390 (0.210) | N/A | 0.010 (0.129) | N/A | -0.042 (0.491) | N/A | -0.058 (0.114) |
| Obs. | 4,078 | 4,078 | 1,122 | 1,122 | 1,319 | 1,319 | 463 | 463 | 1,873 | 1,873 |
| *Employment after 12 months* | | | | | | | | | | |
| Allocation of voucher | -0.008 (0.010) | N/A | -0.017 (0.021) | N/A | 0.003 (0.017) | N/A | -0.050 (0.033) | N/A | -0.002 (0.015) | N/A |
| Take up of the voucher | N/A | -0.025 (0.031) | N/A | -0.043 (0.054) | N/A | 0.010 (0.052) | N/A | -0.269 (0.181) | N/A | -0.004 (0.039) |
| Obs. | 3,392 | 3,392 | 976 | 976 | 1,103 | 1,103 | 389 | 389 | 1,466 | 1,466 |
| *Employment after 24 months* | | | | | | | | | | |
| Allocation of voucher | -0.004 (0.016) | N/A | -0.020 (0.032) | N/A | -0.004 (0.029) | N/A | -0.057 (0.052) | N/A | 0.005 (0.023) | N/A |
| Take up of the voucher | N/A | -0.013 (0.047) | N/A | -0.049 (0.079) | N/A | -0.013 (0.088) | N/A | -0.322 (0.299) | N/A | 0.013 (0.061) |
| Obs. | 3,083 | 3,083 | 866 | 866 | 993 | 993 | 355 | 355 | 1,309 | 1,309 |
| *Employment after 36 months* | | | | | | | | | | |
| Allocation of voucher | 0.006 (0.022) | N/A | -0.045 (0.046) | N/A | -0.039 (0.040) | N/A | 0.020 (0.065) | N/A | 0.042 (0.031) | N/A |
| Take up of the voucher | N/A | 0.019 (0.064) | N/A | -0.112 (0.113) | N/A | -0.118 (0.121) | N/A | 0.110 (0.361) | N/A | 0.107 (0.079) |
| Obs. | 2,849 | 2,849 | 768 | 768 | 932 | 932 | 328 | 328 | 1,207 | 1,207 |

Note: Regressions for percentage change since baseline using our preferred specification, which controls for cohort and month of application and includes the full set of interactions between type of diagnostic, theme of advice and dummy for last month of application. N/A: not applicable. Robust standard errors in parentheses; ** p<0.01, * p<0.05.

Figure A.1. Distribution of the propensity score for treated and control firms
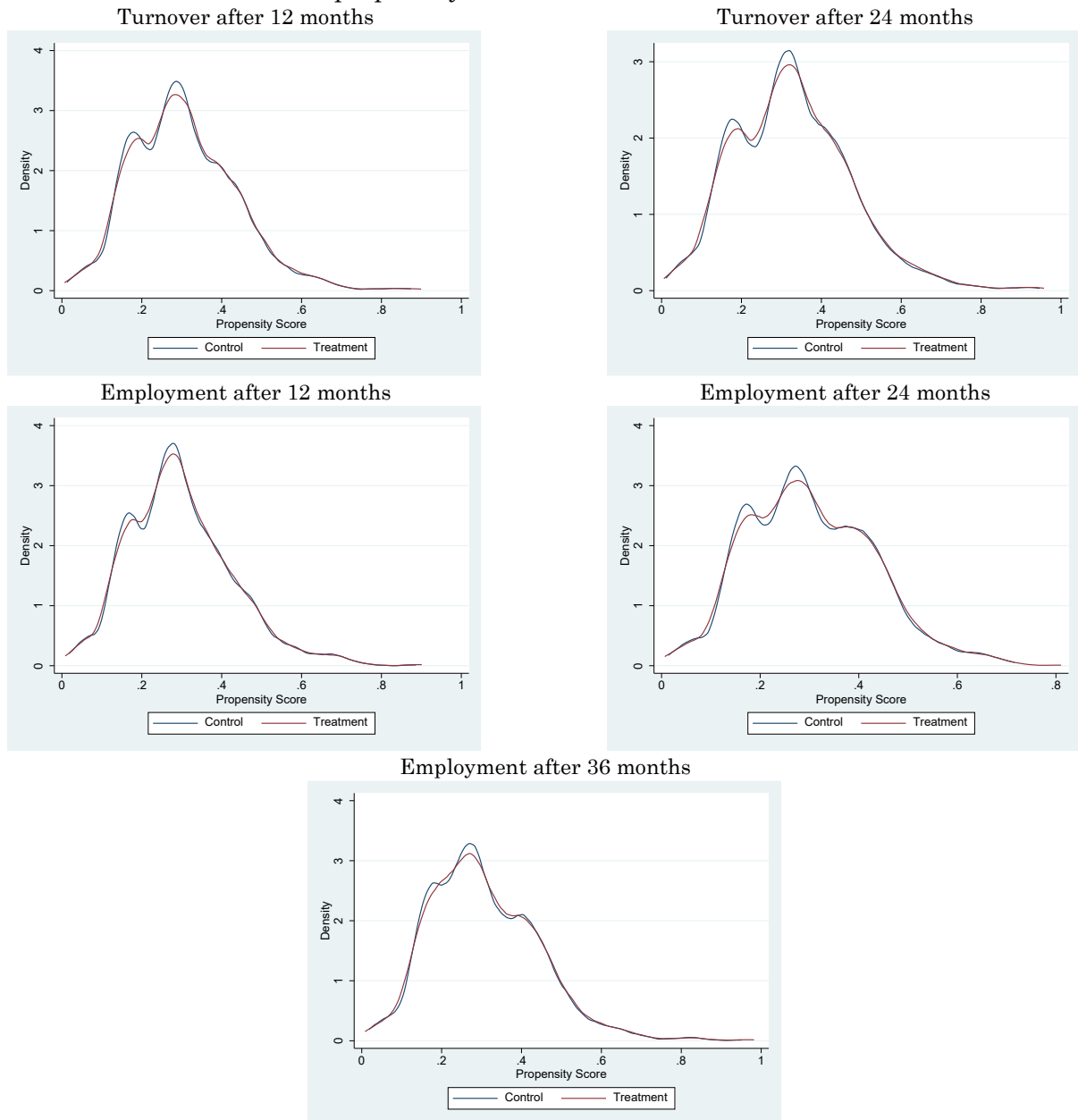


Note: For each outcome and period, the graph shows the distribution of the estimated propensity score using our preferred probit specification for the group of GVP compliers and the matched control group.

Table A.14. Balance of treatment and control groups after controlling for the propensity score for turnover

| | After 12 months | | After 24 months | |
|---|---|---|---|---|
| | Coeff. | Obs. | Coeff. | Obs. |
| Years trading | -0.207 (0.193) | 5,395 | -0.044 (0.198) | 5,153 |
| Turnover in 2015 | -12.307 (54.178) | 5,395 | -49.448 (53.933) | 5,153 |
| Turnover in 2014 | -8.308 (46.952) | 5,395 | -39.343 (46.221) | 5,153 |
| Employment in 2015 | 0.355 (0.225) | 5,395 | 0.176 (0.223) | 5,153 |
| Location | | | | |
| East of England | 0.001 (0.008) | 5,395 | -0.008 (0.009) | 5,153 |
| East of Midlands | 0.008 (0.008) | 5,395 | 0.006 (0.008) | 5,153 |
| Greater London | 0.008 (0.010) | 5,395 | 0.005 (0.010) | 5,153 |
| Home counties | 0.006 (0.007) | 5,395 | 0.010 (0.007) | 5,153 |
| North East | -0.001 (0.006) | 5,395 | 0.006 (0.006) | 5,153 |
| North West | -0.001 (0.011) | 5,395 | -0.002 (0.011) | 5,153 |
| South East | -0.008 (0.008) | 5,395 | -0.003 (0.008) | 5,153 |
| South West | -0.013 (0.010) | 5,395 | -0.005 (0.010) | 5,153 |
| West Midlands | 0.008 (0.010) | 5,395 | 0.001 (0.011) | 5,153 |
| Yorkshire & Humberside | -0.007 (0.010) | 5,395 | -0.008 (0.010) | 5,153 |
| Other | -0.001 (0.004) | 5,395 | -0.002 (0.004) | 5,153 |
| Sector | | | | |
| Agriculture, forestry & fishing | 0.000 (0.003) | 5,395 | -0.001 (0.004) | 5,153 |
| Manufacturing | -0.006 (0.009) | 5,395 | -0.014 (0.009) | 5,153 |
| Construction | 0.007 (0.007) | 5,395 | 0.003 (0.007) | 5,153 |
| Wholesale & retail | -0.012 (0.013) | 5,395 | -0.031* (0.013) | 5,153 |
| Transportation & storage | 0.003 (0.003) | 5,395 | 0.001 (0.003) | 5,153 |
| Accommodation & food services | 0.001 (0.005) | 5,395 | -0.001 (0.005) | 5,153 |
| Information & communication | 0.024* (0.010) | 5,395 | 0.028** (0.010) | 5,153 |
| Real estate | -0.000 (0.005) | 5,395 | -0.002 (0.005) | 5,153 |
| Professional, scientific & technical | 0.005 (0.014) | 5,395 | 0.017 (0.014) | 5,153 |
| Administrative & support services | -0.007 (0.010) | 5,395 | -0.003 (0.010) | 5,153 |
| Education | -0.002 (0.005) | 5,395 | 0.002 (0.005) | 5,153 |
| Other | -0.013 (0.008) | 5,395 | 0.002 (0.008) | 5,153 |

Note: Each row corresponds to an OLS regression of treatment status on a firm characteristic, controlling for the propensity score. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$.

Table A.15. Balance of treatment and control groups after controlling for the propensity score for employment

| | After 12 months | | After 24 months | | After 36 months | |
|---|---|---|---|---|---|---|
| | Coeff. | Obs. | Coeff. | Obs. | Coeff. | Obs. |
| Years trading | -0.086 (0.195) | 5,455 | -0.067 (0.204) | 5,061 | -0.163 (0.211) | 4,688 |
| Turnover in 2015 | -54.690 (47.623) | 5,455 | -31.829 (63.659) | 5,061 | -72.497 (99.004) | 4,688 |
| Turnover in 2014 | -59.197 (43.817) | 5,455 | -4.092 (41.708) | 5,061 | -45.595 (60.023) | 4,688 |
| Employment in 2015 | 0.019 (0.187) | 5,455 | 0.424* (0.201) | 5,061 | 0.434* (0.211) | 4,688 |
| Location | | | | | | |
| East of England | -0.003 (0.008) | 5,455 | 0.006 (0.008) | 5,061 | 0.001 (0.009) | 4,688 |
| East of Midlands | 0.001 (0.008) | 5,455 | -0.015 (0.009) | 5,061 | 0.008 (0.008) | 4,688 |
| Greater London | 0.010 (0.010) | 5,455 | 0.008 (0.010) | 5,061 | 0.014 (0.011) | 4,688 |
| Home counties | -0.004 (0.007) | 5,455 | 0.010 (0.007) | 5,061 | 0.007 (0.007) | 4,688 |
| North East | -0.005 (0.006) | 5,455 | -0.004 (0.006) | 5,061 | 0.003 (0.007) | 4,688 |
| North West | 0.008 (0.011) | 5,455 | 0.000 (0.011) | 5,061 | -0.005 (0.012) | 4,688 |
| South East | -0.002 (0.008) | 5,455 | -0.006 (0.008) | 5,061 | 0.003 (0.009) | 4,688 |
| South West | -0.002 (0.010) | 5,455 | -0.006 (0.010) | 5,061 | -0.015 (0.011) | 4,688 |
| West Midlands | 0.003 (0.011) | 5,455 | -0.002 (0.011) | 5,061 | -0.016 (0.012) | 4,688 |
| Yorkshire & Humberside | -0.004 (0.010) | 5,455 | 0.007 (0.010) | 5,061 | 0.000 (0.011) | 4,688 |
| Other | -0.001 (0.004) | 5,455 | 0.002 (0.003) | 5,061 | -0.001 (0.004) | 4,688 |
| Sector | | | | | | |
| Agriculture, forestry & fishing | -0.000 (0.004) | 5,455 | -0.004 (0.004) | 5,061 | -0.002 (0.004) | 4,688 |
| Manufacturing | -0.013 (0.009) | 5,455 | -0.012 (0.009) | 5,061 | 0.009 (0.009) | 4,688 |
| Construction | 0.005 (0.007) | 5,455 | 0.001 (0.007) | 5,061 | 0.000 (0.007) | 4,688 |
| Wholesale & retail | -0.002 (0.012) | 5,455 | -0.006 (0.013) | 5,061 | -0.003 (0.014) | 4,688 |
| Transportation & storage | -0.003 (0.004) | 5,455 | 0.003 (0.003) | 5,061 | -0.001 (0.004) | 4,688 |
| Accommodation & food services | -0.002 (0.005) | 5,455 | 0.002 (0.004) | 5,061 | 0.002 (0.004) | 4,688 |
| Information & communication | 0.019 (0.010) | 5,455 | 0.017 (0.010) | 5,061 | 0.020 (0.010) | 4,688 |
| Real estate | 0.000 (0.005) | 5,455 | -0.001 (0.005) | 5,061 | 0.004 (0.005) | 4,688 |
| Professional, scientific & technical | 0.021 (0.014) | 5,455 | 0.005 (0.014) | 5,061 | -0.006 (0.015) | 4,688 |
| Administrative & support services | -0.008 (0.010) | 5,455 | -0.005 (0.010) | 5,061 | -0.003 (0.010) | 4,688 |
| Education | -0.003 (0.005) | 5,455 | 0.000 (0.005) | 5,061 | -0.000 (0.005) | 4,688 |
| Other | -0.014 (0.008) | 5,455 | 0.001 (0.008) | 5,061 | -0.021* (0.009) | 4,688 |

Note: Each row corresponds to an OLS regression of treatment status on a firm characteristic, controlling for the propensity score. Robust standard errors in parentheses; ** p<0.01, * p<0.05.

Table A.16. PSM specification search for turnover

| Outcome | No. | Observables used for the matching process | ATT | Avg. diff. in the PS | Significant differences at baseline after controlling for the PS |
|---|---|---|---|---|---|
| Turnover after 12 months | (1) | Turnover & employment in 2015 | 0.073** (0.020) | 0.0037* (0.0017) | Age, 9 regions and 9 sectors |
| | (2) | Turnover & employment in 2015, age and aged squared | 0.076** (0.019) | -0.0002 (0.0019) | 8 regions and 7 sectors |
| | (3) | Turnover & employment in 2015, age, aged squared and region | 0.059** (0.020) | -0.0007 (0.0033) | 1 region and 8 sectors |
| | (4) | Turnover & employment in 2015, age, aged squared, region and sector | 0.064** (0.021) | -0.0008 (0.0037) | 1 region and 1 sector |
| | (5) | Turnover & employment in 2015, age, aged squared, region, sector, turnover & employment in 2014, and interaction term for London and the biggest sectors | 0.063** (0.020) | -0.0014 (0.0041) | 1 sector |
| Turnover after 24 months | (1) | Turnover & employment in 2015 | 0.135** (0.028) | 0.0041* (0.0021) | Age, 9 regions and 9 sectors |
| | (2) | Turnover & employment in 2015, age and aged squared | 0.135** (0.028) | -0.0004 (0.0024) | 10 regions and 9 sectors |
| | (3) | Turnover & employment in 2015, age, aged squared and region | 0.112** (0.031) | -0.0005 (0.0038) | 9 sectors |
| | (4) | Turnover & employment in 2015, age, aged squared, region and sector | 0.126** (0.029) | -0.0010 (0.0043) | 2 sectors |
| | (5) | Turnover & employment in 2015, age, aged squared, region, sector, turnover & employment in 2014, and interaction term for London and the biggest sectors, and interaction term for London and the biggest sectors | 0.125** (0.032) | -0.0012 (0.0047) | None |

Note: Bootstrapped standard errors in parentheses; ** p<0.01, * p<0.05.

Table A.17. PSM specification search for employment

| Outcome | No. | Observables used for the matching process | ATT | Avg. diff. in the PS | Significant differences at baseline after controlling for the PS |
|---|---|---|---|---|---|
| Employment after 12 months | (1) | Turnover & employment in 2015 | 0.049** (0.008) | 0.0043* (0.0017) | Age, 10 regions and 8 sectors |
| | (2) | Turnover & employment in 2015, age and aged squared | 0.036** (0.007) | -0.0001 (0.0018) | 9 regions and 7 sectors |
| | (3) | Turnover & employment in 2015, age, aged squared and region | 0.039** (0.008) | -0.0004 (0.0031) | Turnover in 2014 and 2015 and 6 sectors |
| | (4) | Turnover & employment in 2015, age, aged squared, region and sector | 0.043** (0.008) | -0.0016 (0.0035) | Employment in 2015 and 2 sectors |
| | (5) | Turnover & employment in 2015, age, aged squared, region, sector, turnover & employment in 2014, and interaction term for London and the biggest sectors | 0.031** (0.008) | -0.0008 (0.0041) | None |
| Employment after 24 months | (1) | Turnover & employment in 2015 | 0.074** (0.012) | 0.0016* (0.0007) | Age, employment in 2015, 7 regions and 8 sectors |
| | (2) | Turnover & employment in 2015, age and aged squared | 0.065** (0.013) | -0.0002 (0.0010) | 8 regions and 6 sectors |
| | (3) | Turnover & employment in 2015, age, aged squared and region | 0.072** (0.013) | -0.0003 (0.0032) | Employment in 2015, 1 region and 6 sectors |
| | (4) | Turnover & employment in 2015, age, aged squared, region and sector | 0.072** (0.013) | -0.0010 (0.0036) | Age, employment in 2015, 1 region and 3 sectors |
| | (5) | Turnover & employment in 2015, age, aged squared, region, sector, turnover & employment in 2014, and interaction term for London and the biggest sectors | 0.077** (0.012) | -0.0010 (0.0042) | Employment in 2015 |
| Employment after 36 months | (1) | Turnover & employment in 2015 | 0.124** (0.016) | 0.0011 (0.0009) | Age, 7 regions and 8 sectors |
| | (2) | Turnover & employment in 2015, age and aged squared | 0.108** (0.016) | -0.0002 (0.0012) | Turnover in 2014 and 2015, employment in 2015, and 7 sectors |
| | (3) | Turnover & employment in 2015, age, aged squared and region | 0.114** (0.017) | -0.0007 (0.0035) | Turnover in 2014 and 2015, employment in 2015, and 7 sectors |
| | (4) | Turnover & employment in 2015, age, aged squared, region and sector | 0.101** (0.017) | -0.0011 (0.0041) | Turnover in 2014 and 2015, employment in 2015, and 1 sector |
| | (5) | Turnover & employment in 2015, age, aged squared, region, sector, turnover & employment in 2014, and interaction term for London and the biggest sectors | 0.100** (0.018) | -0.0008 (0.0047) | Employment in 2015, and 1 sector |

Note: Bootstrapped standard errors in parentheses; ** p<0.01, * p<0.05.

## A.3 Heterogeneity by size and age of the firm

The Growth Vouchers Programme was designed with the expectation that small and young firms would benefit the most from receiving diagnostic support and subsidised business advice.[50] The existing literature shows mixed evidence regarding age and size cut-offs above and below which programmes are more likely to benefit participant firms. See for example Criscuolo, et al. (2012), who find that the regional selective assistance (RSA) programme in the UK was only effective for small firms with fewer than 150 employees. In contrast, Wren and Storey (2002) find that marketing advice in the UK had the highest positive impact for 10-80 employee firms, with no impact on smaller firms and a negative impact on survival for large SMEs. Additional evidence suggests that more established firms were more likely to benefit from training on finance and accounting (Anderson, Chandy and Zia, 2018) and that there may be decreasing returns to business advice with firm size (Bruhn et al., 2018). In line with the broader literature, our results from GVP are also inconclusive when obtaining estimates by the size and age of firms at the time of application or baseline (using IDBR data).

Figure A.2 presents the IV results for the impact of GVP on turnover and employment for firms under a specific age threshold at baseline. The figure shows each point estimate and the associated 95% confidence interval. ATT estimates indicate GVP had a positive impact on turnover after 12 months for two firm age groups: among all firms aged 3 years or less, and among all firms aged 4 years or less. This effect is not significant when considering other age cut-offs, nor after 24 months. In addition, we find no effect of GVP on employment for any age sub-group. Overall, there is no clear pattern in terms of the effect of GVP on turnover and employment by firm age.

Figure A.3 presents the IV results for firms of different size or number of employees. GVP had a positive and significant impact on turnover after 12 months for all size cut-offs considered (except for firms with 3 employees or less due to large standard errors). However, IV turnover estimates are not significant after 24 months. Further, the results indicate no significant effects on employment in any period. As with age, we do not find a clear pattern for the effect of GVP on firm performance by size of the firm.
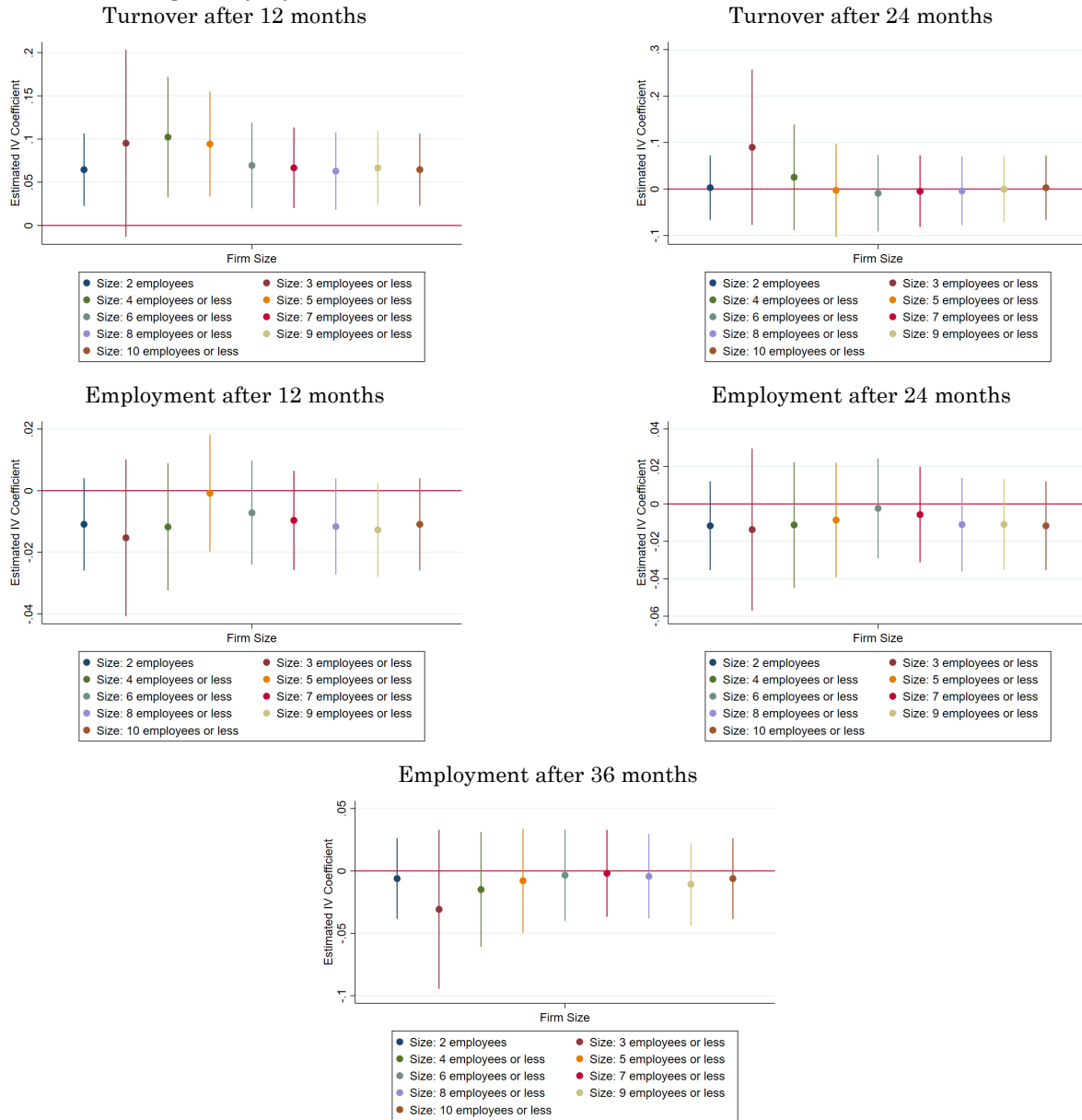
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[50] This is reflected in the eligibility criteria of GVP. See BIS (2013) as well as page 9 of the full trial protocol, available at https://www.gov.uk/government/publications/growth-vouchers-programme-trial-protocol.

# Figure A.2. Heterogeneity by firm age at baseline
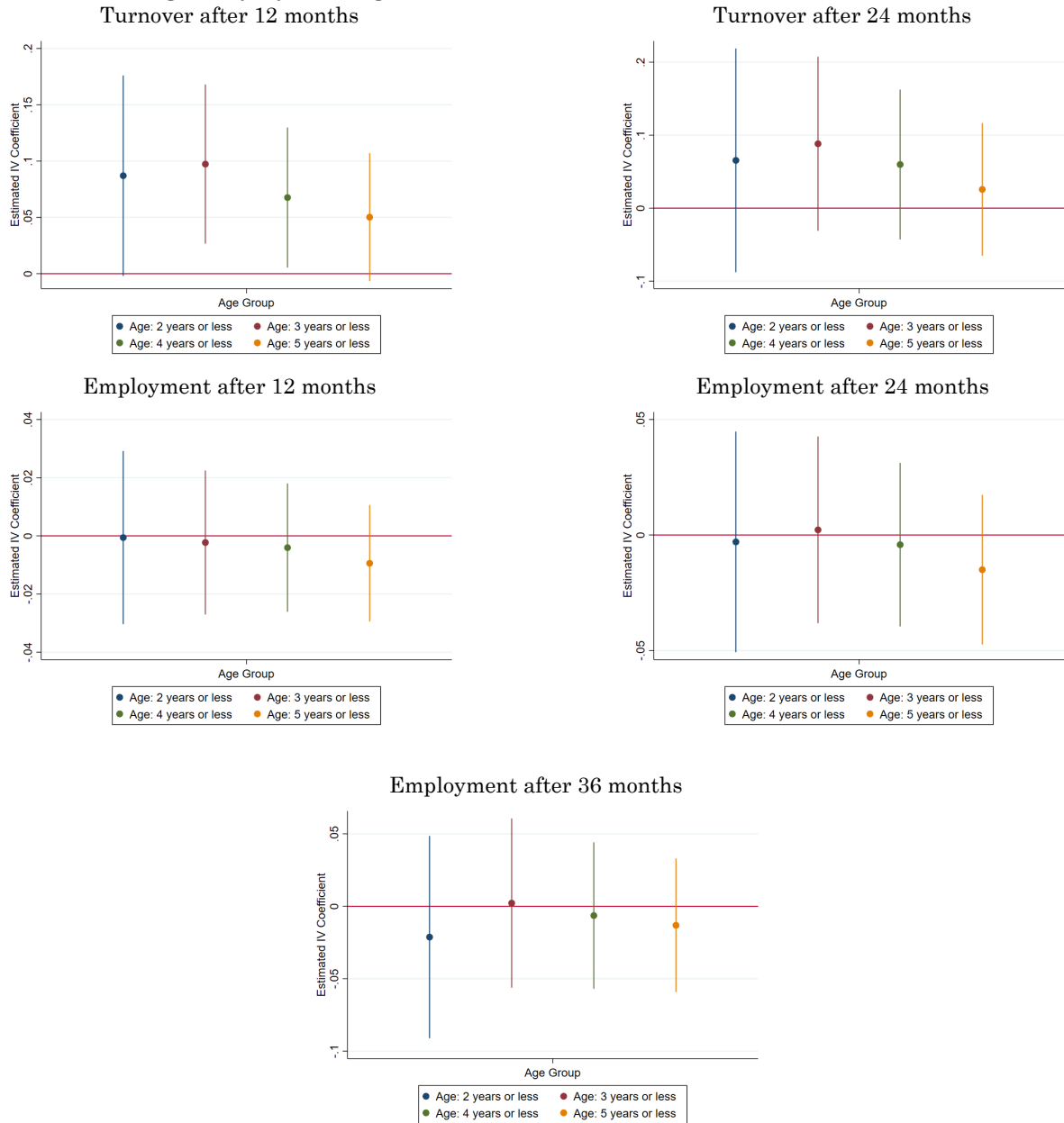


Note: Each coefficient corresponds to an IV estimate considering firms below the indicated age threshold. IV estimates for percentage changes since baseline using our preferred specification, which controls for cohort and month of application and includes the full set of interactions between type of diagnostic, theme of advice and dummy for last month of application. The vertical lines depict 95% confidence intervals.

## Figure A.3. Heterogeneity by firm size



Turnover after 12 months



Turnover after 24 months



Employment after 12 months



Employment after 24 months



Employment after 36 months

Note: Each coefficient corresponds to an IV estimate considering firms below the indicated size threshold. IV estimates for percentage changes since baseline using our preferred specification, which controls for cohort and month of application and includes the full set of interactions between type of diagnostic, theme of advice and dummy for last month of application. The vertical lines depict 95% confidence intervals.

## A.4 Details on the identification strategies

This section presents the instrumental variables identification strategy and the propensity score matching approach.

<u>Instrumental variables</u>

To get an estimate of the effect of treatment on the treated we rely on the fact that the random assignment of a voucher by construction influences the probability of using the voucher (as vouchers are non-transferable). To do this we employ an instrumental variables (IV) approach using the variation in take up of the voucher induced by the allocation of the voucher to estimate the effect of treatment on the treated (ATT). If eligible, the assignment to a group can be defined by:

$Z = 1\ for\ treatment$ and $Z = 0\ for\ control$

while the actual treatment decision $D_z$ corresponds to

$D_1 = 1\ Treatment\ status\ if\ used\ the\ voucher$

$D_0 = 0\ Treatment\ status\ if\ assigned\ to\ control\ group$

Overall, we have 4 groups according to the allocation and use of the voucher

1) Compliers: $D_1 > D_0\ (D_0 = 0\ and\ D_1 = 1)$

2) Always-takers: $D_1 = D_0 = 1$

3) Never-takers: $D_1 = D_0 = 0$

4) Defiers: $D_1 < D_0\ (D_0 = 1\ and\ D_1 = 0)$

To identify the impact of the programme on the treated we estimate:

$$\mathrm{E}\ [Y_1 - Y_0|\ D_1 > D_0] = \frac{\mathrm{E}[Y|Z=1] - \mathrm{E}[Y|Z=0]}{\mathrm{E}[D|Z=1] - \mathrm{E}[D|Z=0]} = \frac{cov(Y,Z)}{cov(D,Z)}$$

where $\mathrm{E}\ [Y_1 - Y_0|\ D_1 > D_0]$ corresponds to the expected change in outcome Y between the treatment and control groups, conditioned on complying with the treatment. In this way, the IV estimator corresponds to the average treatment effect for the subpopulation of compliers. This provides an estimate of the local average treatment effect (LATE) or average treatment effect on the treated (ATT) for the firms whose participation status changes due to their selection into the treatment group. The identification assumptions are:

1) Independence: $(Y_0, Y_1, D_0, D_1) \amalg Z$

2) First stage: $0 < P(Z = 1) < 1$ and $P(D_1 = 1) \neq P(D_0 = 1)$

3) Monotonicity: $D_1 \geq D_0$

For the GVP, we have one-side non-compliance $(D_1 = 0)$, and thus

$$E[Y_1 | D_1 > D_0] = E[Y_1 | D_1 = 1] = E[Y_1 | Z_1 = 1, D_1 = 1] = E[Y_1 | D = 1]$$

Similarly,

$$E[Y_0 | D_1 > D_0] = E[Y_0 | D = 1]$$

such that the local average treatment effect for compliers corresponds to the average treatment effect on the treated (ATT) in the following way

$$\text{LATE} = E[Y_1 - Y_0 | D_1 > D_0] = E[Y_1 - Y_0 | D = 1] = \text{ATT}$$

Therefore, the IV strategy estimates the effects of receiving business support for compliers, which corresponds to the ATT, instead of estimating the effect of being offered subsidised advice (the ITT effect). The first stage of the IV estimation is:

$$P_i = \omega + \delta V_i + \gamma X_i + u_i$$

where $P_i$ measures whether business $i$ used the voucher received as part of the GVP, $V_i$ corresponds to the allocation of voucher for each business and $X_i$ is a vector of baseline characteristics or covariates. The second stage regression for any outcome Y is

$$Y_i = \alpha + \beta \hat{P}_i + \rho X_i + \varepsilon_i$$

where $\hat{P}_i$ is the predicted likelihood for firm $i$ to participate in the GVP given that it was randomly allocated a voucher for business support. As discussed above, the coefficient of interest is $\beta$ which provides an estimate of the ATT.

Propensity score matching

PSM imputes the missing potential outcome of each treated unit using the observed outcome from a similar untreated unit, where similarity is defined in terms of a set of observable covariates, such that:

$$ATT = \frac{1}{N} \sum_{D_i=1} (Y_i - Y_{j(i)})$$

Where $N$ corresponds to the total number of treated observations $D_i$, $Y_i$ to the outcome of the treated observation, $Y_{j(i)}$ is the outcome of an untreated observation such that $X_{j(i)}$ is the closest value for covariate $X_i$ among the untreated observations. If we consider the average of the M closest matches, then

$$ATT = \frac{1}{N} \sum_{D_i=1} \left\{ Y_i - \left( \frac{1}{M} \sum_{m=1}^{M} Y_{jm(i)} \right) \right\}$$

The discrepancy between treated and untreated observations tends to increase with the number of covariates $X_i$ considered, since it becomes difficult to find units that are similar on all of these dimensions. Thus, a common solution is to estimate and use a propensity score (PS), such that this score is identified as the selection probability conditional on the confounding or observable variables. The exogeneity assumption corresponds to:

$$(Y_1, Y_0) \perp D \mid ps(x)$$

That is, the selection on observables conditioning on the propensity score is enough to have independence between the treatment indicator D and the potential outcome of interest Y. A second condition requires having common support or probability of assignment bounded by $0 < ps(x) < 1$. The allocation mechanism can be interpreted as if, within subpopulations of units with the same value for the score, a random assignment was carried out. If both assumptions hold, then:

$$ATT = E[Y_1 - Y_0 | D = 1] = \frac{1}{P(D=1)} E[Y \frac{D - p(X)}{1 - p(X)}]$$

## A.5 Distribution of firms between tradable and non-tradable sectors

In this section we consider how many GVP firms belong to tradable and non-tradable sectors. To do this, we follow the methodology developed by Jensen and Kletzer (2006), which uses the geographical concentration of service activities (within the US) to identify whether activities are traded domestically or not. They assume that geographically concentrated activities are more tradable – because they can serve dispersed customers from a smaller number of locations. They define three degrees of tradability: Gini class 1 (least geographically concentrated, when the Gini index < 0.1); Gini class 2 (when the Gini index is between 0.1 and 0.3); and Gini class 3 (most geographically concentrated, when the Gini index is >0.3).

Table A.18. Breakdown of compliers by sectoral classification

| Group and measure | | Manufacturing sector | Services sector | | | Other sector | Not classified | Total |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| | | | Tradable (Gini 3) | Medium tradable (Gini 2) | Non-tradable (Gini 1) | | | |
| All GVP firms in IDBR | Number | 1,281 | 1,925 | 6,038 | 3,190 | 955 | 55 | 13,444 |
| | Share by sector | 9.5% | 14.3% | 44.9% | 23.7% | 7.1% | 0.4% | 100% |
| GVP compliers | Number | 362 | 460 | 1,615 | 800 | 229 | 13 | 3,479 |
| | Share by sector | 10.4% | 13.2% | 46.4% | 23.0% | 6.6% | 0.4% | 100% |

Note: Non-tradable services also include the public sector and construction services. Gini 1, Gini 2 and Gini 3 are derived following Jensen and Kletzer (2006). 'Other' are sectors that are not classified by degree of tradability because they were either excluded in Faggio & Overman (2014) (e.g., electricity and gas, transport, or telecommunications) or they were not mapped into a degree of tradability in the correspondence process (e.g., some administrative support activities).

To use this classification for UK sectors, we proceed as follows. We first use the mapping by Faggio & Overman (2014) from the US 6-digit codes to the UK 4-digit SIC03 codes based on an industry correspondence table from the US Census Bureau. The correspondence table maps 2002 NAICS (6-digit code) to NACE Rev. 1.1 (4-digit code) which is almost equivalent to the UK SIC03 classification. Together with the 4-digit code, the correspondence table also provides a brief description of the industry. Faggio & Overman (2014) combine information on the industry code and description to match the NAICS and the UK SIC classifications. In a second step, we map the UK 5-digit SIC03 codes to the UK 4-digit SIC07 codes based on ONS conversion tables. In the last step, to mitigate double counting and measurement error, we apply the following rule: if two or more SIC03 codes with different degrees of tradability are mapped to one SIC07 code, we

label the SIC07 code using the highest tradable classification for the corresponding SIC03 codes. For example, if a SIC07 code maps to two tradable and a non-tradable SIC03 code, we classify that SIC07 code as tradable. If a SIC07 code maps to one tradable, one medium-tradable and one non-tradable SIC03 code, we also classify that SIC07 code as tradable.

Table A.18 above presents the resulting breakdown of firms by degree of tradability. Of the 93% of compliers that are in a manufacturing or services sector, we find that just over 23% are in manufacturing or highly tradable services, 46% are in medium-tradable and 23% in non-tradable services.

The remaining 7% of compliers either could not be classified (since IDBR data does not include a SIC07 code for these firms) or are classified in 'Other sectors' because they were excluded in Faggio & Overman (2014), such as firms in electricity and gas, transport, or telecommunications (roughly 62% of the 229 compliers in 'Other') or because they were not mapped in the correspondence process (e.g. some office and other administrative support activities, which account for 38% of the 229 compliers in 'Other'). The resulting 'Other' SIC07 would be considered non-tradable by some definitions.

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