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ABSTRACT

Do Interventions Targeted at Micro-Entrepreneurs and Small and Medium-Sized Firms Create Jobs? A Systematic Review of the Evidence for Low and Middle Income Countries*

Worldwide 600 million jobs are needed over the next 15 years to keep employment rates at their current level. Governments, non-governmental organizations and donors spend on targeted programs and broader policies to enhance employment creation and the creation of new firms. Because most employment in low and middle income countries is in micro, small and medium-sized enterprises, these firms are especially targeted by such interventions. Despite these efforts, not much is known about which of these interventions are really effective and under which conditions particular interventions work. This systematic review synthesizes the existing evidence on the impact of these programs. Overall the review shows that creating employment is a very complex challenge. Many conditions have to be met before interventions in favor of individual enterprises do not only improve business practices and performance but also lead to additional jobs. A striking finding is that the study design matters for the impacts found; randomized controlled trials find systematically smaller effects than quasi-experimental studies. A significant shortcoming of the literature is that almost nothing is known about long term effects and cost effectiveness.

JEL Classification: D22, G21, J21, O10

Keywords: employment, active labor market policy, firm creation, micro, small and medium sized firms, impact evaluations, systematic review

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1. Introduction

Creating new jobs and in particular 'good jobs', i.e. jobs in high productivity sectors and offering decent working conditions, is one of the major challenges most low and middle income countries face. According to the 2013 World Development Report on jobs, worldwide 600 million jobs are needed over the next 15 years to keep employment rates at their current level (World Bank, 2012). Governments, non-governmental organizations and donors spend large amounts of money for targeted programs and broader policies to enhance employment creation and the creation of new firms. Because most employment in low- and middle income countries is in micro, small and medium-sized enterprises (MSMEs) (see e.g. Ayyagari et al., 2011), these firms are often targeted by such interventions. Typical interventions include the provision of finance and financial services, entrepreneurship training, business support services, wage subsidies and measures that transform the business environment for these firms. Despite these efforts, not much is known about which of these interventions are really effective, or, more importantly, under which conditions particular interventions work.

With the trend to conduct rigorous impact evaluations of development interventions, many researchers have started to look more closely at programs and policies that are targeted at MSMEs. This systematic review synthesizes the existing evidence on the impact of these programs and extracts the main lessons. The review reveals several factors and design features likely to increase the probability of interventions aimed at enhancing employment being successful. However, the review also still reveals important knowledge gaps. This stands in sharp contrast to the high number of programs and projects that claim to know how to create jobs and on which considerable funds are being spent. We focus on the following five policy areas: (i) access to finance, (ii) entrepreneurship training, (iii) business development services, (iv) wage subsidies, and (v) improvements to the business environment (e.g. registration procedures). There are many other interventions and policies that may have employment effects such as improvements in energy supply, road construction or trade and exchange rate policies, but given that such policies are typically not targeted it is hard to find a counterfactual and to establish causal evidence. Therefore such interventions and policies are not considered in this review. Our work builds on a few earlier reviews which however have not focused specifically on employment creation in MSMEs in developing countries or considered only a sub-set of the policies we focus on.

McKenzie and Woodruff (2014) review the quality and findings from business training and entrepreneurship evaluations. They focus in particular on statistical power, measurement issues and attrition. Across the reviewed studies, they find only modest impacts on survivorship but stronger impacts on business creation. Bruhn and McKenzie (2013) review the rigorous evidence on entry regulation and formalization of microenterprises. They conclude that formalizing firms is generally difficult and the effects of formalization on firms are in most cases very modest if not insignificant. In an earlier publication McKenzie (2010) reviews a few finance evaluations (all pre-2009) in which the results not only raise questions about the appropriateness of many existing micro-finance programs but also where the pure quantity of studies also suggests that many more impact evaluations need to be conducted to close the relevant knowledge gaps. Tripney et al. (2013) conducted a systematic review of post-basic technical and vocational education and training (TVET) interventions to improve employability and employment of TVET graduates in Low and Middle Income Countries (LMICS). While the authors are concerned with the same outcome as this systematic review, they do not explicitly focus on employment creation in MSMEs. This applies also to Stewart et al. (2012) who assessed whether different financial instruments effectively enable poor people, and especially women, to engage in meaningful economic opportunities in LMICs, Vaessen et al. (2012) who are currently assessing the effect of microcredit on women's control over household spending, Hagen-Zanker et al. (2011) who focused on the impact of employment guarantee schemes and cash transfers on the poor, Duvendack et al. (2011) who analyzed the evidence on the impact of microfinance on the well-being of the poor, Betcherman (2014) who reviews studies that explore the effects of labor market regulations on employment and other outcomes as well as Stewart et al. (2010) who assessed the impact of microfinance on poor households in Sub-Saharan Africa.

Other reviews focus on policy areas we do not cover, for the reasons given above. These include Cirera et al. (2011) who synthesized the evidence of the impact of tariff reductions on employment and fiscal revenue, Cirera et al. (2013) who focused on the impact of free trade zones on employment and wages and Nataraj et al. (2012) who assessed the impact of labor market regulations on employment in low income countries. Other authors focused on youth employment, but again, do not consider MSMEs specifically (Betcherman et al., 2007; Puerto, 2007).

A meta-analysis on entrepreneurship programs in developing countries conducted by Cho and Honorati (2014) is so far the most relevant synthesis of evidence with respect to the purpose of this review. The authors considered a sub-set of the studies we review in the area of finance and training. They focus on a whole range of business outcomes and find that finance and training interventions to promote MSME development are more effective in changing intermediate outcomes, like business knowledge and practice, than increasing a general set of labor market activities. For the latter, the combination of training and finance proves to be the most effective though this depends also on the type of beneficiary that is being targeted. This review will substantially add to the work done by Cho and Honorati (2014), first, by updating and broadening the evidence base considerably, taking into account also policy areas other than finance and training and, second, by choosing a strong focus on employment and business creation.

The remainder of this review is organized as follows. In Section 2 we lay out our inclusion criteria and the search strategy. In Section 3 we propose a theory of change which will guide our analysis along the causal chain, linking program inputs and employment outcomes. In Sections 4 and 5 we present our search results and a narrative synthesis of the evidence. The results from a meta-regression analysis are presented and discussed in Section 6. In Section 7 we conclude.

2. Inclusion criteria and search strategy

2.1 Inclusion criteria

We include studies that explicitly focus on MSMEs in the formal, as well as informal sector. We limit the analysis to urban, as well as rural non-farm employment and firms, i.e. farms and employment on farms are not considered. Although, there are no common criteria that are applied uniformly to identify MSMEs, neither by researchers nor by statistical offices, we use an employment criterion and set the threshold at 250 employees. As micro-enterprises we define firms with less than five workers. Firms are considered being in the category small if they have between 5 and 19 workers and medium sized firms are firms with 20 and more workers (but less than 250). Evaluations of interventions that target the labor force directly are only included if the intervention was implemented to enhance the creation of new MSMEs (incl. self-employment). Our systematic review is focused on the context of developing countries. We use the thresholds of the World Bank and consider countries as developing countries if they show a GNI below USD 12,476 per capita, calculated using the World Bank Atlas method. Thereby, we look at low and middle income countries. Specifically, the following income groups provided by the World Bank classification are included: low income (USD 1,025 or less per person and year), lower middle income (USD 1,026 - 4,035), and upper middle income (USD 4,036 - 12,475).

We define employment creation as the emergence of new jobs in existing MSMEs (whether privately or publicly owned) and as jobs that arise through the creation of new MSMEs. The latter also includes self-employment. Whenever a certain intervention creates some and destroys other jobs simultaneously, we explicitly consider – if the data allows – both gross and net employment generation. However, very often only one of the two is available. We consider any form of employment under acceptable working conditions, conditional on the specific context studied. This includes paid employment, as well as paid and unpaid family employment.[1] The outcome can be measured in the number of employees or its growth rate. Studies are not included if they focus exclusively on hours worked, labor intensity, wages or labor supply without considering employment per se.

[1] The latter usually implicitly paid.

Based on an initial screening we decided to classify all studies into five intervention categories: access to finance, entrepreneurship training, research and development, business development services, and private sector incentive schemes. In addition to these specific programmatic interventions, we also decided to include studies that analyze the impact of more general conditions, generally referred to as the business environment on MSMEs (*policy* interventions) as long as it was possible to establish causality between the policy and employment creation in a credible way.

Studies are included if they can establish a credible causal relationship between a programmatic or policy intervention and job creation in MSMEs. Hence, we include studies if they consist of an impact evaluation based on an experimental design such as a randomized controlled trial (RCT), as well as on quasi-experimental designs including propensity-score matching, instrumental variables, regression discontinuity designs or difference-in-difference estimation. For quasi-experimental designs, we include studies that are based on panel data as well as cross-sectional data, analyzed either at the firm or individual level. We only include studies that report quantitative impacts.

Moreover, inclusion/exclusion is not based on publication status. If an identified study was still ongoing, the authors were contacted in order to check whether the results were already available for inclusion into this review. Evaluations in any of the five following languages are included: English, Spanish, French, Portuguese, and German.

2.2 Search methods to identify relevant studies and data extraction

A range of different search methods have been applied to ensure the identification of recent, ongoing, as well as unpublished studies. These include the searching of electronic databases, screening of relevant websites, hand-searching of key journals, literature snowballing, and contacting researchers and key experts in the topic. Details on the search strategy can be found in Appendix A. Further details are provided in Grimm and Paffhausen (2014).

Information from the included studies has been extracted systematically. The information extracted consisted of the general information of the study, i.e. author(s), title, year and type of publication. Furthermore, the country or countries and the target population were characterized for each study, as well as the outcome(s) measured and the type(s) of intervention. With respect to the type(s) of interventions, we recorded whether the intervention was a stand-alone or joint intervention. If it was the latter, the different components of the intervention have also been documented. Whether employment or firm creation was a primary objective of the intervention was also recorded. Furthermore, included studies were characterized according to their study design, comprising methodology, sampling methodology, sample size etc. Finally, information regarding the internal and external validity was extracted from the studies. Data extracted from included studies, especially those involving judgments by the coder, have been discussed extensively among the two researchers involved.

3. Theory of change

The considered interventions affect firms and prospective firms in many different ways. Some relax capital market constraints, others improve management skills and business practices, again others reduce the cost of labor or ease formalization procedures. The figure below shows a simplified result chain for all interventions together. Next to the final outcome of interest these chains also show intermediate outcomes such as investment, productivity, output and profits. For all interventions it is obvious that employment does not necessarily have to respond. If capital market imperfections are relaxed and investment increases, employment effects will only occur if the investment is large enough and if labor is a complement to the investment and not a substitute. Labor saving investments could even reduce employment. Credits that are used to increase inventories are also unlikely to increase employment. Interventions, such as training, that increase the productivity will only have employment effects if output is increased following falling costs. Hence, the price elasticity of demand and the degree of competition matter. In other words growth at the intensive and extensive margin needs to be distinguished. Only the latter goes hand in hand with more employment. Yet, if

employment is not increased, increasing productivity may secure survival of the firm and therefore prevent jobs from disappearing, so in that sense it would also imply a contribution to employment.

[Figure 1 about here]

Employment effects also require that labor supply is sufficient, i.e. that workers queue for jobs and can be hired at any time. An assumption that is probably realistic for most urban areas in the developing world at least as long as unskilled workers are concerned. If skilled workers are concerned this might not always be a good assumption. Some interventions may also imply negative externalities on non-beneficiaries. In a context in which economic growth is weak or completely absent an increase in productivity in one firm may drive another firm out of the market and hence the net job effect might be zero, or at least reduced.

Temporary wage subsidies will have lasting impacts only if the subsidized job or 'on the job training' increases the worker's productivity to such an extent, that the firm continues hiring that person even if the subsidy expires. This would imply that the temporary subsidy allows reaching a higher growth trajectory that would not be reached without the subsidy. Obviously such programs may also work because they solve a behavioral bias, i.e. it may provide a hiring incentive to those that are very risk averse and reluctant to hire, although it would be beneficial for them.

A simplification of registration procedures will only have any effect on employment if the registration allows to access new markets or to reduce the costs of certain inputs, such as public services or if it improves the access to finance. But even then the above mentioned caveats apply.

Finally, it is important to note that these result chains have been developed from the perspective of the evaluator, based on the objectives of proponents of such interventions, which justify them on the basis that they eventually contribute to employment creation. They are however not necessarily the result chains of the implementers of such interventions and most certainly they are not the result chains of the beneficiaries. For instance, an NGO delivering a microcredit program might do this with the aim of promoting female empowerment or increasing household income only. A researcher who evaluates this program then however might assess the effect on a broad range of outcomes, including employment in household enterprises. The person obtaining the microcredit might be interested in merely smoothing consumption, keeping her business going or creating a new subsistence activity but not necessarily to employ more staff.

4. Search procedures and results

4.1. Search and identification

We organized our search strategy around two alternative search approaches. The first approach combined sets of search terms referring to the population, outcome and type of study and was conducted between February 5 and February 20, 2013. This search resulted in 1,200 hits. After removing duplicates, there were still 932 records left that needed to be screened. The second approach focused on the different intervention categories considered in this systematic review and was carried out from May 13 to June 16, 2013 and resulted in 2,446 hits. A substantial number of these were again duplicates, so that after removing them, there remained 1,343 records to screen.

We then merged the results of both search approaches, which together amounted to 2275 hits. Again, duplicates had to be removed so that the final sample included 1924 records. These records were screened successively, applying inclusion criteria, in a first step, to titles and abstracts only. This was done mainly by one researcher. However, in order to minimize bias, a second researcher randomly double-screened about a quarter of the studies that had been excluded by the first researcher. There were virtually no discrepancies in judgment for this sample of studies. Moreover, unclear cases were screened additionally by the second researcher, and where necessary, a conclusion was reached through discussion. Thereby, already about 85 percent of studies could be excluded. It was not always possible to reach a decision based on only the title and abstract of a study. In those cases, we resorted to screening the text. Overall, the texts of about 300 studies were screened to reach a final decision on

eligibility and eventually inclusion. A common reason for early exclusion of studies was that they did not focus on low and middle income countries. In addition, although many were concerned with SMEs or smaller enterprise, these were no impact evaluations and typically did not document any changes in the outcomes relevant for this review. Obviously, a non-negligible part of the papers was also totally off topic.

The whole procedure left 139 studies for thorough full text screening. Out of these, four records could not be obtained in full text. However, further cross-checks suggested that they were, with a very high probability, irrelevant for the purpose of this review. There was one study that was judged highly relevant based on the abstract, but the study could not be obtained. The database search also identified 15 potentially relevant ongoing impact evaluations. We hence contacted the authors in order to see whether any preliminary results were already available. As of February 15, 2014 we had received information on the status of five ongoing impact evaluations. None of these however were in a stage in which any findings could already be included in our review. Since up to this point, we had not yet identified a single study for inclusion which had been conducted before 1990, we decided to definitely exclude studies that had been conducted before that date. For all remaining papers the final decision on inclusion or exclusion was always based on the full text of the study. Finally, 34 studies were included from the database search.[2]

The screening of websites of key donors and funders of MSME interventions, as well as research institutions resulted in one further study that was included. In addition to the database search and website screening, a number of relevant academic journals were searched for studies to be included. This enabled us to identify three more studies to be included. Literature snowballing of the World Bank World Development Report 2013 and other (systematic) reviews resulted in four more records that were included. The references of included studies were thoroughly screened, resulting in ten more studies to be included in the review. Furthermore, we contacted key researchers and provided them with a preliminary list of included studies as of September 9, 2013, asking whether they were aware of any further studies that met our inclusion criteria which we should include in the review. We received answers from three researchers as of February 15, 2014. This exercise resulted in no further study to be included in the review. Finally, two studies were included in the review that were already known to the authors but had not been identified through any of the sources mentioned above. Altogether, this comprehensive search process hence led to a final sample of 54 studies that have been included in this review. The entire process is illustrated in Figure 2.

[Figure 2 about here]

4.2 Short characterization of included studies

Population and context

The 54 studies included in this review cover a wide range of countries from all major world regions. The majority of studies focuses on Latin America. This can be explained by the fact that in particular Latin American countries have experimented a lot with active labor market policies over the past two decades and that they have earlier than others started to rigorously evaluate many of their policies. Interestingly, the share of studies that have been conducted in Sub-Saharan Africa is with nine studies larger than we had initially expected, but it still constitutes only a small evidence base, given that employment creation is a major preoccupation and considerable amounts of foreign aid are spent on private sector development in that region. Most studies have been conducted in upper middle income countries (32) and lower middle income countries (15). Only 7 studies took place in low income countries. The majority of the studies focus on micro-enterprises (with up to five employees). In this category we also included studies that target individuals and households with the aim of creating new microenterprises or enhancing self-employment. A total of 36 studies fall into this group. Another 18 studies target larger firms, which could be categorized as small and medium sized enterprises. These are generally already established and mostly registered (i.e. formal) enterprises.

[2] Note that the second search approach described above led to a contribution of only three additional studies to that sample that were not found by the first approach, giving us confidence in our search strategy.

[Table 1 about here]

Interventions

Most of the studies in our sample analyze interventions that aim to remove credit constraints of micro, small and medium sized firms (26). All but two of them imply the provision of capital, either in form of micro- and larger loans or through the provision of cash- and in-kind grants. Some of the interventions focusing on access to finance are also combined with other interventions. In most cases these are entrepreneurship training interventions. For the sake of completeness such interventions are analyzed below in both groups - finance and entrepreneurship training - whenever justified, based on the specific features of the intervention evaluated. The sample of studies focusing on entrepreneurship training includes 20 studies. The interventions covered are somewhat diverse in that they consist of general business and financial literacy trainings, as well as more customized and specialized support, or even vocational training with the aim of developing these skills for self-employment. They also differ regarding the duration of the treatment and sophistication of training content. For the other categories, the samples of included studies are all relatively small. There are six studies in the area of business development services. The interventions analyzed in these studies are very heterogeneous; they include for instance counseling, supplier development, product and process innovation, and the provision of working premises. We identified only one relevant study related to research and development. Since the intervention evaluated in this study had many features of business development in general, we decided to review the study together with the five other studies we had identified in the area of business development services. The studies focusing on private sector incentive schemes to enhance employment all rely on wage incentives. Overall we found four relevant studies to be included in this review. Three of them assess the impact of wage subsidies on MSMEs, while one study looks at the effect of minimum wage legislations. They are also analyzed jointly with business development services and research and development. Finally, we identified five studies that assess interventions falling into the category of improvements to the business environment. These studies all deal with interventions to achieve formalization of MSMEs and the impact of formalization on employment creation.

Outcomes and impacts

As set out before, we included studies that assessed the impact of interventions relevant to MSMEs on either changes in employment levels in these enterprises, or the creation of new enterprises, including becoming self-employed. There are also studies that assess the impact on both outcomes which were then also both used. We always extracted the evaluators' most preferred estimate. Robustness checks were considered to assess the quality and reliably of the estimates, but are not retained for the meta regression analysis below.

Some studies provide impacts at different points in time, such as short-term and long-term impacts. Others offer impacts for stand-alone and mixed interventions. In these cases we have more impact observations than studies in our sample. Hence, from the 54 studies we included in this review, we have a total of 116 impact estimates. Most of the impacts we retrieved (about 60%) represent the impact on employment in treated firms. The remaining impacts measure business creation and self-employment. We do not consider explicitly firm survival as only few studies provide results for this outcome. We express impacts in terms of their standardized mean difference (SMD), i.e. as the ratio between the change in the outcome due to the intervention divided by the standard deviation of the outcome in the control group (or at baseline). If the outcome is a binary outcome such as 'having set up a firm or not' we use the risk ratio. In those studies where such impact measurements were not directly provided (in fact the large majority) we computed them ourselves. However, some studies do not provide all the necessary information, in these cases we based the estimate just on the reported *t*-values of the impact and the sample sizes of treatment and control groups using the formulas given in Lipsey and Wilson (2001). Using different computation methods, on the one hand, reduces the comparability of the estimates. On the other hand it limits the number of studies for which no effect size can be computed.

Study design and methods applied

Of the 54 studies that have been included in this review, 28 studies employ quasi-experimental designs while 26 studies are based on RCTs. RCTs are especially relevant for impact evaluations in the area of entrepreneurship training and finance. Only one fourth of the studies that evaluate the impact of entrepreneurship training use a quasi-experimental design. Studies that assess finance interventions are divided relatively evenly across different study designs, with 13 studies employing an experimental design and another 13 an RCT. Within the group of RCTs, the studies focus exclusively on microenterprises and potential entrepreneurs. Note that this is also the case for the entrepreneurship training interventions. The RCTs have all been emerging in the recent years, and a number of studies are ongoing at this moment. This shows the high interest in credibly establishing the impact of popular interventions such as microfinance, as well as increasing possibilities to conduct such trials in this area. Studies included in the areas of business development services, wage subsidies, and business environment, almost exclusively rely on quasi-experimental designs such as difference-in-difference, propensity score matching or instrumental variables to deal with possible selection effects.

5. Synthesis of the evidence

5.1 Access to finance interventions

Most of the 26 studies that are concerned with finance interventions examine the effectiveness of microcredit schemes (20 studies), followed by conditional or unconditional cash- or in-kind grants (four studies) and two studies that just introduce changes to existing credit schemes, such as Field et al. (2011), who evaluate the extension of the period until the first repayment is due and de Mel et al. (2013b) who provide savings incentives. Not a single study could be identified that looks at the employment effects of micro-insurance. All studies are listed and briefly presented in Appendix B. Some of the interventions analyzed here are stand-alone interventions, whereas others are combined with entrepreneurship training or other interventions with a completely different scope. This is for instance the case in the study of Tarozzi et al. (2013), where microcredit is combined with family planning services. The amount of finance involved is typically between USD 100 and USD 2,000. By definition micro-credit programs target poor households and micro firms. This must be taken into account when compared with other types of finance interventions that target larger enterprises (small to medium sized firms), which are already formalized, with the aim of expanding these businesses. The range of these studies reflects well the dominance of micro-credit in the debate about firm support as well as the common belief that limited access to financial services is a major constraint for the expansion of micro, small and medium enterprises. However, it should not be forgotten that the range of studies was limited by the interventions' (quasi-)experimental evaluability. Interventions to overcome financial impediments via developing financial markets in general, for example by extending refinancing maturities for banks, were not covered.

With respect to employment creation most micro-credit schemes turned out to be rather unsuccessful; only 16 out of 45 impact estimates, which were measured in the 26 studies on access to finance, show a statistically significant increase in employment or firm creation. 27 out of the 45 treatment effects were not statistically significant. In two cases, a statistically significant negative effect was found. The sign and significance of program effects can be found in Appendix B. Table 2 provides the distribution of effect sizes. The generally small, and often statistically insignificant, effects reflect of course also the often very low power of these evaluations. In some studies power is further reduced by rather low program take-up rates. Positive effects on employment, if found at all, were only small, especially for already existing small and micro enterprises. Major effects were achieved with respect to the creation of new (mostly micro) enterprises and the expansion of already larger, well established and profitable firms. The success cases are more concentrated in (upper) middle income countries rather than low income countries, where the focus however is mainly on small to medium sized firms that are already established and not micro-credit programs but rather larger scale public credit lines and guarantee schemes. Yet, those interventions are predominantly evaluated using quasi-experimental study designs which may, on the one hand, imply that selection effects could not be eliminated entirely, or, on the

other hand, that these evaluations often rely on larger samples and less heterogeneous firms which increases the power of these analyses and hence the probability of detecting significant effects.

[Table 2 about here]

Apart from power considerations, the high proportion of statistically insignificant results does not necessarily reflect the futility of this measure but is probably due to the fact that employment generation is typically not a primary objective of micro-credit programs. Rather, income stabilization most frequently seems to be the major intention. Most enterprises make use of the credit or cash grants, if directly offered, but the studies' findings suggest that the financial resources are primarily used as working capital, i.e. invested into inventories. Only on rare occasions would these result in fixed capital investments in machines or buildings. De Mel et al. (2013b) for example detect a significant effect of a savings scheme on investment in inventories, while there is no significant effect on fixed capital investment. Likewise Arraiz et al. (2012) find that credit had no impact on capital stocks and suggest that firms use credit rather to increase their working capital. Hence, such interventions might have no employment effects, but in many cases they show significant impacts on sales and revenues.

With regard to business profits, the majority of studies does not detect an impact (see Angelucci et al., 2013; Augsburg et al., 2012; Barnes, 2011; Crepón et al., 2011; De Mel et al., 2013b; Karlan and Zinman, 2011; and Nelson, 2011). Karlan et al. (2012) find that a capital grant even lowered profits, while Banerjee et al. (2013) detect a positive impact, but only for existing, relatively larger and the most profitable microbusinesses. Only Marcours et al. (2012) find that cash grants for investment resulted in higher profits from non-agricultural self-employment activities more generally. These effects were substantial even two years after the end of the intervention.

It is debatable whether the limited evidence for an improvement in business profits is due to rather short follow-up periods. Of course, it might be that it takes some time for the entrepreneur to be able to use the capital treatment effectively and adjust accordingly in order to reap the benefits of increased investment. However, as Augsburg et al. (2012) argue, most microenterprises are very simple in nature so that it should not take all too long to generate profits as a result of the investment. It also turns out that most loans seem to be simply too small and their maturities too short to lead to large changes in the capital stock and the production technology. For instance, a tailor who – thanks to a micro-credit – switches from a mechanical to an electric sewing machine may neither have the need nor the profitability to immediately hire an additional worker. Nevertheless, he may well see an increase in performance as measured by revenues, profits and, of course, business investment. Hence, we find growth at the intensive rather than at the extensive margin. Field et al. (2011) indirectly provide supportive evidence to this hypothesis in showing that the details of the loan contract matter. They find that short repayment periods, which over the loan period translate into lower outstanding loans and shorter maturities, prevent poor entrepreneurs from investing since they fear not being able to repay on time (Field et al., 2011). Moreover, in the cases where potential business starters or subsistence-type of enterprises are targeted, a reason for failure may obviously also be that very often there are competing needs and hence instead of investing, borrowers spend the credit on health care, education, housing improvements and so on. Based on such insights Karlan and Zinman (2011) suggested that microcredit may need to be combined with detailed business planning and extraordinarily close monitoring in order to assure that it leads to increased investment in the business.

Programs targeting particularly women also appear to be less successful in employment creation than programs without such a focus. This suggests that women face additional constraints which need to be overcome in order to increase the return to finance. Mothers, for instance, tend to spend on average more on food, clothes and health for the household, when compared to fathers, and may therefore have less to spend on capital goods. Resisting pressure from family members and relatives to share financial resources might also be more difficult for women, obliging them to share funds even when they would prefer to invest. In many settings, women still have lower education than men, they have no access to formal banking services without consent of their husband, they have no property rights and are not allowed to leave their house alone. All these factors may explain why, on average, loans to women have lower returns than loans to men.

Finally, most of the studies in this sample generally assess the effect at one point in time. A notable exception is De Mel et al. (2013b). They observe treatment effects at seven different points over a horizon of two years and conclude that positive effects on business performance arise only in the short-run, suggesting that the savings program speeds convergence to the steady state of the businesses but has no transformative effect by changing the long-term steady state of the business itself. So in the end, while the interventions may not be able to increase profits or employment in existing enterprises, they nevertheless may have an important impact on securing the continuation of existing businesses, thereby also contributing to employment protection.

5.2 Entrepreneurship training

The second most frequent intervention category in the retained sample was entrepreneurship training. The review includes 20 studies that fall into this category (see Appendix B). Training measures comprise business skills training, business plan development, financial literacy training, technical and vocational training (in-class and on the job), and life skills training. Hence, not surprisingly, even within the category of entrepreneurship training interventions the heterogeneity is quite substantial. Entrepreneurship training provided to beneficiaries varies in that it can be either general, or specifically tailored to the businesses and difficulties of the beneficiaries. Most evaluations assess the impact of general entrepreneurship training in the areas of business management, accounting, financial literacy or the development of vocational skills (15 cases). Bandiera et al. (2012), for instance, analyze the provision of vocational training in activities like hair-dressing, tailoring, computing etc. to adolescent girls. Other examples can be found in Bruhn and Zia (2011), Calderon et al. (2013) and de Mel et al. (2012), who assess interventions that provide general business and financial education trainings to microcredit clients and individuals interested in starting a new business. A few interventions provide more specifically tailored assistance (4), which may come in the form of business plan development assistance (see Klinger and Schündeln, 2011; Jaramillo and Parodi, 2003) or management consulting services focusing on problem diagnosis and solving (see Bruhn et al., 2013). One study analyzes both, an intervention that provides general business training to microentrepreneurs as well as the combination of this general training with individualized support (see Valdivia, 2011). The interventions analyzed here further vary substantially regarding their duration. While some training courses are delivered over a few days only (see for instance Bruhn and Zia, 2011), others are more substantial. The business training evaluated by Valdivia (2011) for instance was delivered over twelve weeks in three sessions per week that lasted three hours each. Vocational entrepreneurship training tends also to be more substantial in duration, varying further with the specific occupations for which the training is provided.

From this sample of studies it appears that skill constraints are believed to be more relevant to microenterprises than for already established SMEs: the majority of interventions targets microenterprises with up to five employees or aims to enhance self-employment in groups highly at risk of unemployment, such as the youth. Often entrepreneurship training interventions, especially when provided for business start-up, are delivered jointly with start-up finance, indicating that skill constraints are typically not assumed to exist in isolation. The majority of studies included here are based on RCTs; only five employ a quasi-experimental design. All together 25 treatment effects were analyzed.

Looking across all studies, 11 out of 28 treatment effects show significant positive employment effects (see Appendix B). 17 treatment effects were not statistically significant. As for finance interventions, low statistical power of many evaluations is probably partly causing the large number of null results. Interestingly, a few studies found negative employment effects. Drexler et al. (2013) found that training led to a reduction of employees for low-skilled business owners, and Valdivia (2011) as well as Calderon et al. (2013) found microentrepreneurs that participated in general business training to be more likely to close poorly performing businesses. This suggests that entrepreneurship training enhances the entrepreneurial spirit and forces (potential) entrepreneurs to think more carefully about the business model and its profitability. In fact, even non-existent or negative employment effects can be good news for trainings' effectiveness as entrepreneurship training was found to help non-profitable firms either to become profitable or to close down. Likewise, entrepreneurship training can prevent non-profitable business ideas from being started.

Overall, most training interventions have difficulties in changing actual business performance like profits. Yet most programs produce significant improvements in business skills and behavioral skills, and sometimes also higher optimism and motivation, although some of these changes might be due to a changed reporting behavior after the start of the intervention (Drexler et al., 2012). Thus, employment seems to come last in the result chain of entrepreneurship training. Some studies report higher investment, very few studies report process or product innovations and sometimes also improvements in sales and revenues. Even fewer studies measure higher profits and, fewer again, employment. Furthermore, short-term positive effects often seem to vanish in the long run.

There are no straightforward results on the influence of targeting. The evidence is mixed on whether trainings' return is higher for those with initially lower skills and whether it is more helpful for male or female owned enterprises. The review suggests that training is more helpful for start-ups than for business expansion. However, since many interventions that aim at business start-up often also include some form of financial assistance, it is difficult to isolate the effect of the training. De Mel et al. (2012) for instance find the combination of a cash-grant with entrepreneurship training to be especially successful; also because it led to the creation of more successful businesses. The more tailor-made and substantial the training the better, but it is not necessarily the more-complex programs that are the most successful. Drexler et al. (2013) for instance observe that general accounting training led to some improvements in objective reporting quality and business performance for higher skilled participants, but had the opposite effect for less educated individuals, while a simpler, rule-of-thumb training was more effective for those participants with lower educational levels. From the studies it further appears that training needs to address specific knowledge gaps and be 'substantial' in order to be effective. The consulting and mentoring services analyzed by Bruhn et al. (2013) eventually led to increases in the number of employees of treated businesses. These services were provided to beneficiary enterprises over a period of one year, in weekly four-hour consulting sessions. Likewise, Premand et al. (2012) find positive treatment effects of an intervention that is provided over a period of one academic semester.

5.3 Business development services and wage subsidies

In this sub-section we cover a set of ten studies, which are rather heterogeneous in the specific nature of the underlying interventions (see Appendix B). Broadly, they fall under the heading of business development services and targeted subsidies. Four of the ten studies cover business development services in the narrow sense (supplier development, support for environmental audit, provision of working premises, etc.). One of these studies covers conditional tax-breaks and fiscal incentives for technological innovations as well. Two further studies measure the employment impact of grants for product and process innovations. An additional three studies cover supply or demand side wage subsidies, and one study measures the impact of minimum wage legislation on employment. All the studies on wage-related interventions focus on Turkey or South and South-East Asia, while the other studies cover almost exclusively Latin-American countries. Only one of these ten studies is based on an RCT design, while the others use a quasi-experimental approach or exploit the variation in the policy across time and space to identify effects.

The studies show mostly positive and statistically significant employment effects (see Appendix B). Nevertheless, general conclusions have to be treated with care due to small sample sizes and selection biases that are possibly not entirely removed by the evaluation design. Overall it seems that business support services and targeted subsidies can contribute to employment generation if they are demand driven, tailor-made and focused. Larger firms may need quite specific and sophisticated support, whereas small firms just need very rudimentary improvements to their business.

Interestingly, Kluve (2010) who compared the effectiveness of active labor market programs across European countries came to a very similar conclusion as we do: The direct employment effects were the largest for wage subsidies and 'services and sanctions' conditional on certain productivity enhancing activities. According to the studies we reviewed, tax-breaks and fiscal incentives conditional on process and product innovations seem to be particularly effective. However, the robustness of the findings is somewhat low, first, because the sample of studies is quite small and, second, because almost all studies have to rely on a rather weak identification strategy and hence a

bias through firms selecting themselves into such programs cannot be ruled out completely. It is also remarkable that nothing can be said about the East- and South-East-Asian context, where at least in some countries business support services may have played an important role.

The studies on wage subsidies suggest that targeting matters for job creation. Two different programs that have been examined in a similar context in Turkey allow for an interesting comparison of supply and demand driven subsidies. One program targets the employers who benefit from reductions in social security contributions for additionally hired workers. This was found to increase the rate of employment growth and business growth substantially (Betcherman et al., 2010). A supply-driven program through which workers received the subsidy in the form of vouchers that allowed them to be hired and get training on the job, turned out to not increase employment chances. Only a few beneficiaries were retained in their job once the subsidy came to an end (Fretwell et al., 1999). Various reasons could explain why this program failed. One could be that this has to do with the targeting. The on-the-job training program targeted employees whereas the more conventional wage subsidy programs targeted employers. Employers may keep workers hired at a reduced rate, when they are free to choose the workers they actually prefer. If an unemployed person applies for a job with a voucher, not only may the profile not fit, but a voucher may have a negative signaling effect. Hence direct wage subsidies may have more positive employment effects than voucher based-programs. However, they may have very different income distributional effects. These interpretations are based on only two studies and eventually, whether they are really valid can only be found out if several wage subsidy programs test and compare such specific design features.

Finally, it is obvious that wage subsidies are in general a quite expensive intervention and the programs covered here are no exception. The pure wage subsidy program in Turkey entails costs per job-month created that correspond to roughly 94% of the total cost of employing a minimum wage worker. This may still seem acceptable, if the jobs created are sustainable, but evidence whether this is really the case is scarce (Betcherman et al., 2010). A major cost component is the dead weight loss produced by the fact that many workers that are hired under a subsidized rate would have been hired anyway. This is also confirmed by the experimental study in Sri Lanka (De Mel et al., 2013b and 2010), where the authors find a strong correlation between pre-program hiring intentions and program uptake.

5.4 Interventions to promote formalization

In most low and middle income countries the bulk of urban micro and small enterprises are informal, i.e. they are not registered with the tax authority and operate outside most regulations. A key policy question is whether the performance of these firms could be improved and their size in terms of employed capital and staff be expanded through formalization. On the one hand, it is believed that formalization increases access to credit and other resources important for business success and expansion, even if some argue that most micro and small firms have little to gain here because their business is simply too small to benefit from any services offered to formal firms and in many countries the government has not much to offer anyway (Maloney 2004). On the other hand, formalizations could imply a significant increase in tax payments which have to be added to the bureaucratic act of formalization, which according to De Soto (1989) already can be so significant that they alone prevent firms from becoming formal.

As both costs and benefits of formalization are involved, the policy problem of formalization is two-fold: What interventions are suited to enhance firms' formalization, and what are the effects of becoming formal? As this review focuses on employment effects, formalization studies were only included if they covered effects on employment. Five studies were identified that can credibly establish a link between formalization and employment (see Appendix B). Four of them concentrate on Brazil and Mexico, where significant reforms have been implemented to reduce the costs of formalization. The fifth focuses on Sri Lanka and is based on a randomized field experiment where cash rewards are offered to firms if they formalize.

All studies show that it is difficult to get the average firm formalized as it is simply too small and not profitable enough to make use of the potential that formality offers. Programs that offer cheaper and

easier formalization procedures seem to work for a relatively small group of entrepreneurs and firms that show already a higher initial performance. It also seems easier to formalize firms while they are being set up than formalizing firms that already exist. This has in particular been shown in the case of Mexico (Bruhn, 2011). De Mel et al. (2013a) showed for the case of micro and small firms in Sri Lanka that even if the equivalent of one month of the median firm's profits are offered only around one-fifth of all firms register the business. Interestingly, in this particular case the lack of property rights for the ground they work on was a major deterrent to formalization for many entrepreneurs. Complementary evidence comes from Andrade et al. (2013), a study not included in our systematic review because it does not assess employment effects, who directly tested the effectiveness of various treatments intended to increase formality in Brazil based on an RCT . They found that more information on procedures and lower costs did not work either; only inspection visits had some effect. Receiving an inspection gave a 21 to 27 percentage point increase in probability of formalizing.

In Brazil and Mexico, among those firms that do formalize, performance in terms of revenue and profits typically improves, including employment and capital investment, but for most only modestly. For Sri Lanka, De Mel et al. (2013a) do not find any employment effects as a result of formalization.[3] Of course, it will always increase the government's tax revenues, which is typically the main objective of formalization anyway. Yet, it seems that programs that "force" firms to formalize are unlikely to produce any significant employment effects as for many formerly informal firms formality does not translate into extra profits but into additional costs. It seems the best incentive governments can provide for formalization is to offer useful public services in return. This does of course not imply that policies should not simplify administrative procedures, but efforts need to go beyond. It is not the costs of registration but the expected benefits of formality that is pivotal for the decision to formalize and only if such benefits exist it is likely that formality also leads to additional jobs.

6. Meta-regression analysis

6.1 Method

To implement a meta-regression analysis we use two alternative impact measures: first, whether a given intervention had a positive significant impact on employment, firm ownership (start or continuation) or self-employment;[4] and, second, the standardized effect size. How we derived the latter was explained above. Relying on standardized effect sizes ensures a certain comparability of impacts across studies. However, measured impacts still differ in terms of the temporal horizon they refer to and of course in the program costs that had to be incurred to produce a particular change in the outcome.

For the sets of estimates where we just use a binary variable taking the value one if the effect of program *i* was significantly positive, we run a simple probit regression and explore the variation of that binary variable across large set of study characteristics X_i. Hence, the model reads:

$$\text{Probit}(y_i=1|x_i)=\Theta(X_i'\beta + \varepsilon_i), \quad (1)$$

where X_i includes the type and characteristics of the intervention, the term, ε_i, is the error term and Θ stands for the cumulative normal distribution. Since coefficients of a probit model cannot be directly interpreted, we compute and show marginal effects, i.e. the change in the probability of success if one explanatory variable is increased by one unit while all other explanatory variables are kept at their mean. Since some studies contribute with more than one outcome, we correct the standard errors for within-group correlations. The application of weights ensures that each intervention counts only once in the sample.

We further conduct simple linear regressions, where the dependent variable is the standardized effect size. The regression model reads:

$$y_i = X_i'\alpha + \eta_i, \quad (2)$$

[3] Similar evidence comes from McKenzie and Sakho (2010), but they do not focus on employment effects.

[4] Since we had only four statistically significant negative impact estimates in the sample, we decided to lump together insignificant and statistically significant negative estimates.

On the right hand side we use the same explanatory variables as in the probit model above. The term η_i captures the unexplained part of the variance in *y* including measurement error.

As explanatory factors we include the type of the intervention, whether the intervention explicitly aimed at employment creation, whether it was provided in combination with other interventions, whether it targeted women, the types of enterprises targeted, the study design of the impact evaluation, as well as the country income category, and finally, the outcome measure, i.e. whether it is employment or firm ownership. Table 3 below shows the summary statistics for the dependent and explanatory variables. We also considered testing whether the type of the implementing agency plays a role, for instance whether this was a public or private entity. However, this did not lead to any meaningful results. First many studies are not clear about the status of the implementing agency. In other cases the implementing agency might be private, but the intervention was developed and designed by a public agency, which at the end makes it hard to interpret the results. Apart from that the sample size also puts a limit on the extent heterogeneity - in particular in terms of context and program characteristics - can be captured by the Meta-regression analysis.

[Table 3about here]

In total, we have 116 observations that can be used for the meta-regression analysis. 40 percent of these relate to business creation while the remaining relate to changes in employment in existing firms. Overall, 46 percent of impact estimates are positive and statistically significant at the 10 percent level. Most impact estimates relate to the effect of finance interventions, followed by training interventions. About 12 percent of impact estimates represent the impact of business development services, wage incentives and policies to enhance research and development. Interestingly, around 70 percent of the estimates come from evaluations of interventions that explicitly aimed at creating employment or new enterprises. Almost half of the impact estimates come from joint interventions, and more than half of the impact estimates have been obtained through randomized controlled trials. Furthermore, the majority of evaluations focused on existing and potential microenterprises and was conducted in upper middle income countries.

6.2 Results

Looking at the effectiveness across the different intervention categories, it can be seen that the included finance interventions had on average lower employment effects than the included training interventions (see Table 4). This is confirmed by both specifications, i.e. whether we use the dichotomous outcome or the standardized effect sizes. There are however, no systematic and significant differences between training interventions and business development services, wage incentive schemes and interventions that improved the business environment.

In our sample, there is some indication that interventions of the type we consider have often more chances in establishing new firms than expanding existing firms. A micro-credit program, for example, may enable many households to start a small business, but may enable only few established microentrepreneurs to expand their business. However, as McKenzie and Woodruff (2014) pointed out, this finding may partly be due to the fact that power is generally higher for detecting binary outcomes (such as whether a new business started) than for detecting changes in rather continuous variables (such as employment). Those interventions that target small enterprises appear to be more successful in achieving significantly positive employment effects as compared to those that target microenterprises, implying that only a small share of microenterprises graduates, or that it is at least difficult to expand microenterprises in general. The 'IFC Jobs Study' comes to a similar conclusion based on a review of the literature (IFC, 2013). However, since specific types of microenterprises might still have the potential to grow, targeting is crucial.

[Table 4 about here]

Surprisingly, whether employment creation has been an explicit objective of the evaluated intervention does not correlate with larger employment effects. Combined interventions did also not systematically lead to larger employment effects, although we had seen above that the specific combination between finance and training often seems to work better than each of these two interventions in isolation.

Those interventions in our sample that targeted women specifically apparently had a lower chance of success, although this finding is only statistically significant in some specifications. This is consistent with a number of recent studies that find capital or business training to have no effect on female enterprise performance (see for instance de Mel et al. 2009; Fafchamps et al., 2011). Yet this does not mean that the return of targeting women is lower, it rather suggests that programs targeted at women typically need to account for other, complementary constraints as well. For instance, if women have less control over their budget than men, then cash grants paid to women have a lower impact on investment than for men. Women typically also have lower levels of formal education, have often only limited access to other relevant services, often need to work from home and are often deprived of property rights – land rights in particular.

Finally, the regressions also reveal that programs that have been evaluated experimentally show fewer significant employment effects than programs that have been evaluated with quasi-experimental methods. Looking at finance interventions, for example, shows that 20 out of the 26 treatment effects that are based on an RCT show insignificant effects, while this is the case for only 8 out of the 18 treatment effects based on quasi-experimental methods. Hence, the failure rate is almost twice as high. Obviously, the most intuitive explanation is that the quasi-experimental studies cannot entirely deal with selection effects and hence employment effects are often over-estimated. We constructed further variables measuring the risk of bias, based on our own assessment for various dimensions of internal and external validity and following the criteria proposed in Waddington and Hombrados (2012). Although, low statistical power of many RCTs is an important shortcoming controlling for this problem and other risk of biases could not substantially reduce the estimated coefficient associated with RCTs. However, investigating the included RCTs in more detail also shows another potential explanation: RCTs systematically focus more prominently on small programs, very poor areas and very specific target groups (as compared to evaluations based on quasi-experimental designs), all of which may increase the probability of a failure. In other words, RCTs are often applied in very specific cases and hence one needs to be careful to generalize their findings.

7. Conclusion

Overall the review shows that creating and enhancing employment is a very complex challenge. Many conditions have to be met before interventions in favor of individual enterprises do not only improve business performance but also lead to additional jobs. Phrased differently, it is typically ‘a long way’ in the result chain from policy inputs to employment impacts, even more so if employment is supposed to be sustainable and tied to acceptable and secure working conditions. Given the discrete nature of decisions to found a new business or to hire an additional employee, not a minor change but rather a major push is needed to have an impact. It seems much easier to have an effect on management practices, sales and (short term) profits than on employment. Many interventions seem to lead to changes at the intensive margin, but fail to deliver productivity increases that go hand in hand with more jobs. This is coherent with the findings by Cho and Honorati (2014). Their meta-analysis of finance and training programs shows that in particular vocational and business training programs have positive effects on business outcomes such as improved knowledge and practice and sometimes income, but effects on a general set of labor market activities are generally quite weak and very often insignificant.

Yet, our review also revealed that about a third of the interventions covered by this review are not primarily designed to create employment but rather strive for income stabilization and poverty reduction. Hence, one should not expect massive impacts on job creation if interventions were not even intended to deliver this result. It also seems easier to create new businesses than to expand existing firms. Obviously, it is also far from certain, whether these new firms survive in the longer term. Most of the studies included do not provide an answer to this question, as their follow-up period is relatively short. Targeting seems to be key to achieve positive employment effects. Furthermore, not all potential and actual entrepreneurs can make good use of support. Different types of interventions will be required to increase employment for different groups. Interventions also need to pay attention to the interaction between different binding constraints. For instance, just improving business skills

without facilitating access to capital (and vice versa), is often not enough to have an effect on investment and employment.

The review also shows that the available evidence is still sketchy in particular for large parts of Sub-Saharan Africa and Asia, regions in which, in the coming decades, the need for jobs is the highest. Findings from Latin-America, which dominate in this review, cannot necessarily be generalized and applied to other regions. Also only very few studies are able to assess the longer term effects of their interventions and policies and many studies fail to provide a detailed analysis of why certain effects occurred or did not occur – making it hard to extrapolate lessons. Moreover, it is also hard to tell from the results whether new jobs were created or whether workers were just tracked away from other activities. Furthermore, almost none of the 54 studies provided a detailed cost effectiveness analysis, i.e. how much does it cost to create an additional job with a certain program compared to another? This gap should alert both implementers and researchers. Implementers should provide the necessary numbers and researchers should go beyond the estimate of simple impacts, which is not really helpful for those who have to allocate resources across different interventions.

Many of the studies covered are based on RCTs, whereas others use a quasi-experimental design such as difference-in-difference estimation, propensity score matching techniques, regression discontinuity designs or several of these in combination. A striking finding of our review is that the study design matters for the impacts found. RCTs, which are typically seen as the 'gold standard', find less often positive employment effects than other methods, controlling for the type of intervention, type of country and type of firm that is targeted. This may suggest that in many of the studies that are based on a weaker identification strategy, selection bias is still an issue. However, it can also not be ignored that many RCTs have low statistical power due to small sample sizes and that they are applied particularly to small programs, very poor areas and very specific target groups. Under these conditions the generation of employment might be particularly difficult; hence these findings cannot necessarily be generalized. Finally, the results from RCTs might also be biased due to Hawthorne and John Henry effects, attrition and spill-overs, although many RCTs address the potential for such biases quite carefully.

Finally it is important to note that the methodology of a systematic review, because of its focus on rigorous evidence, must systematically ignore untargeted policies, such as financial sector development, large-scale infrastructure projects, trade policies and alike, which may be particularly beneficial for SMEs. Other approaches must be applied to find out how effective these interventions are in creating employment. This will of course imply to rely on less rigorous methods and hence the reliability of the findings need to be assessed very carefully in each case, but ignoring that literature may paint an overly pessimistic picture with respect to the potential of such policies and interventions to create jobs.

Appendix

Appendix A: Search strategy

Electronic databases

RePEc (Research Papers in Economics) / IDEAS (IDEAS uses the RePEc database); SSRN; EconLit; Labordoc (ILO); 3ie's database of policy briefs, systematic reviews and impact evaluations; Innovations for Poverty Action Publication Database; JPAL Evaluation Database; JPAL Publication Database; ILO Youth Employment Inventory; Research for Development; Web of Science.

Following the inclusion criteria, as specified above, we defined different sets of search terms which we combined using the Boolean operator 'AND' for the searches in electronic databases. In a first step, we combined search sets referring to the population, outcome and type of study, since the outcome is of main interest in this systematic review. However, in order to avoid missing potentially relevant studies by not searching sets for the interventions considered in this review, we conducted a second search that focused on the intervention categories set out in section 3, combining search sets for population, intervention and type of study. See Grimm and Paffhausen (2014) for the specific search terms used and results of the search.

Websites screened

AFD (Agence Francaise de Developpement); African Development Bank; Asian Development Bank; AusAid (Australian Agency for International Development); CIDA (Canadian International Development Agency); DFID (Department for International Development); GDI (German Development Institute); GIZ (Deutsche Gesellschaft für Internationale Zusammenarbeit); IADB (Inter-American Development Bank); IDS (Institute for Development Studies); IFC (International Finance Corporation); ILO (International Labor Organization); KfW Development Bank; ODI (Overseas Development Institute (ODI); OECD (Organisation for Economic Co-operation and Development) Development Center; SIDA (Swedish International Development Cooperation Agency); The SME Initiative at Innovations for Poverty Action (IPA); UNDP (United Nations Development Program); USAID; World Bank Group.

Hand-searches of key journals

Agricultural Economics; American Economic Review; American Economic Journal: Applied Economics; American Journal of Agricultural Economics; Econometrica; Economic Development and Cultural Change; ESR Review (previously Journal of Microfinance); Journal of African Economies; Journal of Development Economics; Journal of Development Effectiveness; Journal of Development Studies; Journal of Labor Economics; Journal of Political Economy; Journal of Small Business Economics; Quarterly Journal of Economics; Review of Economic Studies; World Bank Economic Review; World Development.

Literature snowballing

For included studies, citation tracking was conducted (forward searching). Moreover, their references have been screened for further relevant studies (backward searching).

Furthermore, the bibliographies of the World Bank's World Development Report 2013 on jobs and the following completed reviews have been hand-searched for relevant studies: Cho and Honorati (2014); Cirera et al. (2011); Duvendack et al. (2011); Hagen-Zanker et al. (2011); McKenzie and Woodruff (2012); Nataraj et al. (2012); Stewart et al. (2012); Stewart et al. (2010).

Ongoing studies:

For ongoing studies we heard of, we decided on inclusion, i.e. if they were relevant or marginally relevant, based on their title, and, if available, their statement of objectives. We then contacted the authors in order to check whether the results were already available for inclusion into the systematic review.

Appendix B: Overview of included studies

Ref.	Intervention	Job priority	Country	Size of Enterprises Targeted	Women targeted	Study Design	Length of follow-up	Program effect(s)
			Access to finance interventions					
Angelucci et al. (2013)	Microcredit, joint-liability, stand-alone, targeted at women that have a business or self-employment activity or intend to start one.	yes	Mexico (urban, peri-urban, and rural)	Micro	yes	RCT	up to three years; on average 26 months	BC: insignificant E: insignificant
Arraíz et al. (2012)	Government-backed partial credit guarantees for Colombian MSMEs without enough collateral, stand-alone.	yes	Colombia (national)	Small and medium	no	Quasi-experimental: PSM + DID	up to two years	E – current yr: positive E – 1 yr after: positive E – 2 yrs after: positive
Attanasio et al. (2011)	Small loans; two different treatments: group-lending and individual loans, stand-alone. Target group: Relatively poor women in rural areas	yes	Mongolia (rural)	Micro	yes	RCT	1.5 years	BC – indiv. lending: insignificant BC – group lending: insignificant female BC – indiv. lending: insignificant female BC – group lending: positive
Augsburg et al. (2012)	Individual-liability micro-credit to 'marginal' borrowers, i.e. loan applicants who would otherwise be excluded from loans because of a lack of collateral., stand-alone.	yes	Bosnia and Herzegovina (national)	Micro	no	RCT	14 months	BC: positive
Banerjee et al. (2011)	Direct transfer of productive assets combined with provision of training (inoculation of savings habits and integration into microfinance groups) to the 'ultra poor', particularly women.	no	India (rural West-Bengal)	Micro	yes	RCT	18 months	BC: insignificant
Banerjee et al. (2013)	Group-liability microcredit loans ranging between $200 at market exchange rates (or $1,000 at PPP-adjusted exchange rates) $400, stand-alone and targeted to women and the poor, but not the very poor.	no	India (Hyderabad)	Micro	yes	RCT	3 to 3.5 years	E –short term: insignificant E – long term: insignificant BC – short term: insignificant BC – long term: insignificant

Barnes (2001)	Group-liability microcredit, accompanied by an orientation session that teaches sound business management practices, and loan officers provide management advice.	yes	Zimbabwe (urban)	Micro	no	Quasi-experimental: Matching	2 years	E: insignificant
Blattman et al. (2012)	Nearly unconditional, unsupervised group cash transfers to pay for vocational training, tools, and business start-up costs, stand-alone, targeted at the poor and underemployed youth.	no	Uganda (Northern Region)	Micro	no	RCT	2 years	E – males: significant E – females: negative
Bruhn and Love (2009)	Opening of Banco Azteca in pre-existing stores for electronics and household goods, offering a variety of financial services for low and middle-income customers, previously underserved by the traditional banking industry.	yes	Mexico (national)	Micro	no	Quasi-experimental: DID	up to 9 quarters	BC: positive E: insignificant
Crepón et al. (2011)	Microcredit, joint-liability, as well as individual-liability targeted at existing enterprises.	no	Morocco (rural/semi-urban)	Micro	no	RCT	2 years	E: insignificant BC: insignificant
Da Silva et al. (2006)	Constitutional financing funds, stand-alone Target group: firms in the northeastern region of Brazil, in particular SMEs	yes	Brazil (North/North-East)	Small and medium	no	Quasi-experimental: PSM	3 years	E – program 1: positive E – program 2: positive
De Mel et al. (2013b)	Three different interventions: (1) a subsidized savings program, (2) temporary wage subsidies to incentivize hiring additional employees, and (3) a five-day training program based on the ILO's Improve Your Business (IYB) program, provided jointly (combination of two of these interventions) as well as stand-alone to male-owned enterprises with two or fewer paid employees.	yes	Sri Lanka (urban)	Micro	no	RCT	up to 2 years	E- F: insignificant E – F+T: insignificant E – F+WS: positive
DeNegri et al. (2011)	Public credit lines, stand-alone.	yes	Brazil (national)	Mostly small and medium	no	Quasi-experimental: DID	up to 5 years	E – short term: positive E – long term: positive
Eshetu et al. (2013)	Joint intervention: enabling legal framework and streamlining regulatory conditions as well as specific support services (financial and business development services including a credit and saving scheme trainings, technology transfer, counseling, provision of working premises etc.	yes	Ethiopia (urban Dire Dawa)	Micro	no	Quasi-experimental: PSM	up to 6 years	E: positive
Eslava et al. (2012)	Public credit lines, stand-alone.	yes	Colombia (national)	Small and medium	no	Quasi-experimental: DID and PSM	up to 4 years	E: positive
Field et al. (2011)	Introduction of a grace period of two months for the initiation of the repayment of microloans. Normally repayments start after two weeks.	no	India (Kolkata)	Micro	yes	RCT	3 years	BC: insignificant E: insignificant

Gubert and Roubaud (2011)	Individual-liability loans averaging €500 for urban microbusinesses and longer-term loans (from 24 to 36 months) to small and medium-sized enterprises (SMEs) averaging € 8,000 euros.	no	Madagascar (urban)	Micro	no	Quasi-experimental: Matching	up to 4 years	E: insignificant
Kaboski and Townsend (2005)	Village-level (micro-finance) institutions: (1) Production micro credit groups; (2) Rice bank; (3) Women's group; (4) Buffalo banks.	no	Thailand (rural/semi-urban North-East and Central)	Micro	no	Quasi-experimental: IV	unclear	BC: insignificant
Karlan et al. (2012)	Two types of interventions: (1) specific management consulting services and (2) unconditional cash grant of approximately US $133-Provided stand-alone as well as jointly to tailors and seamstresses.	no	Ghana (urban)	Micro	no	RCT	up to 11 months after the consulting stopped and 14 months after the capital drops	E – F: insignificant E – F+T: insignificant
Karlan and Zinman (2011)	Individual liability microcredit, loans ranging from 5,000 to 25,000 pesos for existing enterprises, stand-alone.	yes	Philippines (two provinces and capital region)	Micro	no	RCT	11-22months	E:negative BC: negative
Kondo et al. (2008)	Group-liability microcredit, stand-alone.	no	Philippines (national)	Micro	no	Quasi-experimental: DID	3 to 5 years	BC: positive E: positive
Macours et al. (2012)	CCT complemented either by vocational training or a productive investment grant targeted at rural households.	no	Nicaragua (rural)	Micro	no	RCT	2 years	BC: positive
Nelson (2011)	Large-scale, publicly-funded microfinance initiative which helped to set-up and to support independent village banks.	yes	Thailand (rural/semi-urban North-East and Central)	Micro and small	no	Quasi-experimental: IV	up to 6 years	BC – low wealth: negative BC – middle wealth: positive BC – high wealth: insignificant
Resende (2012)	Constitutional Financing Funds: Loans at subsidized interest rates, stand-alone, targeted at micro and small rural and industrial producers.	yes	Brazil (North/North-East)	Small and medium	no	Quasi-experimental: Matching	up to 6 years	E – 3yr period: positive E – 6yr period: positive
Tan (2009)	Seven different matching grants and credit programs and two other, open-ended, residual programs.	yes	Chile (national)	Small and medium	no	Quasi-experimental: DID + PSM	up to 10 years	E – techn. Assistance (BDS): insignificant E – cluster formation (BDS): insignificant E – technology dev. (BDS): insignificant E –any BDS: positive

Tarozzi et al. (2013)	Joint-liability microcredit, combined with family planning program	yes	Ethiopia (rural)	Micro	no	RCT	up to 2 years	BC – Oromiya: insignificant BC – Amhara: insignificant
Entrepreneurship training								
Bah et al. (2011)	Financial and/or technical assistance analyzed jointly.	yes	Macedonia (national)	Mostly micro and small	no	Quasi-experimental: matching	up to 3 years	E – 1st yr: positive E – 2nd yr: positive E – 3rd yr: positive
Bandiera et al. (2012)	Joint intervention that simultaneously provides: (1) vocational training to run/start small-scale enterprises; and (2) information on health and risky behaviors. Courses are supplemented by financial literacy courses and targeted at adolescent girls aged 14-20.	yes	Uganda (rural, urban and semi-urban)	Micro; target group not necessarily existing entrepreneurs	yes	RCT	2 years	BC: positive
Banerjee et al. (2011)	Direct transfer of productive assets combined with provision of training (inoculation of savings habits and integration into microfinance groups) to the 'ultra poor', particularly women.	no	India (rural West-Bengal)	Micro; target group not necessarily existing entrepreneurs	yes	RCT	18 months	BC: insignificant
Bruhn and Zia (2011)	Three-day business and financial education training for microcredit clients.	yes	Bosnia and Herzegovina (urban)	Micro	no	RCT	6 months	E: insignificant BC: insignificant
Bruhn et al. (2013)	Subsidized consulting and mentoring services for owners/managers of formal businesses. Consultants were asked to (1) diagnose the problems that prevented the enterprises from growing, (2) suggest solutions and (3) assist in implementing the solutions.	no	Mexico (Puebla)	Mostly micro and small	no	RCT	up to one year (short term) and between 1-3 years (long term)	E – short term: insignificant E – long term: positive
Calderon et al. (2013)	Stand-alone basic business training provided at no cost, focusing on the application of the concept discussed in class on the participants' businesses. Target group: small, female headed firms in the retail and production sector	no	Mexico (rural)	Micro	yes	RCT	up to 1 year (short term effects) and about 2.5 years (medium term effects)	E – below median profit: insignificant E – above median profit: insignificant
Cho et al. (2012)	Vocational training apprenticeship combined with entrepreneurial support and life skills training and, in some cases, start-up capital. Target group: vulnerable youth who are poor, orphaned, HIV/AIDS vulnerable, school dropouts.	yes	Malawi (national)	Micro; target group not necessarily existing entrepreneurs	no	RCT	4 months	BC: insignificant

De Mel (2013b)	Three different interventions: (1) a subsidized savings program, (2) temporary wage subsidies to incentivize hiring additional employees, and (3) a five-day training program based on the ILO's Improve Your Business (IYB) program, provided jointly (combination of two of these interventions) as well as stand-alone to male-owned enterprises with 2 or fewer paid employees.	yes	Sri Lanka (urban)	Micro	no	RCT	up to 2 years	E – T: insignificant E – T+F: insignificant E – T+WS: positive
De Mel et al. (2012)	ILO's 'Start-and-Improve Your Business program' provided to female current as well as potential business owners.	yes	Sri Lanka (urban)	Micro	yes	RCT	2 years	BC – T – short term: positive BC – T – long term: insignificant BC – T+F – short term: positive BC – T+F – long term: insignificant
Drexler et al. (2013)	Two different stand-alone training interventions: (1) Standard accounting training, and (2) rule-of-thumb training, which taught participants simple rules for financial decision making. Target group: microentrepreneurs interested in training.	no	Dominican Republic (urban)	Mostly micro	no	RCT	1 to 2 years	E: insignificant
Galasso et al. (2004)	Vouchers that entitled to hire an employee at a subsidized wage for 18 months. In a variant of that intervention employees received special skill training.	yes	Argentina (urban)	Micro	no	RCT	18 months	BC: positive
Giné and Mansuri (2011)	Hands-on business training based on the ILO's 'Know About Business' modules was added to microfinance. In addition, one-on-one follow-up training sessions and beneficiaries had the opportunity to participate in a lottery for a loan up to seven times the average loan size.	no	Pakistan (rural)	Micro	no	RCT	18 months after training and 6 months after loan lottery	BC – beneficiary involved: insignificant BC – beneficiary not involved: insignificant
Jaramillo and Parodi (2003)	Two different programs providing training and finance to low-income/poor youth (18-25 years) either already owning a microenterprise or interested in establishing one. Focus was on business plan development.	yes	Peru (urban)	Micro; not necessarily existing entrepreneurs	no	Quasi-experimental: PSM	3 months (CID program) and 11 months ('JUMP' program)	E – CID: positive BC – JUMP: positive
Karlan and Valdivia (2011)	Training is added to microcredit program. The training included general business skills and strategy training, not client-specific problem solving. Target group: female microentrepreneurs who are microcredit clients.	yes	Peru (regional)	Micro	yes	RCT	up to 2 years	E: insignificant BC: insignificant
Klinger and Schündeln (2011)	Multi-phased business plan competition	yes	El Salvador, Guatemala and Nicaragua (within-country distribution	Micro and small; not necessarily existing entrepreneurs	no	Quasi-experimental: RDD	1 to 3 years	BC: positive

			unclear)					
Lopez-Acevedo and Tinajero-Bravo (2010)	Subsidies for SMEs to (1) hire independent instructors to design and deliver training, and (2) to reduce the costs of producing training materials, developing training programs, and assessing workers' skills based on labor competency standards.	yes	Mexico (national)	Mostly medium size	no	Quasi-experimental: PSM	up to 5 years	E: negative
Macours et al. (2012)	CCT complemented either by vocational training or a productive investment grant. Target group: Rural households	no	Nicaragua (rural)	Micro; not necessarily existing entrepreneurs	no	RCT	2 years	BC: positive
Premand et al. (2012)	Introduction of an innovative entrepreneurship track in the university curriculum, including entrepreneurship courses, external private sector coaching delivered by entrepreneurs, and business plan development. Target group: University students.	yes	Tunisia (national)	Micro; not necessarily existing entrepreneurs	no	RCT	up to 1 year	BC: positive
Steiner et al. (2010)	Stand-alone business training program promoting productive activities in the agricultural sector; and agro-industrial sector, as well as in services and industry and targeted at the unemployed youth (16-25) in rural and remote areas.	no	Colombia (rural)	Micro; not necessarily existing entrepreneurs	no	Quasi-experimental: PSM and DID	about 1 year	BC: significant
Valdivia (2011)	Stand-alone training (general and individualized). There were two different treatments: (1) Regular business training consisting of personal development, business development and management and productivity improvements; and (2) Additional individualized support in the form of technical assistance (TA) Target group: Female microentrepreneurs in Lima.	no	Peru (urban)	Micro	yes	RCT	10 months	BC – general training: insignificant BC – general + specific training: insignificant
Business development services (BDS)								
Arráiz et al. (2013)	Supplier development program providing public subsidies for projects aimed at strengthening the management of SMEs that supply large firms. Each project must include between 10 to 20 SMEs.	no	Chile (national)	SMEs	no	Quasi-experimental: PSM	up to three years	E: positive
Benavente (2007)	Technology development fund providing matching-grants for projects aimed at developing new products and improving production processes. Covers the development of prototypes and market testing.	yes	Chile (national)	SMEs	no	Quasi-experimental: PSM+DID	unclear	E: positive
Castillo et al. (2011)	Co-financing (up to 50%) for product innovation or process innovation.	no	Argentina (national)	SMEs	no	Quasi-experimental: PSM + DID	up to 8 years	E – product innovation: positive E – process innovation:

								positive
Eshetu et al. (2013)	Joint intervention: enabling legal framework and streamlining regulatory conditions as well as specific support services (financial and business development services including a credit and saving scheme trainings, technology transfer, counseling, provision of working premises etc.).	yes	Ethiopia (urban Dire Dawa)	Micro and small	no	Quasi-experimental: PSM	up to 6 years	E: positive
Lopez-Acevedo and Tinajero-Bravo (2010)	Four different programs including: (1) tax breaks, (2) finance audits and support for investments to reduce environmental risks, (3) fiscal incentives for technological innovation, and (4) a training of the industrial workforce.	yes	Mexico (national)	MSMEs	no	Quasi-experimental: PSM	up to 5 years	E – tax breaks: positive E – environment audit: negative E – fiscal incentives and techn. innovation: positive E – other state support: insignificant E – any program: positive
Tan (2009)	Seven different matching grants and credit programs, and two other, open-ended, residual programs.	yes	Chile (national)	Mostly medium sized	no	Quasi-experimental: DID+PSM	up to 10 years	E – techn. assistance (BDS): insignificant E – cluster formation (BDS): insignificant E – technology dev. (BDS): insignifciant E –any BDS: positive
Wage incentives								
Alatas and Cameron (2010)	Introduction of a minimum wage legislation (province-specific wage levels).	no	Indonesia (urban)	Small and medium sized formal firms	no	Quasi-experimental: Matching + DID	Average exposure time not reported	E - small firms: negative E – large domestic firms: positive E – large foreign firms: positive
Betcherman et al. (2010)	Social security contribution and wage subsidies as well as land and electricity subsidies (the latter for particular cases).	yes	Turkey (national)	Small and medium sized formal firms	no	Quasi-experimental: PSM + DID	1-2 years	E: positive

De Mel et al. (2013b)	Three different interventions: (1) a subsidized savings program, (2) temporary wage subsidies to incentivize hiring additional employees, and (3) a five-day training program based on the ILO's Improve Your Business (IYB) program, provided jointly (combination of two of these interventions) as well as stand-alone to male-owned enterprises with 2 or fewer paid employees.	yes	Sri Lanka (urban)	Micro	no	RCT	up to 2 years	E – WS: insignificant E – WS + F: positive E – WS + T: positive
Fretwell et al. (1999)	On-the-job training, averaging 4.5 months in length. Contracts with training agencies and enterprises were 'performance-based' with pre-negotiated job placement rates and trainees were provided with a token amount for living and travel expenses.	yes	Turkey (urban)	Small and medium sized	no	Quasi-experimental: Matching	1-2 years (unclear)	E: insignificant
Business environment: Interventions to promote formalization (BE)								
Bruhn (2011)	Federal program targeted at municipalities that 'one-stop' firm registration offices allowing small firms to obtain a license to operate in two days or less and to postpone health and social security inspections for three months. The program reduced registration procedures from 30 to 2 days.	yes	Mexico (national)	MSMEs	no	Quasi-experimental: DID	up to 4 years	BC: positive
Courseuil and Moura (2011)	A tax incentives program that combines, simplifies and promotes the collection of federal taxes from micro-firms and small companies, with lower, though progressive, tax rates on the same base for calculation (gross revenue). The program combines reductions both in monetary and administrative costs of tax payment.	yes	Brazil (national)	Mostly medium sized	no	Quasi-experimental: RDD	1 year	E: positive
de Mel et al. (2013a)	Information about procedures and costs and benefits of formalization, provided either alone or with capital.	yes	Sri Lanka (urban)	Mostly micro	no	RCT	up to three years	E: insignificant
Fajnzylber et al. (2011)	Simplified tax system for micro-firms, including also an overall reduction of taxes of up to 8%.	yes	Brazil (urban)	Micro	no	Quasi-experimental: RDD, IV and DID	up to 1 year	E – all micro-firms: positive E – firms with at least one employee: positive
Kaplan et al. (2011)	Federal program targeted at municipalities that 'one-stop' firm registration offices allowing small firms to obtain a license to operate in two days or less and to postpone health and social security inspections for three months. The program reduced registration procedures from 30 to 2 days.	yes	Mexico (national)	MSMEs	no	Quasi-experimental: DID	up to 4 years	E: positive BC: positive

Source: Own representation.

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Tables and Figures

Table 1: Regional distribution and basic characteristics of included studies

	Finance	Training	BDS/Wage	Business E.	Total
Region					
Latin America & Caribbean	9	11	5	4	29
Sub-Saharan Africa	6	2	1	0	9
South Asia	4	4	1	1	10
East Asia & Pacific	5	0	1	0	6
Europe & Central Asia	1	2	2	0	5
Middle East & North Africa	1	1	0	0	2
Firm size					
Micro	20	17	2	2	41
Small	3	2	2	2	9
Medium	30	1	6	1	38
Stand-alone or joint					
Stand-alone	18	6	2	3	29
Joint	6	12	7	2	27
Both	2	2	1	0	5
Empl. creat. primary objective					
Yes	16	11	7	5	39
No	10	9	3	0	22
Total (per intervention area)	26	20	10	5	61

Notes: The total count does not add up to 54 because some studies have been included in more than one category.
Source: Own computations based on information made available by the evaluations.

Table 2: Distribution of standardized effect sizes by intervention area

	Finance		Training		BDS/Wage		Business E.	
	Count	Share (%)	Count	Share (%)	Count	Share (%)	Count	Share (%)
Negative effect size (<0)	13	24.1	8	22.2	2	10.5	0	0
Small effect size (>0, <0.2)	33	61.1	16	44.4	12	57.9	5	71.4
Medium effect size (>0.2, <0.5)	7	13.0	5	13.9	2	10.5	0	0
Large effect size (>0.5, <1)	1	1.9	7	19.4	3	15.8	2	28.6
Total	54	100	36	100	19	100	7	100

Notes: Effect sizes are computed as the standardized mean difference (SMD), i.e. as the ratio between the change in the outcome due to the intervention divided by the standard deviation of the outcome in the control group (or at baseline). If the outcome is a binary outcome such as 'having set up a firm or not' the risk ratio is computed (-1). In those studies where such impact measurements were not directly provided they were computed based on the available information. However, some studies do not provide all the necessary information, in these cases we based the estimate just on the reported *t*-values of the impact and the sample sizes of treatment and control groups using the formulas given in Lipsey and Wilson (2001). This implies that effect sizes are not fully comparable across studies and hence can only roughly reflect the order of magnitude of program impacts. For one intervention an effect size measure could not be computed.
Source: Own computations based on information made available by the evaluations.

Table 3: Description of sample of impacts used for the meta-regression analysis

Variable	Observations	Mean	Std. Dev.	Min	Max
Positive significant program effect	116	0.457	0.500	0	1
Effect size[a)]	115	0.145	0.283	-0.891	1.5
Training	116	0.310	0.465	0	1
Finance	116	0.466	0.501	0	1
BDS	116	0.121	0.327	0	1
Private sector incentives and business environment	116	0.103	0.306	0	1
Empl. creat. primary objective	116	0.690	0.465	0	1
Joint intervention	116	0.491	0.502	0	1
Intervention targets women	116	0.198	0.400	0	1
Evaluation based on an RCT	116	0.534	0.501	0	1
Microenterprises	116	0.664	0.474	0	1
Small enterprises	116	0.147	0.355	0	1
Medium enterprises	116	0.190	0.394	0	1
LIC	116	0.086	0.282	0	1
LMIC	116	0.345	0.477	0	1
UMIC	116	0.569	0.497	0	1
Measured outcome is business creation	116	0.371	0.485	0	1

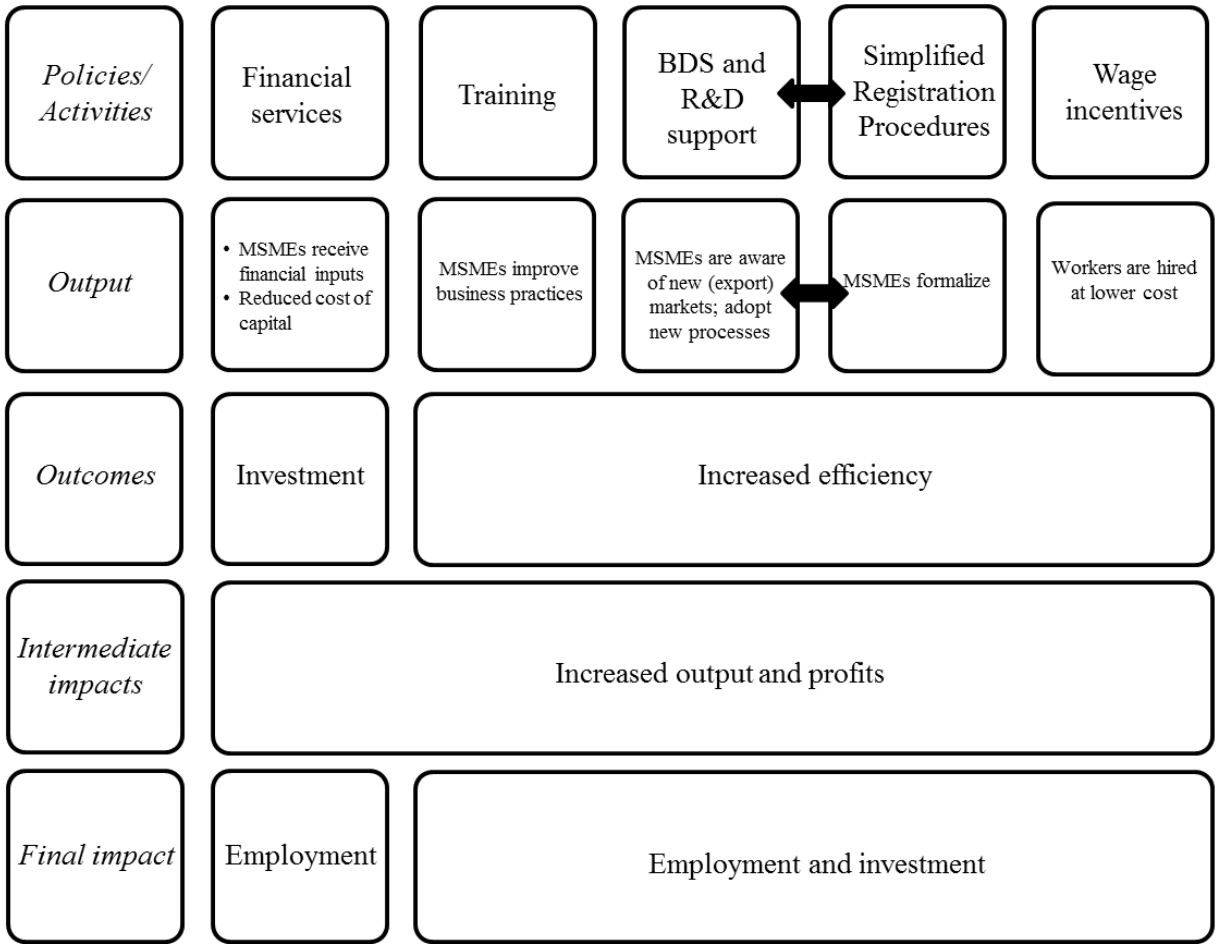
Notes: [a)] Regarding the computation of effect sizes, see Note to Table 2. For one intervention an effect size measure could not be computed.
Source: Own representation.

Table 4: Results from the meta-regression analysis

	(1) Positive significance, unweighted	(2) Positive significance, unweighted	(3) Positive significance, unweighted	(4) Positive significance, unweighted	(5) Positive significance, weighted	(6) Effect size, unweighted	(7) Effect size, weighted
Program type							
Training	Ref.			Ref.	Ref.	Ref.	Ref.
Finance	-0.048			-0.227	-0.294**	-0.217**	-0.245*
	(0.123)			(0.147)	(0.136)	(0.102)	(0.130)
Business development services	0.225			-0.012	-0.007	-0.0723	-0.082
	(0.156)			(0.186)	(0.197)	(0.123)	(0.134)
Private sector incentives and business environment	0.332**			0.154	0.060	-0.105	-0.106
	(0.138)			(0.205)	(0.225)	(0.112)	(0.144)
Firm size							
Micro-enterprises		Ref.		Ref.	Ref.	Ref.	Ref.
Small enterprises		0.482***		0.410**	0.371**	0.126	0.0457
		(0.104)		(0.173)	(0.186)	(0.136)	(0.154)
Medium-sized enterprises		0.176		0.107	0.0712	-0.102	-0.182
		(0.170)		(0.250)	(0.243)	(0.141)	(0.174)
Empl. creat. primary objective		0.0856		0.050	0.024	0.022	0.040
		(0.133)		(0.137)	(0.152)	(0.083)	(0.098)
Joint intervention		0.0001		-0.110	-0.209	0.0004	-0.032
		(0.128)		(0.170)	(0.148)	(0.0739)	(0.0871)
Intervention targets women		-0.244*		-0.216	-0.272*	-0.027	-0.072
		(0.128)		(0.140)	(0.143)	(0.081)	(0.101)
Evaluation based on an RCT				-0.334*	-0.365**	-0.107	-0.087
				(0.176)	(0.172)	(0.140)	(0.156)
Outcome is business creation			-0.135	0.130	0.240*	0.014	0.011
			(0.097)	(0.132)	(0.138)	(0.051)	(0.055)
Country income category							
LIC				Ref.	Ref.	Ref.	Ref.
LMIC				-0.053	-0.035	-0.102	-0.111
				(0.214)	(0.235)	(0.114)	(0.125)
UMIC				-0.186	-0.146	-0.106	-0.072
				(0.233)	(0.247)	(0.150)	(0.180)
Effect size measure is SMD						0.138	0.154
						(0.098)	(0.121)
Square root of sample size				-0.0001	-0.0001	-0.0002	-0.0002
				(0.0002)	(0.0002)	(0.0001)	(0.0001)
Intercept						0.317	0.340
						(0.215)	(0.243)
Observations	116	116	116	116	116	115	115
Pseudo R2	0.051	0.156	0.013	0.210	0.205	0.220	0.197

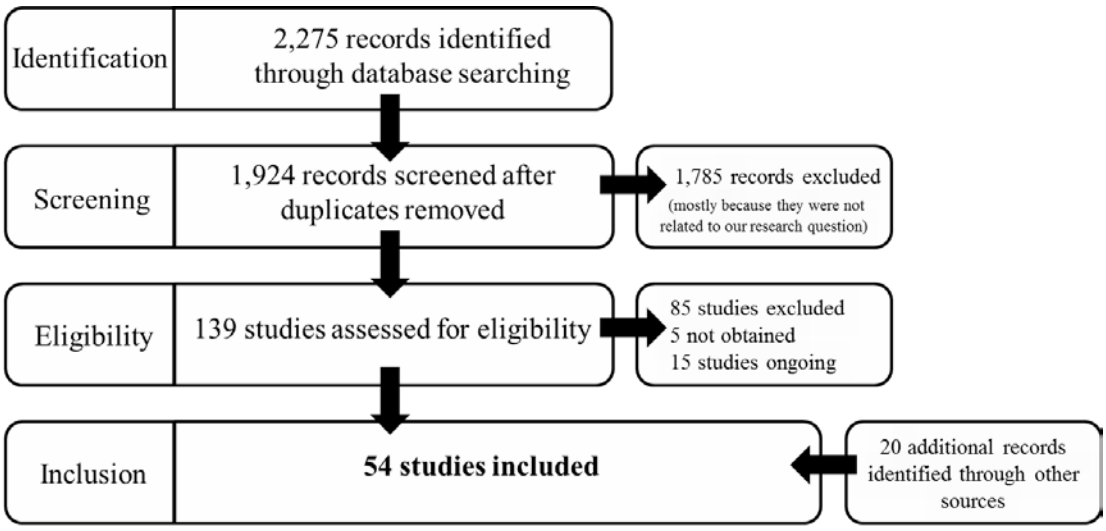
Notes: Robust standard errors clustered at the level of studies in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Source: Own data.

Figure 1: Simplified results chain linking interventions and employment outcomes



Source: Own representation.

Figure 2: Selection of studies



Source: Own representation.