

Evidence Review 4

Access to Finance

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what works centre for
local economic growth



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Preface

This report presents findings from a systematic review of evaluations of access to finance programmes aimed at improving business growth and other outcomes.

It is the fourth of a series of reviews that will be produced by the [What Works Centre for Local Economic Growth](#). The What Works Centre is a collaboration between the [London School of Economics and Political Science](#), [Centre for Cities](#) and [Arup](#) and is funded by the [Economic & Social Research Council](#), [The Department for Communities and Local Government](#) and [The Department for Business Innovation & Skills](#).

These reviews consider a specific type of evidence – impact evaluation – that seeks to understand the causal effect of policy interventions and to establish their cost-effectiveness. To put it another way they ask ‘did the policy work’ and ‘did it represent good value for money’? By looking at the details of the policies evaluated we can also start to answer questions about delivery issues – for example, whether access to finance programmes that directly provide loans or loan guarantees perform better than interventions that simply provide information about existing sources of credit.

We see these impact-focused reviews as an essential part of more effective policy making.

We often simply do not know the answers to many of the questions that might reasonably be asked when implementing a new policy – not least, does it work? Figuring out what we do know allows us to make better decisions and to start filling the gaps in our knowledge. **This also helps us to have more informed discussions and to improve policy making.**

These reviews therefore represent a first step in improving our understanding of what works for local economic growth. In the months ahead, we will be working with local decision makers and practitioners, using these findings to help them generate better policy.

Henry Overman

Director, What Works Centre for Local Economic Growth



Executive Summary

This report presents findings from a systematic review of evaluations of policies designed to improve access to finance for businesses, with the goal of improving business growth and other outcomes. It is the fourth of a series of reviews that will be produced by the What Works Centre for Local Economic Growth.

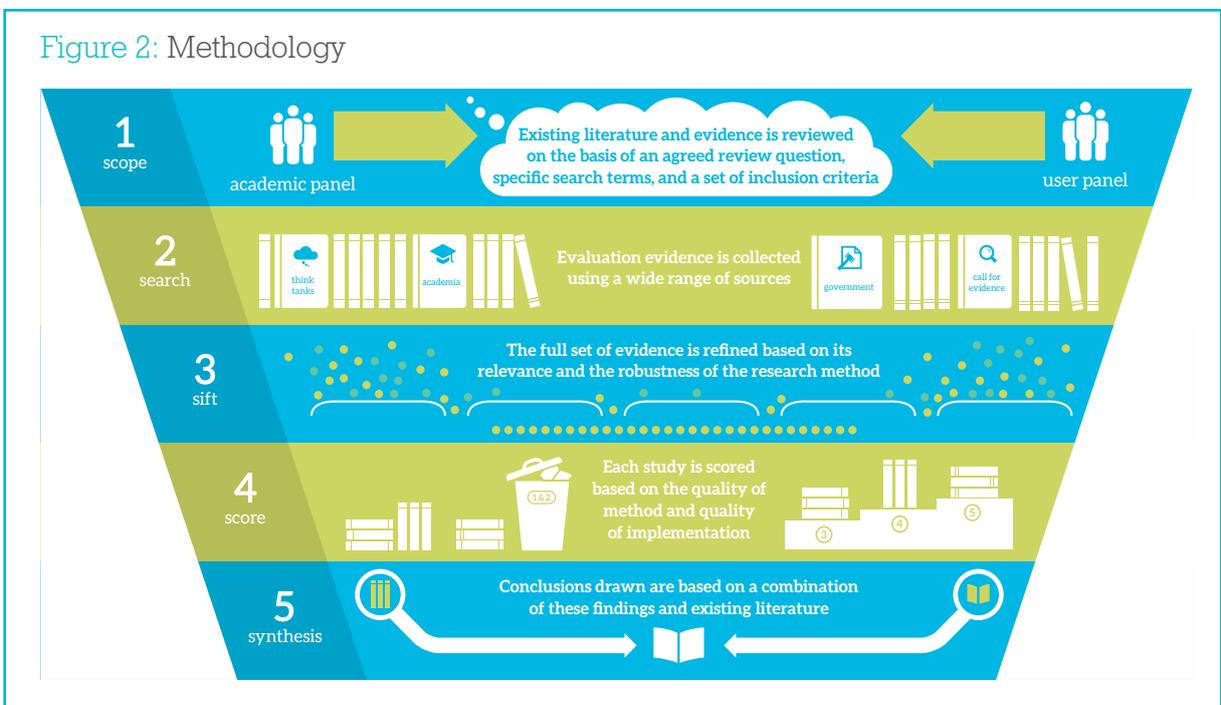
The review considered almost **1,450 policy evaluations** and evidence reviews from the UK and other OECD countries.

It found **27 impact evaluations** that met the Centre's minimum standards. This is a smaller evidence base than for our first and third reviews (on employment training and the impact of culture and sports projects) although a little larger than for our second review (on business support). It is a very small base relative to that available for some other policy areas (e.g. medicine, aspects of international development, education and social policy).

Overall, of the 27 evaluations reviewed, **17 found positive impacts on at least one firm outcome**. Seven evaluations found mixed results (at best providing only weak evidence of positive effects, at worst a mix of positive and negative effects). Two evaluations found that the programme didn't work (had no effect) and one found that the programme might be harmful.

Approach

The Centre seeks to establish causal impact – an estimate of the difference that can be expected between the outcome for firms in the programme and the average outcome they would have experienced without the programme (see Figure 1). Our methodology for producing our reviews is outlined in Figure 2.



Summary of findings

Helping businesses access finance can have two kinds of effects on firm outcomes. There may be immediate effects on improved access to finance (credit availability, cost of borrowing, etc.). In turn, these need to translate into improved firm performance (captured by employment, productivity and so on) if the programme is to have an impact on local economic growth. Even if these positive effects occur at the firm level, however, access to finance could have an adverse impact on local economic growth if it helps weaker firms survive.

What the evidence shows

- Access to finance programmes had a positive impact on at least one firm outcome (e.g. credit, employment, sales) in 17 out of 27 evaluations.
- Programmes have a positive effect on firm access to *debt* finance either in terms of the availability of credit or the cost of borrowing (or both). The impact on access to equity finance is mixed (and available evidence limited).
- The impact of policies on investment and assets is mixed.
- There is some evidence that loan guarantees may increase default risk.
- Access to finance had a positive impact on at least one aspect of firm performance (e.g. employment and sales) in 14 out of 17 evaluations.
- However, these overall patterns hide much more mixed results for specific aspects of firm performance, with only half the evaluations typically recording a positive effect when looking at a specific aspect of firm performance (e.g. employment).

Where the evidence is inconclusive

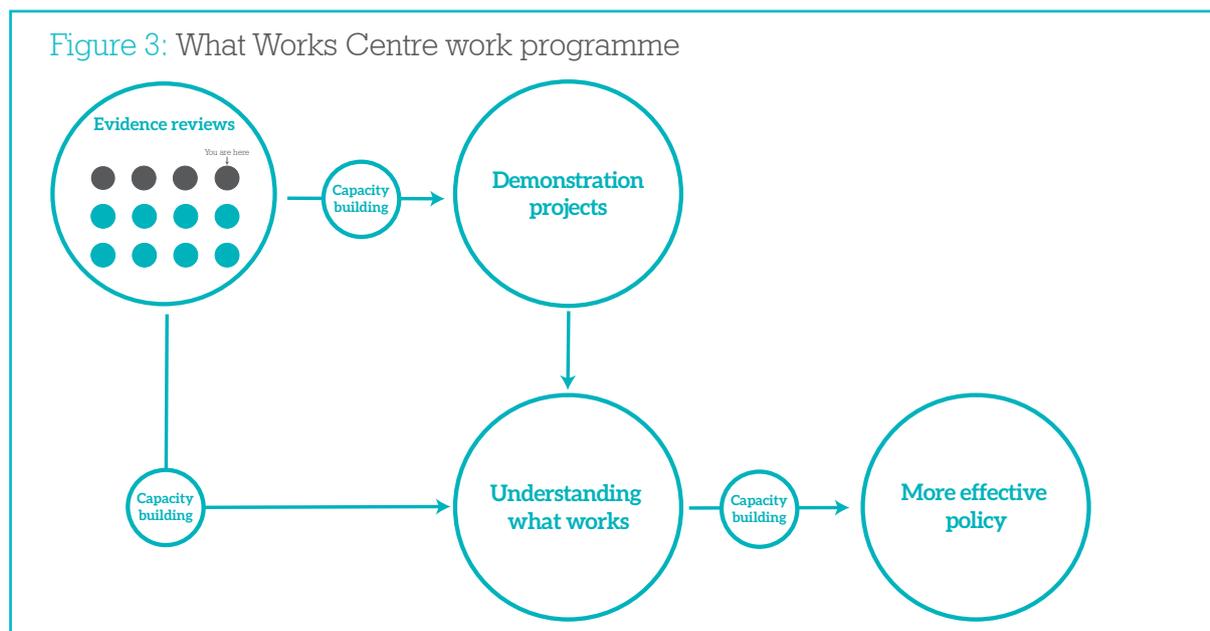
- There is no evidence that programmes targeted at Small and Medium Sized Enterprises are more or less effective than non-targeted programmes. Other targeted programmes (taken as a group) appear to perform slightly less well.
- The overall results for loan guarantees and alternative investment mechanisms are broadly similar. Loan guarantee schemes introduced in response to economic crisis perform somewhat worse than long term development schemes.
- The overall results for public, private or hybrid programmes are broadly similar.

Where there is a lack of evidence

- We found very few studies that look at the impact of schemes on both access to finance (direct effect of the scheme) and on the subsequent performance of firms (indirect effects of the scheme).
- While most programmes appear to improve access to finance, there is much weaker evidence that this leads to improved firm performance. This makes it much harder to assess whether access to finance interventions really improve the wider economic outcomes (e.g. productivity, employment) that policymakers care about.
- As with other reviews, we found very few studies that gathered (or had access to) information on scheme costs. As a result, we have very little evidence on the value for money of different interventions.

How to use these reviews

The Centre's evidence reviews consider a specific type of evidence – impact evaluation – that seeks to understand the causal effect of policy interventions and to establish their cost-effectiveness. In the longer term, the Centre will produce a range of evidence reviews that will help local decision makers decide the broad policy areas on which to spend limited resources. Figure 3 illustrates how the reviews relate to the other work streams of the Centre.



Supporting and complementing local knowledge

In our employment training and business advice reviews, we set out a number of ‘Best Bets’ which outlined what tends to work in those two policy fields based on the best available impact evaluations. These best bets might be in terms of the intended outcomes (e.g. business advice programmes show better results for sales than for productivity or employment) or in terms of policy design features (e.g. on the job training is more effective than class-room based training). These ‘Best Bets’ do not generally address the specifics of ‘what works where’ or ‘what will work for a particular individual’. But they do provide an important complement, rather than a substitute, for local, on-the-ground knowledge.

As should be clear from our summary above, despite the availability of relatively high quality evaluations, the evidence provides no such guidance for access to finance programmes. Impacts across key outcomes such as employment, profit and sales are generally similar with around 50% of programmes having a positive impact on any given outcome. Overall programme impacts do not differ much between targeted and non-targeted programmes, between loan guarantee and alternative finance vehicles or between private, public and hybrid programmes. In short, the available evaluation evidence provides little guidance to local policymakers on the detail of policy design in this area.

Providing general guidance on what works

Despite their continued popularity with policy makers we have very limited evidence that access to finance interventions improve firm performance. Note that we have good in-principle reasons to think

these programmes may improve growth across the local economy – but the evidence suggests that such impacts are not consistently achieved in practice.

This also suggests that direct programme outputs (e.g. loans made or guaranteed) are unlikely to be good indicators of programme impact on wider local economic growth. Similarly, sustainability of a programme may provide a useful indicator that there has been no increase in default risk, but this is no guarantee of an impact on the local economy further down the line.

In short, standard monitoring of performance indicators appears to provide no guidance to policy effectiveness in terms of improving local economic growth. More evidence is needed to understand whether access to finance improves firm performance and thus whether the large amount of money committed to such programmes is justified.

Filling the Evidence Gaps

This review has not found answers to many of the questions that will be foremost in policy makers' minds. This makes it much harder to assess whether access to finance interventions really perform against the economic performance measures policymakers care about.

These gaps highlight the need for improved evaluation and greater experimentation, specifically experiments that focus on:

- setting out and evaluating whether access to finance models improve both access to finance and firm performance.
- identifying how different types of access to finance programmes contribute to better or worse firm and economy outcomes; and,
- the value for money of different access to finance approaches.

Addressing these gaps requires evaluation to be embedded in the access to finance policy design process, and thinking differently about the access to finance policy cycle as a whole.



Introduction

Financial markets play an important role in the efficiency and growth of the economy. They mobilise savings but also pool capital, select projects, monitor and enforce contracts, and manage risks.¹ Obtaining, processing and managing information is fundamental to well-functioning financial markets. This means that market failures are – arguably – more likely in financial markets than for many other goods and services.

Governments have always played a role in financial markets. Most obviously, the state steps in during financial crises – such as the 2007 Crash, when governments around the world intervened to prevent bank collapses and stimulate the supply of credit. In recent years governments have also used financial tools – such as loans and tax breaks – to stimulate growth in high-value sectors of the economy.²

Market failures provide the rationale for government intervention in business finance markets (and this review considers a number of such interventions as described in detail below). While many businesses can obtain the finance they need, there are a number of structural market failures affecting the supply of both debt and equity finance to certain businesses such as start-ups, micro enterprises and Small and Medium Sized Enterprises (SMEs). This leads to some potentially viable businesses being refused finance, which may be sub optimal for economic growth.

As suggested above, these market failures mainly relate to imperfect or asymmetric information. When future profitability is hard to predict it may be difficult to distinguish between good and bad prospects. Those firms most keen to borrow may be least likely to pay back.³ Even highly informed lenders such as venture capital firms find that only about 6% of investments pay off more than five times their initial stake.⁴ High street lenders, who operate across a much broader pool of clients, typically respond to these challenges by ‘profiling’ borrowers into blocks, then adjusting the cost of borrowing and/or restricting supply.

In economists’ jargon, this makes markets ‘incomplete’ – lenders respond to uncertainty and risk by reducing the supply of finance below what the market demands. For these reasons there is often limited

1 Stiglitz (1994) provides a useful overview.

2 Rodrik (2004), Lerner (2009).

3 Economists term this ‘moral hazard’. Akerlof (1970) provides the classic discussion.

4 Kerr, Nanda and Rhodes-Kropf (2014).

competition in the market: 'riskier' borrowers may find that only a few lenders will actually do business with them. These information failures may also become exacerbated in uncertain economic conditions (such as recessions) when lenders become more risk adverse and there is greater uncertainty.

In addition, there are information market failures affecting the demand side for businesses seeking finance. Some entrepreneurs and businesses may not fully understand the potential benefits to their business of raising finance or their likely chance of success in gaining finance, which ultimately means they do not apply. This may restrict the growth of businesses. Business owners can also lack knowledge of funding sources available or lack the skills to present themselves as strong opportunities to investors.

Financial markets also typically disregard social returns. Banks and other lenders fund projects with the highest private returns. But society may benefit from other projects being funded. For example, investing in early stage innovative businesses can lead to a number of positive spill-over effects – through innovation and knowledge transfers – to other parts of the economy. Private investors do not take this into account when making a decision to invest, meaning they may not fund such firms at all.

In recessions wider factors also apply: sustaining firms' viability and preventing job losses is important for preventing negative spillovers across the economy. In some countries (and cities) there may also be a shortage of private sector specialist finance such as angel investors or venture capital firms.

In theory, then, there are public interventions in financial markets that can make everyone better off. Through legal powers and scale, governments can compel the disclosure of information, pool risk and handle externalities (for example, acting as a public venture capitalist). At a basic level, government can:

- Regulate
- Provide information
- Provide incentives to lenders (e.g. tax breaks)
- Indirectly provide finance (e.g. loan guarantees, or 'funding for lending' type schemes)
- Directly provide finance (e.g. loans).

In practice, government policy can fail too: the public sector faces the same information gaps as the private sector; political considerations may shape decisions, and interest groups may 'capture' a policy.⁵

New technology is helping to tackle some of the market failures discussed above: P2P lending platforms can help pool risk and reduce operating costs; online aggregators can improve consumer information; and big data may allow more sophisticated profiling of borrowers and their payment prospects.⁶ In turn, these developments will change the space for government to act, and the tools it needs to use.

Evaluating the success of these policies is difficult due to the complexity of the rationales for intervention. If the government programme is mainly fixing problems arising from information problems then the benefits should show up at the firm level. These firm level benefits are the focus of most evaluations that tend to consider outcomes such as access to finance, firm survival rate, and firm employment/wages. It is possible, however, that programmes aimed at maximizing social (as opposed to private) returns might deliver benefits at the wider national or area level so that focusing on firm effects understates the impact. In practice, however, most (if not all) of the evaluations covered in this review consider schemes where benefits to participants are expected and would be necessary to underpin any wider benefits (e.g. improved performance of the local economy). This justifies our focus on firm level outcomes in what follows.

5 Lerner (2009) and Rodrik (2004) both discuss these issues.

6 See for example Bahkshi and Mateos-Gracia (2012) and Einav and Levin (2013). Mayer-Schonberger and Cukier (2013) provide a basic overview.



Impact evaluation

Governments around the world increasingly have strong systems to monitor policy inputs (such as the amount of loans guaranteed) and outputs (such as the number of firms that have received loan guarantees). However, they are less good at identifying policy outcomes (such as the effect of providing a loan guarantee on firm employment). In particular, many government-sponsored evaluations that look at outcomes do not use credible strategies to assess the causal impact of policy interventions.

By causal impact, the evaluation literature means an estimate of the difference that can be expected between the outcome for firms ‘treated’ in a programme, and the average outcome they would have experienced without it. Pinning down causality is a crucially important part of impact evaluation. Estimates of the benefits of a project are of limited use to policy makers unless those benefits can be attributed, with a reasonable degree of certainty, to that project.

The credibility with which evaluations establish causality is the criterion on which this review assesses the literature.

Using Counterfactuals

Establishing causality requires the construction of a valid counterfactual – i.e. what would have happened to programme participants had they not been treated under the programme. That outcome is fundamentally unobservable, so researchers spend a great deal of time trying to rebuild it. The way in which this counterfactual is (re)constructed is the key element of impact evaluation design.

A standard approach is to create a counterfactual group of similar places not undertaking the kind of project being evaluated. Changes in outcomes can then be compared between the ‘treatment group’ (those affected by the policy) and the ‘control group’ (similar individuals not exposed to the policy).

A key issue in creating the counterfactual group is dealing with the ‘selection into treatment’ problem. Selection into treatment occurs when participants in the programme differ from those who do not participate in the programme.

An example of this problem for access to finance programmes would be when more ambitious firms

apply for and obtain support. If this happens, estimates of policy impact may be biased upwards because we incorrectly attribute better firm outcomes to the policy, rather than to the fact that the more ambitious participants would have done better even without the programme.

Selection problems may also lead to downward bias. For example, firms that apply for support might be experiencing problems and such firms may be less likely to grow or succeed independent of any advice they receive. These factors are often unobservable to researchers.

So the challenge for good programme evaluation is to deal with these issues, and to demonstrate that the control group is plausible. If the construction of plausible counterfactuals is central to good policy evaluation, then the crucial question becomes: how do we design counterfactuals? Box 1 provides some examples.

Box 1: Impact evaluation techniques

One way to identify causal impacts of a project is to randomly assign participants to treatment and control groups. For researchers, such Randomised Control Trials (RCTs) are often considered the 'gold standard' of evaluation. Properly implemented, randomisation ensures that treatment and control groups are comparable both in terms of observed and unobserved attributes, thus identifying the causal impact of the project. However, implementation of these 'real world' experiments is challenging and can be problematic. RCTs may not always be feasible for local economic growth policies – for example, policy makers may be unwilling to randomise.⁷ And small-scale trials may have limited wider applicability.

Where randomised control trials are not an option, 'quasi-experimental' approaches of randomisation can help. These strategies can deal with selection on unobservables, by (say) exploiting institutional rules and processes that result in some locations quasi-randomly undertaking projects.

Even using these strategies, though, the treatment and control groups may not be fully comparable in terms of observables. Statistical techniques such as Ordinary Least Squares (OLS) and matching can be used to address this problem.

Note that higher quality impact evaluation first uses identification strategies to construct a control group and deal with selection on unobservables. Then it tries to control for remaining differences in observable characteristics. It is the combination that is particularly powerful: OLS or matching alone raise concerns about the extent to which unobservable characteristics determine both treatment and outcomes and thus bias the evaluation.

Evidence included in the review

We include any evaluation that compares outcomes for firms receiving treatment (the treated group) before and after an intervention, relative to a comparison group used to provide a counterfactual of what would have happened to these outcomes in the absence of treatment.

This means we look at evaluations that do a reasonable job of estimating the impact of the project using either randomised control trials, quasi-random variation or statistical techniques (such as OLS and matching) that help make treatment and control groups comparable. We view these evaluations as providing credible impact evaluation in the sense that they identify effects which can be attributed, with a reasonable degree of certainty, to the project in question. A full list of shortlisted studies is given in Appendix A.

⁷ Gibbons, Nathan and Overman (2014).

Evidence excluded from the review

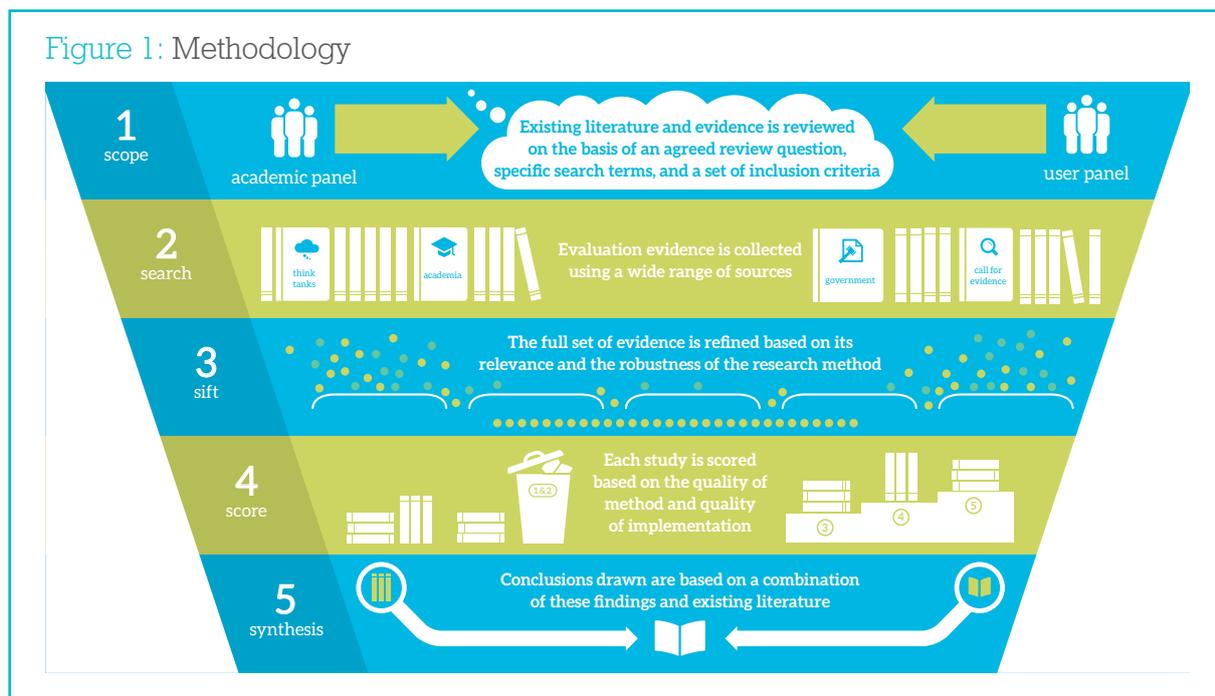
We exclude evaluations that provide a simple before and after comparison only for those receiving the treatment because we cannot be reasonably sure that changes for the treated group can be attributed to the effect of the project.

We also exclude case studies or evaluations that focus on process (how the programme is implemented) rather than impact (what was the effect of the programme). Such studies have a role to play in helping formulate better policy but they are not the focus of our evidence reviews.

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Methodology

To identify robust evaluation evidence on the causal impact of access to finance programmes, we conducted a systematic review of the evidence from the UK and across the world. Our reviews followed a five-stage process: scope, search, sift, score and synthesise. These stages are set out in Figure 1.



Stage 1: Scope of Review

Working with our User Panel and a member of our Academic Panel, we agreed the review question, key terms and inclusion criteria. We also used existing literature reviews and meta-analyses to inform our thinking.

Stage 2: Searching for Evaluations

We searched for evaluation evidence across a wide range of sources, from peer-reviewed academic research to government evaluations and think tank reports. Specifically, we looked at academic databases (such as EconLit, Web of Science and Google Scholar), specialist research institutes (such as CEPR and IZA), UK central and local government departments, and work done by think tanks (such as the OECD, ILO, ippr and Policy Exchange.) We also issued a call for evidence via our mailing list and social media. This search found just over 1450 books, articles and reports. Appendix B provides a full list of sources and search terms.

Stage 3: Sifting Evaluations

We screened our long-list on relevance, geography, language and methods, keeping impact evaluations from the UK and other OECD countries, with no time restrictions on when the evaluation was done. We focussed on English-language studies, but would consider key evidence if it was in other languages. This left us with 150 studies. We then screened the remaining evaluations on the robustness of their research methods, keeping only the more robust impact evaluations. We used an adjusted version of the Maryland Scientific Methods Scale (SMS) to do this.⁸ The SMS is a five-point scale ranging from 1, for evaluations based on simple cross sectional correlations, to 5 for randomised control trials (see Box 2). We shortlisted all those impact evaluations that could potentially score three or above on the SMS.⁹ For examples of impact evaluations that score three, four or five on the SMS scale, see www.whatworksgrowth.org.

Stage 4: Scoring Evaluations

We conducted a full appraisal of each evaluation on the shortlist, collecting key results and using the SMS to give a final score for evaluations that reflected both the quality of methods chosen and quality of implementation (which can be lower than claimed by some authors). Scoring and shortlisting decisions were cross-checked with the academic panel member and the core team at LSE. The final list of included studies and their reference numbers (used in the rest of this report) can be found in Appendix A.

Stage 5: Synthesising Evaluations

We drew together our findings, combining material from our evaluations and the existing literature.

8 Sherman, Gottfredson, MacKenzie, Eck, Reuter, and Bushway (1998). Note that in our first three reports 'Box 2' provided a description of the original Maryland Scale. However all our reports actually score on the basis of the adjusted scale as described in Box 2. Following the publication of our Scoring Guide, we have decided to describe the adjusted scale in this and all future evidence reviews.

9 Sherman et al. (1998) also suggest that level 3 is the minimum level required for a reasonable accuracy of results.

Box 2: The Scientific Maryland Scale

Level 1: Either (a) a cross-sectional comparison of treated groups with untreated groups, or (b) a before-and-after comparison of treated group, without an untreated comparison group. No use of control variables in statistical analysis to adjust for differences between treated and untreated groups or periods.

Level 2: Use of adequate control variables and either (a) a cross-sectional comparison of treated groups with untreated groups, or (b) a before-and-after comparison of treated group, without an untreated comparison group. In (a), control variables or matching techniques used to account for cross-sectional differences between treated and controls groups. In (b), control variables are used to account for before-and-after changes in macro level factors.

Level 3: Comparison of outcomes in treated group after an intervention, with outcomes in the treated group before the intervention, and a comparison group used to provide a counterfactual (e.g. difference in difference). Justification given to choice of comparator group that is argued to be similar to the treatment group. Evidence presented on comparability of treatment and control groups. Techniques such as regression and (propensity score) matching may be used to adjust for difference between treated and untreated groups, but there are likely to be important unobserved differences remaining.

Level 4: Quasi-randomness in treatment is exploited, so that it can be credibly held that treatment and control groups differ only in their exposure to the random allocation of treatment. This often entails the use of an instrument or discontinuity in treatment, the suitability of which should be adequately demonstrated and defended.

Level 5: Reserved for research designs that involve explicit randomisation into treatment and control groups, with Randomised Control Trials (RCTs) providing the definitive example. Extensive evidence provided on comparability of treatment and control groups, showing no significant differences in terms of levels or trends. Control variables may be used to adjust for treatment and control group differences, but this adjustment should not have a large impact on the main results. Attention paid to problems of selective attrition from randomly assigned groups, which is shown to be of negligible importance. There should be limited or, ideally, no occurrence of 'contamination' of the control group with the treatment.

Note: These levels are based on but not identical to the original Maryland SMS. The levels here are generally a little stricter than the original scale to help to clearly separate levels 3, 4 and 5 which form the basis for our evidence reviews.



Definition

We included in our definition of access to finance programmes which:

- Directly lend all or part of money to firms (for example public loans or subsidised loans);
- Guarantee or partly guarantee loans;
- Provide financial education or information to firms (for instance about financial services available);
- Facilitate alternative forms of lending (for example business angels, micro-finance, venture capital and group lending), by creating networks, incentivising or matchmaking lenders and firms.

In practice, the vast bulk of the shortlisted evaluations cover loan guarantees, or alternative investment tools (mainly venture capital). We also include one study which covers a relevant regulatory change (study 620).

In what follows, we talk about programme effects on ‘businesses’ or ‘firms’ interchangeably. However, we distinguish between ‘financial outcomes’ (direct effects on credit constraints, levels of debt or probability of default), and effects on ‘firm performance’ which are likely to appear later (indirect effects on firms’ productivity, sales/profits or employment). From a local economic growth perspective we care more about the latter than the former, although ideally we need information on both types of impacts to judge whether a given intervention has been successful.



Findings

This section sets out the review's findings. We begin with a discussion of the evidence base, and then explore the overall pattern of positive and negative results. After this we consider specific programme features in more detail.

Quantity and Quality of the Evidence Base

The review considered just over 1,450 policy evaluations and evidence reviews from the UK and other OECD countries, which were identified during the initial keyword search.

Following a further high level review, just over 1,300 were sifted out as not relevant (e.g. because they were theoretical rather than data-based; reviewed non-OECD countries; were written in a foreign language or because of subject relevance). From the remaining 150, we discarded 20 purely qualitative evaluations. A further 28 clearly did not meet the centre's minimum standard of quantitative evidence (i.e. scored 2 or below on the SMS scale). The remaining 92 studies were shortlisted for detailed review.

Of those 92 shortlisted studies reviewed in detail, a further 30 were ultimately discounted on grounds of relevance, and 35 on grounds of not meeting the Centre's minimum standard of evidence (i.e. scored 2 or below on the SMS scale). The remaining 27 have been included in this review.

This is a smaller evidence base than for our first and third reviews (on employment training and the impact of culture and sports projects) although a little larger than for our second review (on business support). This may also be larger than the evidence base for many other local economic growth policies. It is a small base relative to that available for some other policy areas (e.g. medicine, aspects of international development, education and social policy). Table 1 shows the distribution of the studies ranked according to the SMS.

Table 1: Ranking Studies by Quality of Implementation

SMS Score	No. of studies	Evaluation reference numbers
5	2	542, 562
4	8	546, 558, 584, 617, 619, 648, 651, 742
3	17	548, 564, 565, 570, 572, 573, 583, 588, 590, 592, 599, 620, 622, 623, 650, 737, 740
Total	27	

There are two randomised control trials both of which scored 5 on the SMS.¹⁰ We found eight studies that used credible quasi-random sources of variation (i.e. scored 4 on the SMS) to identify policy impacts.

The remaining 17 studies scored 3 on the SMS, and use variations on OLS or matching techniques. The techniques applied in these SMS 3 studies mean that we can be reasonably confident that the evaluation has done a good job of controlling for all observable characteristics of firms or individuals (for example: firm age; size; sector) which might explain differences in firm outcomes. However, for these studies, it is likely that unobservable characteristics such as entrepreneurial talent or firms' desire to grow may still be affecting the results. This raises concerns that the evaluation incorrectly attributes beneficial outcomes to the programme rather than to these firm characteristics. We can only be fully confident that selection on unobservables has been eradicated with an RCT methodology, where participants are randomly assigned to treatment or control groups.

Compared to previous evidence reviews the proportion of studies ranking 4 or 5 is quite high. That is, the overall standard of evidence on access to finance is quite high. It is worth re-emphasising, however, that the overall volume and quality of evidence remains low compared to a number of other public policy areas (e.g. medicine, aspects of international development, education and social policy).

Type and Focus of Support

The two largest categories of support in our shortlisted studies are guarantees on conventional lending methods, usually through private financial institutions; and alternative lending or investment mechanisms. Specifically:

- 11 evaluations focus on state-led loan guarantee programmes¹¹, whereby loans provided through private banks were either partially or fully guaranteed by public sector organisations in cases where it was deemed too risky by the bank (for example, in the case of start-ups or young SMEs). Within this group, five were introduced in response to credit shocks resulting from wider financial crises and six were introduced with a more long-term, economic development logic;
- Two cover programmes which involved state subsidy or provision of loans themselves to firms;¹²

¹⁰ Studies 542, 562.

¹¹ Studies 548, 564, 565, 570, 572, 584, 588, 590, 592, 599 and 623

¹² Studies 558 and 562

- One covers a hybrid which combined elements of both of the above;¹³
- One covers an alternative micro-finance lending method employed in Mexico¹⁴;
- Two cover alternative investment mechanisms, encompassing venture capitalist and business angel interventions;¹⁵
- Four evaluations consider state-led or state-sponsored venture capital schemes¹⁶, whereby the state provided funding opportunities for start-ups; two of these were specifically targeted at hi-tech firms¹⁷ and one also looked at ‘hybrid’ examples whereby firms were supported by a consortium of public and private venture capitalists;¹⁸
- Two cover government interventions which aimed to create a favourable environment to induce private sector venture capitalist funding.¹⁹

Of the other four evaluations:

- One covers the Japanese Small Business Credit Scoring (SBCS) programme which aims to improve the accuracy of credit scoring for small firms;²⁰
- Two look at how local market conditions, particularly in terms of the number of banks, affect firms’ ability to access credit and the wider local economy;²¹ and
- One covers a public intervention in the regulation of inter-state banking in the USA.²²

Programme objectives and outcomes

As with our business support review, a number of the access to finance evaluations are unclear about what the programme objectives are. Objectives are clearly stated for 17 of the 27 evaluations. Where objectives are clearly stated, the majority list multiple objectives. In some cases, these may reflect poor or imprecise policy design; in other cases, policymakers’ legitimate desire to influence multiple aspects of firm performance via improving access to finance.

Multiple objectives make clean evaluation harder. Indeed, in quite a few cases specified objectives are not assessed in the evaluation. In other cases, objectives which do not appear to be part of the programme rationale are included in the evaluation. Sometimes these outcomes may act as a proxy for the true objective when data is not available. In other cases, researchers may pay little attention to policy detail, but evaluate the outcomes they are most interested in or for which quantitative data is available. More worryingly, this weak link between objectives and outcomes evaluated may reflect policymakers’ desire to find some positive outcome for a programme on which considerable resources have been expended.

For all of these reasons, we also look directly at the effect of programmes on different outcomes, regardless of stated objectives. This provides some indication of whether access to finance programmes work better for some firm outcomes than others (even when these outcomes are not the objective of policy).

13 Study 583

14 Study 542

15 Studies 546 and 619

16 Studies 648, 650, 740 and 742

17 Studies 740 and 742

18 Study 650

19 Studies 737 and 740

20 Study 573

21 Studies 617 and 622

22 Study 620

In the remainder of this section, we consider a number of outcomes in turn. Table A1 in Appendix A summarises our findings for all of the different outcomes considered in the evaluations. Note that some of the evaluations cover multiple outcomes, so category counts in table 3 do not sum to the total count. This table clearly highlights the issues with respect to multiple objectives and the link to outcomes evaluated.

As set out in section 1, we expect the direct effects of these programmes to be felt on access to finance measures (credit availability, cost of borrowing, etc.) which in turn may improve firm performance (employment, productivity, etc). Therefore, in terms of understanding the effects of these programmes, it is useful to start by looking at access to finance and then work up to the effects on firm outcomes that relate closely to key local growth outcomes.

Financial outcomes

Access to debt finance

Policies generally have a positive effect on firm access to finance either in terms of the availability of credit or the cost of borrowing (or both).

As shown in Table 3, eight out of 11 evaluations that consider access to debt financing show positive effects. Seven of these consider credit availability,²³ while four consider the cost of borrowing. Interestingly, one study²⁴ saw increased credit availability for firms despite the fact that credit ratings worsened. Two further studies²⁵ report beneficial effects on both the availability of credit and the cost of borrowing. While it is theoretically possible that participation in such schemes could have a perverse effect on access to finance there is little evidence of this happening in practice.

Table 3: Access to Debt Finance

Outcome	Evaluation references	Total	Positive	Zero	Negative	Mixed	Share +ve
Credit availability	565, 570, 572, 584, 623, 737, 740	7	565, 572, 623, 740	-	-	570, 584, 737	4/7
Reduced borrowing costs	572, 584, 590, 623	4	572, 584, 590, 623	-	-	-	4/4

Access to equity finance

The impact of policies on access to equity finance is mixed (and available evidence limited).

Only three evaluations look at the impact on access to equity via Initial Public Offerings. One scheme has positive effects, one mixed and one negative. All three of these schemes involve alternative

²³ Study 584 considers debt which we treat as a proxy for credit availability.

²⁴ Study 565

²⁵ Studies 572 and 623

investment finance vehicles (either business angels²⁶ or venture capital²⁷). The availability of follow-up investment is presumably conditional on improved firm performance and the mixed results for these three policies is consistent with the evidence that these firm performance effects are in turn mixed (as we discuss further below).

Borrowing, investment and assets

The impact of policies on investment and assets is mixed.

Table 4 reports results from evaluations that look at the impact on investment and assets. Unlike measures of access to finance, we should not necessarily expect these programmes to increase investment or assets as this would depend on the way in which available funds are used by the firm. Still, it is interesting to see whether such effects occur.

Table 4: Investment and Assets

Outcome	Evaluation references	Total	Positive	Zero	Negative	Mixed	Share +ve
Investment	542, 564, 565, 584, 592	5	542,	565, 592	-	564, 584	1/5
Assets	542, 590, 599, 648, 742	5	590, 599	542	648	742	2/5

Five studies consider the effect on investment. One of these (an SMS4) finds positive effects. Study 542 finds microcredit in Mexico increases household investment, particularly in existing businesses. Two studies (both SMS3) find zero effect on firm level investment. These are both evaluations of credit guarantee schemes, one in Korea (592) and one in Japan (565). Two studies report mixed effects; study 584 finds a credit guarantee scheme in Italy slightly increases firm investment in year one of the programme, but not thereafter, whilst study 564 reports mixed results from a credit guarantee scheme in Korea.

Overall, the evidence suggests that credit guarantee schemes, at least, do not have lasting impacts on investment. Five studies consider the effect on assets and the picture in terms of effects is mixed. Study 542 looks at microcredit aimed at households, and among other things measures the likelihood of buying and selling assets such as property and vehicles. In this case microcredit reduces the likelihood of buying these large items, and the likelihood of selling them to service other debts. The total asset effect is therefore likely to be zero, consistent with the study's findings. Studies 648 and 742 both look at public venture capital programmes, where the public sector makes investments in firms that (if successful) will increase their value (in terms of balance sheet assets, or sale price when acquired or IPO'd). Here the results are mixed. Canadian public VC funds (study 648) result in a small but significant decrease in total value as measured by IPO / acquisition price. Pan-European analysis of government and university-backed VC (742) finds zero overall effects, but that government schemes have a positive effect on firm balance sheets, and that both government and university-backed schemes increase young firms' total assets. Study 599 however, finds a positive return on assets of 0.5% above non-users of the Special Credit Guarantee Programme (Japan), whilst study 590 similarly finds positive results on assets from the SOFARIS/OEEO Loan Guarantee Programme (France).

26 Study 619

27 Studies 548 and 648

Default risk

There is some evidence that loan guarantees may increase default risk (albeit on the basis of limited available evidence).

Because these schemes increase access to finance it is theoretically possible that they also increase default risk (especially if they do not lower borrowing costs). Evidence on this is somewhat mixed although not particularly reassuring. For study 573, which looked at a programme aiming to improve credit scoring, risk of default increased where loans were taken from secondary banks (suggesting these institutions only adopted the programme to save costs), whereas default risk fell for primary banks. Unfortunately, for the three other studies of loan guarantees that looked at default risk, the effect was to increase risk in two cases,²⁸ with no effect in the third.²⁹ Albeit on the basis of limited evidence, this would suggest that concerns over adverse effects on default risk may be valid (in contrast to concerns over the possibility of adverse effects on the availability of finance, which appear to be invalid as discussed above).

Firm performance

Access to finance had a positive impact on at least one aspect of firm performance in 14 out of 17 evaluations. But the effect on any specific aspect of firm performance was much more mixed with only around half the evaluations reporting a positive effect.

Studies for a number of firm performance outcomes are reported in Table 5. The tendency of studies to look at multiple firm performance outcomes (frequently driven more by data availability than policy objectives) raises concerns about the overall results on firm performance. At best, we might interpret this as telling us that it is hard to target particular outcomes. At worst, the overall finding of at least one positive impact in 14 out of 17 studies significantly overstates the likely impact of policies against any given stated objective. The effects on individual outcomes, which we now briefly consider, clearly highlight these concerns.

Table 5: Economic outcomes

Outcome	Evaluation references	Total	Positive	Zero	Negative	Mixed	Share positive
Firm survival	542, 564, 592, 619, 648	5	564, 619	542, 592, 648	-	-	2/5
Employment (firm-level)	542, 548, 564, 565, 592, 619, 622, 650, 740, 742	11	548, 564, 619, 622, 740, 742	542, 565, 592, 648, 650	-	-	6/11
Employment (wider)	588, 590, 617	3	590, 617	-	-	588	2/3
Wages and Incomes	542, 548, 564, 583, 588, 592, 620, 622	8	564, 583, 620, 622	548, 592	588	542	4/8

²⁸ Studies 584 and 590

²⁹ Study 570

Outcome	Evaluation references	Total	Positive	Zero	Negative	Mixed	Share positive
Sales and Turnover	542, 548, 564, 565, 592, 617, 650, 740, 742	9	542, 548, 564, 617, 740	592	565	650, 742	5/9
Profit	542, 562, 565, 599	4	562, 599	542, 565	-	-	2/4
New Start-ups	542, 590, 617, 622	4	617	542, 590	-	622	1/4

Firm survival

Of the five evaluations that looked at firm survival as an outcome, two found positive results while three report no effect on firm survival.

Firm level employment

Six out of 11 evaluations that look at firm level employment find positive effects.

The majority of these programmes are loan subsidies/guarantees, but there is one example of an alternative investment mechanism (US business angels³⁰) that generates positive employment effects. In the cases where there is no statistically significant effect, one evaluation encouraged R&D investment but not growth in additional employment.³¹ In another case,³² the programme is linked to greater access to finance but no further investment or employment growth. Once again, this highlights concerns over the evaluation of multiple outcomes that are not policy objectives.

Profit

Two out of four evaluations that look at profit as a programme outcome find positive effects.

One of the studies reporting positive effects, 562, is an SMS 5 RCT (the highest scoring evaluation type). The other positive evaluation³³ finds positive effects for the majority of firms (those in the middle of the net asset distribution) but no significant impact for a minority of high or low net worth businesses.

One of the evaluations that did not find a positive impact³⁴ focused on the Emergency Credit Guarantee (ECG) in Japan, which was implemented during a severe recession. It found that profitability of firms did not show any significant improvement, implying that conditions attached to the finance encouraged cost-cutting restructuring which impacted on performance. The other evaluation³⁵ found significant effects on other outcomes including business growth, but not profit – both revenues and expenses increase.

30 Study 619

31 Study 592

32 Study 565

33 Study 599

34 Study 565

35 Study 542

Sales and turnover

Five out of nine evaluations found that programmes had positive impacts on firm sales.

Some of the positive effects are quite large (in one case³⁶, revenues grew by 37%, whilst in another³⁷ sales grew by 16-27%³⁸). In the two studies where the results were mixed, study 650 found that sales growth increased with Independent Venture Capital, but not with Government Managed Venture Capital. For the mixed results of study 742, the impact on sales growth is only positive for very young high tech firms, but not significant for older firms. In the case where sales fell (Japan in 2008-9³⁹), the authors note that the decline is again sizeable (equivalent to 12.9% of the volume of assets).

Wages and income

Four out of eight evaluations that look at wages and income as a programme outcome show positive impacts.

The single evaluation that recorded a negative impact⁴⁰ on wages and income found that, while the programme led to employment growth, earnings per capita fell, at least in the short term. This may be a direct result of participation in the programme, which reduced immediate earning opportunities (i.e. income may reduce in the initial stages of expansion as firm re-structuring takes place).

Employment wider economy

Two out of three evaluations which looked at wider employment impacts found positive impacts on job growth in the wider economy, while one found mixed results.

One evaluation⁴¹ which looked at aggregated data at the county level found employment growth to be 4% higher in counties in receipt of the loan guarantees relative to non-recipient counties (for a loan totalling \$1000 per capita); however, mean employment-per-capita growth was found to follow the same downward trend as in non-recipient counties after two years, so results are mixed. Another looked at employment growth across a French industry where firms were in receipt of loan guarantees,⁴² finding both short and long term increases in employment growth in relation to other industries (25% points and 16% points higher respectively).

New start-ups

Of the four evaluations that looked at new start-ups as an outcome, only one found positive results.

36 Study 542

37 Study 564

38 Depending on the type of the firm

39 Study 565

40 Study 588

41 Study 588

42 Study 590

One Italian evaluation⁴³ found positive coefficients related to firm creation. Two evaluations found no statistically significant impacts. One evaluation⁴⁴ found no impact on entry of firms into the market,⁴⁵ whilst another⁴⁶ found there was no increase in the number of firms created relative to industries not in receipt of the guarantee (though the firms that are created are larger in terms of assets and employment). One Mexican evaluation⁴⁷ found that, in areas where new bank branches were opened, there was a statistically significant increase in the number of informal business owners, but no impact on the number of formal businesses. In two of the cases where the impact is zero (in Mexico⁴⁸ and in France⁴⁹), the programmes are targeted quite specifically across a wide range of different criteria, including area, firm size, gender of entrepreneur, industry affiliation and group affiliation.

Tracing through the effects of access to finance programmes

In principle, access to finance programmes are intended to have direct effects (e.g. easing credit constraints), which then feed through to improved firm performance (e.g. survival, productivity, employment growth and so on).

Given the concerns about a) vague objectives and b) multiple evaluation outcomes, we have more confidence in the limited number of evaluations that are able to show an effect on both these immediate outcomes (such as access to finance) and wider economic performance in firms (such as via increased investment).

Of the 11 studies that consider either credit availability or the cost of borrowing (studies 572 and 623 do both), five do not consider any further economic outcomes. One shows that there is a mixed impact on investment.⁵⁰ Of the five remaining studies, only one⁵¹ finds a positive effect on both the economic outcomes – employment and sales - that it evaluates. One study⁵² finds positive effects on survival and employment as well as on future funding (via IPOs). Study 542 finds a positive effect on investment but can only find a positive effect on one out of six economic indicators (sales and turnover). Results for study 590 are similarly mixed, with a positive effect on assets and wider economy employment, but zero effect on start-ups. Results for study 565 are weaker still – finding no effect on investment, employment or profit and a negative effect on sales.

In short, while most programmes appear to improve access to finance, there is much weaker evidence that this leads to improved firm performance. This makes it much harder to assess whether access to finance interventions really improve the wider economic outcomes (e.g. productivity, employment) that policymakers care about and thus whether the large amount of money committed to such programmes is justified.

We also need more evidence on how different programme design elements might impact on effectiveness. It is to this issue that we now turn.

43 Study 617

44 Study 542

45 The authors use a measure of the number of businesses started in the last twelve months as a proxy

46 Study 590

47 Study 622

48 Study 542

49 Study 590

50 Study 584

51 Study 740

52 Study 619

Programme design elements

Targeting

There is no evidence that programmes targeted at SMEs are more or less effective than non-targeted programmes. Other targeted programmes (taken as a group) appear to perform slightly less well.

One important dimension on which programmes differ is in terms of whether they are targeted. Table 6 breaks down studies by the type of targeting.

Table 6: Types of Targeting Utilised

Targeting	Evaluations	References
SMEs	14	548, 562, 564, 565, 570, 572, 573, 584, 590, 592, 599, 623, 740, 742
Area	2	542, 588
Sector	4	562, 572, 590, 592
Scale of Angel investment	1	619
Female micro-entrepreneurs	1	542
No targeting	5	558, 583, 620, 622, 648
Not stated	5	546, 617, 650, 651, 737

By far the largest type of targeting, unsurprisingly, is at small and medium size enterprises. 14 evaluations consider programmes that target SMEs. Of these, 11 report positive effects on one or more outcomes, with five reporting consistently positive effects.⁵³ Five studies report at best mixed results when looking at particular outcomes (in all cases a mix of positive and negative results) while one study found no effect in all but one of the outcomes that it evaluated.⁵⁴ In addition, four studies reported a mix of positive results for some outcomes and negative⁵⁵ or zero⁵⁶ results for others. This is broadly in line with the ten studies that report no targeting (either because they are unclear on whether the programme is targeting or because they explicitly state the programme is not targeted).

Most SME programmes targeted support across a broad range of SME firms, although some focused more specifically on sub-categories of SME; for example, targeted at small retailers,⁵⁷ tech SMEs⁵⁸ and mature SMEs.⁵⁹

53 Studies 562, 572, 599, 623 and 740.

54 Study 592

55 In the three studies (565, 584 and 590), the negative coefficients were for financial outcomes, specifically worsening credit scores (565) and increased probability of default (584, 590) and for firm performance (590; with the authors suggesting that firms were using loans to fund operating losses).

56 Study 548

57 Study 562

58 Study 564

59 Study 590

A small number of programmes were targeted by area,⁶⁰ sector or industry,⁶¹ the scale of Angel investment, or at female entrepreneurs. Generally there are too few evaluations to reach any general findings.

Type of Intervention

The overall results for guarantees and alternative investment mechanisms are broadly similar across various outcomes. Loan guarantee schemes introduced in response to economic crisis perform somewhat worse than long term development schemes in terms of firm performance indicators.

As described above, there are two broad categories of support which may be offered to firms to improve their access to finance: guarantees on conventional lending methods, usually through private financial institutions; and alternative lending or investment mechanisms.

Table 7 divides the evaluations using these two broad categories in addition to reporting results for the other types of schemes which do not fit within them. The table also breaks down guarantees depending on whether they were introduced in response to a structural crisis (such as a recession) or are long-term development schemes.

Table 7: Type of Intervention

Intervention	Evaluations	References
Guarantees	11	548, 564, 565, 570, 572, 584, 588, 590, 592, 599, 623
Crisis	5	564, 565, 570, 592, 599
Long-term development	6	548, 572, 584, 588, 590, 623
Alternative investment (venture capital / angels)	8	546, 619, 648, 650, 651, 737, 740, 742
Loan subsidies	1	562
Lending	1	558
Hybrid (guarantees and lending)	1	583
Informational asymmetries	1	573
Improving local market conditions	2	617, 622
Banking regulation	1	620
Alternative lending (P2P / microfinance / revolving loans)	1	542

Overall the results for guarantees and alternative investment mechanisms are broadly similar. As might be expected, the loan guarantee schemes set up to respond to a crisis on balance perform somewhat worse than those that are long term development schemes. Detailed investigation reveals that this slightly inferior performance is driven by weaker impacts on firm performance (consistent with our findings that all such schemes generate positive effects on credit availability if that is evaluated).

60 Studies 542 and 588

61 Studies 562, 572, 590 and 592

As with sector targeting for the remaining alternative schemes there are too few evaluations to reach any general findings. However, we can see that these programmes appear to perform slightly better than either loan guarantees or the more common alternative investment vehicles (although again the differences are not large).

Public versus private programmes

The overall results for public and private are broadly similar; for hybrid programmes the results are more mixed.

Finally we have broken down the results according to whether the programme is public, private or hybrid. Results are reported in Table 8. The majority of programmes are hybrid involving both public and private elements. Public programmes appear to perform positively across a range of outcomes including income, output and employment. Four out of five studies which are purely public find positive effects in at least one outcome. Five out of the six evaluations which examine purely private programmes report positive effects in at least one outcome; however only half of these report positive impacts in all outcomes that they evaluate. There is no particularly strong pattern in terms of the effectiveness of hybrid programmes.

Table 8: Programme Design

Intervention	Evaluations	References
Public	5	546, 620, 651, 740, 742
Private	6	542, 558, 573, 617, 619, 622
Hybrid	16	548, 562, 564, 565, 570, 572, 583, 584, 588, 590, 592, 599, 623, 648, 650, 737



Summary of findings

Helping businesses access finance can have two kinds of effects on firm outcomes. There may be immediate effects on improved access to finance (credit availability, cost of borrowing, etc.). In turn, these need to translate into improved firm performance (captured by employment, productivity and so on) if the programme is to have an impact on local economic growth. Even if these positive effects occur at the firm level, however, access to finance could have an adverse impact on local economic growth if it helps weaker firms survive.

What the evidence shows

- Access to finance programmes had a positive impact on at least one firm outcome (e.g. credit, employment, sales) in 17 out of 27 evaluations.
- Programmes have a positive effect on firm access to *debt* finance either in terms of the availability of credit or the cost of borrowing (or both). The impact on access to equity finance is mixed (and available evidence limited).
- The impact of policies on investment and assets is mixed.
- There is some evidence that loan guarantees may increase default risk.
- Access to finance had a positive impact on at least one firm performance outcome (e.g. employment, and sales) in 14 out of 17 evaluations.
- However, these overall patterns hide much more mixed results for specific firm performance outcomes, with only half the evaluations typically recording a positive effect when looking at a given outcome.

Where the evidence is inconclusive

- There is no evidence that programmes targeted at Small and Medium Sized Enterprises are more or less effective than non-targeted programmes.
- The overall results for loan guarantees and alternative investment mechanisms are broadly similar. Loan guarantee schemes introduced in response to economic crisis perform somewhat worse than long term development schemes in terms of outcomes for firms.

- The overall results for public and private are broadly similar, the results for hybrid programmes are more mixed.

Where there is a lack of evidence

- We found very few studies that look at the impact of schemes on both access to finance (direct effect of the scheme) and on the subsequent performance of firms (indirect effects of the scheme).
- While most programmes appear to improve access to finance, there is much weaker evidence that this leads to improved firm performance. This makes it much harder to assess whether access to finance interventions really improve the wider economic outcomes (e.g. productivity, employment) that policymakers care about.
- As with other reviews, we found very few studies that gathered (or had access to) information on scheme costs. As a result, we have very little evidence on the value for money of different interventions.



How to use this review

This review considers a specific type of evidence – impact evaluation. This type of evidence seeks to identify and understand the causal effect of policy interventions and to establish their cost-effectiveness. To put it another way they ask ‘did the policy work’ and ‘did it represent good value for money’?

The focus on impact reflects the fact that we often do not know the answers to these and other basic questions that might reasonably be asked when designing a new policy. Being clearer about what is known will enable policy-makers to better design policies and undertake further evaluations to start filling the gaps in knowledge.

Supporting and complementing local knowledge

In our employment training and business advice reviews, we set out a number of ‘Best Bets’ which outlined what tends to work in those two policy fields based on the best available impact evaluations. These best bets might be in terms of the intended outcomes (e.g. business advice programmes show better results for sales than productivity or employment) or in terms of policy design features (e.g. on the job training is more effective than class-room based training). These ‘Best Bets’ do not generally address the specifics of ‘what works where’ or ‘what will work for a particular individual’. But they do provide an important complement, rather than a substitute, for local, on-the-ground knowledge.

As should be clear from our summary above, despite the availability of relatively high quality evaluations, the evidence provides no such guidance for access to finance programmes. Impacts across key outcomes such as employment, profit or sales are generally similar with around 50% of programmes having a positive impact on any given outcome. Overall programme impacts do not differ much between targeted and non-targeted programmes, between loan guarantee and alternative finance vehicles or between private, public and hybrid programmes. In short, the available evaluation evidence provides little guidance to local policymakers on the detail of policy design in this area.

Providing general guidance on what works

While most programmes appear to improve access to finance, there is much weaker evidence that this leads to improved firm performance. Note that we have good in-principle reasons to think these programmes may improve growth outcomes across the local economy – but the evidence suggests that such impacts are not consistently achieved in practice.

This also suggests that direct programme outputs (e.g. loans made or guaranteed) are unlikely to be good indicators of programme impact on wider local economic growth. Similarly, sustainability of a programme may provide a useful indicator that there has been no increase in default risk, but this is no guarantee of an impact on the local economy further down the line.

In short, standard monitoring of performance indicators appears to provide no guidance to policy effectiveness in terms of improving local economic growth. More evidence is needed to understand whether the access to finance interventions really improve the wider economic outcomes (e.g. productivity, employment) that policymakers care about and thus whether the large amount of money committed to such programmes is justified.

Helping to fill the evidence gaps

The Centre's longer term objectives are to ensure that robust evidence is embedded in the development of policy, that these policies are effectively evaluated and that feedback is used to improve them. To achieve these objectives we want to:

- work with local decision makers to improve evaluation standards so that we can learn more about what policies work, where.
- set up a series of 'demonstration projects' to show how effective evaluation can work in practice.

Interested policymakers please get in touch.

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Appendix A: Findings by Outcome

Table A1: Findings by outcome: financial outcomes.

Outcome	Evaluation references	Total evaluated	Positive	Zero	Negative	Mixed	Share positive
Improved credit rating	565	1	-	-	565	-	0/1
Credit availability	565, 570, 572, 584, 623, 737, 740	7	565, 572, 623, 740	-	-	570, 584, 737	4/7
Reduced borrowing costs	572, 584, 590, 623	4	572, 584, 590, 623	-	-	-	4/4
Increased debt	584	1	584	-	-	-	1/1
Investment	542, 564, 565, 584, 592	5	542	565, 592	-	564, 584	1/5
Assets	542, 590, 599, 648, 742	5	590, 599	542	648	742	2/5
Reduction of default risk	570, 573, 584, 590	4		570	584, 590	573	0/4
IPO exit	546, 619, 648	3	619	-	648	546	1/3

Table A2. Findings by outcome: economic outcomes.

Outcome	Evaluation references	Total evaluated	Positive	Zero	Negative	Mixed	Share positive
Firm survival	542, 564, 592, 619, 648	5	564, 619	542, 592, 648	-	-	2/5
Employment (firm-level)	542, 548, 564, 565, 592, 619, 622, 648, 650, 740, 742	11	548, 564, 619, 622, 740, 742	542, 565, 592, 648, 650	-	-	6/11
Employment (wider)	588, 590, 617	3	590, 617	-	-	588	2/3
Wages and Incomes	542, 548, 564, 583, 588, 592, 620, 622	8	564, 583, 620, 622	548, 592	588	542	4/8
Sales and Turnover	542, 548, 564, 565, 592, 617, 650, 740, 742	9	542, 548, 564, 617, 740	592	565	650, 742	5/9
Profit	542, 562, 565, 599	4	562, 599	542, 565	-	-	2/4
New Start-ups	542, 590, 617, 622	4	617	542, 590	-	622	1/4
Productivity	564, 592	2	-	564, 592	-	-	0/2
Research and development	564, 592, 648, 651	4	592, 651	564	648	-	1/2
GDP	617	1	617	-	-	-	1/1
Age of entrepreneur	617	1	-	-	617	-	0/1

Appendix B: Evidence Reviewed

Ref No.	Reference
542	Angelucci, M., Karlan, D., and Zinman, J. (2014). Microcredit Impacts: Evidence from a Randomized Microcredit Program Placement Experiment by Compartamos Banco.
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