

**Three empirical essays on micro-,
small and medium-sized
enterprises and entrepreneurship
promotion in developing
countries**

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To my parents

Abstract

Eradicating poverty through inclusive economic growth has become a central aim of the international community. This entails poverty reduction approaches that see the poor as contributors to the growth process instead of mere beneficiaries of its outputs. As such, the promotion of productive employment and earnings increases, especially for the poor, have become important elements of poverty reduction strategies.

The private sector is seen as the key driver of employment creation. In developing countries it consists of a large number of very small firms. Many workers, especially among the poor, are self-employed own-account workers and very few firms hire more than ten workers. Moreover, the majority of these firms are of low profitability and productivity, and do not have the potential to grow substantially, while there are a few small firms which are being held back by constraints such as a lack of finance or a hostile business environment. Policymakers are hence on the one hand interested in identifying the constrained high-potential firms and entrepreneurs, the barriers they face to growth, and in finding effective policy measures and interventions to promote the growth of these enterprises. On the other hand, they are in need to know which constraints low-income households face in engaging profitably in small-scale productive activities and how they can be removed, as these activities provide the main source of income for their owners and their households.

In this thesis I empirically assess a wide range of constraints faced by different types of entrepreneurs in developing countries and the effectiveness of different policy interventions aimed at removing them. Thereby I acknowledge the heterogeneity of entrepreneurs and enterprises of small size in developing countries with regard to their growth and transformational

potential and provide information that helps to identify these different types. The thesis contributes to the growing literature on enterprises and entrepreneurial activities in developing countries, by producing valuable evidence on which are the constraints faced by which types of entrepreneurs in starting, surviving, and growing, as well as on which interventions work, and for whom. Thereby it provides important insights for policy formulation aimed at inclusive growth. It consists of three self-contained papers, which put different emphasis on three different areas in which important research gaps remain.

The first paper, which is presented in chapter 2, focuses on interventions aimed at removing firm-level shocks. It consists of a systematic review of evaluations of targeted programs and broader policies that intend to promote micro-, small and medium-sized enterprises (MSMEs) in developing countries, and assesses which of these interventions are effective in creating jobs. The review systematically identifies and assesses 53 studies, which together cover all major world regions and evaluate interventions of the following types: (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). Together with my co-author Michael Grimm, I extracted information from the 53 included studies in a structured way. We synthesize and assess the resulting evidence first in a narrative review and then using a meta-regression analysis.

The results show that the employment effects of these types of interventions have so far been very modest, with the majority of standardized effect sizes found in the studies being small, i.e. between 0 and 0.2 standard deviations. Furthermore, the interventions analyzed here turned out to be more successful in creating new microenterprises, rather than in expanding employment in already existing firms. In addition, for already existing firms these interventions are more effective in generating employment in small and medium-sized firms, compared to microenterprises, which suggests that many of these microenterprises rapidly attain their optimal firm size. The effects of finance interventions have, on average, been weaker than the effects

of entrepreneurship training or business development services. A further important finding is that the study design matters for the impacts found; randomized controlled trials find systematically smaller effects than quasi-experimental studies. Finally, the review highlights existing knowledge gaps and significant shortcomings of the literature, including the very limited evidence on long term effects.

The paper presented in chapter 3 considers death of small, mainly one-person and household businesses. Together with my co-author David McKenzie I collated panel data on more than 14,000 small firms from 16 firm panel surveys conducted in 12 developing countries. We use this unique panel dataset to provide answers to the following questions: What is the rate of firm death over different horizons? Which firms are more likely to die? Why do they die and what happens afterwards? We consider three potential reasons for firm death. A first set of reasons comes from firm-level shocks that reduce the profitability of the firm and cause it to make a loss. A second set of reasons comes from shocks on the household side; for instance, with imperfect labor markets or other missing markets, illness of the business owner or of household members that the owner has to look after may cause the owner to shut down the business. Finally, firm death can also arise from positive shocks to the entrepreneur's outside options such as an attractive wage job offer, or the entrepreneur coming up with an idea for a different firm.

Our research shows that firm death is very common among small firms, with half of the current stock of small firms in a developing country likely to die within the next six years. We find that younger firms, less profitable firms, and firms of younger and older entrepreneurs in comparison to those of middle-aged owners, are much more likely to die. Furthermore, firm death rates are higher in richer developing countries than in poorer countries. The most common reason for firm death is that less profitable and less productive firms end up making losses and close. However, other small firms, particularly those run by women, close because of illness and family reasons, suggesting non-separability between the household and firm, while

a minority of firms close because better opportunities arise for the owner. As a result, on average, firm death is associated with a large fall in the labor earnings of the owner. Finally, we also find that firm death is not necessarily permanent, with approximately 40 percent of owners, whose firms have closed, opening a new firm again within three years.

In chapter 4, I take a closer look at household-level shocks and non-separability between the household and the firm, and assess whether extending health insurance to previously uninsured households leads to increased investment in productive activities. Health shocks are an important source of risk for poor households in developing countries, which have traditionally been excluded from formal health insurance mechanisms. Insuring households against the financial impact of these shocks might lead to increased investment in productive activities by eliminating the need to engage in costly self-insurance strategies, such as selling productive assets and holding pre-cautionary savings instead of investing in entrepreneurial activities. The paper assesses the impact of a large national health insurance scheme, the Mexican *Seguro Popular*, on investment in productive agricultural and non-agricultural assets and activities in rural areas. Using panel data from the Mexican rural evaluation surveys of the *Oportunidades* cash transfer program, I estimate the effect of the program on out-of-pocket health care expenditures and productive assets and account for possible self-selection of households into the program, using difference-in-differences estimation, as well as a propensity score matched difference-in-differences specification.

I find that, although health shocks led to increases in health care expenditures and reductions in productive assets prior to the introduction of *Seguro Popular*, the insurance program did not increase these assets for insured households and did not reduce their health care expenditures substantially. The program mainly reduced spending for medical consultations but was not successful in protecting households from expenditures for medication, which is the more important component of their health care expenditures. Assessing heterogeneity of the effect with regard to household income shows

that these results hold also for households with higher incomes whose productive investments might be more reactive to financial protection from health shocks. Average impact estimates are hence not driven by poorer households foregoing expenditures for health care before the introduction of *Seguro Popular*. Problems in the implementation of *Seguro Popular*, in particular the severe underfunding of the program, could be an explanation for the absence of effects on productive investments.

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I am also very grateful to David McKenzie for giving me the opportunity to learn so much while working with him, for letting me participate in a very interesting research project that led to the paper included in this thesis in Chapter 3, for allowing me to include the output of this project in my thesis, and for his consideration for my doctoral studies while I was working with him. I would also like to thank Stefan Bauernschuster and Rodrigo Isidor for serving on my dissertation committee.

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The paper presented in Chapter 4 uses data from the Mexican rural evaluation surveys of the *Oportunidades* cash transfer program. This data is made available publicly by the federal government of Mexico and has been collected by the International Food Policy Research Institute (IFPRI), the

Mexican National Institute of Public Health (INSP, after its initials in Spanish), and the Mexican Social Anthropology Research and Higher Studies Center, West Campus (CIESAS-Occidente, after its initials in Spanish) at the request of the federal government of Mexico. A second source of data that I use in the paper is administrative data from the Mexican social health insurance program *Seguro Popular*. I thank Raymundo Miguel Campos-Vázquez, who has worked with this data before, for sharing it.

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Chapter 1

Introduction

The study of poverty is a central subject of the economic sciences and its elimination the principal goal of the international community. In their *2030 Agenda for Sustainable Development*, adopted in 2015, the United Nations have reiterated that the eradication of poverty is the '*greatest global challenge and an indispensable requirement for sustainable development*' (United Nations General Assembly, 2015). Starting from a narrow focus on economic growth as a way to eliminate poverty, which was mostly perceived as income poverty, the prescriptions and policies for poverty reduction have broadened considerably in scope since the 1990s. Taking into account the multidimensionality of poverty, consideration is now also given to factors such as equality of opportunities and empowerment, peace and security, health, and environmental protection. Nevertheless, there remains widespread consensus, supported by empirical evidence (e.g. Santos et al., forthcoming; Kraay, 2006; Ravallion & Chen, 1997), that economic growth is vital for poverty reduction. But the focus is now on the specific patterns of this growth that ensure poverty reduction, which is encompassed in the concept of *inclusive* economic growth. As such, ensuring that economic growth is inclusive is now a central concern of the development community and policy makers.¹

Despite the universal importance given to inclusive growth, the concept is not uniquely and unambiguously defined (see Ranieri & Almeida Ramos, 2013, for a review). Yet there are certain commonalities among definitions and policies aimed at achieving

¹See for instance the Sustainable Development Goals Declaration of the United Nations (United Nations General Assembly, 2015), the World Bank Group Corporate Strategy (World Bank Group, 2013), and South Africa's National Development Plan 2030 (Republic of South Africa, National Planning Commission, 2012).

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inclusive growth, which delineate and distinguish it from other similar concepts such as broad-based and pro-poor growth. They share an attention to the participation of the whole population in the *process* of growth by contributing to it, instead of focusing only on participation in the output or benefits of growth. As a result, the promotion of productive employment and earnings increases, especially for the poor, becomes an important element of inclusive growth (e.g. Klasen, 2010; Ranieri & Almeida Ramos, 2013; World Bank, 2012; Ianchovichina & Lundstrom Gable, 2012). Goal 8 of the Sustainable Development Goals, which aims to *"promote sustained, inclusive and sustainable economic growth, full and productive employment and decent work for all"*, incorporates this definition of inclusive growth and expresses the emphasis that is given universally to employment creation as an important instrument for poverty reduction (see United Nations General Assembly, 2015).

This then leads to the questions of where and how employment and productivity increases can be generated, and turns the focus on the private sector and the productive activities poor households in developing countries engage in. The private sector is seen as the key driver of employment creation, as it accounts for 90 percent of all jobs in developing countries (World Bank, 2012). Contrary to high-income countries, however, in developing countries a large number of firms is very small, with the modal firm size being one worker, who is also the owner of the firm (McKenzie, 2017). Moreover, only very few firms hire more than ten workers (McKenzie, 2017; Hsieh & Olken, 2014). Indeed, many workers in developing countries are self-employed, own-account workers, as opposed to employers, and this is especially the case for the low-income population in these countries (e.g. Gindling & Newhouse, 2014; Fields, 2014; Banerjee & Duflo, 2007). The majority of these tiny enterprises are of low profitability and productivity and usually do not grow (e.g. Banerjee & Duflo, 2011; Gindling & Newhouse, 2014).

An extensive amount of research has assessed what prevents the many small-scale firms in developing countries from growing. Often this has been done simultaneously with testing the impact of interventions that remove certain constraints. In fact, impact evaluations of a number of different policy measures and targeted interventions aimed at promoting potential and current small firm owners in developing countries, including the self-employed, have proliferated over the past ten years (see Table 2 in Cameron et al., 2016). The following types of constraints and associated policy responses have been the frequent focus of research: capital constraints and access to finance, including

microfinance, (e.g. de Mel et al., 2008; Banerjee & Duflo, 2014; Grimm et al., 2011; Banerjee et al., 2015), managerial or organizational constraints and business training (e.g. Drexler et al., 2014; de Mel et al., 2014; Bruhn et al., 2018), labor market frictions and wage subsidies (e.g. de Mel et al., forthcoming; Hardy & McCasland, 2015; Cohen, 2016), and bureaucratic barriers and interventions to reduce the costs of formalization, including streamlining of registration processes (e.g. de Mel et al., 2013a; Bruhn, 2011; Kaplan et al., 2011). While this research has highlighted the importance of some constraints to enterprise growth in developing countries, the combined evidence is very mixed as to whether the targeted enterprises are able to grow when provided with one or some of these interventions.

Furthermore, underlying this type of research has been the assumption that all of these enterprises have the potential to grow, once their constraints are removed. Empirical research, which has assessed this hypothesis, however points to considerable heterogeneity among the many small enterprises in developing countries (e.g. de Mel et al., 2010; Gindling & Newhouse, 2014; Dodlova et al., 2015). There is now a consensus that the majority of these firms are operated by subsistence entrepreneurs, while there are a few small firms with high growth potential, but which are being held back by constraints such as a lack of finance or a hostile business environment (e.g. Schoar, 2010; Banerjee & Duflo, 2011; World Bank, 2012). Policymakers are hence interested in identifying these types of firms and entrepreneurs, the barriers they face to growth, and in finding effective policy measures and interventions to promote the growth of these enterprises. At the same time, even though the majority of economic activities low-income households engage in are unlikely to grow substantially, there is an agreement that they are elementary for their owners and their households, as they provide their main source of income (e.g. de Mel et al., 2010; Banerjee & Duflo, 2011). There is hence a need to assess, which constraints low-income households face in engaging profitably in small-scale productive activities and how they can be removed.

The research on enterprises and entrepreneurial activities in developing countries has provided important contributions to understanding the factors inhibiting enterprise start-up and growth in developing countries, determining the effectiveness of associated responses, and distinguishing the different types of enterprises. There remain, however, many important questions, of which this thesis aims to answer some. First, the types of firms, countries, contexts and interventions which have been examined in the literature

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are very heterogenous. The conclusions made by each individual study generally hold for the particular context in which the research has taken place, which limits external validity. Policymakers and donors, however, are interested in knowing whether interventions can be scaled up or implemented in other contexts, and whether interventions, which do not provide the intended result, do this because the assumed constraint is not binding or because the specific intervention studied does not address it adequately. Second, the literature has mostly focused on constraints to start-up and growth. Death of these firms has received less attention. There is no systematic evidence available on the rate at which small firms die, which firms are more likely to die, what are the reasons for firm death, and what happens after a firm dies. Answers to these questions however provide essential input for the formulation of policies aimed at increasing employment, incomes and productivity in developing countries. This is because the consequences of firm death for aggregate productivity and individual welfare, and hence the need for policy intervention, differ depending on whether the least or most productive firms die, and whether firm death reduces the incomes of the owners.

Third, most of the research has highlighted constraints at the firm-level and evaluated associated interventions. This ignores the importance of the owner, and the household more in general, for the small-scale activities operated in developing countries. In the development economics literature the close relationship between production and consumption decisions of households engaging in productive activities has been captured and analyzed in the context of the agricultural household model (see Singh et al., 1986; Bardhan & Udry, 1999). In this framework households consume at least part of the good they produce and supply labor as an important production factor. In the presence of missing markets, production and investment decisions do not derive from profit maximization, but depend on consumption decisions, preferences, and endowments, which is termed non-separability. Microenterprises in the non-farm sector share characteristics of the stylized agricultural household and can hence be studied within this framework. If consumption and production decisions of households who operate microenterprises in developing countries are not separable, policies and interventions, which affect household consumption, might have effects on their investment decisions too. There has been a remarkable rise in social protection programs in developing countries targeted at low-income households (Barrientos, 2011). These include cash-transfer programs, employment guarantee schemes and social health insurance

programs. Studying how these programs affect productive activities of beneficiaries is important in order to understand the constraints faced by household enterprises in increasing and stabilizing their income from productive activities, and to identify appropriate policy interventions.

This thesis addresses these three points through an empirical analysis of a wide range of constraints faced by different types of entrepreneurs in developing countries and the effectiveness of different policy interventions aimed at removing them. Thereby it not only acknowledges the heterogeneity of entrepreneurs and enterprises of small size in developing countries with regard to their growth and transformational potential, but also provides information that helps to identify these different types. The thesis contributes to the growing literature on enterprises and entrepreneurial activities in developing countries, producing valuable evidence on which are the constraints faced by which types of entrepreneurs in starting, surviving, and growing, as well as on which interventions work, and for whom. The thesis hence provides important insights for policy formulation aimed at inclusive growth.

It consists of three self-contained papers, which put different emphasis on each of the three aspects described above. The first paper, which is presented in chapter 2, focuses on interventions aimed at removing firm-level shocks. It consists of a systematic review of evaluations of targeted programs and broader policies that intend to promote micro-, small and medium-sized enterprises (MSMEs) in developing countries, and assesses which of these interventions are effective in creating jobs. The review systematically identifies and assesses 53 studies, which together cover all major world regions and evaluate interventions of the following types: (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). Together with my co-author Michael Grimm, I extracted information from the 53 included studies in a structured way. We synthesize and assess the resulting evidence first in a narrative review and then using a meta-regression analysis.

The results show that the employment effects of these types of interventions have so far been very modest, with the majority of standardized effect sizes found in the studies being small, i.e. between 0 and 0.2 standard deviations. Furthermore, the interventions analyzed here turned out to be more successful in creating new microenterprises, rather than in expanding employment in already existing firms. In addition, for already

1. INTRODUCTION

existing firms these interventions are more effective in generating employment in small and medium-sized firms, compared to microenterprises, which suggests that many of these microenterprises rapidly attain their optimal firm size. The effects of finance interventions have, on average, been weaker than the effects of entrepreneurship training or business development services. Finally, a further important finding is that the study design matters for the impacts found; randomized controlled trials find systematically smaller effects than quasi-experimental studies.

An important contribution of the paper lies in the wide scope it takes with regard to the number of different types of interventions, studies, and contexts it assesses. Furthermore, contrary to similar reviews of interventions targeted at MSMEs in developing countries, this research provides evidence on the ultimate outcome of interest to policymakers, by focusing on employment effects. It also informs the debate on whether and how the many small firms in developing countries can be upgraded, and which policy measures are most effective in doing so. Moreover, the review highlights existing knowledge gaps. First, the available evidence is still sketchy particularly for large parts of Sub-Saharan Africa and Asia, regions in which the need for jobs will be the highest in the coming decades. Second, only very few studies are able to assess the longer-term effects of the interventions and policies they study. Third, many studies fail to provide a detailed analysis of why certain effects occurred or did not occur, making it difficult to extrapolate lessons. Fourth, it is hard to tell from the results whether new jobs were created or whether workers were just tracked away from other activities. And fifth, almost none of the 53 studies provided a detailed cost effectiveness analysis of their interventions.

The paper presented in chapter 3 considers death of small, mainly one-person and household businesses. Together with my co-author David McKenzie I collated panel data on more than 14,000 small firms from 16 firm panel surveys conducted in 12 developing countries. We use this unique panel dataset to provide answers to the following questions: What is the rate of firm death over different horizons? Which firms are more likely to die? Why do they die and what happens afterwards? We consider three potential reasons for firm death. A first set of reasons comes from firm-level shocks that reduce the profitability of the firm and cause it to make a loss. A second set of reasons comes from shocks on the household side; for instance, with imperfect labor markets or other missing markets, illness of the business owner or of household

members that the owner has to look after may cause the owner to shut down the business. Finally, firm death can also arise from positive shocks to the entrepreneur's outside options such as an attractive wage job offer, or the entrepreneur coming up with an idea for a different firm.

Our research shows that firm death is very common among small firms, with half of the current stock of small firms in a developing country likely to die within the next six years. We find that younger firms, less profitable firms, and firms of younger and older entrepreneurs in comparison to those of middle-aged owners, are much more likely to die. Furthermore, firm death rates are higher in richer developing countries than in poorer countries. The most common reason for firm death is that less profitable and less productive firms end up making losses and close. However, other small firms, particularly those run by women, close because of illness and family reasons, suggesting non-separability between the household and firm, while a minority of firms close because better opportunities arise for the owner. As a result, on average, firm death is associated with a large fall in the labor earnings of the owner. Finally, we also find that firm death is not necessarily permanent, with approximately 40 percent of owners, whose firms have closed, opening a new firm again within three years.

A major contribution of this paper is the provision of systematic data on the rates, causes and correlates of small firm death through the construction of a unique dataset. Panel surveys of such firms are still relatively rare, often do not track firms for long periods, and often lose firms which die to attrition. Moreover, the research can be of value for a number of different purposes. First, it can serve as a benchmarking tool for researchers or policymakers tracking the failure rates of firms in their study or intervention, and who so far have had little evidence to compare to and know whether the rates they see in their data are high or low. Second, it provides important insights for targeting of programs for small firms in developing countries. Programs that want to work actively with businesses for several years may wish to avoid small firms at greatest risk of closing down, while other programs that are designed to help firms at risk of failure to survive may wish to use the information provided in the paper to decide which firms to offer the program to. Finally, the results may be of interest in deciding whether interventions are needed, what types of interventions, and for whom. To the extent that firms are exiting because they are of low productivity and competition is reallocating production to more efficient firms, or because owners

1. INTRODUCTION

pursue better opportunities, policymakers may not want to intervene. In contrast, if firms die because of a lack of separability with household shocks, or because of firm shocks that were not able to be insured, policymakers may want to experiment with policies to insure these types of shocks. The paper suggest that there is a subset of firms, particularly those run by women, for whom such policies may be of particular importance.

In chapter 4, I take a closer look at household-level shocks and non-separability between the household and the firm, and assess whether extending health insurance to previously uninsured households leads to increased investment in productive activities. Health shocks are an important source of risk for poor households in developing countries, which have traditionally been excluded from formal health insurance mechanisms. Insuring households against the financial impact of these shocks might lead to increased investment in productive activities by eliminating the need to engage in costly self-insurance strategies, such as selling productive assets and holding pre-cautionary savings instead of investing in entrepreneurial activities. The paper assesses the impact of a large national health insurance scheme, the Mexican *Seguro Popular*, on investment in productive agricultural and non-agricultural assets and activities in rural areas. Using panel data from the Mexican rural evaluation surveys of the *Oportunidades* cash transfer program, I start with an assessment of the relevance of uninsured health shocks for out-of-pocket health care expenditures and productive assets prior to the introduction of *Seguro Popular*, to strengthen the motivation for the analysis of the impact of *Seguro Popular* on productive activities. I then estimate the effect of the program on these outcomes and account for possible self-selection of households into the program, using difference-in-differences estimation, as well as a propensity score matched difference-in-differences specification.

I find that, although health shocks led to increases in health care expenditures and reductions in productive agricultural assets prior to the introduction of *Seguro Popular*, the insurance program did not increase agricultural assets for insured households and did not reduce their health care expenditures substantially. The program mainly reduced spending for medical consultations but was not successful in protecting households from expenditures for medication, which is the more important component of their health care expenditures. Assessing heterogeneity of the effect with regard to

household income shows that these results hold also for households with higher incomes whose productive investments might be more reactive to financial protection from health shocks. Average impact estimates are hence not driven by poorer households foregoing expenditures for health care before the introduction of *Seguro Popular*. Problems in the implementation of *Seguro Popular*, in particular the severe underfunding of the program, could be an explanation for the absence of effects on productive investments.

The paper contributes to the small but growing literature which assesses the interplay between an important household level shock and the enterprises poor households operate in developing countries. It provides a valuable contribution to identifying interventions aimed at stabilizing and increasing the incomes low-income households earn through their productive activities. Moreover, over the past decades, many developing countries have been introducing or expanding health insurance to the large part of the population previously not covered by these schemes. The literature evaluating these schemes has predominantly focused on their effect on out-of-pocket expenditures, utilization of health services, and health status. Few evidence exists on the impact of health insurance on the strategies households have used to self-insure against the consequences of health shocks. By analyzing this aspect, the paper also contributes to a comprehensive evaluation of the welfare effects of these schemes. Finally the paper highlights the importance of assessing the implementation of policies when analyzing their success and establishing external validity.

1. INTRODUCTION

Chapter 2

Do interventions targeted at microentrepreneurs and small and medium-sized firms create jobs? A systematic review of the evidence for low and middle income countries

with Michael Grimm

2.1 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, 600 million jobs are needed worldwide 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

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employment in low and middle income countries is in micro-, small and medium-sized enterprises (MSMEs) (e.g. Ayyagari et al., 2011), these firms are often targeted by such interventions.

This systematic review synthesizes the existing evidence on the employment impacts of these interventions and extracts the main lessons. 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 establish causal evidence.

Our work builds on a few earlier reviews which however have not focused specifically on employment creation in MSMEs or they considered only a subset 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. McKenzie (2010) reviews a small set of finance evaluations (all pre-2009) and concludes that many existing microfinance programs fail to achieve their targets. 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. While the authors are concerned with the same outcome as this systematic review, they do not explicitly focus on employment creation in MSMEs. Betcherman (2014) provides a more narrative review of studies that explore the effects of labor market regulations on employment and other outcomes. Finally, other authors focused on youth employment, but again, do not consider MSMEs specifically (see Betcherman et al., 2007; Puerto, 2007).

A meta-analysis of studies 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. Cho and Honorati (2014) focus on various

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 in increasing a general set of labor market outcomes. 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 substantially adds to the work done by Cho and Honorati (2014), first, by updating and broadening the evidence base considerably, while taking into account also policy areas other than finance and training, and second, by choosing a strong focus on employment and business creation. Because of this difference in scope and the advancement in time, we cover 39 evaluation studies that have not been covered by Cho and Honorati (2014). Hence, the major originality of our review is the focus on the ‘*job creation*’ outcome. Considering a wide range of programs will give insight to which instruments under which conditions contribute the most effectively to this goal.

Our review shows that overall the impacts on employment are very modest. Although many interventions successfully affect intermediate outcomes such as management skills, only very few interventions enhance job creation. The picture is a bit more optimistic if the focus is just on the set-up of microenterprises. The evaluated business training and business development programs show on average better results than the access to finance programs. Comparing successes and failures suggests that ‘*intense treatments*’ combined with the appropriate targeting are needed to have an impact. Interestingly, we also find that interventions evaluated with randomized controlled trials (RCTs) show substantially less often significantly positive results than interventions evaluated with quasi-experimental designs. While this has certainly to do with the context and type of intervention RCTs have been used for, it may also imply that many quasi-experimental designs cannot fully get rid of selection effects. In addition, as we show, the very small sample sizes and hence the low power of many RCTs are responsible for their inability to detect significant effects if these are small.

The remainder of this review is organized as follows. In Section 2.2 we lay out our inclusion criteria and the search strategy. In Section 2.3 we propose a theory of change that links program inputs and employment outcomes and will guide our analysis along the causal chain. In Sections 2.4 and 2.5 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 2.6. In Section 2.7 we conclude.

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2.2 Inclusion criteria and search strategy

2.2.1 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 is no common definition to identify MSMEs, neither by researchers nor by statistical offices, we use an employment criterion and set the threshold at 250 employees. We define microenterprises as firms with less than five workers. Small firms are firms with 5 to 19 workers and medium-sized firms are firms with 20 or more, but less than 250 workers. We limit our review to firms in low and middle income countries. We use the thresholds of the World Bank and consider countries as developing countries if they show a gross national income (GNI) below USD 12,476 per capita, calculated using the World Bank Atlas method. 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).

Regarding the outcome, we include studies if they measure the creation of jobs specifically for MSMEs. That is, we included any evaluation study that provides evidence for MSMEs separately from large firms. Whether the policy under study also affects large firms does not matter. 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. 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. Studies are not included if they focus exclusively on hours worked, labor intensity, wages or labor supply without considering employment per se. Interventions that target the youth or the labor force directly are only considered if they aim at the creation of new MSMEs or self-employment. 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,

2.2 Inclusion criteria and search strategy

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.

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 an RCT, as well as on quasi-experimental designs including propensity-score matching, instrumental variables, regression discontinuity designs or difference-in-differences estimation. Since different methods have different strengths and weaknesses, we discuss the problems that come with these techniques in Section 2.4.1.

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.

2.2.2 Search methods to identify relevant studies and data extraction

A range of different search methods were applied to ensure the identification of recent, ongoing, as well as unpublished studies. These include the screening of electronic databases, relevant websites, key journals as well as literature snowballing and contacting of researchers and key experts. Details on the search strategy can be found in Grimm and Paffhausen (2014).

From each included study we systematically extracted information about the author(s), the title, the year and the 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 intervention, we recorded whether the intervention was a stand-alone or joint intervention. If it was the latter, the different components of the intervention were 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, including their methodology, sampling procedures etc. Finally, information regarding the internal and external validity was extracted from the studies. Data

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extracted from included studies, especially those involving judgments by the coder, have been discussed extensively among the two involved researchers.

2.3 Theory of change

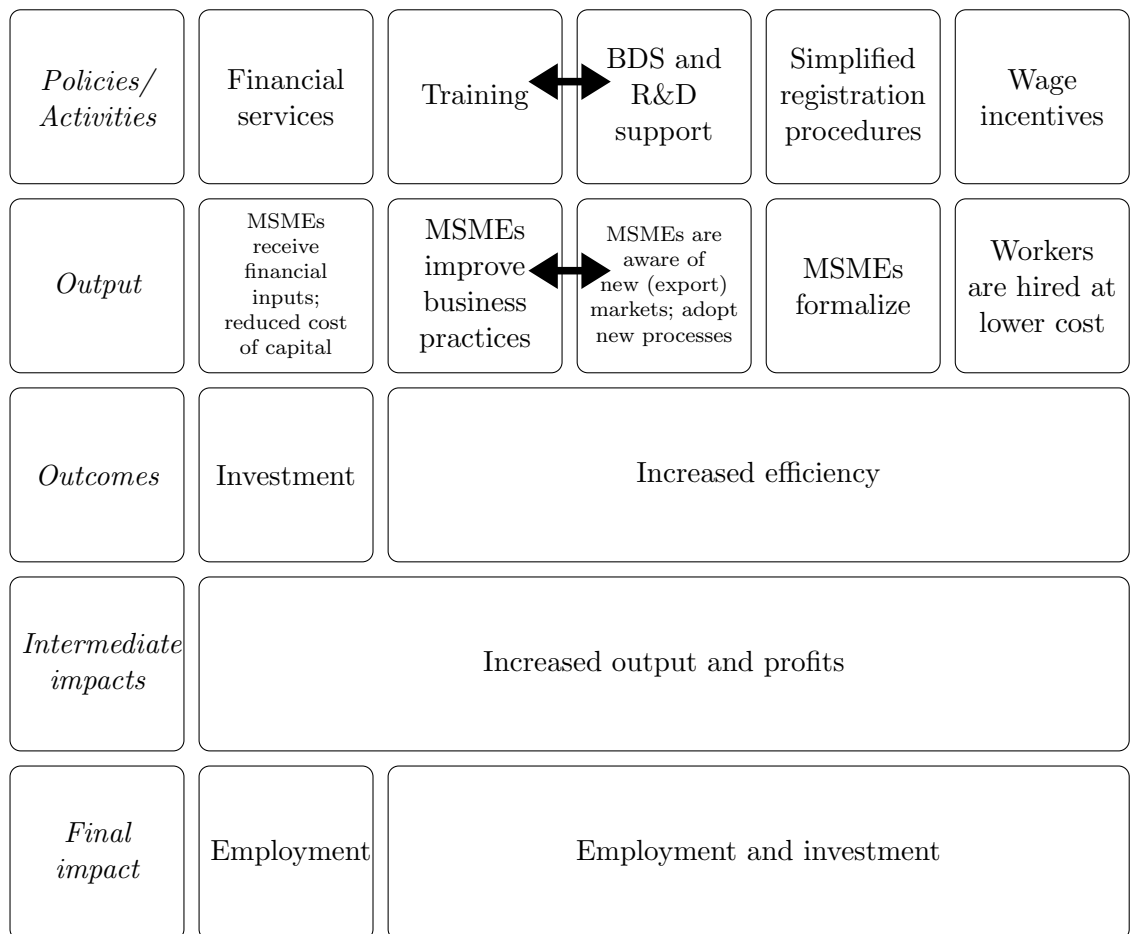
The considered interventions affect firms and prospective firms in many different ways. Some relax capital market constraints, others improve management skills and business practices, and again others reduce the cost of labor or ease formalization procedures. Figure 2.1 below shows a simplified result 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 that increase productivity, such as training, 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 and thereby also make a contribution to employment.

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 then 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

2.3 Theory of change

Figure 2.1: Simplified results chain



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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, even if it would be beneficial for them.

A simplification of registration procedures will only have any effect on employment if the registration allows access to new markets or a reduction of costs of certain inputs such as public services or if it improves 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 in employing more staff.

2.4 Search procedures and results

2.4.1 Search and identification

We organized our database search strategy around two alternative search variants. The first variant combined sets of search terms referring to the population, outcome and type of study and was conducted in February 2013. This search resulted in 1,200 hits. After removing duplicates, there were still 932 records left that needed to be screened. The second variant focused on the different intervention categories considered in this systematic review and was carried out in May/June 2013, resulting in 2,446 hits.¹ After

¹Note that the second database search variant led to a contribution of only three additional studies to the final sample of included studies from the database search (a total of 33) that were not found by

2.4 Search procedures and results

removing duplicates, 1,343 records remained for a careful screening.

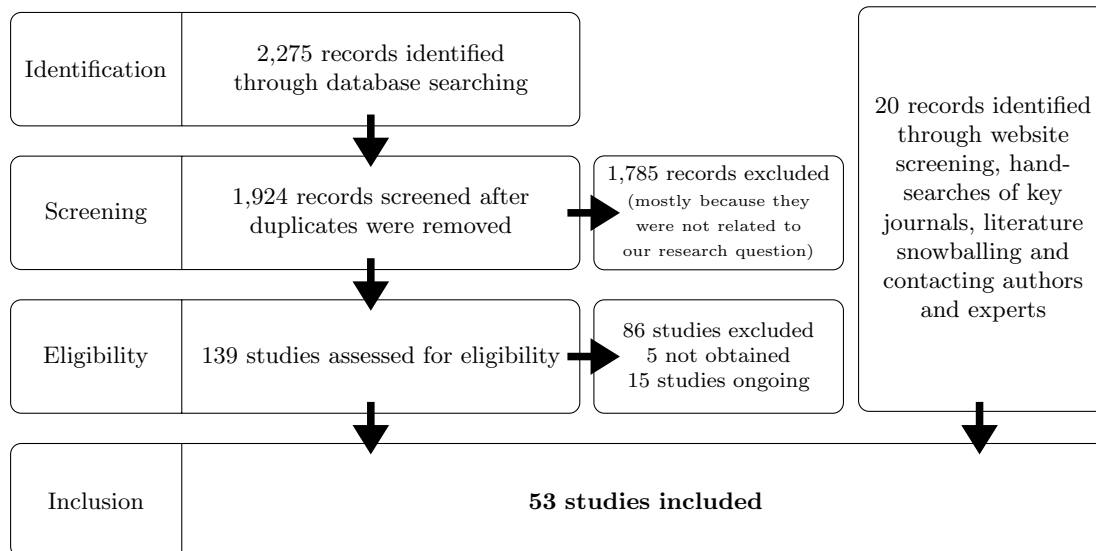
We then merged the results of both database search variants, which together amounted to 2,275 hits. Again, duplicates had to be removed so that the final sample included 1,924 records. These records were screened successively, applying inclusion criteria, in a first step, to titles and abstracts only. 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 a conclusion was reached through discussion, where necessary. Thereby, already about 85 percent of studies could be excluded. 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 small and medium-sized enterprises (SMEs) or smaller enterprises, 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 relevant based on the abstract, but the study could not be obtained. The database search also identified 15 potentially relevant ongoing impact evaluations. We 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 definitively 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, 33 studies were included from the database search.

the first approach. This result gave us confidence in our search strategy and we decided to stop the database search here, not adding further database search variants since the expected benefits in terms of additional studies would not justify the corresponding effort of searching and screening.

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Figure 2.2: Selection of studies



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 for inclusion. Literature snowballing of the World Bank World Development Report 2013 and other 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 led to a final sample of 53 studies that have been included in this review. The entire process is illustrated in Figure 2.2.

2.4.2 Short characterization of included studies

2.4.2.1 Population and context

The 53 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 Latin American countries in particular have often experimented with active labor market policies over the past two decades and that they have started earlier than others to rigorously evaluate many of their policies. There are still only few studies that evaluate interventions implemented in Sub-Saharan Africa, although 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 (14). Only 7 evaluations were implemented in low income countries. The majority of the studies focuses on microenterprises and self-employment (with up to five employees). A total of 36 studies fall into this group. Another 17 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

2.4.2.2 Interventions

Most of the studies in our sample analyze interventions that aim to remove credit constraints of MSMEs (26). 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. There are six studies in the area of business development services including for instance counseling, supplier development, product and process innovation, and the provision of working premises. The studies focusing on private sector incentive schemes all evaluate wage incentives. In

²The size classification is based on the thresholds described in Section 2.2.1. Self-employment is classified as ‘*microenterprise*’. When a study was not clear on the definition that had been used to classify enterprises, but when it reported at least summary statistics regarding the number of employees, we considered the mean size of the enterprises. A number of studies do not in fact report the exact size of the firms analyzed, but have an obvious focus on MSMEs. In those cases we relied on the classification provided in the study.

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Table 2.1: Regional distribution and basic characteristics of included studies

	Finance	Training	BDS/Wage Inc.	Formalization	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	0	0	5
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	3	1	5	1	10
Stand-alone or joint:					
Stand-alone	18	6	1	3	28
Joint	6	12	7	2	27
Both	2	2	1	0	5
Employment creation is primary objective:					
Yes	16	11	7	5	39
No	10	9	2	0	21
Study design:					
RCT	13	15	1	1	30
Quasi-experimental	13	5	8	4	30
Total	26	20	9	5	60

Note: The total count does not add up to 53 because some studies are included in more than one category.

this category we found three relevant studies, which all assess the impact on MSMEs. Finally, we identified five studies that assess interventions falling into the category of improvements to the business environment. These studies all deal with business registration.

2.4.2.3 Outcomes and impacts

As laid 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 reliability 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 standalone and mixed interventions. In these cases we have more than one impact per study. In total, from the 53 included studies, we drew a total of 116 impact estimates. Most of these impacts (about 60 percent) relate to employment. The remaining impacts measure business creation and self-employment. We do not consider firm survival explicitly 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.

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2.4.2.4 Study design and methods applied

Of the 53 studies that have been included in this review, 27 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. This is also the case for the entrepreneurship training interventions. RCTs are often considered the ‘*gold-standard*’ in the evaluation literature as, in principle, they avoid selection effects that can bias the results.

However, RCTs also come with significant drawbacks. The main problem we noticed is the weak power of many of these studies. Most of the included RCTs do not provide any power calculations. Hence, to assess their power to detect a 20 percent and 50 percent change in the outcome, we drew information on the sample size, on the control group mean, on the standard deviation (if available), on stratification, on up-take, and on attrition from a sample of studies. For that purpose we concentrated on the 17 included RCTs that look at the number of jobs created as an outcome (ignoring the set-up of businesses).³ Based on the information collected, we computed the absolute effect size corresponding to a 20 percent and 50 percent change in the outcome. Similar to McKenzie and Woodruff (2014), we then compared these changes to the standard errors of the estimated impacts to make a judgment whether these changes could be detectable. Based on this assessment, we conclude that only about 5 out of the 17 studies are able to detect effect sizes of 20 percent and less. Although the picture looks much better for effect sizes of 50 percent and more, as 11 to 15 of the 17 studies would be able to detect such changes, it will be seen below that in 60 to 80 percent of the studies – depending on the type of intervention considered – the point estimate of the actual effect size is smaller than 20 percent. Hence the upper-bound effect size of 50 percent is hardly relevant. The second drawback with RCTs is their often low external validity, i.e. many of the programs evaluated through RCTs are targeted to specific

³Since continuous outcomes require different formulas to make the power calculations than binary outcomes, we limited the exercise to the continuous outcome to have fully comparable results.

groups and areas and run at a very small scale. Up-scaling those programs may not only be institutionally difficult and costly, but it may also come with a whole range of additional problems. We get back on this issue when discussing the studies below.

In contrast to the areas of finance and training, 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-differences, propensity score matching or instrumental variables to deal with possible selection effects. These studies typically do not have problems with low power and perform much better with respect to external validity. However, they cannot always fully rule out potential selection effects. This is particularly the case for those studies that just rely on cross-sectional matching (10 out of the 28 quasi-experimental studies), although studies where we had serious concerns about causality (see inclusion criteria above) were not included. Most included quasi-experimental designs rely on a difference-in-differences design (14 studies) where the remaining concern is typically only with unobservable time-variant confounders. Studies that just use either an instrumental variable for identification or a regression discontinuity design are rare (2 studies each).

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2.5.1 Access to finance interventions

Most of the 26 studies that deal 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. Field et al. (2011), for instance, evaluate the extension of the period until the first repayment is due. de Mel et al. (2013b) examine savings incentives. Not a single study could be identified that looks at the impact of microinsurance on employment. All studies under consideration are listed and briefly presented in Appendix 2.A. Some of the interventions analyzed here are stand-alone interventions, whereas others are combined with different forms of entrepreneurship training or even other interventions with a completely different scope. This is, for instance, the case for the study by Tarozzi et al. (2013), who look at the combination of microcredit and family planning services. The credits and grants that are typically considered by the evaluations under review

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are in the range of USD 100 to USD 2,000. Loans provided through public credit lines targeted at small and medium-sized firms are on average larger.

With respect to employment creation most microcredit schemes turned out to be rather unsuccessful; only 20 out of 54 impact estimates, which were drawn from the 26 studies on access to finance, show a statistically significant increase in employment or business creation. 32 out of the 54 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 2.A. Table 2.2 provides the distribution of effect sizes. The high frequency of statistically insignificant effects also reflects the often very low power of these evaluations (see our discussion above). Positive effects on employment – if found at all – were only small, especially for already existing small and microenterprises. Major effects were achieved with respect to the creation of new firms (mostly microenterprises) and the expansion of already larger, well-established and profitable firms. But there is hardly any evidence for employment expansion in microenterprises, notable exceptions being a cash transfer program in Uganda (Blattman et al., 2012) and a group lending scheme in the Philippines (Kondo et al., 2008).

The majority of insignificant effects (29 out of 32), as well as the two statistically significant negative employment effects are indeed found in evaluations that focus on microenterprises. More than 70 percent of the effects associated with microenterprises are statistically insignificant. In contrast for small firms, all treatment effects are positive and statistically significant. For medium-sized firms about half of the studies find positive significant effects. All this suggests that it is easier to create jobs in small and medium-sized enterprises than in microenterprises. However, three comments are in order. First, it is not straightforward to disentangle how, on the one hand, firm size and, on the other hand, the country context shape impacts. In low income countries most interventions target microenterprises whereas in middle and in particular in upper middle income countries interventions tend to target small to medium-sized firms. Second, very often the programs that target microenterprises as opposed to those that target small and medium-sized firms do not have employment creation as one of their primary objectives; they are in many cases primarily concerned with income stabilization and female empowerment. Third, the evaluations that deal with small and medium-sized firms predominantly use quasi-experimental evaluation designs which, as explained above, cannot always claim to fully get rid of potential selection effects.

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Table 2.2: Distribution of standardized effect sizes by intervention area

	Finance		Training		BDS/Wage Inc.		Formalization	
	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	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

Note: 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 and we subtract 1 from it. 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.

However, in contrast to experimental methods, evaluations based on quasi-experimental designs often rely on larger samples and less heterogeneous firms. Therefore their power and hence the probability of detecting significant effects is higher. These aspects will be further explored in the framework of a meta-regression analysis below.

Programs which explicitly target women also appear to be less successful in employment creation than programs without such a focus, suggesting that women in particular face additional constraints which need to be overcome in order to increase the return to finance. Women typically have lower levels of formal education, often have only limited access to other relevant services, are exposed to higher consumption demands from their family and relatives, often need to work from home and are often deprived of property rights – land rights in particular.

Analyzing impacts along the full causal chain, linking program inputs with final outcomes, can reveal whether such interventions have an impact on intermediate outcomes and under what conditions changes in intermediate outcomes also lead to changes in the employment outcome. We screened the 26 studies for information regarding the impact on investment, hours worked, productivity, output, sales, revenues, and profits.

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The findings are selective, however, as not all studies explore the same intermediate outcomes.

First, it can be stated that most enterprises make use of the credit or cash grants provided, if they are 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 do they result in fixed capital investments such as machines and 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 Arráiz et al. (2012) find that credit had no impact on capital stocks and suggest that firms rather use credit to increase their working capital.

Studies that assess the impact on labor supply in terms of hours worked in the business find mixed results, with two studies detecting an increase in labor supply as a result of the intervention (see Blattman et al., 2012; Banerjee et al., 2013), but others finding no impact (see Crépon et al., 2011; de Mel et al., 2013b; Karlan et al., 2012) or inconclusive evidence (Augsburg et al., 2012).

A few studies assess the impact on productivity. The majority of these studies focuses on small and medium-sized enterprises and suggests that finance interventions do not affect productivity of established enterprises. It is however important to note, as De Negri et al. (2011) rightly point out, that this lack of impact might also be due to the fact that labor productivity is often proxied by real wages, which are more rigid than actual productivity.

Enterprise performance is typically measured in terms of output, sales, revenues, business expenditures, and profits. Most studies considering the impact on sales do not find any impact, or only in the short-run, such as de Mel et al. (2013b). Of the studies focusing on revenues and business expenditures, most do not detect any effects either (see Banerjee et al., 2011; Karlan et al., 2012; Nelson, 2011) or find that both revenues and costs increase proportionally (Angelucci et al., 2013). Only very few studies assess effects on physical output, but those who do find an increase (see Arráiz et al., 2012; Eslava et al., 2011). Finally, with respect to business profits, the majority of studies does not detect an impact (see Angelucci et al., 2013; Augsburg et al., 2012; Barnes et al., 2001; Crépon et al., 2011; de Mel et al., 2013b; Karlan & Zinman, 2011; Nelson, 2011). Karlan et al. (2012) find that capital grants even lowered profits. Banerjee et al. (2013) detected a positive impact, but only for existing, relatively large and the

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very profitable microbusinesses. Only (Macours 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 could be that the limited evidence for an improvement in business profits is also due to rather short follow-up periods in the evaluation design. It might take 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. Otherwise it may simply imply that employment effects do not materialize because the finance interventions already fail in changing the necessary intermediate outcomes.

Apart from the general observation that many of these programs do not even intend to create jobs, reasons for low or no impacts seem to be that most loans are simply too small and their maturities too short to lead to large changes in the capital stock and the production technology which in turn could generate employment effects. For instance, a tailor who – thanks to a microcredit – switches from a mechanical to an electric sewing machine may still not reach the profitability to immediately hire an additional worker although this tailor may well see an increase in performance as measured by revenues, profits, and business investment. Put differently, if the studies find any growth at all, it is rather at the intensive than at the extensive margin. Field et al. (2011) show that the repayment period also matters. Short grace periods seem to prevent poor entrepreneurs from investing since they fear not being able to repay on time. Moreover, in the cases where potential business starters or subsistence-type enterprises are targeted, a reason for failure may also be that very often there are competing needs. 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 ensure that it leads to increased investment in the business. Yet all this also shows that capital constraints interact with other constraints and therefore, only addressing capital constraints is often not enough.

In summary, the programs under consideration are more effective in creating new firms. In contrast, employment expansion happens in small and medium-sized firms. The weak evidence for employment expansion in microenterprises has different causes. Most importantly, many of these programs do not even intend to create jobs and even if

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they do, these programs are often not well designed (i.e. loans are often too small and repayment periods too short) and they often lack a clear targeting strategy. Further flaws come from the evaluations themselves. Low power of many experimental studies is probably the most serious problem in this respect.

2.5.2 Entrepreneurship training

Overall we reviewed 20 studies that considered entrepreneurship training (see Appendix 2.A). These studies cover quite a heterogeneous set of policies: business skills training, business plan development, financial literacy training, technical and vocational training (in-class and on the job), and life skills training. Entrepreneurship training is sometimes quite general and sometimes 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, and computing to adolescent girls. Other examples can be found in Bruhn and Zia (2011), Calderon et al. (2013) and de Mel et al. (2012a), which 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 & Schündeln, 2011; Jaramillo & 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 & Zia, 2011), others are more substantial. The business training evaluated by Valdivia (2011) for example was delivered over twelve weeks in three sessions per week that lasted three hours each. Vocational entrepreneurship training also tends to be more substantial in duration, varying further with the specific occupations for which the training is provided.

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

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as youth. Often entrepreneurship training interventions, especially when provided for business start-up, are delivered jointly with start-up finance. The majority of studies included here are based on RCTs; only five employ a quasi-experimental design. All together, 36 treatment effects were analyzed.

Looking across all studies, 15 out of 36 treatment effects show significant positive employment effects (see Appendix 2.A). 21 treatment effects were not statistically significant. As discussed above and also shown for finance interventions, the low statistical power of many RCTs is probably partially 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 both Valdivia (2011) and Calderon et al. (2013) found that microentrepreneurs who participated in general business training were 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 their business model and its profitability. Entrepreneurship training can also prevent non-profitable business ideas from being started.

The majority of firms in this sample are microenterprises. Only two studies assess entrepreneurship training interventions for small-sized firms and there is only one study focusing on medium-sized firms (see Table 2.1). Of the 30 treatment effects we collected for microfirms, 63 percent are insignificant with the remaining 37 percent being statistically significant and positive. Out of the five effect sizes for small firms, four are positive and statistically significant. The effect size for medium-sized firms is insignificant.

With respect to the impact of entrepreneurship training along the causal chain, information on a range of intermediate outcomes is available. Employment creation is typically not the primary aim of entrepreneurship training. Instead, those interventions rather aim at improvements in business and entrepreneurial skills, which are supposed to lead to improved business performance. Hence, most studies provide information on those types of intermediate outcomes. Most training interventions have, in fact, difficulties in changing actual business performance. Most programs produce significant improvements in business skills and behavioral skills, and sometimes even higher optimism and motivation (see Cho et al., 2012; Premand et al., 2012), although some of these changes might be due to a change in reporting behavior after the start of the

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intervention (Drexler et al., 2013). In addition, although business skills and practices are significantly higher after the entrepreneurship training interventions, this might not necessarily imply that skills are sufficiently developed to run or expand an enterprise, an issue also being raised by Cho et al. (2012). Of those studies that look at the impact of training on investment, only one study finds a significant positive effect. Yet, in this particular intervention, investment was also a core concept emphasized in the training (see Bruhn & Zia, 2011). Only few studies assess the effect on labor supply, in terms of hours worked. The results from these studies are inconclusive. With very few exceptions, most evaluations do not find that entrepreneurship training led to increased sales or revenues. Finally, the training interventions analyzed here do not generally succeed in increasing profits. Bruhn and Zia (2011), for instance, do not find any treatment effects of business and financial education training on business performance, but they do find changes in business practices consistent with the topics taught in the course. Similarly Gine and Mansuri (2014) find that, especially for male entrepreneurs, business training increased business knowledge and practices, but had no impact on sales or profits. Furthermore, short-term positive effects often seem to vanish in the long run. Hence, in the case of training interventions, it appears that impacts can only be detected very early in the causal chain in the form of improved business skills and, in some cases, practices. But those effects are not sufficient enough to trigger some sort of virtuous cycle, which could lead to improvements in business performance and, ultimately, employment.

Several studies assess the influence of targeting. The evidence is mixed on whether the return to training 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 not always possible to isolate the effect of the training. de Mel et al. (2012a), for instance, find the combination of a cash-grant with entrepreneurship training to be especially successful. The more tailor-made and substantial the training, the better, but the more complex programs are not necessarily the most successful. Drexler et al. (2013), for example, observe that general accounting training led to some improvement 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. Furthermore, from the studies it 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.

To summarize, whereas many of these programs indeed are able to improve business skills, there is only weak evidence for substantial employment effects. However, low power of many of the evaluations is again part of the story, i.e. some of these programs may in fact have employment effects but they are too small to be detected by the studies. The combination of finance and training seems to work better than either of them in isolation. Moreover, training needs to be intensive: short-term interventions do not have a lasting impact.

2.5.2.1 Business development services and wage subsidies

In this sub-section we review nine studies, which are rather heterogeneous in the specific nature of the underlying interventions (see Appendix 2.A). Broadly, they fall under the heading of business development services and targeted subsidies. Four of the nine studies cover business development services in the narrow sense, i.e. they deal with 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 studies measure the employment impact of grants for product and process innovations and three studies cover supply or demand side wage subsidies. The studies dealing with wage subsidies focus on Turkey and Sri Lanka, while the other studies cover Latin American countries almost exclusively. Overall, circumstances were less challenging in these countries than in those that hosted many of the microcredit and entrepreneurship interventions analyzed before. Only one of these nine studies is based on an RCT design, while the others use a quasi-experimental approach, typically by exploiting variation in the policy across time and space in a difference-in-differences framework.

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The studies show mostly positive and statistically significant employment effects (see Appendix 2.A). This holds regardless of the size of firms that are being evaluated. For medium-sized firms, about 62 percent of the thirteen treatment effects are statistically significant; for small sized firms, one out of two treatment effects is statistically significant and positive, while the other is insignificant; and for microenterprises three out of four treatment effects are statistically significant and positive. 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 can be helped with rather simple improvements to their business. Wage subsidies as well as tax-breaks and fiscal incentives conditional on process and product innovations seem to be particularly effective. Interestingly, this has also been found by Kluve (2010) for a sample of European countries. His review showed that direct employment effects were the largest for wage subsidies and ‘*services and sanctions*’ conditional on certain productivity enhancing activities in comparison to other active labor market policies such as training or out-of-work income support.

However, the findings for low and middle income countries covered here are somewhat fragile, first, because the findings are based on a very small set of studies 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 impact of business support services in the East and South East Asian context, where at least in some countries such programs may have played an important role.

With respect to the impact of business development services along the causal chain, most studies consider effects on sales, productivity, and sometimes output, exports, and fixed capital. Generally, the interventions are successful in increasing sales significantly, as well as exports, outputs, and fixed capital. When productivity is considered, effects are also predominantly positive, which is additionally corroborated by studies assessing the impact on wages, since these studies find overall positive impacts. Tan (2009) is an excellent example of a study that assesses the effects of an intervention along the causal chain. He first assesses the impact of the services evaluated on a number of intermediate outcomes where effects were intended, and finds that beneficiary enterprises were more likely to be involved in research and development, to have introduced new product

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lines and production processes, to have obtained internationally recognized quality certification, and to have provided their workers with training. These can be seen as underlying mechanisms which explain why the programs led to increases in sales, labor productivity, and finally employment.

Overall, the studies conclude that the interventions considered here were successful in raising the competitiveness of beneficiary firms, which enabled business expansion. The general positive employment effects of business development services can therefore be seen as a consequence of improvements in business performance which was a result of the intervention. Furthermore, there is also evidence that the time it takes for these effects to occur is substantial. For example, Lopez-Acevedo and Tinajero (2010) assess the impact of different business support programs in Mexico over ten years; only after a four year period the effects become statistically significant, but then they even gain in magnitude as time elapses. Two different programs that were evaluated in a similar context in Turkey allow for an interesting comparison of supply- and demand-oriented subsidies. The supply-oriented on-the-job training program targeted employees (Fretwell et al., 1999), whereas the more conventional wage subsidy programs targeted employers (Betcherman et al., 2010). Whereas the first was a failure, the second was a success, suggesting that employers may keep workers hired at a reduced rate when they are free to choose the workers they actually prefer. In contrast, voucher-based programs may leave little choice to firms and firms may interpret vouchers as a negative signal about the workers' productivity, reducing their chance of being offered a long-term contract. Again, these interpretations are based on only two studies and eventually, whether or not they are really valid can only be determined if several wage subsidy programs test and compare such specific design features.

Indeed the three studies on wage subsidies do not provide much evidence about the underlying mechanisms that are important to understand why or why not such employment effects are lasting beyond the period over which the subsidy is paid. Only de Mel et al. (2013b) systematically assess the effect of the wage subsidy on outcomes such as management practices, investment, sales, profits, and personal income. Delivered as a stand-alone intervention, the wage subsidy has no effect, neither on management practices nor on investment. It neither led to increased sales nor profits. Only when combined with savings or training interventions, the wage subsidy significantly improved investments and sales. Profits, however, remained unchanged.

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Finally, it should be noted 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 percent of the total cost of employing a minimum wage worker. This may still seem acceptable if the jobs created are sustainable, but this is far from obvious (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., 2010, 2013b) which shows a strong correlation between pre-program hiring intentions and program uptake.

2.5.2.2 Interventions to promote formalization

In most low and middle income countries, the bulk of urban microenterprises and small firms is informal, i.e. most enterprises are not registered with the tax authority and operate outside most regulations. A key policy question is whether the performance of these firms can be improved and their size in terms of capital and staff be expanded through formalization. On the one hand, one may argue that formalization increases access to credit and other resources important for business success and expansion. 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 some think is typically already so costly that it alone prevents firms from becoming formal (De Soto, 1989).

As both costs and benefits of formalization are involved, the policy problem of formalization is two-fold: Which interventions are suited to enhance firms' formalization, and what are the effects of becoming formal? We identified five studies that can credibly establish a causal link between formalization and employment (see Appendix 2.A). 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 an RCT.

All studies show that it is difficult to get the average firm formalized, because the average firm 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 for firms that show already a higher initial performance (see de Mel et al., 2013a). Inspections seem to

work somewhat better (see de Andrade et al., 2013)⁴. It also seems easier to formalize firms while they are being set up than to formalize firms that already exist. This has been shown particularly in the case of Mexico (see Bruhn, 2011).

Overall, based on the sample of studies analyzed here, we find that formalization – to the extent it works – yields positive employment effects. Out of the seven effect sizes we drew from the included studies only one is insignificant. For small and medium-sized firms, all effect sizes are statistically significantly positive, while two of the three effect sizes for microenterprises are statistically significantly positive and the other is insignificant.

According to Bruhn (2011) the Rapid Business Opening System (Sistema de Apertura Rápida de Empresas, SARE) reform in Mexico increased the number of registered firms by five percent and wage employment by 2.2 percent. However, employment decreased in ineligible firms (high risk industries) and hence the net effect is less impressive. Kaplan et al. (2011) use a different identification strategy and a different data source to evaluate the same reform. They only observe formal firms that also have formally registered employees and must ignore formal self-employment and new informal jobs in newly formally registered firms. Similar to Bruhn (2011), Kaplan et al. (2011) confirm that the program enhanced formalization: the monthly number of new formal firms increased by 4.7 percent. It is however not possible to say whether these firms are new or whether they existed previously as informal firms. Moreover, according to the findings, the program caused a monthly increase in formal employment of 10 percent. Again, some of these workers may have worked before as informal workers. Overall Kaplan et al. (2011) judge these effects to be rather modest in terms of their magnitude and not necessarily lasting. Over two years, according to the authors, the program would imply 42,408 additional formal jobs, corresponding to just 0.2 percent of informal employment.

In Brazil, the tax simplification program called SIMPLES offered a simplified tax system and lower taxes for microenterprises. The program permitted an overall reduction of taxes of up to eight percent with the main objectives to increase formality and to enhance the competitiveness of microenterprises relative to larger firms. Fajnzylber et al. (2011) find that the policy led to an increase in employment of 0.4–0.5 employees per firm. When the authors account for selection effects by instrumenting formal

⁴This study is not included in our systematic review because it does not assess employment effects.

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status, they even find an effect double that size. The results show that the increase is particularly high for firms with at least one employee. Moreover, the increase is largely in paid employment, as the share of paid employees in employment increases by 10 to 40 percent. The authors attribute the positive employment effects of the SIMPLES program in Brazil to the reductions in social security payments for hired labor. They also correctly point out that this effect does not necessarily apply to any informal firm that would be formalized, but rather reveals what would happen if entrepreneurs register their firm at the time of start-up. Corseuil and Moura (2010) also analyze the SIMPLES reform, but focus on firms with at least five employees. They find an increase of 6 to 7.5 percent in the average employment level.

In contrast, de Mel et al. (2013a) focus on microenterprises in Sri Lanka and do not find any employment effect as a result of formalization.

Of the five studies considered here, only two studies present detailed evidence on causal mechanisms linking formalization and employment outcomes. For Brazil, Fajnzylber et al. (2011) find that, among those firms that do formalize, performance improves in terms of revenue and profits. Effects on fixed capital are inconclusive, since formalization appears to have no impact on access to credit or sales to larger firms, but rather on the probability that firms operate from a fixed location. In conjunction with the positive employment effect mentioned earlier, the authors conclude that the mechanism through which formalization improves business performance is the creation of more permanent and larger scale operations (Fajnzylber et al., 2011). de Mel et al. (2013a) also do not find any evidence that formalization improves access to credit. They further cannot detect any impacts on sales, profitability, or capital stock, though business practices, in particular the use of receipt books and advertising, improve as a result of formalization. Formalization appears to have a substantial positive effect on profits. But this effect is driven by very few firms in the upper tail of the distribution. Providing qualitative evidence on these firms, de Mel et al. (2013a) show that they indeed used their formal status to develop new forward and backward linkages, to receive official quality labels, or to take a loan and invest in capital. Notwithstanding, these effects were not strong enough to have a sizeable effect on employment.

de Mel et al. (2013a) also consider potential impacts on attitudes of the entrepreneurs and find that trust in local governments they interacted with during the

intervention improved and they tend to agree more often that it is important to pay taxes. However, there are no effects on actual tax payments.

In conclusion, it seems that programs that *'force'* firms to formalize are unlikely to produce any significant employment effects as for many previously 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, of course, does not imply that policies should not simplify administrative procedures, but that efforts need to go further. It is not the cost 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 will also lead to additional jobs.

2.6 Meta-regression analysis

2.6.1 Method

To implement the 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 at the 10 percent significance level⁵, 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:

$$Pr(y_i = 1|X_i) = \theta(X_i'\beta + \epsilon_i), \quad (2.1)$$

where X_i includes the type and characteristics of the intervention, the term ϵ_i is the error term and stands for the cumulative normal distribution. Since coefficients of a probit model cannot be directly interpreted, we compute and show marginal effects. We

⁵Since we had only four statistically significant negative impact estimates in the sample, we decided to lump together insignificant and statistically significant negative estimates.

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correct the standard errors for within-group correlations, since some studies contribute with more than one outcome. 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:

$$y_i = X_i' \alpha + \nu_i. \quad (2.2)$$

On the right hand side we use the same explanatory variables as in the probit model above. The term ν_i captures the unexplained part of the variance in the outcome, which also includes measurement error.

As explanatory factors we include the type of the intervention, whether the intervention was 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 and the way the standardized effect size is computed. Table 2.3 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 to which heterogeneity – in particular in terms of context and program characteristics – can be captured by the meta-regression analysis.

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. Around 70 percent of the estimates come from evaluations of interventions that are 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 RCTs. Fur-

2.6 Meta-regression analysis

Table 2.3: Description of the 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
Employment creation is 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
Micro-enterprises	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

^aRegarding the computation of effect sizes, see Note to Table 2.2.

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thermore, the majority of evaluations focused on microenterprises and was conducted in upper middle income countries.

2.6.2 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 2.4). 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, or wage incentive schemes, or interventions that improved the business environment.

The multivariate analysis confirms many of the findings discussed in the more narrative review above. Interventions of the type we consider seem to have more chances in establishing new firms than expanding existing firms.⁶ 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. Interestingly, the impacts in medium-sized firms are not significantly different from the impacts measured for microenterprises, although descriptively we find positive significant impacts in medium-sized firms more often than in microenterprises. It is only if we control for the intervention category that the effect associated with medium-sized firms loses significance, though it remains positive. Separating intervention category effects from firm size effects is not straightforward as the distribution across different intervention categories is very different between microenterprises and medium-sized firms. Interventions targeted at microenterprises are mainly in the area of finance and training. Interventions targeted at medium-sized firms are mainly in the area of business development and wage subsidies and only to a lesser extent in the area of finance. If we look at finance interventions only, we see that 57 percent of the interventions targeted at medium-sized firms have a positive significant employment effect but only 28 percent of the interventions targeted at microenterprises.

⁶However, 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).

In conclusion, we think it is safe to say that it is less difficult to expand employment in medium-sized firms than in microenterprises.

Surprisingly, whether employment creation has been an explicit objective of the evaluated intervention does not correlate with larger employment effects. Combined interventions also did 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 systematic effect on female enterprise performance (see for instance de Mel et al., 2008, 2009; Fafchamps et al., 2011). The causal mechanisms underlying this finding were discussed above.

Finally, the multivariate analysis also confirms 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 22 out of the 29 treatment effects that are based on an RCT show insignificant effects, while this is the case for only 10 out of the 25 treatment effects based on quasi-experimental methods. Hence, the failure rate is almost twice as high. To explore this further, 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, as we have explained above, controlling for this problem and other risk of biases (such as John Henry and Hawthorne effects, attrition, non-compliance, spill-overs and sample selection) could not substantially reduce the estimated coefficient associated with RCTs. However, we think that the fact that RCTs focus more prominently on small programs, very poor areas and very specific target groups (as compared to evaluations based on quasi-experimental designs) also explains why they show more frequently weak or null effects. In other words, RCTs are often applied in very specific cases and therefore their findings cannot always be generalized.

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Table 2.4: Results from the meta-regression analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Positive signifi- cance, un- weighted	Positive signifi- cance, un- weighted	Positive signifi- cance, un- weighted	Positive signifi- cance, un- weighted	Positive signifi- cance, un- weighted	Effect size, un- weighted	Effect size, weighted
Program type:							
Training	Ref.			Ref.	Ref.	Ref.	Ref.
Finance	-0.048 (0.123)			-0.227 (0.147)	-0.294** (0.136)	-0.217** (0.102)	-0.245* (0.13)
Business development services	0.225 (0.156)			-0.012 (0.186)	-0.007 (0.197)	-0.0723 (0.123)	-0.082 (0.134)
Private sector incentives and business environment	0.332** (0.138)			0.154 (0.205)	0.06 (0.225)	-0.105 (0.112)	-0.106 (0.144)
Firm size:							
Microenterprises		Ref.		Ref.	Ref.	Ref.	Ref.
Small enterprises		0.482*** (0.104)		0.410** (0.173)	0.371** (0.186)	0.126 (0.136)	0.0457 (0.154)
Medium-sized enterprises		0.176 (0.17)		0.107 (0.25)	0.0712 (0.243)	-0.102 (0.141)	-0.182 (0.174)
Employment creation is primary objective		0.0856 (0.133)		0.05 (0.137)	0.024 (0.152)	0.022 (0.083)	0.04 (0.098)
Joint intervention		0.0001 (0.128)		-0.11 (0.17)	-0.209 (0.148)	0.0004 (0.0739)	-0.032 (0.0871)
Intervention targets women		-0.244* (0.128)		-0.216 (0.14)	-0.272* (0.143)	-0.027 (0.081)	-0.072 (0.101)
Evaluation based on an RCT				-0.334* (0.176)	-0.365** (0.172)	-0.107 (0.14)	-0.087 (0.156)
Outcome is business creation			-0.135 (0.097)	0.13 (0.132)	0.240* (0.138)	0.014 (0.051)	0.011 (0.055)
Country income category:							
LIC				Ref.	Ref.	Ref.	Ref.
LMIC				-0.053 (0.214)	-0.035 (0.235)	-0.102 (0.114)	-0.111 (0.125)
UMIC				-0.186 (0.233)	-0.146 (0.247)	-0.106 (0.15)	-0.072 (0.18)
Effect size measure is SMD						0.138 (0.098)	0.154 (0.121)
Square root of sample size				-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0002 (0.0001)	-0.0002 (0.0001)
Intercept						0.317 (0.215)	0.34 (0.243)
Observations	116	116	116	116	116	115	115
Pseudo R2	0.051	0.156	0.013	0.21	0.205	0.22	0.197

Note: Robust standard errors clustered at the study level are given in parentheses.

*, **, and *** denote significance at the 10, 5, and 1 percent levels respectively.

SMD: Standardized Mean Difference.

LIC: Low income country.

LMIC: Lower middle income country.

UMIC: Upper middle income country.

Ref.: Reference Category.

2.7 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. It is *'a long way'* in the result chain from policy inputs to employment impacts, even more so if employment is intended to be sustainable and tied to acceptable and secure working conditions. Given the discrete nature of decisions to create a new business or to hire an additional employee, *'intense treatments'* combined with the appropriate targeting are needed to have an impact. Surprisingly, the available evidence suggests that it is easier to enhance the creation of a microenterprise than expanding employment in a microenterprise, which would suggest that many of these microenterprises rapidly attain their optimal firm size. McKenzie and Woodruff (2014) found similar evidence for smaller set of business training and entrepreneurship evaluations. Based on our review, we can generalize this finding also to other types of interventions.

It seems that particularly (micro)finance and training interventions achieve positive effects only very early in the result chain, improving management practices, skills, and investments but without further or lasting results on business performance and, finally, 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 vocational and business training programs in particular have positive effects on business outcomes such as improved knowledge and practice and sometimes income, but effects on a general set of labor market outcomes are quite weak and very often insignificant. They even find training alone to have a negative effect on labor market activity and to be effective only in combination with finance. This is something we cannot confirm. Our results show that even as stand-alone intervention training leads at least on average to positive employment outcomes, above all in terms of the creation of new businesses.

Business development services seem to generate somewhat better employment outcomes, but the evidence mainly comes from small and medium-sized enterprises in Latin American countries and therefore it is difficult to say whether these programs would be equally successful when targeted at microenterprises or at any enterprises in the

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African or Asian context. For interventions that intend to enhance formalization, we faced a similar problem. The evidence mainly comes from Latin America. The overall effects of formalization on employment have been positive in Brazil and Mexico; the magnitude of these effects is still debated. However, these studies also show that it is not easy to encourage firms to formalize in the first place.

Interestingly, our study 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, a fact that has so far been ignored in the literature. Hence, one should not expect massive impacts on job creation if interventions were not even intended to deliver this result. We believe this is basically an issue of design and targeting. If business development in the form of capital accumulation and employment expansion is the objective, the type of entrepreneurs that needs to be targeted is different from those that are targeted through finance and entrepreneurship interventions that aim to reduce poverty and to empower women.

The purpose of such a review is also to highlight still existing knowledge gaps. We show that the available evidence is still sketchy particularly for large parts of Sub-Saharan Africa and Asia, regions in which the need for jobs will be the highest in the coming decades. 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. Many studies also 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 53 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.

Finally, 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 frequently 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 which 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. Hence, RCTs seem to paint an overly pessimistic picture with respect to the potential of such policies and interventions to create jobs. So far this has been largely ignored. This contrast is not only important with respect to the implications of this study; it is also – we believe – an important insight that can enrich the debate about the advantages and disadvantages of different evaluation designs.

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Appendix 2.A Overview of included studies

Table 2.A.1: Overview of included studies

Reference	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)	Micro-credit, 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
Arráiz 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 BC – indiv. lending: insignificant BC – group lending: insignificant female BC – indiv. lending: insignificant female BC – group lending: positive BC: positive
Artanasio 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	
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	
Banerjee et al. (2011)	Direct transfer of productive assets combined with provision of training (incultation of savings habits and integration into micro-finance groups) to the 'ultra poor', particularly women.	no	India (rural West-Bengal)	Micro	yes	RCT	18 months	BC: insignificant

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2.A Overview of included studies

Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
Banerjee et al. (2013)	Group-liability micro-credit loans ranging between USD 200 at market exchange rates (or USD 1,000 at PPP-adjusted exchange rates) USD 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 E: insignificant
Barnes et al. (2001)	Group-liability micro-credit, 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
Crépon et al. (2011)	Micro-credit, 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/Northeast)	Small and medium	no	Quasi-experimental: PSM	3 years	E – program 1: positive E – program 2: positive

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Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
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
De Negri 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 E: 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
Esilava et al. (2011)	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 micro-loans. Normally repayments start after two weeks.	no	India (Kolkata)	Micro	yes	RCT	3 years	EC: insignificant E: insignificant
Gubert and Roubaud (2011)	Individual-liability loans averaging EUR 500 for urban micro-businesses and longer-term loans (from 24 to 36 months) to small and medium-sized enterprises (SMEs) averaging EUR 8,000.	no	Madagascar (urban)	Micro	no	Quasi-experimental: Matching	up to 4 years	E: insignificant

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Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
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 USD 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 micro-credit, 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 micro-credit, 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 micro-finance 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 (2014)	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

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Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
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. insignificant Assistance (BDS): insignificant E – cluster formation (BDS): insignificant E – technology dev. (BDS): insignificant E – any BDS: positive BC – Oromiya: insignificant BC – Amhara: insignificant
Tarozzi et al. (2013)	Joint-liability micro-credit, combined with family planning program	yes	Ethiopia (rural)	Micro	no	RCT	up to 2 years	
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 BC: 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	
Banerjee et al. (2011)	Direct transfer of productive assets combined with provision of training (inculation of savings habits and integration into micro-finance 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 micro-credit clients.	yes	Bosnia and Herzegovina (urban)	Micro	no	RCT	6 months	E: insignificant BC: insignificant

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2.A Overview of included studies

Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
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 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 – T: insignificant E – T+F: insignificant E – T+WS: positive

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Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
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 E: 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: micro-entrepreneurs interested in training.	no	Dominican Republic (urban)	Mostly micro	no	RCT	1 to 2 years	
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
Gine and Mansuri (2014)	Hands-on business training based on the ILO's 'Know About Business' modules was added to micro-finance. 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 micro-enterprise 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

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2.A Overview of included studies

Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
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 micro-entrepreneurs who are micro-credit clients.	yes	Peru (regional)	Micro	yes	RCT	up to 2 years	E: insignificant BC: insignificant
Klinger and Schündel (2011)	Multi-phased business plan competition	yes	El Salvador, Guatemala and Nicaragua (within-country distribution unclear)	Micro and small; not necessarily existing entrepreneurs	no	Quasi-experimental: RDD	1 to 3 years	BC: positive
Lopez-Acevedo and Tinajero (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

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2. DO INTERVENTIONS TARGETED AT MICROENTREPRENEURS AND SMALL AND MEDIUM-SIZED FIRMS CREATE JOBS? A SYSTEMATIC REVIEW OF THE EVIDENCE FOR LOW AND MIDDLE INCOME COUNTRIES

Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
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 micro-entrepreneurs in Lima.	no	Peru (urban)	Micro	yes	RCT	10 months	BC – general training: insignificant BC – general + specific training: insignificant
Business development services (BDS)								
Arráziz 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 et al. (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 E: 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

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2.A Overview of included studies

Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
Lopez-Acevedo and Tinajero (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 E – techn. assistance (BDS): insignificant E – cluster formation (BDS): insignificant E – technology dev. (BDS): insignificant E – technology dev. (BDS): insignificant E – any BDS: 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	
Private sector incentive schemes (wage subsidies)								
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

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2. DO INTERVENTIONS TARGETED AT MICROENTREPRENEURS AND SMALL AND MEDIUM-SIZED FIRMS CREATE JOBS? A SYSTEMATIC REVIEW OF THE EVIDENCE FOR LOW AND MIDDLE INCOME COUNTRIES

Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
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
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
Corseuil and Moura (2010)	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

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2.A Overview of included studies

Table 2.A.1 – continued from previous page

Reference	Intervention	Job priority	Country	Size of enterprises targeted	Women targeted	Study design	Length of follow-up	Program effect(s)
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 EC: positive

BC: Business creation. E: Employment creation. DID: Difference-in-differences. IV: Instrumental Variables. PSM: Propensity Score Matching. RCT: Randomized Controlled Trial. RDD: Regression Discontinuity Design.

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Chapter 3

Small firm death in developing countries

with David McKenzie

3.1 Introduction

Twenty-seven percent of the non-agricultural labor force in developing countries consists of self-employed business owners with no employees (Gindling & Newhouse, 2014), and 99 percent of the firms in many poor countries have 10 workers or fewer (McKenzie, 2017). These firms are an important source of income for the poor, and many policy interventions are designed to help people start and grow such firms. Yet there has been much less attention devoted to the death of such firms, with no systematic evidence available as to the rate of small firm death, which firms are more likely to die, and why they die. Almost two-thirds of published randomized experiments testing policy interventions for small firms in developing countries ignore firm death completely, neither reporting the death rate nor examining it as an outcome.¹

Understanding the prevalence, characteristics, and causes of firm death is important for poverty, productivity, and policy. Since self-employment is an important source of income for the poor, firms shutting down could mean a large loss in income for firm

¹Appendix Table 3.A.1 provides details from 35 published randomized experiments testing different interventions for small firms in developing countries. Out of these 35 experiments, only 12 report the firm death rate and 13 examine firm death as an outcome.

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

owners. A growing body of literature (e.g. Hsieh & Klenow, 2009) has noted large misallocation of resources in developing countries. Firm death can improve aggregate productivity if less productive firms die and reallocate resources and customers to more efficient competitors. The optimal policy response to firm death then depends on whether it involves large income losses for the poor, and whether it is efficiently selecting out the least productive firms or not.

This paper provides systematic evidence on firm death in small firms in developing countries by collating data on more than 14,000 small firms from 16 firm panel surveys in 12 countries, enabling estimation of the rate of firm death over horizons as short as 3 months, and as long as 17 years. This overcomes many of the limitations of the existing literature on firm death in developing countries, which have had to rely on a small number of time periods, usually just in a single country, often with small samples of micro firms, and high rates of attrition.² Moreover, we included detailed questions in nine of these surveys to measure cause of death, and most of our surveys also continue to track the firm owner after their firm has died to provide data on what they do once their firm dies.

We begin by estimating the prevalence of firm death. Existing estimates of the rate at which small firms die in developing countries range from as low as 3 percent per year (Frazer, 2005; Davies & Kerr, 2018) to over 30 percent annually (Fajnzylber et al., 2006; Nagler & Naudé, 2017). We find the rate of firm death is approximately linear over time horizons up to five years, averaging 8.2 percent per year, but then declines over longer time horizons, so that firm death rates average less than 5 percent per year when looking over intervals of 10 years or longer. The result is that half the stock of firms operating at a given point in time will die within the next six years.

We then examine which firms are more likely to die, through examining the extent to which firm and owner characteristics predict firm death. Younger firms are much more likely to die: 17 percent of firms die in their first year, compared to only 4 percent of five- to six-year old firms dying in their next year. Less profitable firms are more likely to die. Death rates are higher for younger and older entrepreneurs than for middle-aged owners, and higher for female owners than males, although this gender

²We discuss how firm death has been measured in the existing literature in the next section, and compare our estimates of death rates to those in existing studies in Appendix Figure 3.B.1 and Table 3.B.1.

3.2 Measurement of firm death in the existing literature

difference is no longer statistically significant after controlling for other firm and owner characteristics. Firm death rates are found to be higher in richer developing countries than poorer countries.

Finally, we turn to why firms die and what happens after death. Firms are more likely to close because they are making a loss, or because the owner suffers household shocks, than because of positive shocks to the outside opportunities facing the owner. As a result, firm death is associated with a large fall in the labor earnings of the owner. We find firm death need not be forever, with approximately 40 percent of owners whose firms have closed opening a new firm again within three years.

The remainder of this chapter is structured as follows: Section 3.2 discusses the different approaches to measuring firm death that have been used in the existing literature. Section 3.3 outlines our approach to firm death and the sample we have put together. Section 3.4 examines the prevalence of firm death, Section 3.5 which firms are more likely to die, Section 3.6 why firms die and what happens afterwards, and Section 3.7 concludes.

3.2 Measurement of firm death in the existing literature

At least four different approaches have been used to measure firm death in the existing literature, each with particular strengths and drawbacks.³ A first approach is seen in some of the earliest empirical work on the topic, undertaken by Carl Liedholm and Donald Mead in the early 1990s in several sub-Saharan African countries (Liedholm & Mead, 1995; Mead & Liedholm, 1998). They carried out cross-sectional “closed firm” surveys by asking a random sample of households about enterprises they previously ran, but no longer operate, in addition to those they currently run. Based on this data, they report an average closure rate of 12.9 percent per year; although using the same data McPherson (1995) reports annual hazard rates of firm death of 3 to 4 percent per year. This approach has the advantage of allowing for large samples, and for representativeness of the areas in which sampling occurs. However, as the authors themselves acknowledge, it is likely to be much less accurate than panel surveys which

³A fifth, related, approach has been to use labor force panel surveys to track movements into and out of self-employment status (e.g. Demirgüç-Kunt et al., 2011; Fajnzylber et al., 2006). This does not enable examination of whether a particular firm survives, since individuals can shut down one firm and start another one, whilst remaining self-employed. Furthermore, labor force surveys typically do not contain information on the firm such as firm age, number of employees, or profitability.

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

track businesses over time, since people may forget or not want to talk about businesses that failed, and there is likely to be substantial recall error as to exactly when the business closed. Moreover, it will not capture people who close their businesses and then migrate out of the area.

A second approach has been to use manufacturing censuses or company registers. Examples include Roberts and Tybout (1996) who provide annual firm exit rates of 10.8 percent in Chile, 11.1 percent in Colombia, and 6.0 percent in Morocco; Bartelsman et al. (2009) who graphically show annual exit rates of approximately 4 to 5 percent in 10 Latin American and Eastern European economies; Klapper and Richmond (2011) who use a firm census of formal firms in Côte d'Ivoire and find annual exit rates of around 10 percent; and Shiferaw (2009) who reports annual exit rates of 16 percent for privately-owned Ethiopian manufacturing firms in the census. These censuses offer the advantage of providing population rather than sample data on the dynamics of larger firms. However, they are unsuited for examining the death of small firms for several reasons. First, many of the censuses are restricted to firms with 10 or more workers, and/or those which are formally registered, and they are extremely unlikely to capture non-visible businesses operating within households. Second, Shiferaw (2009) notes that it is not possible to distinguish firms that have closed from those that have switched sector out of manufacturing, or which have fallen below the cut-off point for inclusion. Third, they do not include firms in the retail and service sectors, which comprise the majority of small businesses.

A third approach is to use multi-purpose household living standards panel surveys such as the World Bank's Living Standards Measurement Study (LSMS). This approach was used in Vietnam by Vijverberg and Haughton (2004) who report 61 percent of firms dying over a five- year period, and McCaig and Pavcnik (2016) who find 30 to 35 percent death rates over two year periods. Krafft (2016) uses the Egypt Labor Market Panel Survey to find 51 percent of firms died over an 8-year period, and 61 percent over a subsequent 6-year period. The advantages of these datasets are that the samples can be large and representative, and they capture micro and small businesses operating within households. However, these authors note several challenges to the use of these household surveys to track enterprises. Since the surveys do not identify specific enterprises by either name or code, authors need to match firms over time based on characteristics such as the age of the firm, identity of which household member runs the

3.2 Measurement of firm death in the existing literature

firm, and its sector. This can result in measurement errors if firms switch their sector of business or owners have trouble recalling the age of the firm. Further downsides are that the surveys are often conducted at irregular intervals, limiting the periods over which death rates can be measured, and often contain relatively little information on firm characteristics to enable exploration of which firms die and why.

The final approach is then to use dedicated firm panel surveys to track a sample of firms over time. The main challenge for this approach has been the dearth of firm panel surveys in developing countries, small sample sizes, and the difficulty of tracking firms with relatively low attrition. This approach was first tried by Mead and Liedholm (1998), who report an annual closure rate for micro and small enterprises in the Dominican Republic of 29 percent in 1992 and 22 percent in 1993, based on a sample of unspecified size with an unspecified attrition rate. They note that they also tried the approach in Zimbabwe, but were unable to relocate 42 percent of firms. Panel surveys of manufacturing firms in several sub-Saharan African countries were collected as part of the Regional Program on Enterprise Development (RPED) by Oxford University and the World Bank, and have been used in several papers on firm dynamics (see Frazer, 2005; Söderbom et al., 2006; Sandefur, 2010). These surveys typically include 100-200 manufacturing firms per country, and do not include household enterprises. They report firm death rates of 6.3 percent over two years (Frazer, 2005) and 19 to 44 percent over 5 years (Söderbom et al., 2006), although they do not report attrition rates. Davies and Kerr (2018) conducted a 10-year follow-up survey of a random sample of 1,000 firms surveyed in the 2003 Ghanaian manufacturing census. They could not find what had happened to 29 percent of the 386 firms with under 10 workers in their sample, while 25 percent had died.

This review of the existing literature shows that there has been relatively little evidence on the rates of firm death in the most prevalent types of enterprises found in developing countries: micro and small household enterprises that include retail and service firms in addition to manufacturing. Few of these existing studies measure attrition rates, nor do any provide bounds on what the rate of firm death will be when this is taken into account. A number of the studies provide descriptive information on what types of firms are more likely to die, and we will compare our findings on these correlates in Section 3.5. However, the fact that firm death is only a fraction of the sample, and that single country samples have often been relatively small, has meant that

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

studies typically have relatively few deaths with which to examine the characteristics of who dies. For example, Frazer (2005) pools four rounds of firm surveys from Ghana, and still has only 30 deaths versus 479 survival episodes to compare; while Davies and Kerr (2018) have 95 deaths of firms with 0 to 9 workers.

3.3 Our approach to measurement and our sample

Our conceptual unit of analysis is the firm, rather than the entrepreneur, and our focus is on non-farm micro and small enterprises in developing countries. The vast majority of such firms have fewer than ten workers, with the modal firm in many developing countries consisting of just the owner with no paid workers (McKenzie, 2017). We define firm death as having occurred if a firm is open at one point in time, and then is reported as having shut down by the owner in a subsequent survey round. By shut down, we mean that the owner of the firm has decided to stop operating the firm, and no one else is operating it. It is not intended to include temporary closures of a few days or weeks that may occur when the owner is ill or away.

Seasonal firms, that regularly open for only part of the year, close for part of the year, and then re-open again will be rare in our data for a combination of sectoral choice and sampling reasons. First, our focus is on firms outside the agricultural and fishing sectors, and so excludes seasonality that arises in those sectors. Second, the majority of our sample comes from firm panels that rely on firms operating full-time for inclusion. Nevertheless, we discuss this issue in more detail with regard to the different surveys below.

3.3.1 Putting together a large panel of micro and small firms

We draw on information on firms from both multi-purpose household and firm panel surveys in order to construct a large panel with which to examine death rates. Our resulting dataset combines data from 16 surveys in 12 developing countries, and includes a total of 14,426⁴ firms at baseline, with a longitudinal dimension that is able to cover the dynamics of these firms in the short, medium and long term. Table 3.1 lists the surveys underlying the dataset, Appendix 3.C discusses more details of each survey and how firm death was measured in each case.

⁴This number refers to the total number of unique firms we observe.

3.3 Our approach to measurement and our sample

Table 3.1: Overview of included surveys

Survey	Rounds	Number of Rounds	Sample Size
<i>A. General Household Surveys:</i>			
Indonesian Family Life Survey (IFLS)	2000,2007/2008,2014/2015	3	2,462
Mexican Family Life Survey (MxFLS)	2002,2005/2006,2009/2010/2011/2012	3	723
Nigeria General Household Survey (NGGHS)	2010/2011 - 2012/2013 (post-planting and post-harvest each)	4	1,804
Townsend Thai Survey (TTHAI)	1997 - 2014 (Annual)	18	1,122
<i>B. Specialized Firm Impact Evaluation Surveys</i>			
Cotonou Informal Enterprise Survey (BJINFORMALITY)	2014 - 2016	3	1,197
Egypt Macroinsurance for Microenterprises Survey (EGMACROINSURANCE)	2012-03 - 2012-11	2	1,441
Ghana Microenterprises Survey (GHMS)	2008 - 2010	6	397
Kenya GET Ahead Business Training Program Impact Evaluation Survey (KEGETAHEAD)	2013 - 2016	3	2,365
Malawi Business Registration Impact Evaluation Survey (MWBRIE)	2011/2012 - 2013/2014	5	757
Nigeria YouWiN! National Business Plan Competition Impact Evaluation Survey (NGYOUWIN)	2011 - 2016	4	280
Sri Lanka Microenterprise Survey (SLMS)	2000 - 2016	15	269
Sri Lanka Impact Evaluation Survey of Business Training for Women (SLFEMBUSTRAINING)	2009 - 2015	5	260
Sri Lanka Informal Enterprises Survey (SLINFORMALITY)	2008 - 2011	4	105
Sri Lankan Longitudinal Survey of Enterprises (SLLSE)	2008 - 2014	11	284
Lomé Informal Enterprise Survey (TGINFORMALITY)	2013/2014 - 2015	5	499
Uganda Women's Income Generating Support Impact Evaluation Survey (UGWINGS)	2009 - 2011	2	461

Note: We use only the control group from surveys that are part of randomized experiments.

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

Our inclusion criteria for using multi-purpose household panel surveys was to include nationally representative surveys that included a detailed non-farm business module, had low rates of attrition, a relatively large sample size, and enabled us to measure firm survival and death rates over at least three or more waves. This yielded four surveys. The first two are the Mexican Family Life Survey (MxFLS) and the Indonesian Family Life Survey (IFLS), both of which provide large samples of households. These surveys occur at low frequency, enabling us to measure firm death over horizons of three, four and a half, and eight years in Mexico, and seven, eight, and fifteen years in Indonesia. We examined the set of LSMS surveys to determine the ease at which they could be used to construct household firm panels. This led to the inclusion of the Nigeria General Household Survey (NGGHS), which allows measurement of firm death over 0.5, 1.5, 2, and 2.5 year horizons. Finally, we also include data from the Townsend Thai Project. This is a smaller sample, but resamples households annually, over an extended period of time, enabling firm death to be measured over periods from one to seventeen years.

These household panel surveys were not designed to track businesses over time, and do not link the businesses reported from one survey wave to the next. We therefore consider households that only operate one enterprise, and follow Vijverberg and Haughton (2004), McCaig and Pavcnik (2016) and Krafft (2016) in using information on firm age, business ownership, and sector to attempt to link firms over time.⁵ Since these household panels also contain data on individuals who are not operating firms in the first period, they enable us to also capture information on new firms that households start between one survey round and the next.⁶ We then pool together multiple survey rounds to get a larger sample; Appendix 3.C details these procedures in more detail.

These multi-purpose surveys do not screen on whether firms operate year-round or not, nor do they ask how many months of the year firms are open. The one exception is the NGGHS, which does ask whether closure is seasonal. Only 11.6 percent of firms that closed between rounds are reported to have been closed seasonally.

The second form of data comes from specialized firm panel surveys that have been collected as part of randomized controlled trials to assess different interventions designed to help micro and small firms in different countries. In each case we only use the control group data, in order to be able to assess firm death in the absence of any

⁵The majority of households who operate a business operate only one business. In the 2000 round of the IFLS these are 80 percent, in the 2002 round of the MxFLS 93 percent, in the 2010 round of the NGGHS 58 percent, and in the 1997 round of the Townsend Thai Project 83 percent.

⁶However, these surveys are not well-designed for studying firm entry rates, since we miss firms that open and then close again between survey rounds. Appendix 3.D discusses firm entry in more detail.

3.3 Our approach to measurement and our sample

intervention.⁷ We searched publicly available datasets for impact evaluations targeted at microenterprises, but found most data made available only provided sufficient detail for replicating particular papers, and were not suitable for assessing survival dynamics. We use 11 panel surveys carried out in randomized trials conducted by one of the authors.⁸ These include microenterprise surveys of informal firms used for formalization experiments in Benin, Malawi and Sri Lanka; surveys of microenterprises used in business training interventions in Kenya, Sri Lanka, and Togo; microenterprises that were part of cash grant interventions in Ghana and Sri Lanka; microfinance clients that were part of an insurance intervention in Egypt; business plan competition applicants in Nigeria; and microenterprises that were part of an intervention that gave wage subsidies, savings help, and business training in Sri Lanka. In addition, the one additional publicly available dataset which we were able to include was data from a program for female small businesses in Uganda from Blattman et al. (2016).⁹

Many of these specialized firm surveys are representative of urban microenterprises of particular size cutoffs or informality status, having been found through door-to-door listing surveys. Some of the samples also screen on the gender of the owner. Most samples do screen on the owners working full-time in the business, and will tend to screen out firms that operate only seasonally or for very limited hours. Several surveys are further restricted to firm owners who applied for some program, such as the business plan competition applicants in Nigeria, the microfinance clients in Egypt, and the Uganda study which was targeted at women in marginalized villages in Northern Uganda.

These specialized firm surveys collect data at relatively high frequencies, with a number collecting data at quarterly or semi-annual frequencies. However, few of them follow firms for more than three and a half years. The exceptions are three surveys from Sri Lanka, which tracked firms at 5 to 6 years, and at 10 to 11 years in the case of the Sri Lanka Microenterprise Survey (SLMS). Appendix Table 3.E.1 provides an overview of the follow-up periods provided by each survey.

⁷This relies on the assumption that the control group death rates were not affected by the treatments. The different experiments try to minimize the possibility of this interference through methods such as not sampling firms which are too close to one another. McKenzie and Puerto (2017) formally tests this assumption, finding no impact of their treatment on the survival rates of other firms operating in the same markets.

⁸We excluded data from surveys with high attrition rates for which death rates could not be accurately calculated, including data from a grants experiment in Mexico and a formalization experiment in Brazil.

⁹The Uganda study targeted women, rather than firms, as the unit of observation. We take the sample of control group participants who started a firm and use this sub-sample to track firm dynamics.

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

3.3.2 Dealing with attrition

A key threat to our ability to measure firm death rates comes from survey attrition, in which owners of businesses could not be found or refused to be re-interviewed in the follow-up survey. The studies we draw on mitigate this threat in two main ways. The first has been to conduct multiple follow-up attempts and track individuals who move, thereby reducing overall levels of attrition. Second, the specialized firm surveys usually record whether the business is still in operation or not, even if the owner refuses to be re-interviewed or is away from the business when survey attempts are made. Appendix Table 3.F.1 summarizes the attrition rates in our combined dataset by period of measurement. Attrition rates are relatively low for this combined sample, at 4.8 percent for one year, 10 percent for two years, and 9.5 percent over three years, and are around 20 percent in many periods beyond 5 years, with some exceptions of lower attrition rates.

We then form bounds for death rates to account for attrition. A lower bound for firm death is obtained by assuming that all firms that have missing values for firm death status at follow-up are still operating. An upper bound for firm death is obtained by assuming that all these firms died.

3.3.3 Summary statistics on firm characteristics

Table 3.2 provides baseline summary statistics of our sample of firms in order to provide a description of the types of firms for which we are able to examine firm death rates. The sample consists of small enterprises run by individuals with relatively low levels of education (only 6 percent have any tertiary schooling), and who have an average age of 43 years. 56 percent of the owners are female. The firms have been in business for a mean of 9 and median of 6 years, with just over half (52 percent) in retail, 31 percent in services, and 14 percent in manufacturing. Only 29 percent of firms have any paid workers, with a mean of 0.75 workers per firm. Conditional on having paid workers, the median firm has only 1 worker and only 10 percent have 5 or more workers. Mean (median) monthly profits are 129 US-Dollar (63 US-Dollar). The median firm is thus earning approximately 2.65 US-Dollar per day it operates.

3.4 How much firm death is there?

The first question that we wish to use this data to answer is just what the rate of firm death is for small firms in the economy. That is, starting with a sample of existing

3.4 How much firm death is there?

Table 3.2: Summary statistics on firms

	Number of Obs.	Mean	S.D	10th	Median	90th
<i>A. Full Sample</i>						
<i>Owner Characteristics</i>						
Owner is Female	18,761	0.56	0.5	0	1	1
Age of Owner	17,961	43.41	13.36	27	42	62
Owner has tertiary education	15,294	0.06	0.24	0	0	0
<i>Firm Characteristics</i>						
Age of firm (years)	15,027	9.47	9.5	1	6	22
Monthly sales (USD)	17,159	1,053	18,175	2	205	1,664
Monthly profits (USD)	15,424	129	733	-63	63	417
Any paid worker	15,737	0.29	0.45	0	0	1
Number of paid workers	15,645	0.75	5.02	0	0	2
Retail firm	17,457	0.52	0.5	0	1	1
Manufacturing firm	17,449	0.14	0.34	0	0	1
Services firm	17,449	0.31	0.46	0	0	1
<i>B. Retail firms</i>						
<i>Owner Characteristics</i>						
Owner is Female	9,087	0.66	0.47	0	1	1
Age of Owner	8,529	44.48	13.42	28	43	63
Owner has tertiary education	7,256	0.05	0.22	0	0	0
<i>Firm Characteristics</i>						
Age of firm (years)	7,172	9.33	9.38	1	6	22
Monthly sales (USD)	8,796	944.89	3,055.52	28	333	2,035
Monthly profits (USD)	8,332	140.95	630.41	-29	76	418
Any paid worker	7,556	0.24	0.43	0	0	1
Number of paid workers	7,515	0.49	5.62	0	0	1
<i>C. Manufacturing firms</i>						
<i>Owner Characteristics</i>						
Owner is Female	2,374	0.47	0.5	0	0	1
Age of Owner	2,315	42.77	12.59	28	41	60
Owner has tertiary education	1,982	0.05	0.22	0	0	0
<i>Firm Characteristics</i>						
Age of firm (years)	1,961	10.42	9.98	1	7	23
Monthly sales (USD)	2,238	395.07	1,835.79	0	68	750
Monthly profits (USD)	2,016	45.19	683.39	-154	30	254
Any paid worker	1,999	0.34	0.48	0	0	1
Number of paid workers	1,975	1.11	3.94	0	0	3
<i>D. Services firms</i>						
<i>Owner Characteristics</i>						
Owner is Female	5,249	0.5	0.5	0	0	1
Age of Owner	5,073	42.8	13.21	27	41	61
Owner has tertiary education	4,559	0.08	0.27	0	0	0
<i>Firm Characteristics</i>						
Age of firm (years)	4,488	9.29	9.53	1	6	22
Monthly sales (USD)	4,754	701.23	7,990.38	0	169	1,276
Monthly profits (USD)	4,099	167.9	920.17	-58	64	422
Any paid worker	4,627	0.3	0.46	0	0	1
Number of paid workers	4,603	0.79	4.32	0	0	2

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firms today, how many of them will have closed a year from now, or five years from now? The answer to this question is useful for understanding the rate of churn in these small firms, as well as for policymakers and researchers planning interventions to help these firms.

3.4.1 The fitted relationship between death rates and time

The surveys vary in their frequency, so that we cannot measure firm death for the same interval (e.g. one year) for all the different surveys. We therefore begin by calculating firm death rates for each of the 79 survey-time interval combinations in our data, and report these rates in Appendix Table 3.G.1 along with an upper and lower bound that accounts for survey attrition. We then plot this data in Figure 3.1, using a point to indicate the death rate for a survey-period combination, and bands around the point to indicate the bounds on this rate once attrition is allowed for. We fit a quadratic to these points, to give the predicted relationship:

$$\begin{aligned} \text{Firm death rate} = & 0.006 + 0.093 \text{Years Elapsed} - 0.00217(\text{Years Elapsed})^2 & R^2 = 0.779 \\ & [0.029] \quad [0.026] & [0.00148] \end{aligned}$$

where robust standard errors, clustered by survey, are shown in parentheses. This relationship suggests firms die at the rate of approximately 9 percentage points per year over the first three years of following them, with this rate then declining over time, to around 7 percentage points per year at five years and 4.6 percentage points a year at ten years.

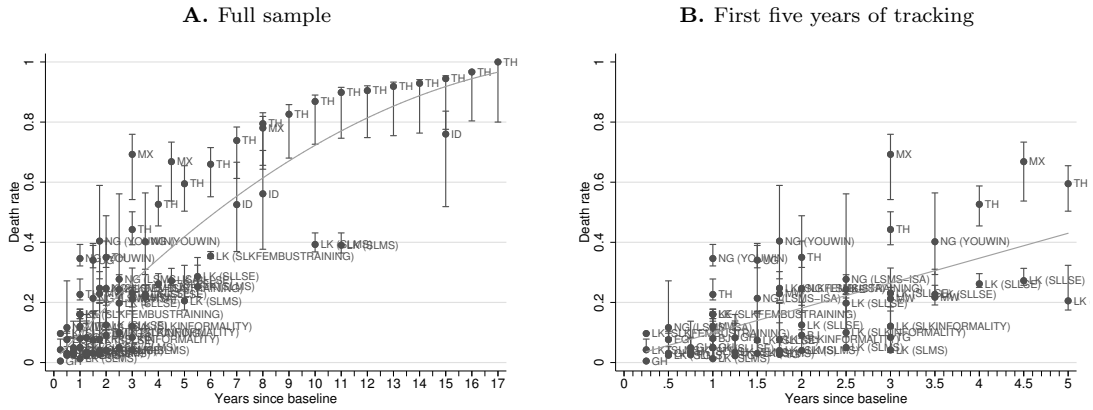
The majority of our data (58 out of 79 survey-time intervals) comes from horizons of up to 5 years. The right panel of Figure 3.1 zooms in on this sub-sample to better visualize the data within this interval. We cannot reject linearity of the relationship between death rates and time over this range (p-value=0.437 on the quadratic term), so we fit the following linear relationship for periods of up to five years:

$$\begin{aligned} \text{Firm death rate} = & 0.016 + 0.083 \text{Years Elapsed} & R^2 = 0.385 \\ & [0.028] \quad [0.021] \end{aligned}$$

That is, firms die at an average of 8.3 percentage points per year over the first five years. Fitting the same slope through the lower bounds of the intervals gives a slope of 6.9 percentage points per year, while fitting it through the upper bounds of the intervals gives a slope of 8.9 percentage points per year.

3.4 How much firm death is there?

Figure 3.1: Firm death rates over different time horizons



Note: Each data point is the firm death rate measured over a survey-time horizon combination. Worst-case bounds that assume all attriting firms are either open or closed are indicated by vertical lines. Left panel shows full sample with a fitted quadratic. Right panel zooms in on death over the first five years of tracking, with a fitted linear line. Letters indicate the surveys, described in detail in the Appendix.

Using these fitted relationships, we can then estimate the half-life of firms that are observed to be in existence at a given point in time. From the quadratic fit, 50 percent of firms are predicted to die within 6.2 years, while from the linear fit, 50 percent are predicted to die within 5.8 years.

In Appendix Table 3.B.1 we collate together estimates of firm death in developing countries from other literature. Appendix Figure 3.B.1 then plots these alongside with our data and fitted quadratic. The firm death rates from other studies are all relatively close to the fitted quadratic, showing that results are generally consistent across a number of contexts.

It may also be of interest to some readers to benchmark these death rates against those in the United States. The main source of firm dynamics data in the U.S. comes from data from the Bureau of Labor Statistics and U.S. Census Bureau for firms with at least one paid employee. Among firms with 1 to 4 paid workers, the one year death rate over 1995 to 2016 averaged 19.7 percent. It was highest for younger firms, averaging 28.4 percent for firms aged 1 to 4 years, 20.8 percent for firms aged 5 to 9 years, and 16.6 percent for firms aged 10 or more years. Death rates were lower for firms with more workers, averaging 4.6 percent for firms with 5 to 9 workers and 4.0 percent for firms with 10 to 19 workers.¹⁰ U.S. data also shows high exit rates from self-employment,

¹⁰Source: Bureau of Labor Statistics: <https://www.bls.gov/bdm/business-employment-dynamics-data-by-age-and-size.htm>. See also Figure 3 in Haltiwanger et al. (2013) for additional U.S. evidence.

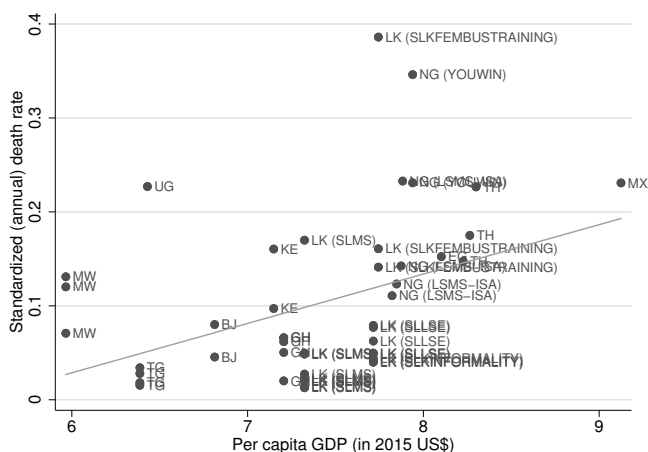
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with 28 percent of the incorporated self-employed exiting over a year (Schweitzer & Shane, 2016). This evidence suggests that firm death rates for small firms are higher in the U.S. than the averages seen in our sample.

3.4.2 Do death rates vary with level of development?

On one hand, we might expect businesses in less developed countries to face more constraints such as less access to credit, and so be less likely to survive negative shocks. However, there may be fewer other options for employment in poorer economies, suggesting fewer voluntary exits. Figure 3.2 plots the observed correlation between annualized firm death rates and per-capita GDP. We see a positive relationship (correlation of 0.41), with firm death rates higher in richer developing countries. Each log point increase in per capita GDP is associated with a 5.3 percentage point higher annualized firm death rate (p-value=0.056). This is consistent with the evidence above that small firm death rates are higher in the U.S. than in our sample.

Figure 3.2: Death rates by per capita GDP



3.5 Which firms are more likely to die?

of firms are more likely to die.

Inspired by Haltiwanger et al. (2013)'s analysis of U.S. firm growth and exit by firm size and firm age, we follow their approach in using a saturated dummy variable regression approach to describe patterns in the data, and show these results graphically. Our main specifications control for time horizon and time horizon squared, following the pattern in Figure 3.1, and include dummies for each of the 16 different surveys. We cluster standard errors at the firm level when generating confidence intervals, given that we have multiple observations per firm.

3.5.1 Are younger firms more likely to die?

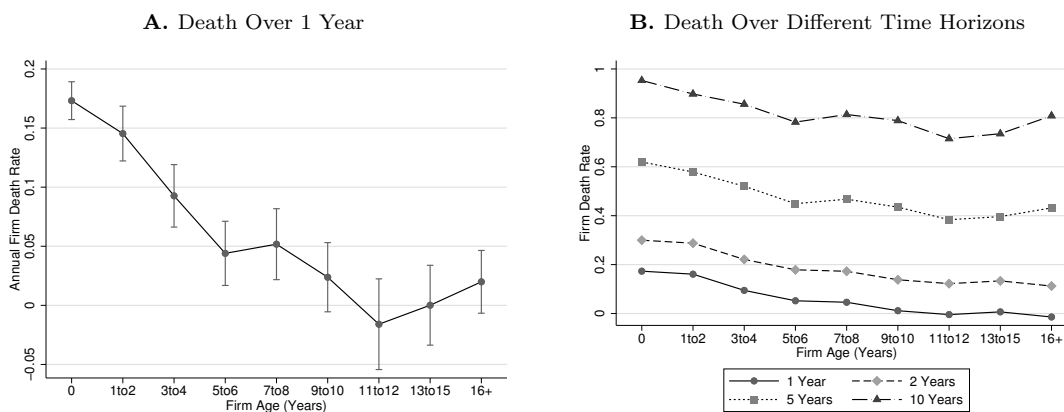
Perhaps the most common stylized fact about firm death is that younger firms are more likely to die than older firms. Theoretically this can arise from new entrepreneurs learning that they are inefficient and exiting as in Jovanovic (1982), from starting with a low level of investment and not being able to keep up with the advances of competitors as in Ericson and Pakes (1995), or from taking on high levels of risk and failing early as in Cressy (2006). Empirically younger firms have been found to be more likely to die in the United States (e.g. Bernard & Jensen, 2002; Dunne et al., 1989; Evans, 1987; Haltiwanger et al., 2013) and in developing countries (e.g. Vijverberg & Haughton, 2004; Frazer, 2005; Shiferaw, 2009) although Söderbom et al. (2006) found no significant relationship between firm age and death rates, and Davies and Kerr (2018) find that firms in the middle age category (5-14 years old) are less likely to die than younger firms (less than 5 years old), while older firms (older than 14 years) do not have higher likelihoods of dying than younger firms.

We regress firm death on a set of dummy variables for firm age, along with the time horizon and survey controls. We plot the fitted firm age coefficients in Panel A of Figure 3.3. As in Haltiwanger et al. (2013), to facilitate interpretation, we scale the data so that firms in the baseline group (here firms of age less than 1 year), are shown at their unconditional mean, and rescale the other effects by adding the difference in coefficients compared to this baseline group. We see a strong negative relationship, with older firms considerably less likely to die in the next year than younger firms. A firm in its first year is estimated to have a 17 percent chance of dying, compared to 14 percent for firms aged 1 to 2, and only 4 percent for firms aged 5 to 6.

Of course, this relationship between firm death and firm age is likely to vary with the time horizon considered, and should flatten over longer time horizons given the upper bound of 100 percent death. We therefore interact each firm age dummy with the time horizon and time horizon squared, and in Panel B of Figure 3.3, show how

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Figure 3.3: Relationship between firm death rate and firm age



Note: Fitted coefficients from regression of firm death on firm age category dummies, survey, time horizon, and time horizon squared. Panel B includes interactions between age dummies and a quadratic in time horizon. 95 percent confidence intervals shown in Panel A.

the firm death and firm age relationship varies over different time periods. We see that the negative relationship does not flatten very much over the time horizons of most firm panel surveys. A new firm is then predicted to have a 62 percent chance of dying within 5 years, and 95 percent chance of dying within 10 years.

3.5.2 Are smaller firms more likely to die?

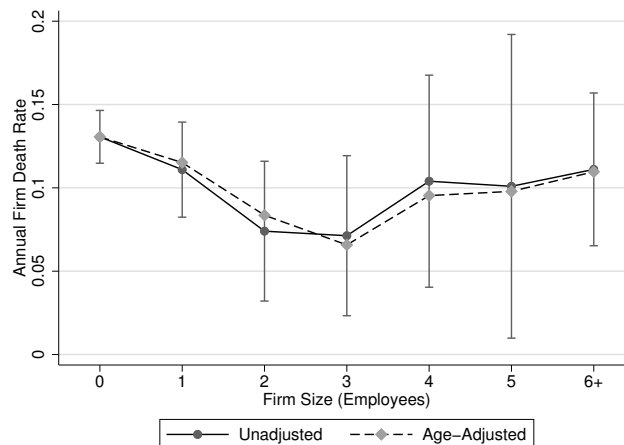
Firm size is closely linked to firm age in many theoretical models, with firms starting small and either growing or dying. Lower death rates for larger firms do appear to be the case in studies which compare medium or large-sized firms to small firms. For example, Davies and Kerr (2018) find Ghanaian firms with 75 or more workers are 5 percentage points more likely to survive over ten years than firms with 0 to 9 workers. Likewise Söderbom et al. (2006) estimate that a firm with 50 workers is 7 percentage points more likely to survive than a firm with 10 workers in their data from Ghana, Kenya and Tanzania, and Shiferaw (2009) finds Ethiopian firms with more than 100 employees are more likely to survive than those with 10 to 30 workers. The relationship has been less strong when examining smaller firms and household enterprises. Vijverberg and Haughton (2004) find larger firms less likely to die in Vietnam, whereas McPherson (1995) finds no significant relationship with firm size in Swaziland and Botswana, and even finds larger firms being more likely to die in Zimbabwe.

Figure 3.4 examines the relationship between firm death and firm size, measured in terms of number of employees. Eighty percent of firms in our sample have no paid

3.5 Which firms are more likely to die?

workers, nine percent have one worker, 3.6 percent have two workers, and only 3.6 percent have 6 or more workers. Although the firm death rates are lower for firms with one or two workers than those with 0 workers, the death rate then rises again with firm size, and the overall relationship is fairly flat. This is accompanied by quite wide confidence intervals, showing that we cannot measure firm death very precisely for firms with more workers. Haltiwanger et al. (2013) find that, in the full sample of U.S. firms, the firm death and firm size relationship disappears once one controls for firm age. In our setting, the correlation of firm age and number of employees is only 0.02. Not surprisingly then, adjusting for firm age by fully interacting firm size with our firm age dummies, and then holding constant the age distribution, leads to very little change in the fitted relationship in Figure 3.4.

Figure 3.4: Relationship between firm death rate and firm size (employees)

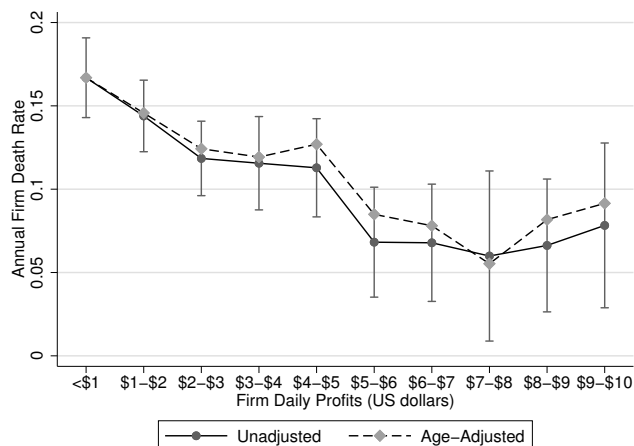


Note: Fitted coefficients from regression of firm death on firm size category dummies, survey, time horizon, and time horizon squared. 95 percent confidence intervals shown for this regression. Age-adjusted line comes from fully interacting firm size dummies with firm age group dummies, and then holding the age distribution constant at the sample mean.

In contrast, there is a much stronger relationship between firm death and firm profitability, with Figure 3.5 showing more profitable firms within a country are more likely to survive than less profitable firms: a firm earning less than 1 US-Dollar per day in profits has a 16.7 percent chance of dying in the next year, compared to a 6 to 8 percent chance for firms earning 5 US-Dollar or more per day in profits. This is not driven by younger firms earning less profits, as holding the age distribution constant leads to very little change in this fitted relationship.

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Figure 3.5: Relationship between firm death rate and firm profitability



Note: Fitted coefficients from regression of firm death on firm profit category dummies, survey, time horizon, and time horizon squared. 95 percent confidence intervals shown for this regression. Age-adjusted line comes from fully interacting firm profit category dummies with firm age group dummies, and then holding the age distribution constant at the sample mean.

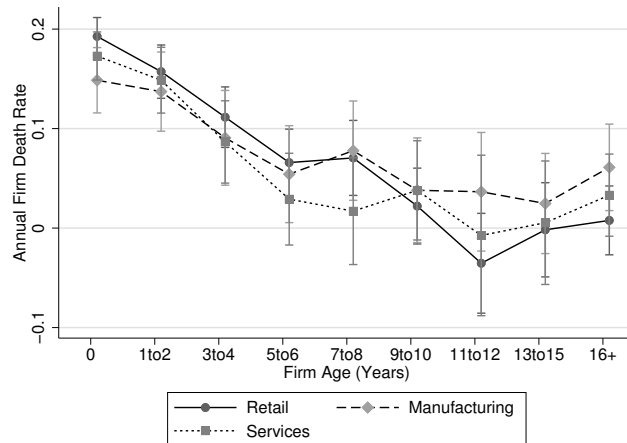
3.5.3 Are firms in certain sectors more likely to die?

While many studies have just used data from manufacturing firms, studies which have included other sectors have found death rates to vary with sector. However, they come to different conclusions about which sectors have higher death rates. McPherson (1995) and Mead and Liedholm (1998) find that firms in the retail sector are more likely to close than firms in the services or manufacturing sector; Klapper and Richmond (2011) find manufacturing firms to face a higher risk of closing compared to firms in the services sector and Vijverberg and Haughton (2004) find manufacturing and services firms to be significantly more likely to close than those in the retail sector. Figure 3.6 shows that in our data, retail firms have the highest death rates, and manufacturing firms the lowest, with services in the middle.¹¹ However, the confidence intervals all overlap, and we cannot reject that the firm death versus firm age relationship is constant across the three sectors (p-value=0.180).

¹¹Three percent of our firms are classified as “other sector”. These include firms in forestry, mining and quarrying, electricity, gas, steam and air conditioning supply, water supply, sewerage, water management, and other unspecified business areas. We exclude these firms from Figure 3.6.

3.5 Which firms are more likely to die?

Figure 3.6: Relationship between firm death rate and firm age by business sector



Note: Fitted coefficients from regression of firm death on firm age category dummies interacted with each sector, survey, time horizon, and time horizon squared. 95 percent confidence intervals shown.

3.5.4 How does firm death vary with characteristics of the firm owner?

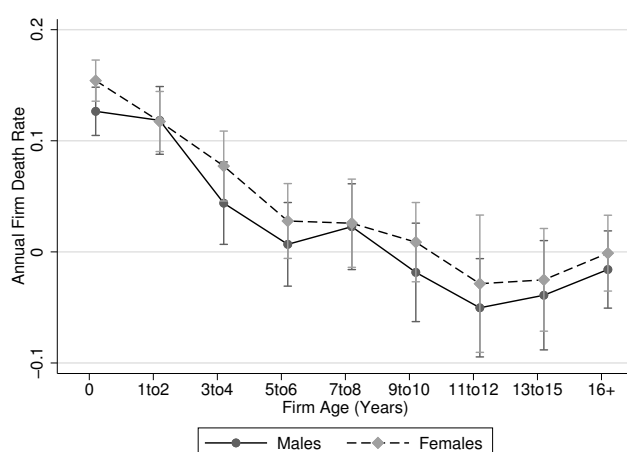
The association between firm death rates and owner characteristics have been less studied in much of the literature, since many surveys of firms do not provide personal characteristics of the owner. Our surveys allow for examination of these relationships.

Are male or female owners more likely to have their firms die? Female-owned firms are, on average, smaller and less profitable than male-owned firms in developing countries. Both these factors would suggest that their firms would be more susceptible to failure. However, the existing literature has found mixed associations between gender and firm death. McPherson (1995) finds female-owned firms are more likely to close in two of the four countries he studies, but finds no significant relationship in the other two. In contrast, Shiferaw (2009) and Vijverberg and Haughton (2004) find female-owned firms to be less likely to die than male-owned firms in Ethiopia and Vietnam respectively. Figure 3.7 shows that the firm death versus age relationship looks similar by gender, but with female-owned firms having slightly higher death rates than male-owned firms at each firm age. Female-owned firms are 1.8 percentage points more likely to die at each firm age (p -value=0.094).

Are firm death rates higher for younger or older entrepreneurs? Younger owners have had less time to accumulate skills and capital, and are at an age where job mobility

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

Figure 3.7: Relationship between firm death rate and firm age by gender of the business owner



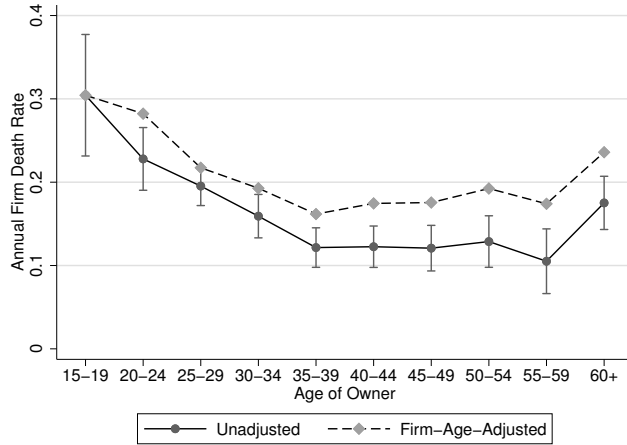
Note: Fitted coefficients from regression of firm death on firm age category dummies interacted with each gender, survey, time horizon, and time horizon squared. 95 percent confidence intervals shown.

is also higher even in wage work. But older entrepreneurs may close as they near retirement, or see less time to recover from shocks. The result might be that survival probabilities are highest for the middle aged. Fajnzylber et al. (2006) find this in the context of the likelihood of staying self-employed (not necessarily in the same business) over a year in Mexico, where the maximum is found for ages 36 to 50. Likewise, Vijverberg and Haughton (2004) find firm death rates to be lower for 26 to 45 year olds than for older or younger firm owners. Figure 3.8 confirms this U-shaped pattern. Firm death rates are highest for youth, with an annual death rate of 30.4 percent for 15-19 year-olds and 22.8 percent for 20-24 year-olds. Death rates then fall until age 35, are stable between 35 and 59, and then rise again from age 60. Since younger individuals operate younger firms (the correlation is 0.30), part of this pattern is due to younger firms being more likely to die. But Figure 3.8 shows the U-shaped pattern continues to apply even when we hold the firm age distribution constant.

Are firms run by more educated owners less likely to die? To the extent that higher education also reflects higher ability, we might expect firms run by more educated owners to be more likely to survive. However, the existing literature has either found no relation between firm death and education (Vijverberg & Haughton, 2004; Krafft, 2016, in one time period), or that firms with more educated owners are actually more likely to die (Fajnzylber et al., 2006, for self-employment; Krafft, 2016, in a second

3.5 Which firms are more likely to die?

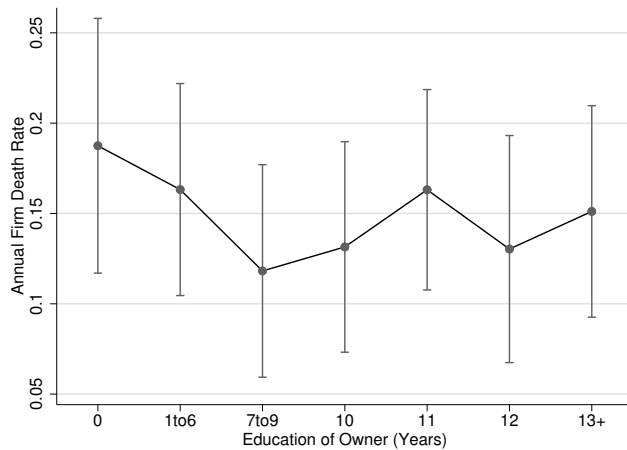
Figure 3.8: Relationship between firm death rate and age of business owner



Note: Fitted coefficients from regression of firm death on owner age category dummies, survey, time horizon, and time horizon squared. 95 percent confidence intervals shown for this regression. Age-adjusted line comes from fully interacting owner age category dummies with firm age group dummies, and then holding the firm age distribution constant at the sample mean.

time period). Figure 3.9 shows that firm death rates are highest for firm owners with no education, and then fluctuate with no simple pattern among firm owners with some education.

Figure 3.9: Relationship between firm death and education of firm owner



Note: Fitted coefficients from regression of firm death on owner education category dummies, survey, time horizon, and time horizon squared. 95 percent confidence intervals shown.

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

3.5.5 Multivariate analysis of the correlates of firm death

The analysis above provides detailed non-parametric examination of the associations between firm death and different firm and owner characteristics. In Table 3.3, we include all the variables together to examine how they jointly predict firm death. Column 1 provides coefficients from a least squares regression of firm death.¹² Columns 2 and 3 examines how sensitive these associations are to attrition by assuming that all attriting firms died (column 2) or remained open (column 3). Column 4 examines robustness to an alternative outcome (to be discussed in Section 3.6.3), of whether the original entrepreneur is not the owner of any firm, capturing exit from self-employment, compared to death of the original firm.

Table 3.3 shows that the associations between firm death and firm and owner characteristics are robust to attrition, and to the precise measure of firm death. Firm death rates are statistically significantly lower as the age of the firm increases (compared to the base group of firms age less than 1 year) as in Figure 3.3, increase with the time horizon at a diminishing rate (as in Figure 3.1), fall as monthly profits increase (as in Figure 3.5), and exhibit a U-shaped pattern with owner age (as in Figure 3.8). Although there is a significant negative association with the number of employees, the magnitude is extremely small (1 more employee is associated with a 0.4 percentage point reduction in the death rate), and we do not see significant differences by sector. The association with owner's education is significant at the 10 percent level in column 1, but also small in magnitude, and the higher death rate of female-owned firms is not significant once we have controlled for these other firm and owner characteristics.

3.6 Why do firms die and what happens after death?

The previous section shows that firm death does not simply occur at random, but is more likely to occur for certain types of firms and firm owners than others. Firm death may arise for a variety of different reasons. A first set of reasons come from firm-level shocks, such as the entry of new competitors, a reduction in product demand, or increases in input prices that reduce the profitability of the firm and cause it to make a loss. A second set of shocks may come from the household side. With imperfect labor markets or other missing markets, illness of the business owner or of household members that the owner has to look after may cause the owner to shut down the business when

¹²Similar results can be obtained using a probit. We use the linear functional form to maintain comparison with the non-parametric models estimated previously, and to aid interpretation of non-linear terms.

3.6 Why do firms die and what happens after death?

Table 3.3: Multivariate correlates of firm death

	(1) Original firm dies	(2) Assume attrit = dead	(3) Assume attrit = live	(4) Does not operate any firm
Firm aged 1 to 2	-0.0218** (0.00994)	0.00577 (0.0102)	-0.0293*** (0.0110)	-0.0116 (0.00942)
Firm aged 3 to 4	-0.0739*** (0.0122)	-0.0396*** (0.0121)	-0.0740*** (0.0130)	-0.0550*** (0.0119)
Firm aged 5 to 6	-0.116*** (0.0127)	-0.0756*** (0.0128)	-0.115*** (0.0130)	-0.0980*** (0.0126)
Firm aged 7 to 8	-0.107*** (0.0142)	-0.0710*** (0.0143)	-0.0973*** (0.0148)	-0.0818*** (0.0138)
Firm aged 9 to 10	-0.133*** (0.0143)	-0.0975*** (0.0140)	-0.120*** (0.0149)	-0.112*** (0.0129)
Firm aged 11 to 12	-0.165*** (0.0198)	-0.130*** (0.0198)	-0.160*** (0.0206)	-0.129*** (0.0186)
Firm aged 13 to 15	-0.159*** (0.0184)	-0.121*** (0.0187)	-0.148*** (0.0177)	-0.126*** (0.0173)
Firm aged 16+	-0.140*** (0.0137)	-0.104*** (0.0134)	-0.130*** (0.0140)	-0.105*** (0.0132)
Time horizon	0.116*** (0.00284)	0.116*** (0.00258)	0.103*** (0.00309)	0.111*** (0.00301)
Time horizon squared	-0.00366*** (0.000170)	-0.00399*** (0.000146)	-0.00294*** (0.000178)	-0.00342*** (0.000174)
Number of Employees	-0.00453* (0.00241)	-0.00217 (0.00215)	-0.00881*** (0.00270)	-0.00521** (0.00253)
Log Monthly Profits	-0.0272*** (0.00316)	-0.0255*** (0.00310)	-0.0267*** (0.00343)	-0.0255*** (0.00305)
Firm is in retail	0.0137 (0.0111)	0.0144 (0.0108)	0.00188 (0.0116)	0.00443 (0.0107)
Firm is in manufacturing	-0.00378 (0.0130)	-0.0147 (0.0131)	1.31e-05 (0.0129)	-0.00650 (0.0130)
Owner's age	-0.00757*** (0.00185)	-0.00975*** (0.00179)	-0.00519*** (0.00185)	-0.00754*** (0.00176)
Owner's age squared/1000	0.0800*** (0.0203)	0.0926*** (0.0193)	0.0640*** (0.0204)	0.0844*** (0.0195)
Owner is female	0.0134 (0.0110)	0.00416 (0.0107)	0.0182 (0.0114)	0.0127 (0.0107)
Owner's years of education	-0.00258* (0.00149)	-0.00226 (0.00140)	-0.00176 (0.00158)	-0.00213 (0.00145)
Observations	56,182	63,137	63,137	57,892

Note: Estimates from linear regressions.

All regressions include survey fixed effects.

Robust standard errors, clustered at the firm level, in parentheses.

*, **, and *** denote significance at the 10, 5, and 1 percent levels respectively.

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

they cannot hire someone else to run the firm. Firm death can also arise from positive shocks to the entrepreneur's outside options, such as an attractive wage job offer, or the entrepreneur coming up with an idea for a different firm. In micro and small firms, the firm is tightly linked to the occupation of the owner, and it is rare for the firm to continue when the owner leaves.¹³

These different reasons for firm death can be difficult to disentangle in the absence of random variation in these different types of shocks. Nevertheless, in contrast to firm censuses, our surveys provide useful suggestive data on why firms shut down, and also enable us to examine what happens afterwards to the firm owner.

3.6.1 Examining reasons for firm death using self-reported cause of death data

Nine out of sixteen surveys directly asked firm owners whose businesses have closed to report the main cause of firm death. The first two columns of Table 3.4 report the results. The most common cause of firm death according to the owners is that the firm is making a loss, which 41 percent of owners say. Household reasons such as sickness or caring for family are the second most common, accounting for 26 percent of closures, while only 11 percent say they are closing for better wage or alternative business opportunities.¹⁴ Appendix Tables 3.H.1 and 3.H.2 show that the cause of death varies by gender, with women more likely to close their business for sickness or family reasons (34 percent) compared to men (12 percent), while male owners are more likely to close because the firm is making a loss or to pursue better opportunities.

¹³For instance, in the SLMS, only 1.5 percent of owners who stopped operating their business from round to round had sold it, while only 2.9 percent said the firm was being operated by another family member. In the Lomé Informal Enterprise Survey, 1.2 percent of owners who were not running their business anymore had sold it, and 1.8 percent had passed it to a family member. In the Ghana Microenterprises Survey, 7.5 percent of the businesses that were not being operated anymore by the original owner were being run by another family member, while no business had been sold.

¹⁴22 percent give "other" as cause of death. This includes some firms in Sri Lanka, where the data for what "other" refers to was never entered, as well as reasons like the government shutting down the firm, disasters causing assets to be destroyed, and international migration.

Table 3.4: Cause of firm death and main activity of owner after firm death

Reason for closure	Percent	Frequency	Working for a wage	Looking for a job	Operating a different business	Housework/ Looking after children	Other	Total
Making a loss	40.55	590	30.92	7.96	18.26	17.54	25.32	553
Sickness	13.54	197	8.13	2.5	10.63	46.25	32.5	160
Care for family	12.37	180	5.29	1.76	4.12	71.18	17.65	170
Better wage job	6.53	95	95.56	1.11	2.22	1.11	0	90
Better business opportunity	4.88	71	10.77	1.54	75.38	6.15	6.15	65
Other	22.13	322	19.51	6.27	18.12	33.1	23	287
Total	100	1,455	342	71	228	392	292	1,325

Note: Data for columns 1-2 from BENINFORM, KENYAGETAHEAD, MALAWIFORM, NGYOUWIN, SLKFEMBUSTRAINING, SLKINFORMALITY, SLLSE, SLMS, and TOGOINF. The category “Other” includes 3.09 percent of businesses, which had to close because of a catastrophe (e.g. fire or burglary), 1.51 percent of business which were shut down by government, a remaining 1.72 percent of owners who discontinued their businesses because either the business was taken from them, they married, or migrated. The remaining 15.81 percent of owners who closed their businesses either did not specify this other reason more in detail or closed because of any other miscellaneous reason, which could not be coded into one of the above categories (for instance because of a lack of workers or inputs, or a machine broke down). Data for columns 3-8 from BENINFORM, KENYAGETAHEAD, MALAWIFORM, SLKFEMBUSTRAINING, SLLSE, SLMS, and TOGOINF.

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3.6.2 What happens to the owner after a firm dies?

One might question whether the data on self-administered firm autopsies reported in Table 3.4 give reliable information on cause of death. One check is to see whether differences in cause of death are accompanied by differences in what the owner is doing after the firm dies. The remaining columns of Table 3.4 show that the reason an owner gives for his or her firm dying is linked to what the owner does afterwards. Those whose firms died because they sought better opportunities are most likely to be in wage work or operating new firms, those whose firms died for family reasons or sickness are most likely to be doing housework and family care, while those whose firms died because the firm was making a loss are spread across a range of activities, including having the highest rate of still looking for a job of any of the causes of death.

We can then examine what happens to the labor earnings of the business owner (the sum of all wage and profit income) when the firm dies. Table 3.5 shows how firm death is associated with changes in labor earnings. We estimated linear regressions of these changes on either a dummy variable for a firm being dead in a given follow-up period (results are presented in panel A) or a variable with categories for survival and three different times of death (results in panel B). All regressions also include survey fixed effects. Using the change in labor income of owners of surviving firms as a counterfactual income trend for the owners who closed their businesses, we see in column 1 of Panel A in Table 3.5 that firm death is associated with a significant reduction in labor earnings. Columns 2 to 4 present results of the same regression specifications on the sample of surviving firm owners and owners who reported either making a loss, or illness and family care, or better opportunities as reasons for closure. We see that firms closing because they were making a loss or because of illness and family care have even larger drops in labor income, whereas owners who closed to pursue better opportunities experience increases in labor income on average. It is possible that owners who close their businesses experience large income drops in the short run, but are able to recover to similar or even higher income levels after some time. In Panel B we consider whether changes in labor income differ depending on the time that has passed since the firm died. For the full sample, we see in column 1 that income drops are the largest if the firm has died in the last year. These drops are reduced to about 68 percent one to two years after firm death. Two to three years after firm death, we observe no significant changes in labor income.

3.6 Why do firms die and what happens after death?

Table 3.5: Changes in labor earnings associated with firm death

	(1) Full Sample	(2) Making a Loss	(3) Illness and Family Care	(4) Better Op- portunities
<i>A. Associations with Firm Death</i>				
Firm Death	-56.3634*** (7.7036)	-97.7698*** (16.1643)	-96.6587*** (16.2955)	65.1027** (31.6603)
Observations	14,653	12,675	12,486	12,303
<i>B. Associations with Time of Death</i>				
Firm Death: (Omitted Category: Firm Survived)				
Last Year	-78.0359*** (8.9252)	-106.6236*** (20.0821)	-100.6150*** (18.3276)	18.9718 (43.9085)
1-2 years ago	-52.7683*** (19.0871)	-122.8290*** (36.9238)	-209.7334*** (72.1518)	99.3019 (106.9451)
2-3 years ago	-20.4288 (23.1555)	-134.4353** (53.8105)	-216.7693*** (43.7517)	191.5198* (104.7777)
Observations	13,870	12,526	12,383	12,259

Note: Results come from linear regressions on the pooled sample of follow-up waves.

The outcome variable is the difference between labor earnings (the sum of wage earnings plus profits) in the month preceding a given survey wave and business profits at baseline.

All regressions include survey fixed effects.

Robust standard errors, clustered at the firm level, in parentheses.

*, **, and *** denote significance at the 10, 5, and 1 percent levels respectively.

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3.6.3 Zombies and phoenixes: Firm death need not mean permanent exit from self-employment

After closing a firm between survey round t and $t+1$, we sometimes observe the business owner operating a firm again in a later round. We can distinguish two types of such firms, using survey questions on the name of the firm, whether it is in the same sector, and whether there is a change in the line of business. The first are “zombies”, in which the owner re-opens the exact same firm they had closed. Such occurrences are relatively rare, with only 6.2 percent of the firms in our sample ever observed to do this. In contrast, it is more common for a firm to close down, and then for the owner to operate a different firm, which we call a “phoenix” arising from the ashes of the old firm. Of all owners that we observe closing a firm within a year, 20 percent operate a new firm again within the same year. Appendix Figure 3.D.2 looks at re-opening rates for the five surveys for which we have the largest numbers of closed firms. We see re-opening rates increase with time, with approximately 40 percent of owners of closed firms opening a different firm within 3 years, although this rate is lower in the Townsend Thai data than in the specialized firm surveys.

Figure 3.10: Sensitivity of age and time horizon relationships to considering exit from self-employment instead of firm death



Note: Panel A shows fitted coefficients from regression of firm death or no longer being self-employed on firm age category dummies, survey, time horizon, and time horizon squared. Panel B shows fitted coefficients for same outcomes regressed on dummy variables for different time horizons, with survey controls included. 95 percent confidence intervals shown.

Figure 3.10 examines how much difference re-opening firms makes to our calculations of the association between firm death and firm age (Panel A), and the cumulative rate at which firms die over time (Panel B). We still see that firm death rates are high-

est at younger ages, although not quite as high when using exit from self-employment compared to death of the original firm. When it comes to tracking a group of firm owners over time, we see that the death rates are quite similar using either measure over shorter periods, and 50 percent of owners still exit self-employment within 7 years (compared to 6 years using closing the original firm). Along with column 4 of Table 3.3, which shows the association of firm death with other firm and owner characteristics is similar across the two measures, the tendency of some firm owners to re-open their firm or to open a new firm does not greatly change any of the conclusions in this paper. One key reason for this is that many of these new firms will themselves fail quickly.

3.7 Conclusion

Firm death is extremely common among small firms, with half of the current stock of small firms in a developing country likely to die within the next six years. Yet because panel surveys of such firms are still relatively rare, often do not track firms for long periods, and often lose firms which die to attrition, systematic data on the rates, correlates, and causes of firm death have not been available. This paper overcomes these issues by pooling together data from 16 panel surveys in 12 countries, in which special efforts were made to limit attrition and ascertain the status of firms which closed down. Using this, we are able to provide stylized facts on the rate of firm death over time, provide descriptive evidence on which types of firms are most likely to die, and provide suggestive evidence on why firms die. Our evidence suggests that the most common reason for firm death is that less profitable and less productive firms end up making losses and closing. However, other small firms, particularly those run by women, close because of illness and family reasons, suggesting non-separability between the household and firm, while a minority of firms, close because better opportunities arise for the owner.

We see several potential uses for this research. The first is as a benchmarking tool. There is currently little evidence for researchers or policymakers tracking the failure rates of firms in their study or intervention to compare to, to know whether the rates they see in their data are high or low. This paper provides guidance as to what death rate can be expected at different time horizons and for firms of different ages. The second potential use is for targeting of programs. Programs that want to work actively with businesses for several years may wish to avoid small firms at greatest risk of closing down, while other programs that are designed to help firms at risk of failure to survive may wish to use the information provided here to decide

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which firms to offer the program to. Finally, the results may be of interest in deciding whether interventions are needed, and what types of intervention. To the extent that firms are exiting because they are low productivity and competition is reallocating production to more efficient firms, policymakers may not want to intervene. Similarly, if firm death arises because of voluntary exit in pursuit of better opportunities, again no intervention may be warranted. In contrast, if firms die because of a lack of separability with household shocks, or because of firm shocks that were not able to be insured, policymakers may want to experiment with policies to insure these types of shocks. Our results suggest that there is a subset of firms, particularly those run by women, for whom such policies may be of particular importance.

3.A Discussion of firm death in published RCTs of small firm interventions

Appendix 3.A Discussion of firm death in published RCTs of small firm interventions

Table 3.A.1: Discussion of firm death in published RCTs of small firm interventions

Author(s)	Type of Intervention	Firm Size	Country	Sample Size	Time Frame (approximate)	Attrition Rate	Firm Death/Survival is an Outcome	Death Rate in Control Group	Note
Angelucci et al. (2015)	microcredit	micro	Mexico	Total: 2,912	20 months	37%	yes	n.a.	a.
Atkin et al. (2017)	export promotion	micro	Egypt	Total: 219 Control: 145	2.5 years (S1) 15 months (S2)	28% (S1) 7% (S2)	no		b.
Beaman et al. (2014)	other	micro	Kenya	Total: 508	n.a. / n.r.	2-3%	no		c.
Berge et al. (2015)	business training and cash grant	micro	Tanzania	Total: 644 Control: 199	3 years	2.6%	no		
Blattman and Dercon (forthcoming)	business training and cash grant	micro	Ethiopia	Total: 643 Control: 358	1 year	14%	no		
Blattman et al. (2014)	cash grants	micro	Uganda	Total: 2,677	4 years	18%	no		
Bruhn et al. (2018)	management consulting	micro, small and medium	Mexico	Total: 432 Control: 282	1 year	12%	no	3.3%	e.
Bruhn and Zia (2013)	business training	micro	Bosnia and Herzegovina	Total: 445 Control: 148	1 year	11%	yes	36%	
de Andrade et al. (2016)	formalization	micro	Brazil	Total: 1,348 Control: 689	1 year	36.8%	yes	14-16%	f.
De Giorgi and Rahman (2013)	formalization	small and medium	Bangladesh	Total: 3,000 Control: 1,500	2 years	n.r.	no		
de Mel et al. (2014)	business training and cash grant	micro	Sri Lanka	Total: 624 Control: 224	2.5 years	7.9%	yes	17%	g.
de Mel et al. (2013)	formalization	micro	Sri Lanka	Total: 520 Control: 105	31 months	11%	yes	n.r.	
de Mel et al. (2012)	cash and in-kind grants	micro and small	Sri Lanka	Total: 387	5.25 years 5.75 years	10% (5.25 years) 8% (5.75 years)	yes	29% (males) 26% (females)	h.
de Mel et al. (2009)	cash and in-kind grants	micro	Sri Lanka	Total: 387	3 years	13.8%	no		
de Mel et al. (2008)	cash and in-kind grants	micro	Sri Lanka	Total: 408 Control: 280	2 years	9.6%	no		

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3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

Table 3.A.1 – continued from previous page

Author(s)	Type of Intervention	Firm Size	Country	Sample Size	Time Frame (approximate)	Attrition Rate	Firm Death/Survival is an Outcome	Death Rate in Control Group	Note
Drexler et al. (2014)	business training formal savings accounts other	micro	Dominican Republic Kenya	Total: 1,193 Control: 387 Total: 392 Control: 197	1.5 years 6-18 months	13% 36%	no no		
Fafchamps and Quinn (forthcoming)		small	Ethiopia Tanzania Zambia	Total: 126 (E), 134 (T), 85 (Z) Control: 40 (E), 44 (T), 22 (Z)	1 year	n.r.	no		i.
Fafchamps and Woodruff (2016)	business training cash and in-kind grants microcredit	small	Ghana	Total: 140 Control: 70	2 years	0%	yes	n.r.	j.
Fafchamps et al. (2014)		micro	Ghana	Total: 793 Control: 396	3 years	14%	no		
Field et al. (2013)		micro	India	Total: 845 Control: 425	3 years	9%	yes	38.6%	
Field et al. (2010)	business training	micro	India	Total: 636 Control: 212	4 months	5.3%	no		
Glaub et al. (2014)	business training insurance	micro and small	Uganda	Total: 100 Control: 53	1 year	0%	yes	9.4%	k.
Groh and McKenzie (2016)	insurance against macroeconomic uncertainty	micro	Egypt	Total: 2,961 Control: 1,481	20 months	1.2%	no		
Higuchi et al. (2015)	business training management consulting services and cash grants	small	Vietnam	Total: 312 Control: 115	2.5 years	12.2%	no		
Karlan et al. (2015)	business training management consulting services and cash grants	micro	Ghana	Total: 160 Control: 45	2 years	6.9%	no		l.
Karlan and Valdivia (2011)	business training microcredit	micro	Peru	Total: 4,591 Control: 1,601	1-2 years	24%	no		
Karlan and Zinman (2011)	business training microcredit	micro	Philippines	Total: 1,601 Control: 329	11-22 months	30%	no		
Mano et al. (2012)	business training business training and in-kind transfer	micro and small	Ghana	Total: 167 Control: 107	3.5 years	10.8%	yes	9.3%	k.
Martínez et al. (forthcoming)	business training and in-kind transfer	micro	Chile	Total: 1,948 Control: 566	45 months	23%	yes	75.1%	m.

Continued on next page

3.A Discussion of firm death in published RCTs of small firm interventions

Table 3.A.1 – continued from previous page

Author(s)	Type of Intervention	Firm Size	Country	Sample Size	Time Frame (approximate)	Attrition Rate	Firm Death/ Survival is an Outcome	Death Rate in Control Group	Note
McKenzie (2017)	large cash grants	micro and small	Nigeria	Total: 541 Control: 263	3 years	28.1% (1 year) 15.6% (2 years) 19.5% (3 years)	yes	13% (1 year) 15.6% (2 years) 24.1% (3 years)	n.
McKenzie et al. (2017)	matching grants	small and medium	Yemen	Total: 416 Control: 200	15-17 months	51%	no	1%	k.
McKenzie and Woodruff (2008)	cash and in-kind grants	micro	Mexico	Total: 207 Control: 77	1 year	33.8%	no		
Premand et al. (2016)	business grants	micro	Tunisia	Total: 1,702 Control: 851	1.5 years	7.2%	no		
Valdivia (2015)	training business grants and technical assistance	micro	Peru	Total: 1,979	1 year (FU1) 2 years (FU2)	16.2% (FU1) 19.8% (FU2)	yes	15.7% (FU1) 20% (FU2)	o.

Note: n.r. denotes not reported. n.a. denotes not applicable.

Where available attrition rate refers to the control group, otherwise attrition rate refers to the total sample.

- Only part of the full sample has a panel structure. Sample size, time frame, and attrition reported here refer only to the panel sample. One of the outcomes the authors assess is whether the respondent no longer has a business. The timeframe is indefinite and the outcome is measured as a binary variable equal to 1 if the respondent used to have a business but no longer has one.
- The authors use different samples in the analysis. S1 denotes sample 1 and S2 denotes sample 2. Sample sizes refer to the combined sample, which is the analysis sample.
- The intervention consisted of different methods to increase the salience of lost sales due to changeouts (running out of change). Firms were surveyed weekly and the experimental design relied on random variation in the introduction into the study. Hence the number of survey waves and length of follow up vary.
- There was an additional treatment arm, to which 304 study participants were assigned. We report only on the entrepreneurship program treatment arm and the control group.
- Data comes from two sources: (1) a one-year follow-up and (7) social security data, covering a horizon of up to 7 years. We do not consider the longer term horizon since the data is not able to assess survival. The death rate of the control group is provided in a footnote in the context of attrition.
- Sample sizes and attrition only refer to the panel sample.
- The authors study two groups of women: women who were already operating a business at baseline (current business owners) and women out of the labor force but interested in starting a business (potential business owners). Survival is only assessed for current business owners, and so we focus only on this sample here.
- Death rates are based on the pooled information from the two long-term follow-up surveys.
- The intervention randomly created social networks among firm owners.
- The business training was part of a larger business plan competition. Training was randomly assigned among enterprise owners who presented their project before the competition panel. We report only on the sample that presented before the panel.
- We calculated the death rate in the control group, based on information provided in the article.
- The authors note that no firm permanently exited the tailoring business during the course of the study, but that some closed their businesses temporarily for health or other reasons.
- Survival is measured as 1 if the individual was self-employed in 2011 and self-employed in 2013.
- Survival is only measured for firms that already existed at the start of the intervention. Part of the full intervention sample consisted of entrepreneurs interested in starting a business. We report sample sizes only for the existing businesses sample.
- Death rates are computed asking business owners whether they closed a business in the last two years.

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3.A.1 References for Appendix Table 3.A.1

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3.B Estimates of firm death rates in the existing literature

Appendix 3.B Estimates of firm death rates in the existing literature

Table 3.B.1: Estimates of firm death rates in existing literature

Study	Country	Type of Firm	Sample Size	Period	Death Rate	Annualized Rate	Attrition Rate	Note
Cabal (1995)	Dominican Republic	micro and small enterprises	n.r.	n.r.	n.r.	22%-29%	n.r.	e.
Daniels (1995)	Zimbabwe	micro and small enterprises	n.r.	n.r.	n.r.	11.5%	42%	e.
Davies and Kerr (2018)	Ghana	All Manufacturing	1,000	10 years	21%	2.1%	22%	
Fajnzylber et al. (2006)	Mexico	0-9 worker manuf. self-employed individuals (own-account workers or employers)	386	10 years	24.7%	2.5%	28.5%	
Fisscha (1994)	Jamaica	micro and small enterprises	n.r.	2 years	n.r.	30.1%	n.r.	
Frazer (2005)	Ghana	selected manufacturing enterprises	n.r.	n.r.	n.r.	4.1%	n.r.	f.
Haddad et al. (1996)	Morocco	Manufacturing firms	200	2 years	6.3%	3.2%	n.r.	a.
Kilby (1994)	Nigeria	micro and small enterprises	n.r.	1 year	4.6%-7.1%	4.6%-7.1%	n.r.	
Klapper and Richmond (2011)	Cote d'Ivoire	Formal firms	n.r.	n.r.	n.r.	1.3%	n.r.	f.
Krafft (2016)	Egypt	Household non-farm	5,941	20 years	n.r.	17%	n.r.	b.
McCaig and Pavcnik (2016)	Vietnam	Household non-farm	828	8 years	51.4%	6.4%	n.r.	
Mead and Liedholm (1998)	Botswana	Household enterprises	1,584	6 years	60.9%	10.2%	n.r.	
	Kenya	Household enterprises	n.r.	2 years	30-35%	15-17%	n.r.	
	Malawi	micro and small enterprises	n.r.	n.r.	n.r.	6%	n.r.	
			n.r.	n.r.	n.r.	15.9%	n.r.	
			n.r.	n.r.	n.r.	15%	n.r.	

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Table 3.B.1 – continued from previous page

Study	Country	Type of Firm	Sample Size	Period	Death Rate	Annualized Rate	Attrition Rate	Note
	Swaziland	micro and small enterprises	n.r.	n.r.	n.r.	10.5%	n.r.	
	Zimbabwe	micro and small enterprises	n.r.	n.r.	n.r.	7%	n.r.	
Nagler and Naudé (2017)	Uganda	Household non-farm	1,849	1 year	30.8%	30.8%	n.r.	
			1,829	1 year	26.2%	26.2%	n.r.	
Nigeria	Household non-farm	1,707	4 years	30.5%	7.6%	n.r.		
		4,448	0.5 years	13.2%	26.3%	n.r.		
		5,367	0.5 years	1.9%	3.8%	n.r.		
		4,516	1.5 years	4.5%	3%	n.r.		
Roberts (1996)	Colombia	Manufacturing firms	n.r.	1 year	8.3%-12.9%	8.3%-12.9%	n.r.	
Sandefur (2010)	Ghana	formal and informal manuf. firms with less than 10 employees	n.r.	n.r.	n.r.	5.1%	n.r.	
Shiferaw (2009)	Ethiopia	Manufacturing with 10+ workers	471-686	1 year	16%	16%	n.r.	c.
Söderbom et al. (2006)	Ghana	Manufacturing firms	154	5 years	19%	3.8%	n.r.	
	Kenya	Manufacturing firms	133	5 years	39%	7.8%	n.r.	
Tybout (1996)	Tanzania	Manufacturing firms	102	5 years	44%	8.8%	n.r.	
	Chile	Manufacturing firms	n.r.	1 year	7.1%-18.2%	7.1%-18.2%	n.r.	
Vijverberg and Houghton (2004)	Vietnam	Household enterprises	2,484	5 years	61%	12.2%	9.6%	d.
Mean Annualized Rate						11.5%		
Median Annualized Rate						9.5%		

Note: n.r. denotes not reported.

Annualized rate is expressed relative to initial stock, and is death rate divided by period over which death is measured.

Sample size reported for experimental studies is number in the control group.

a. Author doesn't report attrition, but reports sample of 800 with only 479 observations on survival status.

b. Death is measured from filings at register of companies, so authors cannot distinguish death from becoming informal/not reporting.

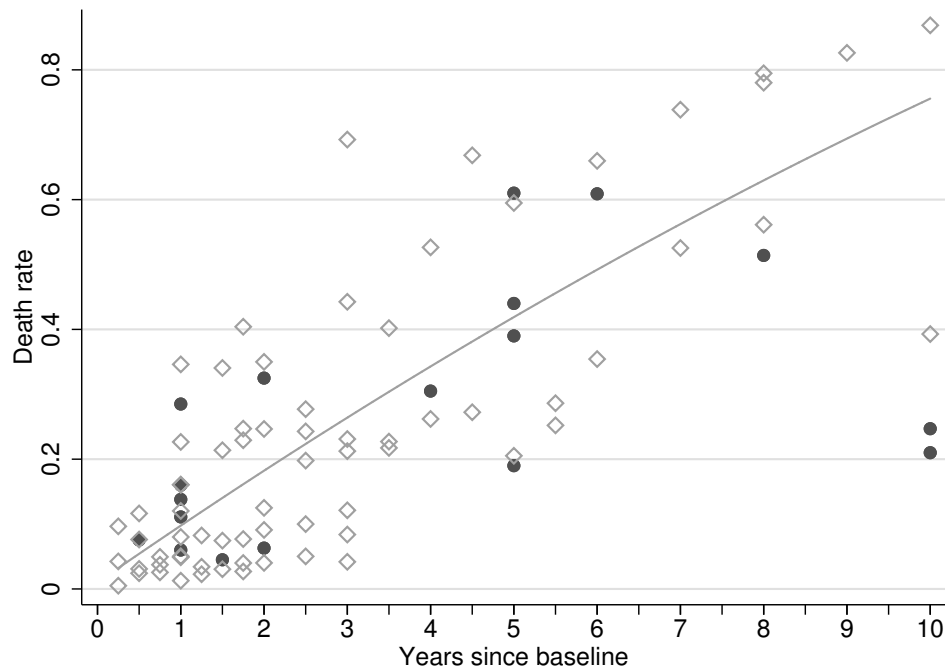
c. Death is measured from manufacturing census, cannot distinguish between dropping below size threshold from death.

d. Attrition rate does not include firms in households that were re-interviewed, but where authors were not able to match the firm over time.

e. Cited in Mead and Liedholm (1998).

f. Cited in Liedholm and Mead (1995). Death rates are based on tracer surveys which are subject to severe selectivity biases that potentially understate these death rates.

Figure 3.B.1: Comparison of our firm death rates to those reported in the existing literature



Note: Black circles are death rates from existing literature in Table 3.B.1. Open diamonds are data points in our study. Curve shown is fitted quadratic in Figure 3.1. The two outliers in the existing literature at 10 years are from Davies and Kerr (2018). Their death rates would look more similar if we assume that the more than 20 percent attrition in their study comes from firms that die.

Appendix 3.C Additional details on surveys

We discuss each of the sixteen surveys used in our study, and how firm death is defined and calculated in each. We begin with the multi-purpose household surveys, before turning to specialty firm panel surveys.

Multipurpose household surveys

Indonesian Family Life Survey (IFLS)

The Indonesian Family Life Survey (IFLS) is a publicly available multi-purpose panel survey of the Indonesian population. It surveys over 7,000 households, consisting of over 30,000 individuals, and is representative of about 83 percent of the Indonesian population. Further details on study design and data can be obtained in Strauss et al.

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(2016) and online at <http://www.rand.org/labor/FLS/IFLS.html>. There are currently five waves of data available and our sample draws from IFLS3, which was fielded in 2000, IFLS4 (fielded in 2007/2008) and IFLS5 (fielded in 2014/2015). Non-farm business modules of IFLS1 and IFLS2 are less detailed, complicating the identification of the business owner as well as the matching of businesses with later rounds, which is why we decided not to include them.

We used the non-farm business modules of the different survey waves to identify household businesses in each of the three waves we included in our sample. A household was coded as operating a business if there was a householder who had worked in a family-owned, non-farm business like trade/retailing or been self-employed in a non-farm enterprise during the past 12 months preceding the survey, which was entirely owned by the household, and still operating at the time of the survey. Owners were identified as those household members who were indicated in the non-farm business module as being primarily responsible for the reported business. Up to two members could be reported per business and their individual survey identification numbers were given, so that information on owner characteristics could be matched from the other survey modules.

Since the IFLS is not a panel of firms, but of households who are asked in each round if they operate a business, it allows us to capture information on firms that were not operating in IFLS3, but in IFLS4. We can thus use two baselines for the analysis of firm dynamics over time, as each round provides us with a sample of firms operating at a given point in time. Taking IFLS3 as baseline, we observe horizons of 7.5 and 14.5 years to IFLS4 and IFLS5, respectively. We observe a horizon of seven years from IFLS4 to IFLS5 with IFLS4 as the baseline. Since horizons using IFLS3 and IFLS4 as baseline differ, we do not pool together data for the same observation period with this approach, but increase the sample with information on different horizons.

The non-farm business modules of the IFLS did not ask whether the business reported in a given round is the same as the business reported in the previous round, or what happened to the one reported earlier. We therefore needed to match reported businesses across rounds in order to be able to determine whether the business survived or not, and in the latter case, whether a new firm was started. To simplify this matching exercise, we only kept firms of households who never operated more than one enterprise in any of the three survey rounds in our sample.

Taking IFLS3 [IFLS4] as our baseline, and IFLS4 [IFLS5] as follow-up round, we then coded the business reported at baseline as surviving, if a business was reported in the follow-up round, and:

3.C Additional details on surveys

- start dates (i.e. date when the business was started) were identical across the two waves; or
- the field of the business was the same across the two waves, and the start date of the business reported in the follow-up round was a date preceding the interview date of the baseline wave; or
- at least one of the business owners (person(s) indicated as being responsible for the business) was the same across the two waves, and the start date of the business reported in the follow-up round was a date preceding the interview date of the baseline wave.

We coded the business reported at baseline as closed, if:

- no business was reported in a subsequent round.

Otherwise, we coded survival/closure as missing.

We coded a new firm start, if:

- the start date of the business reported in the follow-up round was a date after the interview date of the baseline wave.

We coded the variable *newfirmstart* to zero, if:

- the business reported in the baseline round survived; or
- no business was reported in the follow-up round and the business of the baseline round was coded as closed; or
- the start date of the business reported in the follow-up round was a date preceding the interview date of the baseline wave and no business was reported in the baseline wave.

Otherwise, the variable *newfirmstart* was coded as missing.

We used these three types of business characteristics (start date / age of the business, ownership, and business sector) together, because start dates turned out not to be very accurate, and it is possible that ownership of a household business changes over time. Even so, there remained a number of inconsistencies and cases which could not be matched, so that in a final step, we inspected the characteristics of these businesses in all three consecutive survey waves visually and coded survival on a case-by-case basis, again, taking into account information on firm age, ownership and the business sector reported in the three survey waves. For these remaining observations, we decided to exclude firms from the sample if information on firm age was missing, since this information is important in determining survival and *newfirmstart*.

Given that the non-farm business module of the IFLS allowed for more than one owner to be reported, and ownership could change from one round to another, we also had to decide on which of the owners to include in case we identified multiple owners

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

for a business, as we were interested in assessing also owner characteristics in our study. For firms for which we had identified more than one owner, we decided to include the owner, who was reported as the owner in all of the waves in which we observed the firm. If this was the case for both owners, we included the owner,

- who had the lowest household roster number (this chose the household head as the owner if he or she was among the owners), if no other business was reported for at least one of the owners over the whole observation period; or
- who had already been identified as the owner of a different business, observed during one or more different waves over the whole observation period;

and in the remaining cases we chose the owner who was reported as the owner in most of the waves in which we observed the firm, or, if this was the case for both owners, who had the lowest household roster number.

Finally, we excluded firms for which we could not identify at least one owner among the household members who were in the IFLS sample.

Mexican Family Life Survey (MxFLS)

The Mexican Family Life Survey (MxFLS) is a publicly available multi-purpose panel survey conducted in Mexico. It surveys 8,400 households (35,000 individuals) and is representative of the Mexican population at the national, urban, rural, and regional level. Further details on study design and the data can be obtained in Rubalcava and Teruel (2013). Three rounds of data have been collected (2002, 2005-2006, and 2009-2012) and we included all three of them in our sample.

We identified household businesses in the MxFLS using the non-farm business modules of the different survey waves. A household was coded as operating a business if there was a household member who had owned or shared a non-agricultural business during the past 12 months preceding the survey. Owners were identified as those household members who took the most important decisions regarding the business. Given that only their relationship to the respondent was reported, as opposed to their survey identification number, owners could only be uniquely identified, if they were either the respondent or his or her spouse.

Given the household panel structure of the MxFLS, we also extracted information on firms that were not operating in round 1, but in round 2, and use two baselines for the analysis of firm dynamics over time. Taking round 1 as baseline, we observe horizons of three and 7.5 years to rounds 2 and 3, respectively. We observe a horizon of 4.5 years from round 2 to round 3 with round 2 as the baseline. Since horizons using rounds 1 and 2 as baseline differ, we do not pool together data for the same observation period,

3.C Additional details on surveys

but increase the sample with information on different horizons with this procedure.

In order to code whether a business observed at baseline survived or not, and in the latter case, whether a new firm was started, we needed to match reported businesses between rounds based on a number of business characteristics, as the MxFLS does not link household businesses from round to round. For simplification, we decided to only include firms of households in our sample who never operated more than one enterprise in any of the three survey rounds.

Taking round 1 [round 2] as our baseline, and round 2 [round 3] as follow-up rounds, we then coded the business reported at baseline as surviving, if a business was reported in the follow-up round, and:

- start dates (i.e. year the business was started) were identical across the two waves; or
- at least one of the business owners (person(s) taking the most important decisions regarding the business) was the same across the two waves, and the business reported in the follow-up round was not started after 2001 [in or after the year in which the baseline survey was conducted].

We coded the business reported at baseline as closed, if:

- no business was reported in a subsequent round.

Otherwise, we coded *survival* as missing.

We coded a new firm start, if:

- the start date of the business reported in the follow-up round was 2002 or later [the year in which the baseline survey was conducted or later].

We coded the variable *newfirmstart* to zero, if:

- the business reported in the baseline round survived; or
- no business was reported in the follow-up round and the business of the baseline round was coded as closed; or
- the start date of the business reported in the follow-up round was a date preceding 2002 [the interview date of the baseline wave] and no business was reported in the baseline wave.

Otherwise, the variable *newfirmstart* was coded as missing.

We coded survival and *newfirmstart* from round 1 to round 3 as well, based on these same criteria described above.

After this matching and coding, we inspected the characteristics of the businesses for which survival and *newfirmstart* remained unclear or inconsistent on a case-by-case basis, taking into account information on firm age and ownership reported in the three survey waves. For this, we decided to exclude firms from the sample if information on

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firm age was missing, since this information is important in determining survival and *newfirmstart*.

For firms for which we had identified more than one owner, we decided to include the owner, who was reported as the owner in all of the waves in which we observed the firm. If this was the case for both owners, we included the owner,

- who had the lowest household roster number (this chose the household head as the owner if he or she was among the owners), if no other business was reported for at least one of the owners over the whole observation period; or
- who had already been identified as the owner of a different business, observed during one or more different waves over the whole observation period;

and in the remaining cases we chose the owner who was reported as the owner in most of the waves in which we observed the firm, or, if this was the case for both owners, who had the lowest household roster number.

Finally, we excluded firms for which we could not identify at least one owner among the household members who were in the MxFLS sample.

Nigeria General Household Survey (NGGHS)

The Nigeria General Household Survey (NGGHS) is a cross-sectional multipurpose household survey of 22,000 households, carried out annually in Nigeria. Under the World Bank's Living Standards Measurement Study - Integrated Surveys on Agriculture (LSMS-ISA) project a panel component was introduced into the multi-purpose NGGHS, which collects longitudinal data for 5,000 households. The sample is representative at the national level and provides reliable estimates of key socio-economic variables for the six zones in the country. Further details on study design and the data can be obtained in National Bureau of Statistics of the Federal Republic of Nigeria (2014).

The panel component was first introduced in 2010/2011 and has since then been implemented every two years. There are currently three waves available, of which we use the two first ones (2010/2011, and 2012/2013), given that the third wave was not yet available at the time of our data analysis. Each wave consists of two visits, one in the post-planting period and another in the post-harvest period, which we consider separately, resulting in a total of four survey waves from the NGGHS in our sample.

We identified household businesses in the NGGHS panel component using the non-farm business modules of the different survey waves. A household was coded as operating a business if there was a household member who had worked for himself, other than on a farm or raising animals (e.g. operating his or her own business, trade, work-

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ing as a self-employed professional or craftsman) during the past 12 months preceding the survey. Owners were identified using information on who in the household owned the business, who managed it, and who was the survey respondent for the non-farm business module. Up to two members could be reported as owners and/or managers per business and their individual survey identification numbers were given, so that information on owner characteristics could be matched from the other survey modules. We then identified the business owner as the person who was indicated as the business owner if the household indicated only one and the same owner over the whole observation period. For the remaining cases (i.e. households who reported two owners in the same wave, or different owners in different waves) we identified the business owner(s) in a given round as the owner(s):

- who was also reported to be the manager and was the respondent for the non-farm business module; or
- who was/were also the manager of the business, if the respondent was neither a business owner nor a manager of the business; or
- who was also the respondent for the non-farm business module, if no information on the manager was available or none of the owners were identified as the manager; or
- who was/were the business owner(s), if no information was provided on manager and respondent; or
- who was the household head if no information was given on who was the business owner, manager, and respondent; or
- who was also reported to be the manager if one owner was indicated as manager and the other was the respondent for the business module.

In contrast to the other multi-purpose household surveys we use, the non-farm business modules of the NGGHS panel component to check whether the households surveyed reported any non-farm household enterprise or income generating activity in the previous round. If so, they ask, whether this activity is still operating, closed permanently, temporarily or seasonally. We coded enterprises as surviving, if they were still being operated, and as closed if they were either closed permanently, temporarily, or seasonally. Households were also asked if the firm they reported was a new or an original one. We coded the variable *newfirmstart* to one if the reported firm was a new one, and to zero if it was an original one. For simplification, we only included firms in our sample if the household never operated more than one firm in any of the four rounds we considered. Nevertheless, relying on the matching and information on closure and new firm start provided by the NGGHS was not sufficient. There remained a number of inconsistent and unclear cases, which we then coded on a case by case basis. For this

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we considered ownership and business sector over the horizon over which we observed these unclear cases for a given household, as well as, if available, information provided on closure and new firm start.

Given that the NGGHS panel component asks households in each survey round, whether they are operating a business, regardless of whether they were operating one in the previous round, we also extracted information on firms that were not operating in round 1, but in round 2, or 3 and use three baselines for the analysis of firm dynamics over time. Taking round 1 as baseline, we observe horizons of six months, 1.5 years, and two years to rounds 2, 3, and 4 respectively. With round 2 as the baseline, we observe horizons of 1.5 and two years to round 3 and 4, respectively. And with round 3 as baseline, we observe a horizon of six months to round 4. In the analysis, we pool the data for the same horizons with different baselines together to increase the sample size for a given horizon. We also use different baselines when horizons differ to increase the sample with information on different observation periods.

In order to be able to assess owner characteristics in our study, for jointly owned businesses, and businesses for which ownership changed over the observation horizon, we determined one owner as the owner for all waves in which we observed the firm. In the case of jointly owned businesses, we decided to include the owner, who was reported as the owner in all of the waves in which we observed the firm. If this was the case for both owners, we included the owner,

- who had the lowest household roster number (this chose the household head as the owner if he or she was among the owners), if no other business was reported for at least one of the owners over the whole observation period; or
- who had already been identified as the owner of a different business, observed during one or more different waves over the whole observation period;

and in the remaining cases we chose the owner who was reported as the owner in most of the waves in which we observed the firm, or, if this was the case for both owners, who had the lowest household roster number.

Townsend Thai Survey (TTHAI)

The Townsend Thai Project is a data collection project consisting of ongoing annual and monthly multi-purpose household, institutional, and key informant (village leader) panel surveys, in addition to collecting environmental data in Thailand. The baseline was conducted in 1997, collecting data from rural areas in four Thai provinces, two in the Northeast and two in the Central region. In the early and mid-2000s the sample was extended to include data from urban areas as well as from the Southern and Northern

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region of Thailand. Further details on study design can be obtained in Townsend (2016). We use data from the annual household surveys conducted between 1997 and 2014, resulting in the inclusion of 18 survey waves in our sample.

We identified non-farm household businesses using the household business modules of the different survey waves and coded a household as operating a business if anyone in this household owned a business, which was entirely owned by the household or this person. Since our interest was in non-farm enterprises, we did not consider entrepreneurial activities if they were classified as rice mills, shrimp farms, fish farms, shrimp and fish farms, or any other agricultural activity, as provided by the description of the type of business operated. The household business module did not ask who was the owner or manager of the business reported. We therefore identified business owners using the occupation module, as those household members whose occupation was reported to be “*owner of a business*” in case the household was operating a non-farm household business.

Households in the panel were asked in each survey round if anyone in the household owns a business. Each round provides us hence with a sample of firms operating at a given point in time. Taking advantage of this, we pool together data for the same observation period to increase our sample for a given horizon. For instance, we include baseline information from 17 survey waves (with baselines from 1997 to 2013) to assess survival over a horizon of one year, from 16 survey waves (with baselines from 1997 to 2012) for an observation period of two years, and so forth. For the longest observation horizon, 17 years, we can only use information on baseline firms from one survey round, i.e. from 1997.

The annual Townsend Thai Survey (TTHAI) we use, does not link household businesses from round to round. It does hence not provide information about survival of a given firm that has been identified in one of the survey rounds to later rounds. In order to code whether a business observed in a given round survived or not, and in the latter case, whether a new firm was started, we needed to match reported businesses between rounds based on a number of business characteristics. For simplification, we decided to only include firms of households in our sample who never operated more than one enterprise in any of the three survey rounds. We then coded the business of a household reported in a given survey round as surviving to a given follow-up round, if a business was reported for the same household in the follow-up round, and:

- firm ages coincided across the two waves; or
- the sector of the business was the same across the two waves, and the age of the business reported in the follow-up round was not less than the time difference between

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the two survey waves; or

- at least one of the household members identified as the business owners was the same across the two waves, and the age of the business reported in the follow-up round was not less than the time difference between the two survey waves.

We coded the business reported at baseline as closed, if:

- no business was reported in the follow-up round.

Otherwise, we coded survival/closure as missing.

We coded a new firm start, if:

- the age of the business reported in the follow-up round was less than the time difference between the two survey waves.

We coded the variable *newfirmstart* to zero, if:

- the business reported in the baseline round survived; or
- no business was reported in the follow-up round and the business of the baseline round was coded as closed; or
- the age of the business reported in the follow-up round was not less than the time difference between the two survey waves.

Otherwise, the variable *newfirmstart* was coded to missing.

For businesses for which survival and *newfirmstart* remained unclear or inconsistent, we inspected the characteristics on a case-by-case basis, taking into account information on firm age, type of activity, and ownership reported in all survey waves in which we observed these unclear cases. In addition, we used information on whether the household decided to stop running any business in the past 12 months, and if so, the type of activity they were no longer running, as well as whether the business reported in a given round had been founded in the past 12 months, which was provided in the household business modules of the follow-up questionnaires.

Finally, we included one owner per firm in our sample, in case we had identified more than one owner for a given firm, either because we had identified more than one owner in one or more waves, or because different owners were reported in different waves. In these cases, we decided to include the owner, who was reported as the owner in all of the waves in which we observed the firm. If this was the case for both owners, we included the owner,

- who had the lowest household roster number (this chose the household head as the owner if he or she was among the owners), if no other business was reported for at least one of the owners over the whole observation period; or
- who had already been identified as the owner of a different business, observed during one or more different waves over the whole observation period;

and in the remaining cases we chose the owner who was reported as the owner in most of the waves in which we observed the firm, or, if this was the case for both owners, who had the lowest household roster number.

Specialized firm impact evaluation surveys:

We use data from the control group only in a number of panel data surveys collected to measure the impacts of firm interventions in randomized experiments.

Cotonou Informal Enterprise Survey (BJINFORMALITY)

The sample surveyed for the Cotonou Informal Enterprise Survey (BJINFORMALITY) consists of 3,596 informal businesses and is representative of informal businesses operating in Cotonou, Benin. The survey was designed to evaluate the effect of a simple free business registration process on formalization, and test the effectiveness of supplementary efforts to enhance the presumed benefits of formalization by facilitating its links to government training programs, support to open bank accounts, and tax mediation services. Details on sampling and the interventions tested can be found in Benhassine et al. (2016). Survey participants were randomly allocated into three treatment groups and one control group, with the control group consisting of 1,197 informal firms. The baseline survey was conducted between March and April 2014. Two follow-up surveys followed, the first was conducted between April and June 2015, and the second between May and June 2016. Attrition rates at first and second follow-up were 11.8 percent and 15.9 respectively and were not correlated with treatment status.

We coded survival to one if:

- the owner reported to be still operating in the same sector as in the previous wave; or
- the business was said by a person knowledgeable on this matter to be still operating, if the owner refused to be re-interviewed or could not be interviewed when survey attempts were made.

We coded survival to zero if:

- the owner reported not to be operating anymore in the same sector as in the previous wave because of a change in activity; or
- the survey was not completed because the business was found to have been closed definitely; or
- the business was said by a person knowledgeable on this matter not to be operating anymore, if the owner refused to be re-interviewed or could not be interviewed when survey attempts were made.

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We coded *newfirmstart* to one if the owner reported not to be operating anymore in the same sector as in the previous wave because of a change in activity. For the endline wave, we additionally coded new firm start to one if the enterprise had closed definitely and the owner was now operating a different enterprises.

Newfirmstart was coded to zero if:

- an enterprise survived; or
- the survey was not completed because the business was found to have been closed definitely (and in the endline: the owner was not operating a different enterprise); or
- the business was said by a person knowledgeable on this matter not to be operating anymore, if the owner refused to be re-interviewed or could not be interviewed when survey attempts were made.

With this procedure, we coded survival and *newfirmstart* from round to round. Given that we were interested in following only the baseline businesses, and not businesses started in the midline, we recoded survival to be capturing survival of the baseline firm. This implied, recoding survival to zero in the endline wave, if the business had been coded as closed and a new firm had been started in the midline wave.

Egypt Macroinsurance for Microenterprises Survey (EGMACROINSURANCE)

The survey was conducted to evaluate the impact of an insurance to protect microenterprise owners against macroeconomic and political shocks in Egypt, described in detail in Groh and McKenzie (2016). The baseline survey was conducted in March 2012 among a sample of clients from Egypt's largest microfinance institution at particular loan sizes, who were deciding about the renewal of their loans. The sample consisted of 2,980 individuals, with equal numbers assigned to treatment and control. In November 2012, a follow-up survey was conducted, which was able to re-interview 98.8 percent of respondents in the control group and 98.9 percent in the treatment group. We use these two survey waves and are hence able to examine survival over a horizon of approximately 6 months. Using only data from the control group yields 1,441 business owners at baseline, which we include in our sample.

We coded survival to one, if:

- the respondent said that he or she had not closed or sold the business in which he or she worked at the time of the baseline survey in March 2012; or
- the business was said by a person knowledgeable on this matter, to be still being operated by either the business owner, an immediate family member, or by some other person, if the respondent refused to participate in the survey or could not be located.

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We coded survival to zero, if:

- the respondent said that he or she had closed or sold the business in which he or she worked at the time of the baseline survey in March 2012; or
- the business was said by a person knowledgeable on this matter, to be closed, if the respondent refused to participate in the survey or could not be located.

If the respondent said that he or she had closed or sold the business in which he or she had worked at the time of the baseline survey in March 2012, we coded *newfirmstart* to one, if the respondent said that he or she was operating a different business after closing or selling the business previously operated, and had opened this business since March 2012. Otherwise, we coded *newfirmstart* to zero.

Ghana Microenterprises Survey (GHMS) The Ghana Microenterprises Survey (GHMS) was conducted to evaluate the impact of cash and in-kind grants to microentrepreneurs in urban Ghana. Details of the intervention and study design are described in Fafchamps et al. (2014). The baseline survey was conducted in October and November 2009, surveying a representative sample of 907 microenterprise owners in urban Ghana, in particular in the cities of Accra and Tema. A second pre-treatment survey of these firms was conducted in February 2009 to eliminate firms most likely to attrit, yielding a final baseline sample of 793 firms for the experiment, of which 397 were assigned to the control group. Four additional quarterly follow-up surveys followed in May 2009, August 2009, November 2009, and February 2010. In March 2012 a final, longer-term follow-up survey was conducted. We include the 397 baseline control group enterprises in our sample and use data from all six survey waves.

Using information from round to round, we coded survival in each round to one if:

- the owner said that he or she was still owning and operating a business, and the line of the business had not changed; or
- the line of the business had changed, but when asked what happened to the business previously ran, the owner said that he or she was either operating the same business, or that the business was now being run by another family member; or
- the owner or a person knowledgeable of the matter told the enumerator that the owner was still operating the business, if the respondent refused to participate in the survey or could not be located; or
- the owner or a person knowledgeable of the matter told the enumerator that he owner was not operating the business anymore but that someone else was operating it, if the respondent refused to participate in the survey or could not be located.

We coded survival to zero if:

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- the line of the business had changed, and when asked what happened to the business previously run, the owner said that the business had closed down, had been modified to become the current business, or gave any other answer that implied the closure of the original business; or
- the owner or a person knowledgeable of the matter told the enumerator that the business had closed down, if the respondent refused to participate in the survey or could not be located.

We coded *newfirmstart* to one, if the line of the business had changed from one round to another and the respondent said that his or her main activity was then operating a different business. Otherwise, we coded *newfirmstart* to zero.

To be able to assess survival of firms operating at baseline, we recoded survival in a final step, by recoding it to zero for all subsequent rounds, if it had been coded as zero in a given round and a new firm start had been coded in the same round or later.

Kenya GET Ahead Business Training Program Impact Evaluation Survey (KEGETA-HEAD)

The survey was part of an evaluation of a business training program for women in four counties of Kenya: Kakamega and Kisii in the Western region, and Embu and Kitui in the Eastern region. In each county a census was taken of all female-owned businesses in market centers deemed as medium-sized or large, and then a screening was done to select a sample in which the business did not have more than 3 employees; the business had profits in the past week between 0 and 4000 Kenyan Shillings (KSH) (1 US Dollar averaged approximately 85 KSH over the survey period); sales in the past week less than or equal to 50,000 KSH; and the individual had at least one year of schooling. The survey is therefore representative of microenterprises of this size run by women in these four counties.

The baseline survey took place between June and November 2013, and included 3,537 individuals. These individuals were then assigned to treatment and control for the business training intervention in a two-stage process, with markets being assigned to treatment or control in the first stage, and then, within markets that had been assigned to treatment, individuals being assigned to treatment or control. We include all control group observations, i.e. individuals in pure control markets and individuals assigned to the control group in treatment markets, totaling 2,365 firms, in our sample. One year after the baseline survey, between June and October 2014, the first follow-up survey was conducted, which was supplemented by a rapid short survey that was conducted between November 2014 and February 2015, and which we mainly used

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to obtain information on firms not found in the first follow-up. The second follow-up survey took place between February 2016 and July 2016. Further details on the survey, intervention, and study design can be found in McKenzie and Puerto (2017). We used all four survey waves, joining information from the first follow-up survey and the subsequent short follow-up survey and treating them as one round, hence including three rounds of data from this survey in our sample.

Using information from round to round, we coded survival in each round to one if:

- the owner answered that she was still operating the same business as in the previous round; or
- the owner was said by a proxy respondent to still be operating a business, if the respondent refused to participate in the survey or could not be located.

We coded survival in each round to zero if:

- the owner answered that she had closed the business and was now operating a different business; or
- the owner answered that she was no longer operating a business; or
- the owner said that she had never operated a business; or
- the owner said or was said by a proxy respondent, not to be in business anymore or to have left the country, if the respondent refused to participate in the survey or could not be located; or
- the owner had died.

We coded *newfirmstart* to one, if the owner answered that she had closed the business and was now operating a different business. If the owner answered either that she was still operating the same business as in the previous round, or that she was no longer operating a business, we coded *newfirmstart* to zero. In the remaining cases *newfirmstart* was coded to missing.

To transform the coding into survival to a given round for firms that were operating at baseline, and to deal with some inconsistencies that were due to the combination of data from the first follow-up and the subsequent short follow-up, we recoded survival in a final step. If survival to round 2, as well as *newfirmstart* to round 2 had been coded to one, we recoded survival to round 2 to zero, assuming that these were either cases in which the original business had survived to the first follow-up, but not to the subsequent short follow-up survey, and the owner was operating a new firm by the time of the short follow-up survey, or cases in which the owner had closed and opened new firm in the first follow-up and then the business was coded as surviving in the short subsequent follow-up survey, because this new business continued to be open. If a business had been coded to be closed by round 2, but survived to round 3 and a new

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firm had been started by round 2, we recoded survival to round 3 to zero.

Malawi Business Registration Impact Evaluation Survey (MWBRIE)

The Malawi Business Registration Impact Evaluation Survey (MWBRIE) was designed to evaluate the impact of a randomized experiment designed to increase business formalization in Malawi. The survey was conducted on a sample of 3,002 informal firms in urban Lilongwe and Blantyre, which had been identified through a listing of over 100 business centers (i.e. concentrations of firms including industrial parks, markets, streets with shops, set of workshops, etc.). As a result the sample does not include household-based enterprises. Moreover, firms in the sample comply with one of the following criteria: (i) they had at least one worker contracted outside of family members and business owners, (ii) they were operating in a fixed location with more than one person working in the business, (iii) they were at the 25 percentile of revenues or above. More details are provided in Campos et al. (2015).

The baseline survey was conducted between December 2011 and April 2012. After that, the sample was randomly assigned to either one of three treatment arms or the control group, which consisted of 757 firms. Details on the intervention and experimental design can be found in Campos et al. (2015). A first follow-up survey took place between November 2012 and March 2013. Attrition was 5.7 percent, and uncorrelated with treatment status. The second follow-up survey was implemented between November 2013 and March 2014. Attrition for this second follow-up survey was 9.4 percent, and also uncorrelated with treatment status. Between November 2014 and April 2015, a third follow-up survey took place, and a fourth follow-up survey was conducted between July 2015 and January 2016. Including data from all five survey rounds into our sample, we are able to observe firms over horizons of up to 3.5 years.

We coded survival to one if owners said that they were still operating the business that they had when they were first interviewed, i.e. around December 2011 to April 2012. If they said they were not operating this business anymore, we coded survival to zero. *Newfirmstart* was coded to one if the business had closed and the owner had started a new business, and to zero if the business had closed but the owner was engaged in an activity different than operating another business.

Nigeria YouWiN! National Business Plan Competition Impact Evaluation Survey (NGYOUWIN)

The survey sample consists of applicants to a nationwide business plan competition launched in late 2011 and described in McKenzie (2017). Applicants submitted their

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business plans in January 2012, together with baseline information on firm and owner characteristics collected via a baseline data sheet. Business plans were then narrowed down to 2,312 semi-finalists, of which 1,200 received awards averaging USD 50,000 each, and among which 729 winners had been randomly selected from a group of 1,841 semi-finalists. This left 1,112 control group individuals. Three follow-up surveys were taken, approximately at yearly intervals after individuals had applied for the program, with the first follow-up conducted between November 2012 and May 2013, the second between October 2013 and February 2014, and the third between September 2014 and February 2015. A fourth follow-up survey took place between July and November 2016.

Given that at baseline, there were not only already existing firms in the sample, but also individuals who intended to start a new business with the grant, and that, in addition, firm data collected at baseline was limited, we decided to include only the four follow-up surveys in our sample, taking businesses that were existing by follow-up 1 (either because they survived from the baseline to this round, or because they had been created in between these two rounds) as the baseline sample. In addition, we only kept firms, which were not in the business of agriculture, forestry or fishing, whose owners never operated more than one business in any of the rounds in which we observed them, and which had been assigned to the control group. This resulted in the inclusion of 280 firms from the Nigeria YouWin! National Business Plan Competition Impact Evaluation Survey (NGYOUWIN) in our sample at baseline.

We coded survival to one, if:

- the respondent was currently operating a business in the follow-up round and start dates of the business were the same across the follow-up round and the baseline round (i.e. follow-up 1); or
- the respondent was currently operating a business in the follow-up round and the start date of the business was missing in the follow-up round¹⁵; or
- a person knowledgeable on the matter answered that the respondent was currently owning a business in the follow-up round, if the owner could not be interviewed after repeated attempts.

We coded survival to zero, if:

- the respondent was not operating a business in the follow-up round; or
- the respondent was currently operating a business in the follow-up round, but start dates of the businesses reported in the follow-up and baseline round (i.e. follow-up 1) did not coincide; or
- a person knowledgeable on the matter answered that the respondent was not owning a business in the follow-up round, if the owner could not be interviewed after repeated

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attempts.

We coded *newfirmstart* to one, if the respondent was currently operating a business in the follow-up round, but start dates of the businesses reported in the follow-up and baseline round (i.e. follow-up 1) did not coincide. Given that firms were only asked for their start dates in follow-up rounds 2 to 4, if they had been established in the survey year, this should have picked up new firm start.

We coded *newfirmstart*, if the respondent was not operating a business, or survival had been coded to one.

In a final step, we then recoded survival for firms that had been started in a given round and had then been coded as surviving in a later round, because they had appeared with missing start dates in this round.

Sri Lanka Microenterprise Survey (SLMS)

The Sri Lanka Microenterprise Survey (SLMS) was designed to evaluate an experiment to measure the impact of providing capital grants to microenterprises. It took place in three Southern and South-Western districts of Sri Lanka: Kalutara, Galle and Matara. One-time grants of between USD 100 and USD 200 were given in cash or in-kind (as business equipment) to male and female-owned enterprises, some of whom had been affected by the December 2004 tsunami. A door-to-door screening survey of households was used to identify enterprises with invested capital of 100,000 rupees (about USD 1000) or less, excluding investments in land and buildings, resulting in a baseline sample of 617 enterprises in retail trade and manufacturing, operated by owners 22 to 65 years old, and with no paid employees.

The baseline survey was conducted in April 2005. Firms in the sample were then randomly allocated into one of two treatment groups, which received US 100 or 200 either after the first or the third survey round and either in cash or in-kind. Details on the experimental design and intervention can be found in de Mel et al. (2008, 2009, 2012b). Follow-up surveys were conducted at quarterly intervals, from July 2005 to April 2007, and semi-annually in October 2007 and April 2008. Further follow-up surveys were carried out in June and December 2010, in September 2015, and in March 2016. We included all 15 waves of data collection in our sample, which allows us to observe firms for horizons of up to eleven years. Using firms assigned to the control group, we include 269 firms at baseline from the SLMS in our sample.

For rounds 2 to 11, we coded survival from round to round to one, if the owner was still working in the same line of business, he or she had been working in, in the previous round, and to zero if the line of business had changed. *Newfirmstart* was coded to one

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for rounds 2 to 11 if the line of business had changed and the owner stated that his or her main activity was to operate a new business. If the line of business had changed but the main activity was a different one (for instance, working for a wage, looking for work, or housework or looking after children), we coded *newfirmstart* to zero.

In round 12, questions on business continuation referred to the original baseline business. We coded survival to one, if the owner was still working in the same line of business, he or she had been working in, during the baseline survey in April 2005, and to zero if the line of business had changed. *Newfirmstart* was coded to one if either the line of the original business had changed or the owner had started a new line of business while continuing to operate the original business, and the owner stated that his or her main activity after closing, selling or changing the business was either to operate a different business or to cooperate a new business along with the original one. If the line of business had changed but the main activity was different to operating another business, we coded *newfirmstart* to zero. For round 13, we coded survival and *newfirmstart* based on the same criteria, only that the questions asked here referred to the business being operated in round 12 and survival and *newfirmstart* were hence being coded from round to round.

In rounds 14 and 15 we coded survival from round to round to one, if:

- the owner stated that he or she was currently owning a business, and had not changed the industry of the business since the previous round or closed down the business he or she had been running and started a new business; or
- a person knowledgeable of the matter answered that either the enterprise owner was still running the business, or the business was being run by an immediate family member, or the business was being run by some other person, if the respondent refused to participate in the survey or could not be located.

We coded survival from round to round to zero in these rounds, if:

- the owner stated that he or she was currently not owning a business; or
- the owner said that he or she had changed the industry of the business since the previous round, or that he or she had closed down the business he or she had been running and started a new business; or
- a person knowledgeable of the matter answered that the enterprise was closed or not functioning, if the respondent refused to participate in the survey or could not be located.

Newfirmstart was coded in rounds 14 and 15 to one, if:

- the business being owned in the given round had been started after the date of the previous survey round; or

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- the owner stated that he or she had either changed the industry in which the business was operating, or started another business while still operating the original business, or closed down the previously run business and started a new one.

Newfirmstart was coded to zero in these rounds, if:

- the owner stated that he or she was currently not owning a business.

Except for round 12, we then recoded survival in a way that it referred to survival of firms operating at baseline by recoding it to zero for all subsequent rounds, if it had been coded as zero in a given round and a new firm start had been coded in the same round or later.

Sri Lanka Impact Evaluation Survey of Business Training for Women (SLFEMBUS-TRAINING)

The Sri Lanka Impact Evaluation Survey of Business Training for Women (SLFEMBUS-TRAINING) was conducted to evaluate a business training intervention aimed at increasing female labor force participation rates and to raise the income levels of low-earning women already in business. It surveys two different groups of females: (i) a representative sample of 628 current female business owners operating in greater Colombo and greater Kandy areas, who had monthly profits of 5,000 Rs or less (USD 43), and (ii) a representative sample of 628 women between 25 and 45 years of age in greater Colombo and greater Kandy areas, who were out of the labor force, but who were likely to start working within the next year, called potential business owners. These two groups were first interviewed in January 2009, after which 400 current and 400 potential business owners were allocated to treatment. Four rounds of follow-up surveys were then conducted in September 2009, January 2010, September 2010, and June 2011. A fifth and long-term follow-up survey was conducted in June 2015. Details on the survey and study design are described in de de Mel et al. (2014).

Since our interest lies in existing businesses, we use follow-up 1 to 5, including current business owners who were operating a business by the first follow-up round and potential business owners who had started a business by then, and who had been assigned to the control group, as our baseline sample.¹⁵ This resulted in the inclusion of 260 firms at baseline in our sample.

We coded survival from round 1 to round 2 (i.e. from follow-up 1 to follow-up 2) to one, if the owner reported that she was still working in the same line of business as the one she was working in in September 2009. If the owner had changed the line of

¹⁵For current business owners we also use round 1 to extract information on time-invariant characteristics that were not asked in round 2.

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business of the original business, or was not operating a business anymore, we coded survival from round 1 to 2 to zero. *Newfirmstart* was coded to one from round 1 to round 2, if the owner answered that she was either operating a different business or operating a new business along with the old one, after closing, changing or selling the business, and to zero, if she was engaged in a different activity then.

In rounds 3 and 4 (the fourth and fifth follow-up) separate questionnaires were given to females owning only one business and those owning more than one business. For single business owners in rounds 3 and 4, we coded survival from round to round to one, if the owner reported that she was still working in the same line of business as the one she was working in in the previous wave. If the owner had changed the line of business of the original business, or was not operating a business anymore, we coded survival from round to round to zero. In round 4, we additionally used information from the attrition section to code survival, coding a firm of a single business owner as surviving if a person knowledgeable on the matter said that the owner was still running the business, the business was being run by an immediate family member, or the business was being run by some other person, in case the respondent refused to participate in the survey or could not be located. If the proxy respondent answered that the business was closed, we coded survival to zero. We coded *newfirmstart* to one for single business owners in rounds 3 and 4, if the owner answered that she was either operating a different business or operating a new business along with the old one after closing, changing or selling the business, and to zero, if she was engaged in an activity different than operating another business, after closing, changing or selling the business.

For owners of multiple businesses in rounds 3 and 4, we coded survival from round to round to one, if:

- none of the businesses operated in the previous round had been closed and business lines for all businesses had not changed since the previous round; or
- after closing, changing or selling one of the businesses operated in the previous round, the owner was either operating a new business along with one previous business, or operating only one of the previous businesses.

We coded survival from round to round to zero, for owners of multiple businesses in rounds 3 and 4, if, after closing, changing or selling the businesses operated in the previous round, the owner was either working for a wage, looking for work, operating a different business, or doing housework or looking after children.

In round 4, we additionally used information from the attrition section to code survival, coding a firm of a multiple business owner as surviving if a proxy respondent

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answered that:

- the owner was still running all businesses; or
- the owner was not running all businesses, but at least one; or
- the owner had closed down at least one of the businesses, but not all; or
- at least one of the businesses was being run by an immediate family member; or
- at least one of the businesses was being run by some other person, if the respondent refused to participate in the survey or could not be located, and coding survival to zero if the proxy respondent said that all businesses had closed down.

For owners of multiple businesses in rounds 3 and 4, we coded *newfirmstart* to one, if, after closing, changing, or selling at least one of the businesses, or starting another business, the owner was either operating a different business, or operating a new business along with one or both previous businesses, and to zero, if after closing, changing, or selling the business, the owner was either engaged in wage work, looking for work, doing housework or looking after children, or she was operating only one of the previous businesses, or none of the businesses operated in the previous round had been closed and business lines for both businesses had not changed since the previous round.

In the final round, we coded survival to one, if:

- the respondent was currently owning a business and, since January 2009, she had not changed the industry in which the business was operating, or closed down a business she was operating and then started a new business; or
- a proxy respondent reported that the owner was still running the business, or the business was being run by an immediate family member, or the business was being run by some other person, if the respondent refused to participate in the survey or could not be located.

We coded survival to zero in the final round, if:

- the respondent was not owning a business; or
- the respondent said that, since January 2009, she had changed the industry in which the business was operating or she had closed down a business she was operating and then started a new business; or
- a proxy respondent said that the business was closed, if the respondent refused to participate in the survey or could not be located.

We coded *newfirmstart* in round 5 (the final round) to one, if the business had been started after the previous survey round, or, since January 2009, the owner had either changed the industry in which the business was operating, started another business while still operating the original one, or closed down the original business and then

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started a new business. If the respondent was not operating a business in the final round or the business she was operating had been started before the previous survey round, we coded *newfirmstart* to zero.

According to the criteria described above, survival had been coded since baseline only in the second and final round. We hence recoded survival in rounds 3 and 4, in order to reflect survival since baseline, by recoding it to zero for all subsequent rounds, if it had been coded as zero in a given round and a new firm start had been coded in the same round or later.

Sri Lanka Informal Enterprises Survey (SLINFORMALITY)

The Sri Lanka Informal Enterprises Survey (SLINFORMALITY) was part of a field experiment that provided informal firms incentives to formalize. A sample of 520 informal firms owned either exclusively by men or jointly owned by husband and wife, with one to 14 paid employees in the two largest Sri Lankan cities – Colombo and Kandy – were randomly divided into four treatment groups and a control group. Details on sample selection, experimental and survey design can be found in de de Mel et al. (2013a).

The baseline survey was conducted in December 2008. Treatment was then assigned randomly, with 105 businesses being assigned to the control group. Three follow-up surveys were carried out in August 2010, March 2011 and December 2011, and we include data on the control firms from all four survey rounds in our sample.

We coded survival from round to round to one, if, in a given wave, the owner was still engaged in the same line of business as in the previous survey round, and to zero, if the line of business had changed. In the last round, we were able to use information obtained from proxy respondents to code survival, if the owner had refused to participate in the survey or could not be located. If the proxy respondent answered that the owner was still running the business, or the business was being run by an immediate family member, or the business was being run by some other person, we coded survival to one. We coded survival to zero if the proxy respondent said that the business was closed.

For rounds 3 and 4 we then recoded survival, so that it reflected survival since baseline, by recoding it to zero for all subsequent rounds, if it had been coded as zero in a given round and a new firm start had been coded in the same round or later.

Sri Lankan Longitudinal Survey of Enterprises (SLLSE)

The Sri Lankan Longitudinal Survey of Enterprises (SLLSE) is a long-term panel sur-

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vey of microenterprises designed to assess the impact of a wage subsidy, plus a savings and a business training intervention as supplementary interventions (see de Mel et al., 2010, forthcoming, for details on the study and survey design). The baseline survey was intended to be a representative survey of urban microenterprises with two or fewer paid employees, owned by males aged 20 to 45, and operating in non-agricultural sectors, in the Colombo, Kandy, and Galle-Matara areas of Sri Lanka. It was conducted between April and October 2008. The first phase occurred in April 2008 as part of a larger panel survey and also included other urban areas in Sri Lanka. In October 2008, a booster listing exercise and survey took place in order to attain a larger sample for the intervention. The original sample was also re-interviewed during the October 2008 survey, resulting in a total sample of 1,533 firms. These firms were then randomly allocated into treatment and control groups, the latter of which consisted of 286 enterprises. After these two survey rounds, the sample was re-interviewed in 10 additional follow-up rounds, consisting of six-monthly surveys every April and October from 2009 through 2012, and two additional surveys in April 2013 and April 2014.

We included firms allocated to the control group that were either part of the booster sample, or surveyed in April 2008 and reported no change in the line of business in October 2008, taking the survey conducted in October 2008 as the baseline. Our baseline sample then comprises 284 firms, which we follow for a total of eleven rounds (baseline plus ten follow-ups).

We coded survival from round to round to one, if:

- the respondent reported that he was still working in the same line of business as in the previous round; or
- the business was now being operated by another household member; or
- the respondent had sold the business to someone else to run.

Otherwise, survival was coded to zero.

Starting in round 6, we additionally used information from the attrition section to code survival, coding it to one if the respondent could not be located or refused to participate in the survey, and the proxy respondent said that the owner was either still operating the business, or the business was being run by another family member or by someone else. If the proxy respondent said that the business had closed down, we coded survival to zero.

Newfirmstart was coded to one, if the respondent was operating a different business, after closing, changing or selling the business, and to zero otherwise.

In a final step, we recoded survival for rounds 3 to 11, so that it reflected survival since baseline, by recoding it to zero for all subsequent rounds if it had been coded

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as zero in a given round and a new firm start had been coded in the same round or later.

Lomé Informal Enterprise Survey (TGINFORMALITY)

The Lomé Informal Enterprise Survey (TGINFORMALITY) was used for an impact evaluation of two training interventions for informal businesses in Lomé. The sample was drawn from applicants to the government project under which these trainings were conducted and consists of 1,500 microenterprises. Applicants had to be in business for at least 12 months, have fewer than 50 employees, operate outside of agriculture, and not be a formally registered company. Further details on the intervention, sample selection and survey can be found in Campos et al. (2017). The baseline survey was conducted between October 2013 and January 2014. Firms were then randomly assigned into one of two treatment groups and one control group, each of which included 500 firms. The first follow-up survey took place in September 2014, followed by a second in January 2015, a third in September 2015 and a fourth in September 2016. We included data for control group firms from all five survey rounds and excluded firms that were in the agricultural sector, resulting in the inclusion of 499 firms at baseline into our sample.

In each of the follow-up rounds, we coded survival to one, if the owner reported to still be working for the enterprise he or she operated in the previous round, and to zero if the owner said he or she was not working for this enterprise anymore, either because it was closed down or because of another reason. In round five, we additionally used information for owners, who refused to participate in the survey or could not be located, to code survival. If the proxy respondent said that the owner was still working for the enterprise he or she operated in the previous round, we coded survival to one, and if the proxy respondent said that the owner was not working for this enterprise anymore, we coded survival to zero. *Newfirmstart* was coded to one if the owner was operating a business and was not working for the previous business anymore, and to zero, if the owner was engaged in a different activity than operating a business and not working for the previous business anymore. For rounds 3 to 5, in a final step, we then recoded survival to zero, if it had been coded as zero in a given round and a new firm start had been coded in the same round or later, so that it reflected survival since baseline.

Uganda Women's Income Generating Support Impact Evaluation Survey (UGWINGS)

The Uganda Women's Income Generating Support Impact Evaluation Survey (UGWINGS) was conducted to evaluate individual business training, a one-time unconditional cash grant of USD 150, supervision, and business advising, provided to very

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poor Ugandans, mostly women. Details of the intervention and experimental design are described in Blattman et al. (2016). The baseline survey was conducted on a sample of 1,800 individuals from April to June 2009 who had been selected for the intervention. Of these, 896 were randomized to receive either training, or cash and supervision (phase 1 treatment group), and 904 were randomized to waitlist treatments (phase 2). The first follow-up survey was conducted between November 2010 and February 2011. Phase 2 participants then all received business training and cash and in addition supervision and advice, which was randomized among them. They received a short follow-up survey between September and October 2011 (roughly a month after receiving the grant), and a further, more detailed, follow-up survey between June and August 2012.

Individuals were not asked whether they were already operating a small business at baseline, but in the follow-up surveys conducted between November 2010 and February 2011, and between June and August 2012, respondents were asked whether they had started a small business between the last time they had been surveyed and the current survey. In particular they were asked how many small businesses they had started or had tried to start within this period. They were then asked how many of these were still being operated. We used this information to construct the variables *newfirmstart* and *survival*. In particular, we constructed them in the following way:

- We coded *newfirmstart* to 1 if the individual reported to have started or tried to start at least one new small business since the last time he or she was surveyed.
- We coded *survival* to 1 if the individual reported to have started or tried to start at least one new small business since the last time he or she was surveyed and at least one of these businesses was still active. If none of these businesses was active anymore, we coded *survival* to zero.

In the endline for phase-2 individuals (June and August 2012), individuals were not asked if they had started a business between the baseline and the midline survey (conducted between November 2010 and February 2011), so it was not possible to follow the businesses they had started then until the final round. They were only asked whether they had started a new small business between the last time they were surveyed (i.e. midline) and the current survey round (i.e. endline). Furthermore, at that time they had already received treatment. We hence included in our sample data from 461 phase-2 businesses which had been started between round 1 and round 2, and observe them until round 2, which is a maximum horizon of 18 months.

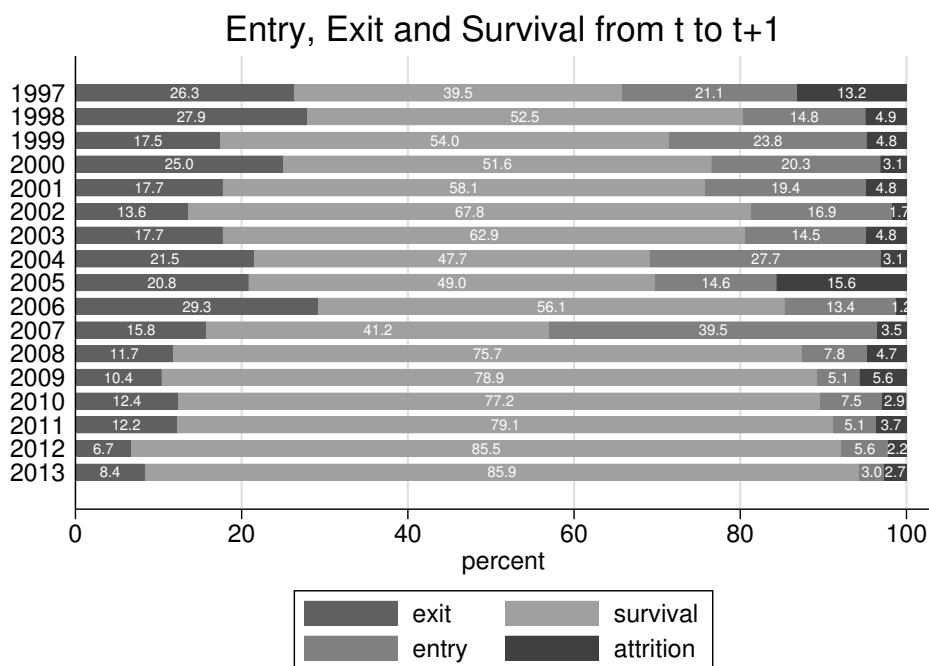
Appendix 3.D Measuring new firm entry

Although the focus in this paper is on firm death, new firm entry is also a key part of firm dynamics. However, the data we have are not well-suited for this purpose. Twelve of our 16 panels come from RCT control group samples, which start with a group of firms and then track them over time. Such panels are useful for measuring the death of these firms, and in some cases allow measurement of whether the owners start-up new firms after firm death, but do not capture new entry of other firms. The two family life surveys (IFLS and MxFLS) have long time intervals between each wave. For example, the three IFLS waves we use are each seven years apart. Again this is fine for measuring firm death, since we can observe whether the firms open at time t are still open at $t+7$. But it is no good for measuring new firm entry, since it will not allow us to capture cases of a firm getting opened and then closed again between survey rounds. Given the estimates in our paper that the half-life of an average firm is six years, and that 17 percent of new firms die in their first year and over sixty percent in their first five years, such surveys would miss over half of new firm entries. The NGLSMS did not ask for firm age in the follow-up rounds, and so also could not be used for this purpose.

The one survey that does allow examining firm entry is then the Townsend Thai data set, which has annual data. It will still miss firms that enter and then close within the same year, but at least provides some sense of how the rate of entry compares to that of firm death. Entry here includes new firms created by owners who had closed previous firms. Figure 3.D.1 shows the year by year entry and exit rates. The average annual firm death rate is 17 percent, and the average entry rate is 15 percent. Moreover, a scatterplot of entry and exit rates (see Figure 3.D.2) shows the two are positively correlated, so that there is more entry in periods of greater exit.

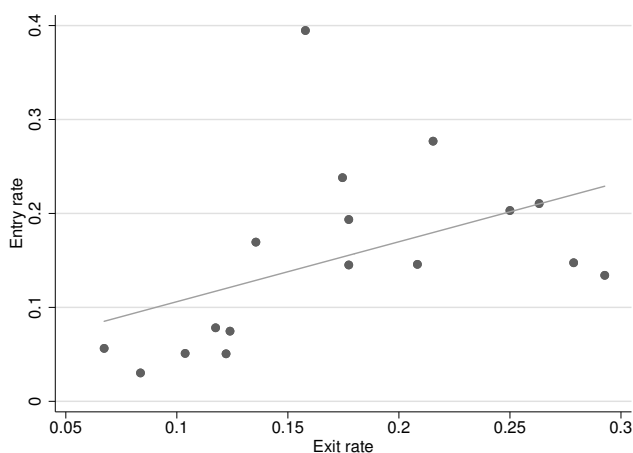
3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

Figure 3.D.1: Annual entry and exit rates in Townsend Thai data, by survey year



Source: Own calculations from Townsend Thai data set.

Figure 3.D.2: Entry rates and exit rates are correlated in Townsend Thai data



Source: Own calculations from Townsend Thai dataset. The correlation between entry and exit rates is 0.64 and statistically significant at the 10 percent level (p-value= 0.063).

Appendix 3.E Coverage of follow-up periods

Table 3.E.1: Overview of the follow-up periods covered, by survey

Years since baseline	BENINFORM	EGYPTMACROINS	GHANAFLYP	IFLS	KENYAGETAHEAD	MALAWIFORM	MXFLS	NGLSMSISA	NGYOUWIN	SLKFEMBUSTRAINING	SLKINFORMALITY	SLLSE	SLMS	TOGOINF	TTHAI	UGWINGS
0.25			✓							✓			✓			
0.5		✓	✓					✓					✓			
0.75			✓										✓	✓		
1	✓		✓		✓	✓			✓	✓		✓	✓	✓	✓	
1.25			✓										✓	✓		
1.5								✓				✓	✓			✓
1.75						✓			✓	✓	✓	✓	✓	✓		
2	✓							✓				✓	✓		✓	
2.5					✓			✓			✓	✓	✓			
3						✓	✓				✓	✓	✓	✓	✓	
3.5						✓			✓			✓				
4												✓			✓	
4.5							✓					✓				
5													✓		✓	
5.5												✓	✓			
6										✓					✓	
7				✓											✓	
8				✓			✓								✓	
9															✓	
10													✓		✓	
11													✓		✓	
12															✓	
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14															✓	
15				✓											✓	
16															✓	
17															✓	

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

Appendix 3.F Attrition and missing information on survival

Table 3.F.1: Attrition and missing information on survival by follow-up horizon

Years since baseline	Percent attrited	Percent missing	Percent attrited + missing
0.25	1,1%	0,1%	1,2%
0.5	2,4%	7,1%	9,5%
0.75	6,1%	0,0%	6,1%
1	4,8%	0,0%	4,8%
1.25	9,2%	0,7%	9,9%
1.5	6,3%	0,4%	6,7%
1.75	10,0%	0,6%	10,6%
2	10,0%	12,0%	21,9%
2.5	7,0%	9,5%	16,5%
3	9,5%	3,6%	13,1%
3.5	11,6%	0,0%	11,6%
4	5,7%	0,0%	5,7%
4.5	5,6%	0,0%	5,6%
5	15,4%	0,0%	15,4%
5.5	9,0%	0,0%	9,0%
6	4,1%	0,0%	4,1%
7	20,0%	0,0%	20,0%
8	15,5%	13,8%	29,3%
9	20,0%	0,0%	20,0%
10	7,4%	0,0%	7,4%
11	8,0%	0,0%	8,0%
12	20,0%	0,0%	20,0%
13	20,0%	0,0%	20,0%
14	20,0%	0,0%	20,0%
15	19,3%	12,4%	31,6%
16	20,0%	0,0%	20,0%
17	20,0%	0,0%	20,0%

3.G Firm death rates over different time horizons

Appendix 3.G Firm death rates over different time horizons

Table 3.G.1: Firm death rates over different time horizons, pooled sample and individual surveys

Years since baseline	Pooled sample	BENINFORM	EGYPTMACROINS	GHANAFIYP	IPLS	KENYAGETAHEAD	MALAWIFORM	MXFLS	NGLSMSISA	NGYUWIN	SLKFEMBUSTRAINING	SLKFEMBUSTRAINING	SLSE	SLMS	TOGOINF	TTHAI	UGWINGS
0.25	4% [4%,5%]		1% [1%,1%]								10% [10%,10%]		4% [4%,8%]				
0.5	9% [8%,18%]		8% [8%,8%]	3% [3%,5%]					12%* [10%,27%]*				2% [2%,10%]				
0.75	4% [3%,10%]			5% [5%,8%]									4% [3%,14%]	3% [2%,8%]			
1	13% [13%,18%]	8% [7%,15%]		5% [5%,15%]	16% [16%,17%]	12% [11%,18%]				35% [32%,39%]	16% [16%,18%]		5% [5%,11%]	1% [1%,14%]	23%* [21%,28%]*		
1.25	5% [4%,14%]			8% [8%,13%]									3% [3%,16%]	2% [2%,14%]			
1.5	20% [17%,34%]							21% [16%,40%]					7% [7%,17%]	3% [3%,17%]			34% [32%,39%]
1.75	17% [15%,28%]					23% [21%,30%]				40% [28%,59%]	25% [25%,25%]		4% [3%,20%]	3% [2%,13%]			
2	20% [17%,33%]	9% [8%,19%]						25%* [17%,49%]*					13% [11%,24%]	4% [3%,20%]	35%* [32%,40%]*		
2.5	23% [19%,36%]							28% [17%,56%]					5% [9%,23%]	5% [19%,21%]			
3	30% [26%,38%]					24% [23%,29%]							4% [23%,25%]	4% [8%,12%]	8% [39%,50%]*		
3.5	25% [22%,35%]									40% [29%,56%]			23% [22%,26%]				
4	36% [32%,43%]												26% [25%,30%]				53%* [45%,59%]*
4.5	51% [44%,58%]												27% [26%,31%]				
5	41% [35%,49%]							67% [54%,73%]					21% [17%,32%]				
5.5	27% [25%,34%]												29% [27%,32%]	25% [22%,35%]			59%* [50%,65%]*

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3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

Table 3.G.1 – continued from previous page

Years since baseline	Pooled sample	BENINFORM	EGYPTMACROINS	GHANAFLYP	IFLS	KENYAGETAHEAD	MALAWIFORM	MXFLS	NGLSMSISA	NGYUWIN	SLKFEMBUSTRAINING	SLKINFORMALITY	SLSSE	SLMS	TOGOINF	TTHAI	UGWINGS
6	50% [43%,57%]										35% [35%,37%]						66%* [55%,72%]*
7	58% [42%,69%]		53% [37%,67%]														74%* [61%,78%]*
8	65% [47%,74%]		56% [38%,71%]					78% [64%,82%]									79%* [66%,83%]*
9	83% [67%,86%]																83%* [68%,86%]*
10	66% [58%,70%]												39% [37%,43%]				87%* [73%,89%]*
11	65% [57%,70%]												39% [36%,43%]				90%* [75%,92%]*
12	90% [74%,91%]																90%* [75%,92%]*
13	91% [75%,93%]																92%* [75%,93%]*
14	92% [76%,93%]																93%* [76%,94%]*
15	77% [53%,84%]		76% [52%,84%]														94%* [78%,95%]*
16	96% [80%,96%]																97%* [80%,97%]*
17	100% [80%,100%]																100% [80%,100%]

Note: Death rate bounds accounting for missing information on survival/death, including attrition, are given in parentheses.

* indicates that multiple rounds are available for this combination of survey and time horizon, so that death rates, lower and upper bounds shown are averages of all their respective round-specific values available for this particular horizon.

Observed drops in death rates for a given survey are possible, since some firm closure is temporary (in the sample of unique firms, we observe 6.22 percent as closed at one point in time and open in a later round), and as a result of attrition (1.1 percent of unique firms in our sample are either closed or open in t and t+2, and not observed in t+1).

For the MxFLS, the baselines for the horizons over which we observe firm death are different. We use the 2002-round for the 3-year and 8-year horizon and the 2005-round for the 4.5 year horizon.

Appendix 3.H Gender and reason of closure

Table 3.H.1: Reason for closure by gender

Reason for closure	Male	Female	Total
Making a loss	49.34%	35.5%	590
Sickness	9.79%	15.69%	197
Care for family	2.45%	18.07%	180
Better wage job	8.1%	5.63%	95
Better business opportunity	7.91%	3.14%	71
Other	22.41%	21.97%	322
Total	531	924	1,455

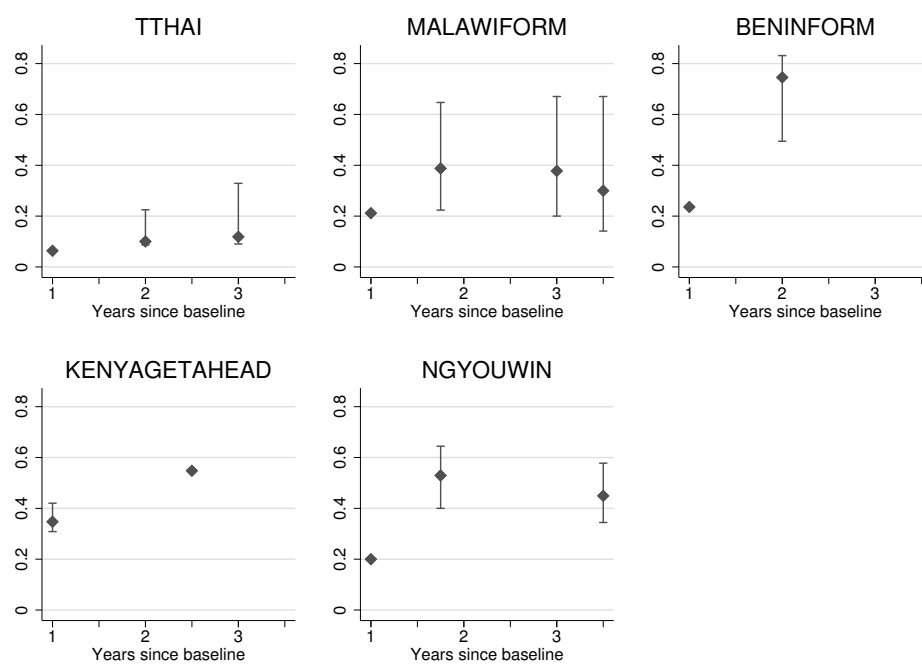
Table 3.H.2: Gender by reason for closure

Reason for closure	Male	Female	Total
Making a loss	44.41%	55.59%	590
Sickness	26.4%	73.6%	197
Care for family	7.22%	92.78%	180
Better wage job	45.26%	54.74%	95
Better business opportunity	59.15%	40.85%	71
Other	36.96%	63.04%	322
Total	36.49%	63.51%	1,455

3. SMALL FIRM DEATH IN DEVELOPING COUNTRIES

Appendix 3.I Reopening rate over different horizons

Figure 3.I.1: Reopening rate over different horizons for firms that closed within 1 year



Note: Only surveys in which 85 or more firms died within a year and subsequent re-opening can then be observed are shown.

Chapter 4

The impact of social health insurance on productive activities in rural Mexico

4.1 Introduction

Health shocks are an important source of risk for poor households in developing countries, which have traditionally been excluded from public and private health insurance schemes. In many developing countries, affluent households, formal sector workers and civil servants have been protected from the financial risk of seeking care in case of a health shock through social health insurance systems and private mechanisms (see Hsiao & Shaw, 2007). The majority of the population in these countries – low income and poor households – however, has been incurring disproportionately high out-of-pocket expenditures for health care, being served by public facilities, which provide free services, but at low quality and reliability (Hsiao & Shaw, 2007). In the absence of formal insurance mechanisms, these households have employed various strategies to cope with the financial burden of health shocks, such as using savings, borrowing, selling assets and livestock, intra-household labor substitution, withdrawing children from school, and child labor (e.g. Sauerborn et al., 1996; Kremer & Glennerster, 2011; Alam & Mahal, 2014). There is also evidence suggesting that uninsured households hold precautionary savings as an ex-ante risk mitigation strategy (see Chou et al., 2003; Pradhan & Wagstaff, 2005). Although these strategies might allow households to deal with the immediate financial impact of health shocks and smooth consumption, they can have important consequences for their income and welfare in the long term.

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For instance, Gertler et al. (2012) find that investment in productive assets, such as farm animals and land, leads to increases in agricultural income and sustained gains in consumption in the long term in Mexico. With regard to child labor, Beegle et al. (2008) find that it reduces educational attainment and marginal labor productivity in Tanzania over a ten-year horizon. And, assessing how a small sample of households in rural Cambodia fares one year after incurring debt to treat dengue, Van Damme et al. (2004) find that the majority had not been able to repay all debt and continued to pay high interest rates. Insuring households against the financial impact of health shocks might hence lead to improvements in household welfare in the long term by eliminating the need to engage in these costly self-insurance strategies.

In this paper, I assess the impact of a large national health insurance scheme, the Mexican *Seguro Popular*, on investment in productive assets and activities in rural areas. Over the past decades, a range of different health insurance schemes have emerged in developing countries with the aim of providing insurance against the adverse effects of health shocks for the big part of the population so far excluded from existing schemes (see Acharya et al., 2012; Wagstaff, 2010a, 2010b; Preker et al., 2002). Assessing their impact on a wide range of adverse outcomes associated with health shocks for their beneficiaries is important in order to be able to evaluate the welfare effects of these schemes comprehensively. So far, the growing literature evaluating the impact of these health insurance schemes has, however, focused mainly on their impact on out-of-pocket expenditures, utilization of health care services, and health status (see Acharya et al., 2012, for a review of nineteen studies on the impact of health insurance schemes for the informal sector on these outcomes in ten developing countries). Very few studies have assessed the impact of formal health insurance on the strategies households use to self-insure in their absence. Among these, most evidence exists on the impact of health insurance on child labor, schooling, and educational achievements, suggesting that health insurance does have beneficial impacts on these outcomes in a range of different countries and contexts (see Strobl, 2017; Alcaraz et al., 2016; Strupat, 2016; Landmann & Frölich, 2015). Less evidence exists regarding other commonly used coping and mitigation strategies (see Dekker and Wilms, 2010, for the impact of a private subsidized small-scale health insurance scheme for informal sector workers on asset sales and borrowing in Uganda; Liu, 2016, for the impact of a national health insurance scheme in rural China on human capital and agricultural investments). For Mexico, and the *Seguro Popular* program in particular, there exists only evidence on its impact on education outcomes, migration, and a portfolio of non-health expenditures, but not on investments in productive activities (see Alcaraz et al., 2016; Mahé, 2017;

Barofsky, 2010). This paper hence contributes to a comprehensive welfare assessment of the Mexican *Seguro Popular*. Furthermore, by focusing on a little researched impact of formal health insurance, but demonstrably important and costly strategy for poor households to deal with health risk, it also contributes to a better understanding of the impact of health insurance on productive activities more in general.

In the absence of insurance, health shocks can affect productive activities through health care expenditures in two ways. First, as an ex-ante risk mitigation strategy, households might choose to hold precautionary savings instead of investing in less liquid productive assets. For example, although not focusing on health related risk in particular, Bianchi and Bobba (2013) find that the ex-ante insurance effect of the Mexican *Oportunidades* cash transfer program is the driving force behind the decision to enter self-employment. And Gertler et al. (2012), who also examine the impact of *Oportunidades*, find that the cash transfer program increased investment in non-farm microenterprises and agricultural activities, which can be explained with the steady source of income implied by the program reducing income uncertainty.¹ Second, health shocks can affect productive assets of uninsured households ex-post (see Mitra et al., 2016, for Vietnam; Sparrow et al., 2014, for Indonesia; Liu, 2016, for China; Islam and Maitra, 2012, for Bangladesh; Bocoum et al., 2018, for Burkina Faso). For Mexico, there is so far no evidence on whether and how health shocks affected productive assets of uninsured households. Using panel data from the Mexican rural evaluation surveys of the *Oportunidades* cash transfer program, I start with an assessment of the relevance of uninsured health shocks for out-of-pocket expenditures and agricultural assets prior to the introduction of *Seguro Popular* to strengthen the motivation for the analysis of the impact of *Seguro Popular* on productive activities.

I then estimate the effect of the program on these outcomes and account for possible self-selection of households into the program using difference-in-differences estimation, as well as a propensity score matched difference-in-differences specification. I find that, although health shocks led to increases in health care expenditures and reductions in productive agricultural assets prior to the introduction of *Seguro Popular*, the insurance program did not increase agricultural assets for insured households and did not reduce their health care expenditures substantially. Moreover, the program mainly reduced spending for medical consultations but was not successful in protecting households from expenditures for medication, which is the more important component of

¹The positive effect of *Oportunidades* on investment found by Gertler et al. (2012) is nevertheless equally consistent with credit constraints preventing households from investing in productive activities, since the authors do not attempt to distinguish between the impact the transfers have through the relaxation of liquidity constraints versus insurance constraints, as Bianchi and Bobba (2013) do.

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their health care expenditures. Assessing heterogeneity of the effect with regard to household income shows that these results hold also for households with higher incomes whose productive investments might be more reactive to financial protection from health shocks. Average impact estimates are hence not driven by poorer households foregoing expenditures for health care before the introduction of *Seguro Popular*. I then discuss potential explanations for the absence of effects on productive investments and point to problems in the implementation of *Seguro Popular*, in particular the severe underfunding of the program, as a possible explanation.

The paper proceeds as follows: Section 4.2 describes the Mexican *Seguro Popular* social health insurance program. I then describe the data and identification strategy used in the analysis in section 4.3. Section 4.4 presents results on the effects of health shocks on out-of-pocket expenditures and productive assets prior to the introduction of the program, on the impact of *Seguro Popular* on these outcomes, and on whether the impact of *Seguro Popular* varies with household income. I discuss potential explanations for the results found here in section 4.5. Section 4.6 concludes.

4.2 The *Seguro Popular* health insurance scheme

The Mexican *Seguro Popular* is a national public health insurance scheme that was introduced in Mexico in 2003 to provide affordable and good-quality health care to the approximately 50 percent of Mexicans who, by then, were not insured through the Mexican social security institutions. Faced with enormous challenges in the provision of social protection in health, in particular the high prevalence of out-of-pocket health care spending, imbalances in the allocation of public funds between different groups of the population, as well as between states, and a low level of overall health spending which manifested itself in a deficient health infrastructure (see Frenk et al., 2006, for details on the challenges confronted by the reform), Mexico's health care system underwent a substantial reform in 2003. A major component of this reform was the introduction of *Seguro Popular*, a social health insurance scheme, with the aim to achieve protection against the financial risk of illness, explicitly entitle its beneficiaries to a comprehensive set of health care interventions, and provide demand-driven financing of health care.

The Mexican health care system had traditionally been characterized by a segmentation in the financing and provision of health care services for different groups of the population. Mexican social security, including health care, was provided, and continues to be provided, to formal-sector workers and public employees, including their families, through the *Instituto Mexicano del Seguro Social* (IMSS) and the *Instituto de Seguri-*

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dad y Servicios Sociales de los Trabajadores del Estado (ISSSTE), respectively. These schemes rely on a tripartite funding structure (contributions are made by the employee, the employer, and the federal government through taxes) and grant salaried workers access to health care from well-funded federal institutions, which is free at the point of delivery (see Frenk et al., 2006; Knaul et al., 2012, for details). All individuals who were not covered by these social security agencies (i.e. the self-employed, the unemployed, non-salaried and informal-sector workers, and those who do not work; hereafter the uninsured) constituted a “residual group” (Frenk et al., 2006) and received health care coverage through the *Secretaría de Salud y Asistencia* (SSA) under the Ministry of Health. Although estimates vary between 47 million (Bosch et al., 2012) and 60 million (Knaul et al., 2012), there is agreement that shortly before the introduction of *Seguro Popular* in 2003 this *residual group* amounted to approximately 50 percent of the Mexican population. Uninsured individuals could access the services provided through SSA at underfunded state and federal health care facilities, but had to pay a user fee (see Knaul et al., 2012; Frenk et al., 2006, for details).

While, at least in theory, the entire population had access to basic health care services, in reality the Mexican public health care system was extremely unequal in coverage, quality, and hence financial protection, with the poor being disproportionately disfavored. Public funds were overproportionately allocated to the population insured under the Mexican social security institutions IMSS and ISSSTE, as well as to richer states (Knaul & Frenk, 2005). In addition, funding for SSA was unstable, the benefit package provided through this scheme not clearly defined, and hence health care services covered under this scheme not comprehensive (see Frenk et al., 2006; Knaul et al., 2012). Waiting times, shortages of medicines, and poor quality of care were common, in particular for the uninsured, which in effect implied a rationing of health care services for this group of the population (Frenk et al., 2006; Knaul et al., 2012). As a result, households had to incur substantial out-of-pocket payments for health care. According to Knaul and Frenk (2005), out-of-pocket spending accounted for more than half of total health spending before the introduction of *Seguro Popular*. Moreover, impoverishing spending for health care was common, with each year between two and four million households either spending 30 percent or more of their disposable income² on health, or falling below the poverty line because of excessive spending for health care (Knaul & Frenk, 2005).

Seguro Popular was explicitly meant to offer protection against the financial consequences of health shocks, increase coverage and quality of public health care, and

²total income less spending on food

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ensure through a demand-driven financing model that the increases in the overall budget for health triggered by the health care reform in 2003 be used efficiently (Bosch et al., 2012; Knaul & Frenk, 2005). Individuals insured under *Seguro Popular* have free access at point of delivery to an explicit and comprehensive set of health care interventions, including essential interventions³, provided ambulatory and in general hospitals, as well as a package of high-complexity and high-cost health care interventions offered at specialized health care providers. As of 2014, the essential package encompasses 634 medicines, and 285 interventions, thereby providing coverage for 100 percent of treatments at the primary and about 90 percent at the secondary level (Comisión Nacional de Protección Social en Salud, 2014). In addition, a separate fund for protection against catastrophic health expenditures (the *Fondo de Protección contra Gastos Catastróficos*) covers 59 non-essential high-cost and high-complexity interventions such as for instance cancer treatments and antiretroviral therapy (Comisión Nacional de Protección Social en Salud, 2014). The explicit entitlement to a clearly defined and extensive set of health care services is an important novelty of *Seguro Popular* (Bosch et al., 2012; Knaul et al., 2012). The program was intended to rely on a tripartite funding structure, with financing coming from federal and state funds, and beneficiaries contributing through a progressive income-dependent copayment⁴ (see Knaul et al., 2012, for details on the financing structure of *Seguro Popular*). However, Knaul et al. (2012) note that by 2012 only about 1 percent of the families enrolled in *Seguro Popular* were paying the premiums, making *Seguro Popular* basically a fully subsidized public health insurance scheme that provides treatment to enrolled families at zero costs.

There is only one single eligibility criterion for enrollment in *Seguro Popular*: Eligible are those households and individuals who are not covered by the Mexican social security system, i.e. who do not work, are self-employed, or are informal workers. Thus, eligibility is independent of income (Bosch et al., 2012). Started as a pilot program in 2001 in five states, and launched gradually in the remaining states throughout 2003 to 2005, *Seguro Popular* has been operating in all 31 states and Mexico City (the Federal District) since 2005. Within states, the program was also rolled out sequentially, targeting first municipalities with high poverty incidence and with indigenous population,

³These are interventions for the basic specialties like internal medicine, general surgery, obstetrics and gynecology, paediatrics, and geriatrics (Frenk et al., 2006). The full list of essential interventions covered by *Seguro Popular* is given in Comisión Nacional de Protección Social en Salud (2014).

⁴The premium is determined based on a sliding-scale subsidy. In the early phases of the program households in the two poorest income deciles were exempted from co-payments. Later this exemption was extended to households in the four poorest income deciles and households with a pregnant woman or at least one child under the age of 4 years. For all other households the prepayment does not exceed 5 percent of disposable income. (see Frenk et al., 2006; Knaul et al., 2012; Bosch et al., 2012, for details on the co-payment modalities of *Seguro Popular*).

but with access to health facilities. The program was operating in all municipalities by the end of 2007. Take-up was rapid and substantial, and by 2012 enrollment of 52.6 million individuals, implying full coverage, had been achieved (Bosch et al., 2012; Knaul et al., 2012).

4.3 Data and identification

4.3.1 The data

The data used in the analyses comes from the *Encuesta de Evaluación de los Hogares Rurales* (ENCEL-rural), a publicly available panel survey implemented for the evaluation of the cash transfer program *Oportunidades* in rural areas.⁵ The ENCEL-rural panel consists of eight rounds of data, having been collected every six months between March 1998 and November 2000, and again once in 2003 and once in 2007 in highly deprived rural localities of seven Mexican states.⁶ The first six rounds covered 506 localities and started with a sample of 24,077 households. For the assessment of the impact of health shocks on health care expenditures, productive assets and activities prior to the introduction of *Seguro Popular*, I use data from the 1999 and 2000 rounds as they contain the information required for the construction of the dependent and independent variables (see Appendix 4.A for an overview of the availability of variables by survey round).

The analysis of the impact of *Seguro Popular* is based on data from the two last survey rounds, which cover the years 2003 and 2007, and focuses on a selected sample of 9,407 households. Although the 2003 survey round included a total of 34,202 households⁷, budgetary reasons, attrition, and the focus of the analysis of this paper led to a substantial reduction of the 2003-2007 panel sample. The 2003 survey round was conducted in 657 evaluation localities, but for budgetary reasons, the 2007 survey round excluded all those localities from the evaluation sample in which fewer than twenty households had been interviewed in the round of 2003, resulting in an exclusion of 103 of the original 657 localities. Furthermore, due to budgetary and operational reasons, in the 2007 survey round, questionnaires were designed in different versions, of which not all collected information on the variables of interest here. Information on the

⁵The ENCEL-rural is available online at: https://evaluacion.prospera.gob.mx/en/eval_cuant/p.bases_cuanti.php.

⁶These states are Hidalgo, Puebla, Guerrero, Veracruz, Michoacán, San Luis Potosí and Querétaro.

⁷For reasons related to the objective of the ENCEL-rural to evaluate the impact of *Oportunidades*, the 2003 round surveyed an additional 151 localities, resulting in a total sample of 34,202 households in this round.

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assignment of questionnaires is not available publicly and could not be obtained from the responsible authorities despite multiple attempts. As a result I determine the baseline sample in 2003 to be used for the analysis using the households resurveyed in the 2007 round. Therefore, households who were intended to be asked information on the variables of interest here in 2007 but attrited are not included in the baseline sample. Finally, I restrict my analysis here to the households eligible for *Seguro Popular* in 2003. This includes households in which the household head did not have any entitlement to health insurance under IMSS, a private health insurance, or any other insurance scheme in 2003, and which were located in municipalities in which the program was not yet operating in 2003.⁸ A detailed overview of the restrictions and corresponding sample reductions that led to the the final analysis sample of 9,407 households, the distribution of questionnaire types in 2007 and its implications for analysis can be found in figure 4.B.1 and Table 4.B.1 of Appendix 4.B, respectively.

4.3.2 Dependent variables

I consider the following dependent variables for health care expenditures and investment in productive activities: Information on household expenditures for health care comes from the household expenditure module. The module asks households for the amounts spent on medicines and on medical consultations in the past month. I consider these amounts, as well as the total amount spent on both of these categories, and binary variables indicating positive expenditures on medicines, medical consultations and their total as dependent variables. For productive assets and activities, I closely follow Gertler et al. (2012) and use information on ownership of animals at the time of the survey, land use over the 12 months preceding the interview, and involvement in self-owned non-farm activities that generated income during the month preceding the interview to construct dependent variables. In addition, I consider data collected in the 2003 and 2007 rounds on household involvement in agricultural and non-farm income generating activities over the past twelve months before the interview and on ownership of agricultural and non-farm equipment and machinery at the time of the survey. Animals are divided into two separate categories as defined in Gertler et al. (2012): production animals, whose meat and/or byproducts (milk, cheese, eggs, etc.) are sold and consumed, and draft animals, used for plowing the fields and for transportation

⁸To decide when *Seguro Popular* started to operate in a given municipality, I use administrative data on enrollment in *Seguro Popular* used in Bosch and Campos-Vázquez (2014), which Raymundo Miguel Campos-Vázquez kindly provided. I follow Bosch and Campos-Vázquez (2014) to establish program start in a given municipality if the number of affiliates was higher than ten individuals in a given quarter and remained higher than ten individuals for at least four consecutive quarters.

purposes. For each of these two categories, I consider indicators for ownership, and continuous variables measuring the quantity owned and the monetary value as dependent variables. Usage of land for agricultural purposes is also examined as a binary variable, and as a continuous variable measuring the number of hectares used. Operation of agricultural and non-farm enterprises is captured through binary variables indicating whether households engaged in the sale of agricultural products or the sale of non-farm goods or services. With regard to machinery and equipment used in agricultural or in non-farm enterprises, I consider binary indicators for ownership as well as monetary values. Data availability for the dependent variables varies over the different survey rounds, so that outcome variables assessed vary depending on the survey waves included in the analysis. Appendix 4.A provides an overview of availability of variables by survey round and describes the construction of dependent variables more in detail.

4.3.3 Key characteristics of the study population prior to *Seguro Popular*

The ENCEL-rural survey was collected in small credit constrained localities with between 50 to 2,500 inhabitants in marginalized rural areas, characterized by very high deprivation levels (see Gertler et al., 2012; Instituto Nacional de Salud Pública, Mexico, 2005). Table 4.1 provides key characteristics of the study population in the period prior to the introduction of *Seguro Popular*.⁹ In the period between 1999 and 2000, over 70 percent of households in the sample own production animals and on average about 60 percent of households use land for agricultural purposes. Overall, about 86 percent of households engage in agricultural activities while, on average, only 3 percent of households operate a non-farm business. Turning to household expenditures for health care, about 11 percent of households spent on medication in the month preceding the survey and about 6 percent on medical consultations. Conditional on spending for health care, total monthly health care expenditures were between 232 and 243 Mexican pesos (approximately 24 to 25 US-Dollar in March 1999)¹⁰ and this amounted to 18-21 percent of total household expenditures on food and non-food items. Expenditures for medicines

⁹Table 4.1 covers the period 1999-2000, which includes the data used for the analysis of the impact of health shocks on health care expenditures and productive investments prior to the introduction of *Seguro Popular*. Although *Seguro Popular* was not yet introduced in a large part of the sample surveyed in the 2003 ENCEL-round, the data from this round are not included in this analysis as it did not provide the same amount of information on the health status of the household head, as the 1999 and 2000 rounds. Table 4.3 however shows that the characteristics of the study population remain similar in 2003.

¹⁰One Mexican Peso was equivalent to 0.10325 US-Dollar on March 15, 1999 (<https://www.oanda.com/currency/converter/>).

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account for about 80 percent of total monthly health care expenditures. According to Gertler et al. (2012), credit opportunities are very limited in the survey localities with approximately 99 percent of these localities not having a formal credit institution. Table 4.1 shows that only 5 to 6 percent of households received a loan in the six months before the survey. This includes informal loans from relatives, friends or other persons, which were also most frequent (67 to 77 percent). Another form of insurance against the cost of health shocks might have been transfers from the *Oportunidades* program. In 1999, 33 percent of households received these cash transfers, with this proportion increasing to 61 percent in November 2000.

Table 4.1: Key characteristics of sample households 1999-2000

	March 1999			Nov. 1999			March 2000			Nov. 2000		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
<i>A. Productive activities</i>												
Draft animals:												
Ownership = 1	22,333	0.29	0.45	23,259	0.30	0.46	22,621	0.31	0.46	22,366	0.62	0.48
Quantity ^a	6,395	1.93	3.03	6,898	2.14	8.72	7,012	2.14	7.38	13,970	5.08	26.41
Production animals:												
Ownership = 1	22,333	0.71	0.45	23,259	0.74	0.44	22,623	0.74	0.44	n.a.	n.a.	n.a.
Quantity ^a	15,957	9.77	9.90	17,135	10.06	13.00	16,827	9.43	11.10	22,366	0.04	0.19
Land:												
Usage = 1	22,334	0.57	0.50	23,259	0.74	0.44	22,627	0.59	0.49	22,366	0.62	0.48
Number of hectares used ^b	12,695	2.94	8.69	14,850	3.06	12.73	13,397	2.57	3.93	13,970	5.08	26.41
Engaged in agricultural activities = 1	22,334	0.84	0.37	23,268	0.86	0.35	22,627	0.87	0.33	n.a.	n.a.	n.a.
Operates a non-farm microenterprise = 1	22,334	0.03	0.16	23,268	0.00	0.07	22,627	0.05	0.22	22,366	0.04	0.19
<i>B. Health care expenditures</i>												
Household expenditures for medication (last month)												
Spending = 1	22,298	0.15	0.35	23,262	0.12	0.33				22,360	0.07	0.26
Amount spent (in pesos) ^e	3,234	184.80	193.83	2,795	188.41	196.99				1,600	190.54	193.99
Proportion of total health care expenditures	3,400	0.81	0.27	2,966	0.82	0.27				1,726	0.79	0.30
Household expenditures for medical consultations (last month)												
Spending = 1	22,312	0.08	0.27	23,267	0.06	0.24				22,362	0.04	0.19
Amount spent (in pesos) ^e	1,760	111.43	141.74	1,424	123.75	161.48				847	137.17	164.39
Proportion of total health care expenditures	3,400	0.19	0.27	2,966	0.18	0.27				1,726	0.21	0.30
Total household expenditures on health care (last month)												
Spending = 1	22,289	0.15	0.36	23,262	0.13	0.33				22,359	0.08	0.27
Amount spent (in pesos) ^e	3,400	231.29	262.81	2,966	236.69	270.65				1,726	243.19	271.95
Proportion of total food and non-food expenditures ^e	3,355	0.19	0.16	2,953	0.19	0.16				1,709	0.19	0.16
<i>C. Household characteristics</i>												
Household size												
Head is male = 1	22,334	5.78	2.83	23,268	5.83	2.87	22,627	5.94	2.90	22,366	6.01	2.94
	22,334	0.89	0.32	23,268	0.88	0.33	22,620	0.87	0.33	22,225	0.89	0.31

Continued on next page

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Table 4.1 – continued from previous page

	March 1999			Nov. 1999			March 2000			Nov. 2000		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Age of household head	22,297	47.61	16.73	22,540	48.12	16.09	22,580	48.92	16.17	22,307	49.32	16.21
Total non-health household expenditures (monthly, in pesos)	21,979	803.58	583.62	23,149	797.68	496.10				22,208	775.62	485.05
Received a loan over past 6 months = 1	22,334	0.05	0.21							22,366	0.06	0.59
Receives <i>Oportunidades</i> transfers	22,334	0.33	0.47	23,268	0.33	0.47	22,627	0.55	0.50	22,366	0.61	0.49

Note: Monetary values are expressed in March 1999 prices.

All monetary values, quantities of animals and land used have been trimmed at the 99 percentile.

n.a.: not applicable

^aConditional on ownership.

^bConditional on usage.

^cInvolvement in agricultural activities is defined as owning animals or using land.

^dConditional on spending.

4.3.4 Estimation and identification strategy

I start with an assessment of the relevance of uninsured health shocks for out-of-pocket expenditures and agricultural assets prior to the introduction of *Seguro Popular* to strengthen the motivation for the analysis of the impact of *Seguro Popular* on productive assets and activities. To eliminate any potential bias that might come from unobservable time-invariant household characteristics which affect both health status and outcomes, such as household preferences for instance, I estimate the impact of health shocks on health care expenditures and productive assets, using the following fixed-effects regression specification:

$$y_{it} = \gamma_i + \lambda_t + \delta Health_{it} + \beta X_{it} + \varepsilon_{it} \quad (4.1)$$

for household i at time t , with y_{it} the outcome of interest, $Health_{it}$ the health status of the household head¹¹, and X_{it} a vector containing the following time-varying covariates: a dummy variable indicating whether the household head is male, age of the household head, household size, a dummy variable indicating whether the household receives cash-transfers from the *Oportunidades* program, and a community-specific wave fixed effect to account for community level shocks that might impact outcomes.¹² For binary outcome variables, I hence estimate a linear probability model with fixed effects. To determine the health status of the household head, I follow Barros (2008) and Gertler and Gruber (2002) and use information on the physical ability to perform activities of daily living (ADLs) collected in the survey, which I combine into an index that takes on a value of one if the household head can perform all ADLs without difficulty, and zero if he or she cannot perform any ADLs (see Appendix 4.C for details on the construction of the index). The parameter δ then gives the impact of moving from complete disability to being completely healthy on health care expenditures and productive assets.

The ENCEL-survey did not ask households for the health insurance status of their members until 2003. There is hence no way of assessing the impact of health shocks

¹¹I follow Barros (2008) and focus only on the impact of health shocks to the household head as he or she is the main source of household income (Barros, 2008).

¹²Since health care expenditures, but also the assets in levels have non-negative, skewed, non-normal distributions, a linear estimation model might not be appropriate as the error terms could be heteroskedastic. The existing literature has approached this differently. While some authors use linear regression frameworks on outcomes in levels (see Barros, 2008; Gertler & Gruber, 2002), others have done so on log-transformed outcomes, estimating in fact the impact at the intensive margin (see Mitra et al., 2016), or applied poisson fixed effects models (see Sparrow et al., 2014; Bocoum et al., 2018). I use robust Huber-White standard errors to account for potential heteroskedasticity. Nevertheless, in order to ensure robustness of the results for outcomes in levels, I also estimate the linear fixed effects regression specification shown above on log-transformed outcomes and use a poisson fixed effects model. Appendix 4.G presents results of these estimation strategies.

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Table 4.2: Enrollment in *Seguro Popular* by survey round

Round	Number of households eligible in 2003		Total
	Not enrolled in <i>Seguro Popular</i>	Enrolled in <i>Seguro Popular</i>	
2003	9,407	0	9,407
2007	6,497	2,910	9,407

on the sample of uninsured households only, and equation (4.1) estimates the average effect of health shocks for all households in the sample. This includes households who have access to health care through the SSA and are the target group of *Seguro Popular*, as well as households with social security provided through IMSS and ISSSTE, or with health insurance provided by other institutions. Therefore, results are likely to rather constitute a lower bound for the impact of health shocks. Nevertheless, given the focus of the *Oportunidades* cash-transfer program and its evaluation sample on poor rural households in highly deprived localities, it can be assumed that the majority of the household heads in the sample does not have a formal or public sector job and hence does not have formal health insurance through social security or other public or private institutions. Table 4.D.1 in Appendix 4.D further corroborates this conjecture by showing that in 2003 only 8 percent of households in the ENCEL-rural had access to health care under IMSS, ISSTE, through other public or parastatal entities, employers, or other institutions.

I next assess, whether *Seguro Popular* was able to insure households against the negative consequences of health shocks through an impact on health care expenditures and, ultimately, on productive assets and activities of beneficiary households. Enrollment in *Seguro Popular* did not occur at random, as participation was voluntary for eligible households once the program was operating in a given municipality. A number of impact evaluations of *Seguro Popular* have exploited the fact that program roll-out was sequenced over time and space, applying difference-in-differences or triple-difference estimation strategies to identify causal effects by eliminating potential sources of bias resulting from self-selection of households into the program (e.g. Azuara & Marinescu, 2013; Barros, 2008; Bosch & Campos-Vázquez, 2014). In the 2007 survey round of the ENCEL-rural *Seguro Popular* was, however, already operating in all municipalities included in the sample. Nevertheless, as Table 4.2 shows, not all eligible households had enrolled in *Seguro Popular* by 2007. I hence use a difference-in-differences strategy that takes the outcome dynamics of households that were eligible for *Seguro Popular* in 2003 and had not enrolled in the program by 2007 as the counterfactual to identify the causal impact of *Seguro Popular* on the variables of interest. According to Bosch

et al. (2012) and Knaul et al. (2012), by 2012 all eligible households were enrolled in the program, so that the difference-in-differences strategy essentially compares early enrollers (until 2007) to late enrollers (after the observation period).

I estimate this using the following fixed-effects regression specification:

$$y_{it} = \gamma_i + \lambda_t + \delta \text{SeguroPopular}_{it} + \varepsilon_{it}. \quad (4.2)$$

for household i at time t , with $t=2003$ (before the introduction of *Seguro Popular*) and 2007 (after the introduction of *Seguro Popular*), y_{it} the outcome of interest, and $\text{SeguroPopular}_{it}$ a dummy variable indicating if household i is enrolled in *Seguro Popular* at time t .¹³ The impact of *Seguro Popular* on the outcome variable of interest is captured by δ . To determine enrollment in *Seguro Popular* at the household level, I use the enrollment status of the household head, given that the ENCEL-rural collects information on enrollment in *Seguro Popular* at the individual level. According to Article 77 Bis 4 of the Mexican General Health Law, the family is the unit of protection in *Seguro Popular* and the family representatives enroll the other family members (children and elderly) in the program, so that a household should be insured if the head is enrolled in *Seguro Popular*.

In order to increase precision of the impact estimate, I also estimate the following specification, which includes additional time-varying covariates :

$$y_{it} = \gamma_i + \lambda_t + \delta \text{SeguroPopular}_{it} + \beta X_{it} + \varepsilon_{it}. \quad (4.3)$$

The vector X_{it} includes a dummy variable indicating whether the household head is male, age of the household head, household size, a dummy variable indicating whether the household receives cash-transfers from the *Oportunidades* program, and a municipality-specific wave effect. The latter would also eliminate potential biases in the treatment effect as estimated in equation (4.2), coming from outcome trajectories that are associated with enrollment of households, if, for instance, municipalities in which the program was rolled out first shared specific outcome trends which differ from those of municipalities in which it was rolled out later, and longer exposure to the program

¹³An alternative to estimating the difference-in-differences model with a fixed-effects regression specification would be an ordinary least squares (OLS) regression specification. While the fixed-effects specification estimates the impact on the balanced panel only, the OLS specification uses the unbalanced panel. Lechner et al. (2016) show that the difference-in-differences estimator is inconsistent in the two-period case for some cases of selective non-response, while the fixed-effects estimator yields a consistent estimator. The authors consider deviating OLS and fixed-effects estimates as evidence that non-response is not ignorable for the difference-in-differences estimation with two periods. I opted for the fixed-effects specification as results differed slightly across the two approaches, although not in a way that would lead to different conclusions.

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increased the probability of enrollment.

Table 4.3 provides summary statistics of household characteristics and outcomes of interest at baseline, i.e. in 2003, and compares households who were enrolled in *Seguro Popular* by 2007, the treatment group, to eligible households who did not enroll, the comparison group. Treatment and comparison households differ significantly with regard to agricultural assets, health care expenditures, household and household head's characteristics, as well as some community characteristics related to economic activity. Overall, households in the treatment group have less agricultural assets at baseline, lower household income and also score worse on other proxies of wealth, such as whether their home has a dirt floor or electricity. They are also significantly more likely to receive cash transfers from the *Oportunidades* program and live in communities which have significantly lower minimum wages. At the same time, treatment households have significantly lower health care expenditures, which are mainly driven by the difference in expenditures for medication, and are significantly less likely to spend for medical consultations than households in the comparison group.

Table 4.3: Baseline characteristics of households and comparison of means (2003)

	Comparison group			Treatment group		
	N	Mean	SD	N	Mean	SD
<i>A. Productive activities</i>						
Operates a microenterprise = 1	6496	0.37	0.484	2910	0.39	0.487
Operates an agricultural microenterprise = 1	6495	0.34	0.475	2910	0.36	0.479
Operates a non-farm microenterprise = 1	6495	0.07	0.254	2910	0.07	0.248
Draft animals:						
Ownership = 1	6475	0.28	0.448	2899	0.25***	0.433
Quantity ^a	1780	1.79	1.135	717	1.63***	1.059
Monetary value (in pesos) ^a	1776	3853.64	4665.205	716	3014.41***	3649.615
Production animals:						
Ownership = 1	6476	0.70	0.460	2901	0.73***	0.443
Quantity ^a	4456	9.91	8.745	2111	9.36**	8.091
Monetary value (in pesos) ^a	4446	3188.45	6747.742	2113	2262.24***	5308.766
Agricultural machinery and equipment:						
Ownership = 1	6474	0.09	0.293	2899	0.07***	0.254
Monetary value (in pesos) ^a	610	4729.90	19910.359	200	5324.10	20278.918
Machinery and equipment used in non-farm business:						
Ownership = 1	6490	0.05	0.210	2910	0.04	0.197
Monetary value (in pesos) ^a	296	2.23	3.626	113	2.08	2.626

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4.3 Data and identification

Table 4.3 – continued from previous page

	Comparison group			Treatment group		
	N	Mean	SD	N	Mean	SD
Land:						
Usage = 1	6492	0.61	0.487	2910	0.65***	0.478
Number of hectares used ^b	3942	4.13	7.744	1884	3.74*	7.535
<i>B. Health care expenditures</i>						
Household expenditures for medication (last month)						
Spending = 1	6469	0.22	0.417	2904	0.22	0.412
Amount spent (in pesos) ^e	1446	280.46	261.807	628	238.44***	246.924
Household expenditures for medical consultations (last month)						
Spending = 1	6475	0.15	0.353	2908	0.12***	0.327
Amount spent (in pesos) ^e	943	211.86	236.295	354	215.85	231.242
Total household expenditures on health care (last month)						
Spending = 1	6463	0.26	0.436	2904	0.24	0.428
Amount spent (in pesos) ^e	1653	363.36	379.372	703	320.84**	356.610
<i>C. Household characteristics</i>						
Household size	6497	5.81	2.898	2910	6.09***	2.888
Children aged 0 to 7 years = 1	6497	0.54	0.499	2910	0.59***	0.491
Children aged 7 to 18 years = 1	6497	0.64	0.481	2910	0.68***	0.466
Adults aged 18 to 54 years = 1	6497	0.93	0.260	2910	0.94***	0.230
Adults aged 55 years or older = 1	6497	0.45	0.497	2910	0.42**	0.494
Women in childbearing age = 1	6497	0.86	0.347	2910	0.90***	0.305
Entrepreneur in the household = 1	6497	0.22	0.413	2910	0.22	0.417
Total monthly household income	6424	2877.62	2648.531	2889	2625.80***	2331.914
Home ownership = 1	6463	0.90	0.302	2895	0.91	0.293
Dirt floor = 1	6452	0.43	0.495	2893	0.53***	0.499
Electricity = 1	6467	0.91	0.288	2902	0.88***	0.327
Oportunidades treatment area = 1	6497	0.75	0.431	2910	0.85***	0.357
Household receives oportunidades = 1	6497	0.45	0.498	2910	0.58***	0.493
<i>D. Head of household characteristics</i>						
Male = 1	6486	0.87	0.340	2906	0.87	0.335
Age	6484	46.59	16.137	2906	45.33***	15.153
Indigenous = 1	6482	0.34	0.473	2906	0.42***	0.494
Alphabetized = 1	6454	0.71	0.454	2891	0.73*	0.446
Never attended school = 1	6468	0.27	0.443	2900	0.25*	0.433
More than primary school education = 1	6468	0.10	0.300	2900	0.11*	0.315
Primary school not completed = 1	6486	0.44	0.496	2906	0.42	0.494
Primary school completed = 1	6486	0.19	0.393	2906	0.21**	0.410
<i>E. Community characteristics</i>						

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Table 4.3 – continued from previous page

	Comparison group			Treatment group		
	N	Mean	SD	N	Mean	SD
Principal productive activity is agriculture = 1	6497	0.94	0.244	2910	0.97**	0.180
Minimum wage	5776	63.16	66.731	2765	50.96**	32.963
Mobile health brigades = 1	6341	0.68	0.468	2910	0.61	0.487
Availability of health care institutions = 1	6497	0.97	0.174	2910	0.98	0.130
Average distance to health care institution is less than 50km = 1	6497	0.71	0.453	2910	0.67	0.470
Availability of health care services = 1	6497	0.98	0.152	2910	0.98	0.127
Average distance to health care services is less than 50km = 1	6497	0.49	0.500	2910	0.47	0.499

Note: For community characteristics, standard errors of differences in means have been clustered at the community level.

Monetary values are expressed in baseline (2003) prices.

All monetary values, quantities of animals and land used have been trimmed at the 99 percentile.

*, **, and *** indicate differences in means between comparison and treatment group at the 10%, 5%, and 1% levels respectively.

^a Conditional on ownership.

^b Conditional on usage.

^c Conditional on operating an agricultural microenterprise.

^d Conditional on operating a non-farm microenterprise.

^e Conditional on spending.

These baseline differences do not necessarily pose a threat to the identification of the causal effect of *Seguro Popular* on the outcomes of interest in specifications (4.2) and (4.3).¹⁴ Nevertheless, in order to construct a comparison group that is more similar to the treatment group, and in order to reduce a potential selection bias present in the specifications above, I also estimate a specification which combines the difference-in-differences approach with propensity score matching to pair treatment group households with observationally similar households that did not enroll in *Seguro Popular* by 2007. Households eligible for *Seguro Popular* are matched on the probability of enrolling in

¹⁴The difference-in-differences estimator $\hat{\delta}$ estimated in these specifications provides an unbiased estimate of the impact of *Seguro Popular* on the variables of interest, under the assumption that the outcome of interest would have followed the same trend over time for insured and uninsured, but eligible, households in the absence of *Seguro Popular*. Appendix 4.E.1 shows trends in outcome variables, for which data exists in the periods prior to the introduction of *Seguro Popular*, as well as afterwards. As the 2003 round of the ENCEL-rural added new households to the survey, not interviewed previously, pre-2003 data is only available for about 50 percent of the sample used in the analysis of the impact of *Seguro Popular* on productive activities and health care expenditures, and Figure A.2 only includes this part of the sample.

the scheme, the so called propensity score, based on pre-program, i.e. 2003, observable socio-economic characteristics of the household and community characteristics, which are believed to influence their decision to enroll in the scheme as well as the outcomes of interest here. In particular, I include characteristics of the household head measured at baseline, such as age, gender and whether he or she is indigenous, socioeconomic characteristics of the household related to composition and wealth, variables capturing the involvement in productive activities, and relevant community characteristics such as availability of health care institutions, in the estimation of the propensity score. In addition, I include the number of quarters the household had been exposed to *Seguro Popular* in 2007 as a determinant of the propensity score. Details on the estimation of the propensity score can be found in Appendix 4.F.

The matched difference-in-differences estimator is then :

$$\hat{\delta}_M(S) = \frac{1}{n_1} \sum_{i \in I_1} [(y_{i,2007} - y_{i,2003}) - \sum_{j \in I_0} W(i, j)(y_{j,2007} - y_{j,2003})], \text{ for } P(X) \in S, \quad (4.4)$$

where I_1 is the set of eligible households who enrolled in *Seguro Popular*, I_0 the set of households in the comparison group, and n_1 the number of households in the treatment group. $W(i, j)$ is the weight attached to household j from the comparison group in constructing the counterfactual outcome trend for treatment household i . The set S is the subset of $(0,1)$ for which values of the propensity score $P(X)$ exist in both the treatment and comparison group samples, called the common support. As an algorithm for the propensity score matching, I chose nearest neighbor matching with replacement. Preference for nearest neighbor matching was given because of its property of bias reduction when constructing comparisons, as opposed to other algorithms, like radius or kernel matching (see Caliendo & Kopeinig, 2008).¹⁵ Likewise, replacement was chosen to ensure closer matches for all treatment group households and this way reduce potential bias. Since there are more than twice as many households in the comparison group than households in the treatment group at baseline, the number of comparison matches, i.e. nearest neighbors, for each treated observation is set to two.¹⁶ In the above specification, $W(i, j)$ is hence 0.5 for the two comparison group households with the propensity score closest to the household of the treatment group who is being matched, and zero for all other comparison group households.

¹⁵As sample size increases, different matching algorithms should however yield the same results, given their consistency (Smith, 2000).

¹⁶A larger number of nearest neighbors reduces treatment effect variance, although at the cost of increased potential bias. As a robustness check, I also estimate specification (4.4) with 1 and 3 nearest neighbors and present results in Table 4.H.1 of Appendix 4.H.

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4.4 Results

4.4.1 The effects of health shocks on expenditures for health care and productive assets prior to *Seguro Popular*

Table 4.4 presents the results for a positive change in the health status (moving from completely sick to completely healthy, as measured by the ADL index) on health care expenditures in panel A, and on productive assets and activities in panel B. Negative coefficients hence imply that deteriorations in the health status of the household head led to increases in the outcome of interest, while positive coefficients represent reductions as a result of illness. Appendix 4.G demonstrates that results for outcomes in levels are robust across different regression models. Panel A of Table 4.4 shows that a negative health shock resulted in increases in the probability of spending for health care and in the amount spent, prior to the introduction of *Seguro Popular*. Households are 18 percentage points more likely to spend on health care if the household head's health status deteriorates and total monthly health care spending increases by about 61 pesos (about 8 percent of total monthly non-health household expenditures)¹⁷ on average. Expenditures for medication are more responsive to health shocks than those for medical consultations, rising by 44 pesos versus 18 and increasing in probability by 17 percentage points versus 10. As regards the impact on productive assets, negative health shocks significantly reduced the probability of holding draft animals by 2 percentage points and the quantity of production animals by approximately 1 production animal. There is also a significant impact on the probability of using land of 4.6 percentage points. Negative health shocks, however, have a statistically significant but small positive effect on operating a non-farm enterprise. Illness of the household head increases the likelihood of operating a non-farm business by 1 percentage point. This seems to contradict the reasoning that uninsured health shocks led to decreases in productive investments and assets prior to the introduction of *Seguro Popular*. Nevertheless, it could also suggest that involvement in non-farm activities is itself a coping strategy of uninsured households when faced by a health shock. Adhvaryu and Nyshadham (2017) provide evidence that, in Tanzania, individuals experiencing health shocks switch from agricultural to non-farm activities, and presume that illness affects productivity more in physically more demanding sectors, like agriculture, than in non-farm enterprises. Moreover, they find that family members of the person hit with a health shock, and who are not ill, also shift labor from agricultural to non-farm activities, and

¹⁷Total monthly household non-health expenditure was 793 Mexican pesos on average in the 1999-2000 period (see Table 4.1).

explain this with complementarities in household labor. As entry costs for non-farm self-employment are low in many sectors in Mexico (McKenzie & Woodruff, 2006), households should be able to switch activities easily when the head's health status deteriorates.

Table 4.4: Impact of health shocks on health care expenditures, productive assets and activities prior to 2003 (see table notes for the underlying estimation specifications)

	(1)	(2)	(3)
<i>Panel A. Impact on Health Care Expenditures</i>			
	(a) Medication	(b) Medical consultations	(c) Total health care expenditures
Probability of spending	-0.173*** (0.012)	-0.102*** (0.009)	-0.176*** (0.012)
Number of Obs.	64,361	64,381	64,352
Amount spent	-43.678*** (3.863)	-18.128*** (2.241)	-61.472*** (5.414)
Number of Obs.	64,361	64,381	64,352
<i>Panel B. Impact on Productive Assets and Activities</i>			
	(d) Draft animals	(e) Production animals	
Ownership=1	0.023** (0.011)	0.016 (0.012)	
Number of Obs.	65,140	65,142	
Quantity	0.001 (0.082)	0.934*** (0.253)	
Number of Obs.	65,140	65,142	
	(f) Land		
Usage=1	0.046*** (0.010)		
Number of Obs.	85,802		
Number of Hectares Used	-0.207 (0.352)		
Number of Obs.	85,802		
	(g) Non-farm microenterprise		
Operates=1	-0.011** (0.005)		
Number of Obs.	85,802		

Note: Monetary values are expressed in March 1999 prices.

Results for binary outcomes (probability of spending, ownership, usage, and operation) are estimated based on a linear probability model with fixed effects. Results for amounts spent, quantities of animals and number of hectares used are based on a linear fixed effects regression model. All specifications include the following time-varying covariates: a dummy variable indicating whether the household head is male, age of the household head, household size, a dummy variable indicating whether the household receives cash-transfers from the Oportunidades program, and a community-specific wave fixed effect to account for community level shocks that might impact outcomes.

Results shown in panel A rely on the following ENCEL-rural rounds: March and November 1999, and November 2000. Results shown in panel B, (d) and (e) rely on data from March and November 1999, and March 2000. Results shown in panel B, (f) and (g) rely on data from all 1999 and 2000 rounds.

Robust standard errors are given in parentheses.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

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4.4.2 The impact of *Seguro Popular* on expenditures for health care and productive investment

Table 4.5 presents results from the difference-in-differences specifications given in equations (4.2) and (4.3) in columns (1) and (2), respectively, and from the matched difference-in-differences estimator in column (3). The estimates of the impact of *Seguro Popular* on health care expenditures are shown in panel A. In this panel, all coefficients but one have negative signs, yet only those for the probability of spending on medical consultations are significantly different from zero in all three specifications. The estimated impact for this outcome ranges from 1.8 to 2.4 percentage points, depending on the specification estimated. With the exception of the results on total health care expenditures, coefficients in panel A are comparable across the three different estimation strategies, suggesting that the results coming from the difference-in-differences specifications (4.2) and (4.3) do not suffer from selection bias. Table 4.H.1 in Appendix 4.H shows that this remains true if specification (4.4) is estimated with 1 or 3 nearest neighbors.

Table 4.5: Impact of *Seguro Popular* on health care expenditures and investment in productive activities (see table notes for the underlying estimation specifications)

	(1)	(2)	(3)
<i>A. Health care expenditures</i>			
Medication, probability of spending	-0.009 (0.013)	-0.011 (0.013)	-0.013 (0.016)
Medication, amount spent (in pesos)	-2.688 (5.034)	-3.746 (5.113)	-3.463 (6.184)
Med. consultations, probability of spending	-0.018* (0.010)	-0.022** (0.010)	-0.024* (0.013)
Med. consultations, amount spent (in pesos)	-8.022** (3.465)	-9.236*** (3.541)	-6.182 (4.311)
Total health care exp., probability of spending	-0.008 (0.013)	-0.013 (0.013)	0.001 (0.017)
Total health care exp., amount spent (in pesos)	-10.795 (7.293)	-12.967* (7.433)	-1.920 (8.654)
<i>B. Investment in productive activities</i>			
Operates a microenterprise = 1	-0.017 (0.013)	0.001 (0.013)	-0.017 (0.013)
Operates an agricultural microenterprise = 1	-0.011 (0.012)	0.009 (0.012)	-0.009 (0.011)
Operates a non-farm microenterprise = 1	-0.001 (0.007)	-0.002 (0.007)	-0.005 (0.007)
Agricultural machinery and equipment: Ownership = 1	0.007 (0.006)	0.002 (0.006)	-0.015** (0.007)
Agricultural machinery and equipment: Monetary value (in pesos)	-124.463 (164.738)	-143.023 (166.814)	-1.887 (190.833)

Continued on next page

4.4 Results

Table 4.5 – continued from previous page

	(1)	(2)	(3)
Draft animals: Ownership = 1	-0.003 (0.010)	-0.002 (0.010)	-0.026** (0.012)
Draft animals: Quantity	0.026 (0.021)	0.022 (0.021)	-0.036 (0.022)
Draft animals: Monetary value (in pesos)	167.408*** (58.829)	162.738*** (60.466)	-39.011 (56.352)
Production animals: Ownership = 1	0.007 (0.014)	0.015 (0.014)	0.030* (0.018)
Production animals: Quantity	0.262 (0.214)	0.387* (0.217)	-0.026 (0.270)
Production animals: Monetary value (in pesos)	434.178*** (118.541)	389.867*** (120.311)	56.449 (113.817)
Machinery and equipment used in non-farm business: Ownership = 1	0.003 (0.005)	0.004 (0.005)	0.005 (0.006)
Machinery and equipment used in non-farm business: Monetary value (in pesos)	20.198 (43.556)	25.522 (47.532)	51.688 (48.849)
Land: Usage = 1	-0.012 (0.013)	-0.003 (0.013)	0.008 (0.016)
Land: Number of hectares used	-0.238 (0.343)	-0.017 (0.349)	0.374 (0.408)
Number of Obs.	18,813	18,787	8,229
Covariates		✓	
Matching			✓

Note: Monetary values are expressed in baseline (2003) prices.

Column (1) shows regression coefficients coming from the fixed-effects specification given in equation (4.2). Robust standard errors are given in parentheses.

Column (2) shows regression coefficients coming from the fixed-effects specification given in equation (4.3), which includes the following covariates: a dummy variable indicating whether the household head is male, age of the household head, household size, a dummy variable indicating whether the household receives cash-transfers from the *Oportunidades* program, and a municipality-specific wave fixed effect. Robust standard errors are given in parentheses.

Column (3) shows regression coefficients for nearest neighbor propensity score matching on first differenced outcomes, using two nearest neighbors (plus potential ties) with replacement. Abadie-Imbens standard errors are given in parentheses.

The number of observations varies depending on the outcome considered. Number of observations are given as the maximum number of observations over all outcomes for a given specification.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Overall, these findings suggest that *Seguro Popular*'s impact on health care expenditures has been modest with reductions on the probability of spending on medical consultations for eligible households of approximately two percentage points, and no effect on expenditures for medication. Results on the amount spent on total health care expenditures and on medical consultations are not robust across the three specifications and insignificant in the matched difference-in-differences specification. A lack of power to detect statistically significant effects could be driving the results for health care expenditures found here, as the health care expenditure outcomes examined here

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are given for a horizon of one month, measured at one follow-up point in time only, and since autocorrelation is presumably low, considering that these expenditures are driven mostly by episodic health conditions.¹⁸ Notwithstanding, the findings on spending for medication are consistent with those of King et al. (2009), who cannot detect a statistically significant impact of *Seguro Popular* on household spending on medicines, using a randomized evaluation design and expenditures given for a longer period. Moreover, assessing the impact of *Seguro Popular* in two different household surveys on a wide range of outcomes and using a triple-difference estimation strategy, Barros (2008) does not find a statistically significant effect of *Seguro Popular* on the amounts spent on neither health care services nor drugs, conditional on spending. And King et al. (2009) find no treatment effect on the total amount of health care expenditures, which includes inpatient and outpatient care, medicines and medical devices.

Turning to the impact of *Seguro Popular* on investment in productive activities, panel B of Table 4.5 considers a range of different investment outcomes and categories. The results of the difference-in-differences specifications presented in columns (1) and (2) find the monetary values of production animals and of draft animals to be significantly affected by the program. The impact on the value of draft animals is estimated to be around 165 Mexican pesos. For the value of production animals the impact estimates coming from the difference-in-differences specifications lie around 400 Mexican pesos. Specification (4.3) also finds a positive impact on the quantity of production animals of about 0.4, which is statistically significant at the 10 percent level. Impact estimates for all other considered outcomes are not statistically significant. Results from the matched difference-in-differences estimator shown in column (3) differ from those of the difference-in-differences specifications. Estimates of the impact of *Seguro Popular* on the monetary values of draft and production animals are insignificant and considerably smaller in magnitude. The point estimate of the impact on the value of draft animals is even negative. This might suggest that the matching procedure does reduce an upward bias in the impact estimates for these outcomes present in the difference-in-differences estimates. Estimates from the matched difference-in-differences estimator though show a statistically significant positive impact on the probability of owning production animals of 3 percentage points. But there are also statistically significant negative impact estimates for the probability of owning agricultural machinery and equipment of 1.5 percentage points and for the probability of owning draft animals of 2.6 percentage points, which rather suggest a reduction of investment in these assets.

¹⁸See McKenzie (2012) on how statistical power for noisy and highly variable outcomes like household expenditures in developing countries can be increased by increasing the number of follow-up rounds.

As Appendix 4.I however shows, after adjusting p-values of estimates given in panel B for multiple hypothesis testing using a two-stage procedure developed by Benjamini et al. (2006) as described in Anderson (2008) to control the false discovery rate, none of the coefficients coming from the matched difference-in-differences remain statistically significant at conventional significance levels. In sum, results presented in Table 4.5 suggest that enrollment in *Seguro Popular* did not increase investment in productive assets or activities of beneficiary households.

4.4.3 Does the impact of *Seguro Popular* vary with household income?

The previous subsection has assessed the average impact of *Seguro Popular* on health care expenditures and investment in productive activities. One potential explanation for the modest effects on health care expenditures and absent effects on productive assets could be that poor households forego health care to ensure consumption and protect their productive assets, as sacrificing them would be too costly. This argument is also discussed by Acharya et al. (2012), who conduct a systematic review of nineteen studies that evaluate health insurance schemes for the informal sector in ten developing countries and conclude that, overall, there is no strong evidence of an impact of such programs on protection from financial risk. Moreover, in the cases in which insurance does provide financial protection, the impact on the poor is weaker (Acharya et al., 2012). Bonfrer and Gustafsson-Wright (2017) provide evidence that foregoing necessary care is indeed a strategy commonly used by agricultural households in Kenya, with one in five households having foregone care over a period of 12 months and poor households having been more likely to do so. In Appendix 4.J, I present a conceptual framework that links health shocks, income risk, and investment in productive activities to show that optimal investment in productive illiquid assets is more sensitive to changes in the prices for health care for households with higher income, if poor households decide to forego treatment more often and hence face less of a trade-off between investing in illiquid productive assets and holding precautionary savings to insure against health shocks ex-ante.

To assess whether this phenomenon is driving the results found on the average impact presented in section 4.4.2, I examine whether and how the impact of *Seguro Popular* changes with household income. For this, I divide the sample into subgroups of equal sizes, according to their household income at baseline, and use the matched difference-in-differences specification on each subgroup. I start with two subgroups, where the first includes households with income below the median income in the sample

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and the second households with income above median. To assess whether there is a gradual increase in the impact of *Seguro Popular*, in addition, I also divide the sample into five equally sized income groups and assess the impact in each of these income quintiles. Results for two income groups are presented in Table 4.6 and for five income groups in Table 4.7. Coefficients and significance levels shown in these tables refer to the impact of *Seguro Popular* in each subgroup. They do not tell us, whether an effect found in one subgroup is significantly different from the coefficient given in another subgroup. To assess this, I use Welch's t-test and test whether the coefficient of a given subgroup is different from the coefficient of the subgroup with the lowest household incomes. Welch's t-values are given below the standard deviation of each coefficient in square brackets, together with stars indicating the statistical significance level.

Table 4.6: Heterogeneous treatment effect of *Seguro Popular* on health care expenditures and investment in productive activities, two income groups (matched difference-in-differences estimation)

	(1)	(2)
Income range	MXN 0 - 398.70	MXN 398.75 - 3655.56
<i>A. Health care expenditures</i>		
Medication, probability of spending	-0.003 (0.021)	0.026 (0.025) [-0.878]
Medication, amount spent	8.180 (7.889)	1.103 (10.040) [0.554]
Med. consultations, probability of spending	-0.012 (0.017)	-0.032 (0.020) [0.761]
Med. consultations, amount spent	-4.606 (5.067)	-9.818 (7.064) [0.599]
Total health care exp., probability of spending	0.017 (0.022)	-0.029 (0.026) [1.386]
Total health care exp., amount spent	15.071 (10.767)	-39.916*** (13.877) [3.131***]
<i>B. Investment in productive activities</i>		
Operates a microenterprise = 1	0.006 (0.017)	-0.003 (0.020) [0.363]
Operates an agricultural microenterprise = 1	0.010 (0.015)	0.008 (0.017) [0.079]
Operates a non-farm microenterprise = 1	-0.002 (0.009)	-0.009 (0.012)

Continued on next page

4.4 Results

Table 4.6 – continued from previous page

	(1)	(2)
Income range	MXN 0 - 398.70	MXN 398.75 - 3655.56
		[0.489]
Agricultural machinery and equipment: Ownership=1	-0.025*** (0.009)	-0.002 (0.012)
		[-1.540]
Agricultural machinery and equipment: Monetary value (in pesos)	-195.003 (193.952)	156.503 (352.357)
		[-0.874]
Draft animals: Ownership=1	-0.031* (0.017)	-0.018 (0.017)
		[-0.541]
Draft animals: Quantity	-0.057** (0.029)	-0.061* (0.035)
		[0.093]
Draft animals: Monetary value (in pesos)	-116.170* (59.262)	41.088 (87.736)
		[-1.485]
Production animals: Ownership=1	-0.007 (0.025)	0.050* (0.026)
		[-1.604]
Production animals: Quantity	-0.608* (0.347)	0.216 (0.382)
		[-1.598]
Production animals: Monetary value (in pesos)	-69.413 (125.234)	272.742 (171.539)
		[-1.611]
Machinery and equipment used in non-farm business: Ownership=1	0.001 (0.008)	0.013 (0.010)
		[-0.923]
Machinery and equipment used in non-farm business: Monetary value (in pesos)	-29.924 (33.227)	61.949 (61.192)
		[-1.319]
Land: Usage=1	0.015 (0.023)	-0.031 (0.023)
		[1.449]
Land: Number of hectares used	-0.939 (0.658)	0.113 (0.545)
		[-1.233]
Number of Obs.	4,123	4,107

Note: Monetary values are expressed in baseline (2003) prices.

Columns (1) and (2) shows regression coefficients for nearest neighbor propensity score matching on first differenced outcomes, using two nearest neighbors (plus potential ties) with replacement, for the sample of households with baseline income below the median and above the median, respectively.

Abadie-Imbens standard errors are given in parentheses.

The number of observations varies depending on the outcome considered. Number of observations are given as the maximum number of observations over all outcomes for a given specification.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

In panel A of Table 4.6 only the estimate for the impact on total health care

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expenditures in the high income group is statistically significant. Households with incomes above the median spend on average 40 Mexican pesos less on health care after enrolling in *Seguro Popular* and this impact estimate is significantly different from the coefficient estimated for the low income group, which is not statistically significant. The point estimate for the impact on the amount spent on medical consultations is lower in the high income group than in the low income group, representing a stronger reduction, and this is also the case for the probability of spending on medical consultations and the probability of spending on health care in general. When the sample is divided into five income groups of equal sizes, a statistically significant effect of *Seguro Popular* is found on the probability of spending for medical consultations in the fourth and fifth income quintiles. These reductions of 6.7 percentage points in the fourth quintile and 8.9 percentage points in the fifth quintile are larger in magnitude than the 2.4 percentage points estimated on the full sample, given in Table 4.5. The coefficient of the fifth income quintile is significantly different from the coefficient of the first income quintile at the 10-percent significance level. Point estimates for the impact on the amount spent on medical consultations show a tendency to decrease with household income. For the fifth income quintile, the reduction of approximately 20 pesos is statistically significant, although not different from the coefficient of the first income quintile. In sum, there is some evidence supporting the hypothesis that *Seguro Popular* did reduce health care expenditures by a larger amount for households with higher incomes. Nevertheless, even when accounting for the possibility of heterogeneity in the treatment effect by household income, the impact of the program remains concentrated on expenditures for medical consultations.

Comparing the impacts on investment outcomes across household income groups, they do not provide evidence for *Seguro Popular* having increased investment in productive activities for households with higher income. In Table 4.6, a number of estimates are statistically significant in the low income group, although all of them suggest a reduction in investment in assets such as agricultural machinery and equipment, draft animals, and production animals. In the high income group, the impact on the quantity of draft animals is also negative and statistically significant at the ten percent significance level, and not statistically different from the coefficient of the low income group. Households in the high income group are, however, 5 percentage points more likely to own production animals after enrolling in *Seguro Popular*. This coefficient is significant at the 10 percent level, but it is not significantly different from the coefficient estimated for the low income group. Estimates of the program's impact on investment outcomes in each income quintile shown in Table 4.7 are qualitatively similar to those shown

in Table 4.6 for agricultural machinery and equipment, draft animals and production animals. Statistically significant coefficients are negative and observed predominantly in the lower income groups. An exception is the positive effect found on the monetary value of production animals in the third quintile, but this is not significantly different from the coefficient of the first quintile. Column 1 of Table 4.7 however shows an impact estimate of 6.1 percentage points on the likelihood of operating a microenterprise in the first income quintile, which appears to be principally driven by the impact on the likelihood of operating an agricultural microenterprise of 5.4 percentage points. Both these coefficients are statistically different from those of higher income quintiles.

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Table 4.7: Impact of *Seguro Popular* on health care expenditures and investment in productive activities by per capita household income, categorical household income variable, five income groups (matched difference-in-differences estimation)

	(1)	(2)	(3)	(4)	(5)
Income range	MXN	MXN	MXN	MXN	MXN
	0 - 200	200.10 - 320	320.21 - 482	482.41 - 760	760.06 - 3655.56
<i>A. Health care expenditures</i>					
Medication, probability of spending	0.015 (0.032)	-0.013 (0.034) [0.589]	-0.060 (0.038) [1.505]	0.051 (0.039) [-0.714]	0.012 (0.042) [0.051]
Medication, amount spent	2.474 (12.173)	6.238 (12.317) [-0.217]	7.300 (12.644) [-0.275]	4.580 (15.060) [-0.109]	-11.689 (16.764) [0.684]
Med. consultations, probability of spending	-0.020 (0.023)	0.011 (0.027)	0.040 (0.024)	-0.067** (0.031)	-0.089*** (0.033)
Med. consultations, amount spent	-4.420 (7.649)	-10.141 (9.738) [0.462]	14.584* (8.756) [-1.635]	-17.848 (11.344) [0.981]	-20.384* (12.316) [1.101]
Total health care exp., probability of spending	-0.021 (0.035)	-0.020 (0.035)	-0.039 (0.039)	0.053 (0.042)	-0.007 (0.045)
Total health care exp., amount spent	-12.211 (16.558)	-3.914 (18.111) [-0.338]	6.764 (18.143) [-0.772]	-8.840 (22.967) [-0.119]	-30.270 (26.259) [0.582]
<i>B. Investment in productive activities</i>					
Operates a microenterprise = 1	0.061** (0.025)	-0.009 (0.028)	-0.007 (0.029)	0.002 (0.030)	-0.042 (0.034)
Operates an agricultural microenterprise = 1	0.054** (0.023)	-0.001 (0.024)	0.004 (0.025)	-0.007 (0.025)	-0.013 (0.028)
Operates a non-farm microenterprise = 1	0.014 (0.014)	[1.664*] (0.009)	[1.490] (-0.008)	[1.794*] (-0.012)	[1.881*] (-0.025)

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Table 4.7 – continued from previous page

	(1)	(2)	(3)	(4)	(5)
Income range	MXN	MXN	MXN	MXN	MXN
	0 - 200	200.10 - 320	320.21 - 482	482.41 - 760	760.06 - 3655.56
Agricultural machinery and equipment: Ownership=1	-0.037*** (0.014)	[0.191] -0.010 (0.012)	[0.874] -0.025* (0.014)	[1.103] 0.000 (0.021)	[1.460] -0.006 (0.022)
Agricultural machinery and equipment: Monetary value (in pesos)	-64.075 (247.789)	79.152 (138.200)	-72.688 (193.564)	-73.196 (388.784)	-307.977 (560.383)
Draft animals: Ownership=1	-0.042 (0.027)	-0.071*** (0.027)	-0.024 (0.030)	-0.030 (0.029)	0.002 (0.032)
Draft animals: Quantity	-0.041 (0.040)	-0.056 (0.048)	-0.049 (0.053)	-0.111** (0.051)	-0.041 (0.061)
Draft animals: Monetary value (in pesos)	-162.441** (77.494)	-109.443 (109.146)	102.242 (101.234)	-152.219 (135.921)	-221.185 (159.807)
Production animals: Ownership=1	-0.020 (0.040)	-0.069* (0.039)	0.011 (0.040)	0.052 (0.038)	0.005 (0.042)
Production animals: Quantity	0.001 (0.515)	-1.024* (0.550)	0.594 (0.509)	0.345 (0.618)	-0.181 (0.632)
Production animals: Monetary value (in pesos)	246.186 (290.639)	-414.697* (224.237)	393.372* (237.038)	208.189 (268.787)	-171.149 (259.798)
Machinery and equipment used in non-farm business: Ownership=1	-0.008 (0.012)	0.011 (0.015)	-0.008 (0.017)	0.015 (0.015)	0.024 (0.017)
Machinery and equipment used in non-farm business: Monetary value (in pesos)	-5.005 (19.622)	21.781 (60.002)	-51.206 (61.487)	34.647 (90.754)	-97.189 (100.052)

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Table 4.7 – continued from previous page

	(1)	(2)	(3)	(4)	(5)
Income range	MXN 0 - 200	MXN 200.10 - 320	MXN 320.21 - 482	MXN 482.41 - 760	MXN 760.06 - 3655.56
Land: Usage=1	0.030 (0.038)	-0.042 (0.036) [-1.386]	0.039 (0.036) [-0.161]	-0.019 (0.036) [0.939]	-0.007 (0.039) [0.693]
Land: Number of hectares used	0.809 (0.899)	-1.824 (1.168) [1.787*]	-0.316 (0.977) [0.847]	-0.059 (0.864) [0.696]	-0.557 (1.188) [0.917]
Number of Obs.	1,671	1,620	1,662	1,657	1,657

Note: Monetary values are expressed in baseline (2003) prices.

Columns (1) to (5) shows regression coefficients for nearest neighbor propensity score matching on first differenced outcomes, using two nearest neighbors (plus potential ties) with replacement, by baseline household income quintiles.

Abadie-Imbens standard errors are given in parentheses.

The number of observations varies depending on the outcome suggested. Number of observations are given as the maximum number of observations over all outcomes for a given specification.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Concluding, there is some evidence that *Seguro Popular* led to stronger reductions in health care expenditures for households with higher income, in particular with regard to expenditures for medical consultations. This is in line with poorer households having resorted to foregoing treatment before the introduction of *Seguro Popular*. Nevertheless, I do not find corresponding differential impacts on productive assets and activities. Households with higher incomes do not have higher levels of investment in productive assets or activities after enrolling in the program. If anything, the program has mostly reduced investment in productive assets of poor households. But the mechanism underlying this effect is not clear. Taken together, results on investment outcomes for the different income groups indicate that *Seguro Popular* did not increase investment of beneficiary households in productive assets.

4.5 Discussion

The previous analyses have shown that, although health shocks led to increases in health care expenditures and reductions in productive agricultural assets prior to the introduction of *Seguro Popular*, the insurance program did not lead to higher levels of agricultural assets for insured households and did not reduce their health care expenditures substantially. I have explored whether these results are driven by poor households having resorted to foregoing health care treatments before the introduction of *Seguro Popular* and found evidence for this behavior being relevant for the impact on health care expenditures. The fact that there are nevertheless no corresponding impacts on investment in productive activities of households with higher incomes could suggest that the ex-ante and ex-post self-insurance strategies these households used before the introduction of the program did not affect productive assets and activities substantially. For instance, they might have relied on other forms of self-insurance, such as borrowing. In section 4.3.3, I have shown, however, that borrowing is very uncommon for the households in the sample. Moreover, even though impacts on health care expenditures are stronger for households with higher incomes, effects are not found for both expenditure categories, medical consultations and medications, but, as is the case for the average effect, concentrated on the less costly category, which is medical consultations.

These modest effects on health care expenditures of *Seguro Popular* might also explain, why there was no impact on productive assets. Although *Seguro Popular* is generally credited with reductions in health care expenditures (e.g. Bosch et al., 2012; Lakin, 2010), there is evidence that the program has not been able to provide full protection against the financial impact of health shocks. For instance, King et al. (2009) find that

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Seguro Popular reduced expenditures for inpatient and outpatient care, but had no significant impact on expenditures for medicines and on total expenditures for health care. Although results of impact evaluations of *Seguro Popular* on different sub-categories of health care spending vary, none of the experimental and quasi-experimental studies assessing the impact on a range of different components of health care expenditures find significant negative impacts on all of them (see King et al., 2009; Barros, 2008; Galárraga et al., 2010; Arenas et al., 2015). Moreover, there is substantial evidence pointing to the existence of important bottlenecks on the supply side, which could explain the modest to non-existent impact on health care expenditures and resulting null effects on productive assets observed here. Lakin (2010) provides evidence that *Seguro Popular* has been severely underfunded; in particular, contributions from states and beneficiaries fell substantially short of the program's financing goals, which were based on costing out the package of treatments and medicines guaranteed under *Seguro Popular*.¹⁹ Probably as a result of this underfunding, the provision of services and especially medicines, actually guaranteed by *Seguro Popular*, was limited. Fieldwork in the Mexican states Chiapas, Oaxaca and Guerrero showed that many clinics and hospitals were not able to provide all of the interventions or medicines included in the *Seguro Popular* package (Lakin, 2010). Barofsky (2010) even finds that availability of doctors and nurses actually decreased by 31 percent in response to the expansion of *Seguro Popular* and explains this with the funding difficulties of the program. With regard to medicines in particular, Servan-Mori et al. (2015) show that, ten years after the introduction of *Seguro Popular*, 17 percent of beneficiaries did not obtain some or all of their prescribed medicines, with unavailability of medicines reported as the reason in 86 percent of cases. This is further corroborated by qualitative evidence from the state Veracruz, in which the ENCEL-rural survey was also fielded (see Montero Mendoza, 2011).²⁰

¹⁹States did not pay their required contributions to the full extent and, in addition, credited other and past spending inappropriately towards most of their contributions (the majority of these credits were not related to the direct provision of health services, as intended originally, but mostly for infrastructure investments, which, in addition, may even have included federal transfers), leading to an estimated financing gap from state contributions of between 171 (approx. 18 percent of the total stipulated state contribution) and 858 million US-Dollar (approx. 90 percent of the total stipulated state contribution) in 2007, depending on the degree to which the credits they took were related to the direct provision of services, with the upper bound being more likely (Lakin, 2010). An additional financing gap came from misclassification of beneficiaries in the calculation of insurance premiums, which resulted in an estimated gap of approximately 160 million US-Dollar in 2004, and which is likely to have increased thereafter (Lakin, 2010).

²⁰When discussing the shortage of medicines, Montero Mendoza (2011) quotes a respondent saying that, although she has *Seguro Popular*, it is of no use, as she usually has to buy all or part of the prescribed medicines on her own.

The findings regarding the impact on health care expenditures made here are hence in line with previous work on *Seguro Popular*, which concludes that the program did lower health care expenditures for the insured population to some degree, but not yet to the extent that they are fully protected against the financial impact of health shocks, as the guaranteed services and especially medications are in practice not always available (e.g. Gakidou et al., 2006; Lakin, 2010; Bosch et al., 2012; Wirtz et al., 2012; Servan-Mori et al., 2015). With regard to the impact of *Seguro Popular* on household coping strategies and investment, this paper also fits the existing evidence, suggesting that the main driver of the results found here is the lack of the program to insure households adequately against the financial impact of health shocks. Barofsky (2010) does not find general expenditure portfolio effects of *Seguro Popular* and concludes that this is likely due to coverage not providing full insurance.²¹ Mahé (2017) observes an impact of *Seguro Popular* on internal migration in Mexico and concludes that this is due to the program relaxing time constraints instead of reducing the financial impact of health shocks, as she does not find a significant negative impact of the program on health care expenditures.

4.6 Conclusion

A large proportion of households in developing countries has traditionally been susceptible to substantial risk from health shocks. On the one hand, social security has excluded these households based on their employment status. On the other hand, public institutions, supposed to provide health services and resources for this population for free, have not been able to do so adequately. As a result these households have engaged in a range of different coping strategies, with potentially high costs in the long run by inhibiting the accumulation of physical and human capital or causing indebtedness. Recognizing the shortcomings of existing national health systems, many developing countries have introduced new health financing and insurance mechanisms to protect poor households from the negative consequences of health shocks. The literature evaluating these schemes has predominantly focused on their effect on out-of-pocket expenditures, utilization of health services, and health status. Few evidence

²¹Using experimental data, Barofsky (2010) finds that *Seguro Popular* reduced out-of-pocket expenditures for health by about 50 percent and had a statistically significant negative impact on expenditures for inpatient and outpatient care, but not for medicines, medical devices and other health expenditures. He then assesses the impact of *Seguro Popular* on seven components of non-health spending and total non-health expenditures. Only for spending on housing he finds a significant positive effect of the program and concludes that portfolio effects are minimal and absent in general, although the impact on housing expenditures might suggest greater saving.

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exists on the impact of these health insurance schemes on the strategies households have used to self-insure against the consequences of health shocks.

In this paper, I have assessed the impact of a large national health insurance scheme for households previously excluded from social security, the Mexican *Seguro Popular*, on productive agricultural assets. I find that, although health shocks led to increases in health care expenditures and reductions in productive agricultural assets prior to the introduction of *Seguro Popular*, the insurance program did not result in higher levels of agricultural assets for insured households and did not reduce their health care expenditures substantially. Significant impacts were only found on spending for medical consultations, but the major component of households' health expenditures, expenditures for medication, was unaffected by *Seguro Popular*. I therefore conclude that the program has not been successful in providing full insurance against the financial impact of health shocks. This is further supported by evidence showing that *Seguro Popular* has suffered from severe underfunding and the provision of services and, especially, medicines guaranteed by the program on paper, has been limited in practice. I propose this as an explanation of why *Seguro Popular* has had no impact on productive agricultural assets of insured households. Concluding, although *Seguro Popular* has led to some improvements in terms of access to health care and out-of-pocket expenditures for its beneficiaries, there remains a need to expand the protective effect of the program. As a first step, this implies ensuring that the guaranteed package of treatments and medicines is actually covered in practice.

Given the increase in health insurance schemes designed to protect the poor in developing countries and the evidence on the adverse effects of health shocks, it is important to evaluate the performance of these schemes, also on potentially harmful risk coping and management strategies. This paper contributes to the emerging literature that assesses the impact of health insurance on household investment behavior and livelihood strategies in developing countries. Although it assesses the impact of a specific program and on one single category of productive assets, using, in addition, data that is not representative at the national level, it adds to the emerging literature in this area and provides important insights for further research. Results compare to related research in Mexico and Colombia. Similar to Barofsky (2010) for Mexico, Miller et al. (2013), do not find any impact of the Colombian *Régimen Subsidiado* health insurance program for the poor on neither changes in the composition of household assets, nor human capital investments, nor household consumption, and attribute this to the failure of the program to provide full insurance. Furthermore, Acharya et al. (2012) show that the difficulties of health insurance programs in providing full protection against

the financial impact of health shocks are not unique to the programs in Mexico and Colombia. It is hence important to improve the protective effect of the emerging health financing and insurance mechanisms in developing countries for poor households. Finally, further research on the impact of health insurance on risk coping and mitigation strategies would benefit from a more in-depth analysis of the mechanisms through which health insurance impacts these strategies and from a simultaneous assessment of a portfolio of strategies. The latter would allow a comprehensive assessment of the welfare-enhancing impact of health insurance, as well as a better understanding of the importance of different behavioral approaches to health related financial risk.

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Appendix 4.A Details on the construction of dependent variables and data availability by survey round

Table 4.A.1: Availability of data for construction of key variables by survey round

	1999 (March)	1999 (Nov.)	2000 (March)	2000 (Nov.)	2003	2007
<i>A. Health insurance status of household head</i>						
Head is enrolled in <i>Seguro Popular</i>					✓	✓
<i>B. Health status of household head</i>						
ADL index of household head	✓	✓	✓	✓		
<i>C. Health care expenditures</i>						
Household expenditures for medication (last month)						
Spending = 1	✓	✓		✓	✓	✓
Amount spent (in pesos)	✓	✓		✓	✓	✓
Household expenditures for medical consultations (last month)						
Spending = 1	✓	✓		✓	✓	✓
Amount spent (in pesos)	✓	✓		✓	✓	✓
Total household expenditures on health care (last month)						
Spending = 1	✓	✓		✓	✓	✓
Amount spent (in pesos)	✓	✓		✓	✓	✓
<i>D. Productive activities</i>						
Operates a microenterprise = 1					✓	✓
Operates an agricultural microenterprise = 1					✓	✓
Operates a non-farm microenterprise = 1	✓ ^a	✓ ^a	✓ ^a	✓ ^a	✓ ^b	✓ ^b
Draft animals:						
Ownership = 1	✓	✓	✓		✓	✓
Quantity	✓	✓	✓		✓	✓
Monetary value (in pesos)					✓	✓
Production animals:						
Ownership = 1	✓	✓	✓		✓	✓
Quantity	✓	✓	✓		✓	✓
Monetary value (in pesos)					✓	✓
Agricultural machinery and equipment:						
Ownership = 1					✓	✓
Monetary value (in pesos)					✓	✓
Machinery and equipment used in non-farm business:						
Ownership = 1					✓	✓
Monetary value (in pesos)					✓	✓
Land:						
Usage = 1	✓	✓	✓	✓	✓	✓
Number of hectares used	✓	✓	✓		✓	✓

Note: The two 1998 survey rounds are excluded completely, as they do not collect the information required for the construction of the ADL index.

^aDefined as one or more household members engaging in a non-farm own-account income generating activity in the month preceding the interview (see further details on construction of this variable in description below).

^bDefined as household receiving income from a non-farm activity over the past 12 months preceding the interview (see further details on construction of this variable in description below).

4.A Details on the construction of dependent variables and data availability by survey round

Household operates an agricultural enterprise:

The 2003 and 2007 rounds of the ENCEL-rural survey ask households, whether they have received income from a range of productive activities over the past 12 months preceding the interview. I define the indicator *household operates an agricultural enterprise* as receiving income from any of the following activities: harvesting the plots owned by the household; collecting field flowers, herbs and mushrooms; preparation and/or sale of products of animal origin, such as dairy products, eggs, honey and other products of animal origin; sale of fattening animals (calves, pigs, chickens and horses); or other products from agricultural activities.

Household operates a non-farm enterprise:

In the survey rounds prior to 2003, a household is defined as operating a non-farm enterprise if a household member engaged in any of the following income generating activities in the month preceding the interview: sewing/ clothe making, preparing food for sale, construction/carpentry, sale/marketing of products other than prepared food (e.g. handicrafts); transport of goods or people; repair of goods or machinery; washing, ironing or cooking for pay; other types of paid own-account activities. In the 2003 and 2007 rounds, the indicator *household operates a non-farm enterprise* is defined as receiving income from any of the following activities over the past 12 months preceding the interview: making food for sale; sewing clothes, carpentry and construction, sale of nonfood items, such as handicrafts, repair of artifacts, or machinery; or other activities done in the context of non-farm self-employment.

Household operates a microenterprise:

In the 2003 and 2007 rounds, receiving income from any of the agricultural or non-farm activities listed above is defined as operating a microenterprise in general.

Production animals, quantity and monetary value:

The ENCEL-rural asks households whether they own animals they use in their agricultural activity, the quantity and, in the 2003 and 2007 rounds, the resale value of these animals. I follow Gertler et al. (2012) in defining production animals as animals whose meat and/or byproducts (milk, cheese, eggs, etc.) are sold and consumed. This category then includes goats and sheep, cows, poultry (chickens, hens, and turkeys), pigs, and rabbits. I then aggregate the number as well as the monetary value of all animals in this category into the total number and total monetary value of production animals, respectively.

4. THE IMPACT OF SOCIAL HEALTH INSURANCE ON PRODUCTIVE ACTIVITIES IN RURAL MEXICO

Draft animals, quantity and monetary value:

Following Gertler et al. (2012), I define draft animals as animals traditionally used for plowing the fields and for transportation purposes. This category then includes donkeys, mules, horses, and oxen. I then aggregate the numbers as well as the monetary values of all animals in this category into the total number and total monetary value of draft animals, respectively.

Agricultural machinery and equipment, ownership and monetary value:

The 2003 and 2007 rounds of the ENCEL-rural survey ask households whether they own a range of goods they use in their agricultural enterprise. These include: tractors, threshing machines, seeders or machinery to make furrows, other agricultural machinery, a plow or yoke (without animals). *Ownership of agricultural machinery and equipment is hence defined as ownership of any of these goods.* Households are also asked the resale value of these goods. I aggregate the resale values of all of these goods into the total monetary value of agricultural machinery and equipment.

Machinery and equipment used in non-farm business, ownership and monetary value:

The 2003 and 2007 rounds of the ENCEL-rural survey ask households whether they own a range of goods they use in their non-farm enterprise. These include: Loom or other machinery for making clothes, napkins or tablecloths; machinery or tools for making handicrafts; machinery or carpentry tools, or other machinery for manufacturing furniture; or any good used in the non-farm enterprise. *Ownership of machinery and equipment used in the non-farm enterprise* is hence defined as ownership of any of these goods. Households are also asked the resale value of these goods. I aggregate the resale values of all of these goods into the total monetary value of non-farm machinery and equipment.

Land, usage and number of hectares used:

The ENCEL-rural survey asks households whether they have used or owned tracts or plots of agricultural, livestock or forestry land over the past 12 months preceding the survey. I consider both, usage (including ownership), as well as the numbers of hectares used or owned as dependent variables.

Appendix 4.B Details on sample selection

Figure 4.B.1: Sequence of steps in sample selection 2003-2007

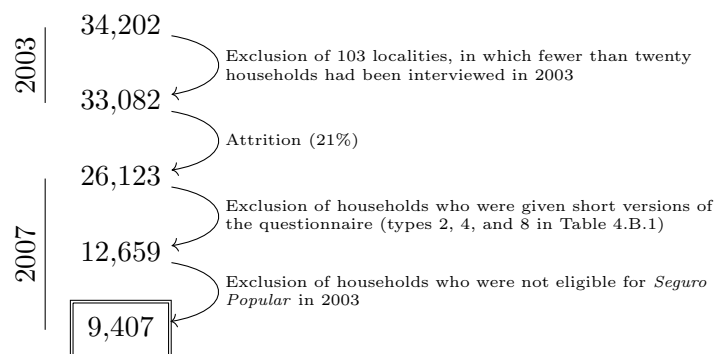


Table 4.B.1: Distribution of questionnaire types in 2007

#	Name	Fequency	Percent	Productive assets	Health care expenditures
1	Oportunidades Cuestionario Largo	6,920	26.49	yes	yes
2	Oportunidades Cuestionario Corto	5,881	22.51	no	no
3	Oportunidades Cuestionario Recuperacion	4,926	18.86	yes	yes
4	Migrantes	0	0	no	no
5	Hogar Adulto Mayor 1a	9	0.03	yes	yes
6	Hogar Adulto Mayor 2a	216	0.83	yes	yes
7	Hogar Adulto Mayor 3a	588	2.25	yes	yes
8	Oportunidades Cuestionario Corto-Largo	7,583	29.03	no	yes
		26,123	100.00	12,659	20,242

Appendix 4.C Construction of the ADL index

In 1999 and 2000, the ENCEL-rural questionnaires included detailed questions about the ability of each adult member to perform the following six activities:

- (1) Vigorous activities, like running, lift heavy objects, play soccer, wash clothes, or carry a bucket full of water,
- (2) Moderate activities, like working in the garden, sweep, lift a baby, walk 5km,

4. THE IMPACT OF SOCIAL HEALTH INSURANCE ON PRODUCTIVE ACTIVITIES IN RURAL MEXICO

- (3) Carry groceries or an object of 10kg for 500m with ease,
- (4) Lift a paper from the ground with ease,
- (5) Walk more 2km,
- (6) Bathe and dress him- or herself.

For each of the six activity categories, three responses are possible, which are coded in the following way:

- 1 – the respondent can perform the activity with ease,
- 2 – the respondent can perform the activity, but with difficulty,
- 3 – the respondent is unable to perform the activity.

For each individual the responses to the six questions are added into a score and combined in the following way to form the health status index as developed for the RAND Medical Outcome Study (Stewart et al., 1990, as cited in Barros, 2008; Gertler and Gruber, 2002):

$$Health_{it} = \frac{ADLScore_{it} - MinimumADLScore}{MaximumADLScore - MinimumADLScore} \quad (4.5)$$

with $MinimumADLScore = 6$ and $MaximumADLScore = 18$.

Appendix 4.D Insurance status of household heads in 2003

The ENCEL-survey did not ask households for the health insurance status of their members until 2003. Table 4.D.1 shows the distribution of household heads' insurance status in 2003 for the 506 original evaluation communities, which have been surveyed in the previous rounds of the ENCEL-rural, as an indicator of the distribution of health insurance in these rounds.

The analysis of the impact of *Seguro Popular* on productive assets and health care expenditures in section 4.4.2 uses the data from the 2003 and 2007 rounds and excludes households located in municipalities in which the program was already operating in 2003. Therefore, in 2003 none of the heads of households included in the main analysis sample is insured through *Seguro Popular*.

4.E Trends in outcome variables 1999-2007

Table 4.D.1: Insurance status of household heads in 2003

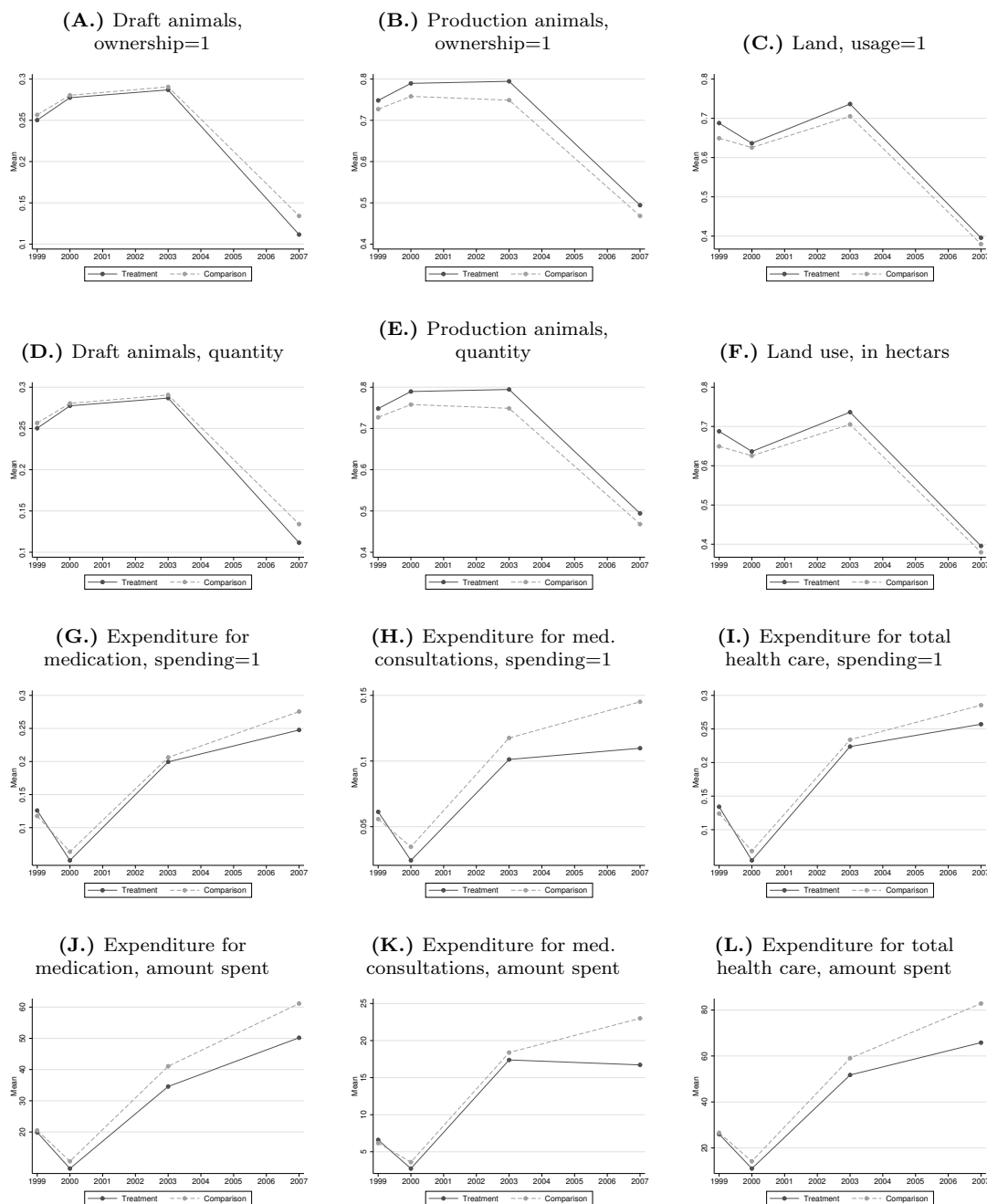
Entitlement to health care services through	Percent	Frequency
IMSS	5.69	1,136
ISSTE	0.9	180
Other public or parastatal institutions (e.g. PEMEX, Army, Marine , etc.)	0.2	40
Employer, at a private hospital or clinic	0.05	9
Other type of institution	0.97	194
Seguro Popular	1.41	281
Does not have entitlement to health care services	90.74	18,114
Does not know	0.04	8
Total	100	19,962

Appendix 4.E Trends in outcome variables 1999-2007

Figure 4.E.1 shows trends in outcome variables, for which data exists in the periods prior to the introduction of *Seguro Popular*, as well as afterwards. When data is available in two rounds of a given year, Figure 4.E.1 shows average values for the entire year. As the 2003 round of the ENCEL-rural added new households to the survey, not interviewed previously, pre-2003 data is only available for about 50 percent of the sample used in the analysis of the impact of *Seguro Popular* on productive activities and health care expenditures, and Figure 4.E.1 only includes this part of the sample.

4. THE IMPACT OF SOCIAL HEALTH INSURANCE ON PRODUCTIVE ACTIVITIES IN RURAL MEXICO

Figure 4.E.1: Trends in outcome variables 1999-2007, comparison vs. treatment group



Note: Monetary values are expressed in March 1999 pesos.

Appendix 4.F Estimation of the propensity score and balance of covariates

From the summary statistics and comparison of means presented in table 4.3, it becomes clear that treatment and comparison groups are not balanced in a number of spheres at baseline, which might jeopardize the common trend assumption, on which the difference-in-differences approach relies on for the identification of a causal treatment effect. In order to make the common trend assumption more credible, I combined difference-in-differences estimation with propensity score matching. This aims at constructing a comparison group that is more similar to the treatment group and this way balancing the distribution of covariates between these two groups at baseline. For this, I estimated the probability of enrolling in *Seguro Popular*, the so called propensity score, on which households eligible for *Seguro Popular* will be matched for the estimation of the treatment effect. Table 4.F.1 presents the results of the specification chosen for the estimation of the propensity score, a probit regression of the household head's enrollment in *Seguro Popular*, as measured in the 2007 round of the ENCEL-rural survey, on covariates that are believed to simultaneously influence the decision of households to enroll in *Seguro Popular* as well as the outcomes of interest here.

The inclusion of covariates in the propensity score model was driven by economic reasoning on potential joint determinants of enrollment and outcomes, and informed by details on the rules of operation of *Seguro Popular*, such as the aim of reaching poorer and indigenous populations first or the possibility of automatic affiliation of beneficiaries of other federal social programs, like the cash-transfer program *Oportunidades* (see Bosch et al., 2012; Scott, 2006). This led to the inclusion of characteristics of the household head, socioeconomic characteristics of the household, variables capturing the involvement in productive activities, and relevant community characteristics, measured at baseline, in the estimation of the propensity score. In addition, I included the number of quarters the household had been exposed to *Seguro Popular* in 2007 as a determinant of the propensity score. This should account for factors at the municipal level, which might jointly influence enrollment and investment in productive activities or health care expenditures. Finally, consideration was given to the model's ability to achieve balance in the distribution of the chosen covariates between treatment and matched control group.

4. THE IMPACT OF SOCIAL HEALTH INSURANCE ON PRODUCTIVE ACTIVITIES IN RURAL MEXICO

Table 4.F.1: Probit estimation results for the propensity score

Dep. variable: household head enrolled in <i>Seguro Popular</i> by 2007	Regression coefficients	Marginal effects
Head is indigenous = 1	0.1072 (0.1049)	0.0358 (0.0349)
Head of household is male = 1	-0.0052 (0.0556)	-0.0017 (0.0186)
Age of head	0.0204*** (0.0078)	0.0068*** (0.0026)
(Age of head) ²	-0.0002*** (0.0001)	-0.0001*** (0.0000)
Children aged 0 to 7 = 1	0.0852** (0.0416)	0.0284** (0.0138)
Women in childbearing age = 1	0.0964 (0.0594)	0.0321 (0.0200)
Dirt floor = 1	0.1356** (0.0649)	0.0452** (0.0216)
Total monthly household income per capita	-0.0001** (0.0000)	-0.0000** (0.0000)
Household receives oportunitades= 1	0.2068*** (0.0756)	0.0689*** (0.0250)
Head has more than primary school education = 1	0.2423*** (0.0784)	0.0808*** (0.0264)
Head completed primary school = 1	0.1903*** (0.0595)	0.0634*** (0.0201)
Operates an agricultural microenterprise = 1	0.0960* (0.0509)	0.0320* (0.0171)
Operates a non-farm microenterprise = 1	-0.0200 (0.0709)	-0.0067 (0.0236)
Draft animals, monetary value (in pesos)	-0.0000 (0.0000)	-0.0000 (0.0000)
Production animals, monetary value (in pesos)	-0.0000 (0.0000)	-0.0000 (0.0000)
Number of hectares used	-0.0029 (0.0023)	-0.0010 (0.0007)
Exposure to <i>Seguro Popular</i> in 2007 (in quarters)	0.0721*** (0.0135)	0.0240*** (0.0042)
Minimum wage	-0.0016* (0.0009)	-0.0005* (0.0003)
Average distance to health care institution is less than 50km = 1	-0.2909** (0.1331)	-0.0970** (0.0433)
Constant	-1.7949*** (0.2485)	
Number of Obs.	8,229	8,229
Prob > χ^2	0	
Pseudo R ²	0.0719	

Note: Monetary values are expressed in baseline (2003) prices.

Standard errors in parentheses, clustered at the community level.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

With regard to the inclusion of statistically insignificant covariates in the propensity

4.F Estimation of the propensity score and balance of covariates

score model, there is no clear guidance (see Caliendo & Kopeinig, 2008). I decided to include statistically insignificant variables, unless they are reasonably unrelated to the outcome variables or not a proper covariate, as recommended by Rubin and Thomas (1996). If, for the same concept, such as education or poverty status of the household, a range of proxies could potentially be used, I only included the ones that were statistically significant, as parsimony can reduce the variance of treatment effects (see Caliendo & Kopeinig, 2008). Health care expenditures at baseline, both the probability as well as the amount spent, were all statistically insignificant in predicting enrollment. Given that the actual date of enrollment in *Seguro Popular* of treatment households is unknown, the reference period for health care expenditures is only one month, and these expenditures are driven mostly by episodic health conditions, I decided not to include these variables, as it is unclear how they might be related to enrollment in this case.

As an algorithm for the propensity score matching, I chose nearest neighbor matching with replacement. Preference for nearest neighbor matching was given because of its property of bias reduction when constructing comparisons, as opposed to other algorithms, like radius or kernel matching, which rely on a larger number of matches for each treated observation and hence perform better at reducing treatment effect variance (see Caliendo & Kopeinig, 2008).²² Given that there are more than twice as many untreated households than treated households in the sample of eligible households at baseline, the number of comparison matches, i.e. nearest neighbors, for each treated observation is set to two.²³ Whereas the replacement reduces potential bias in the treatment effect estimate at the cost of increased variance, increasing the number of nearest neighbors, trades in lower variance for higher potential bias.

Figure 4.F.1 shows the distribution of the propensity scores in the treatment and comparison group before and after matching. There is considerable overlap between these two distributions before matching.²⁴ After matching the two distributions are more similar. Furthermore, the chosen matching approach is able to achieve covariate balance, as can be seen in Table 4.F.2. After matching, there remain no statistically

²²As sample size increases, different matching algorithms should however yield the same results, given their consistency (Smith, 2000).

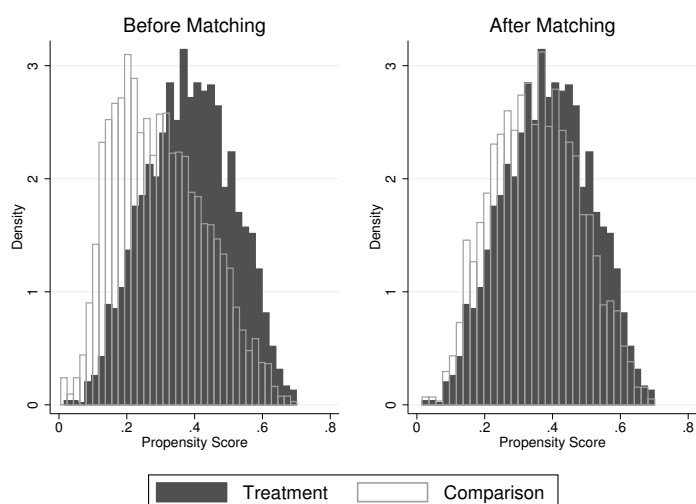
²³As I used the Stata command *teffects psmatch* for the estimation, in cases when there are comparison observations with identical pcores, which are nearest neighbors of a given treatment group observation, all of these comparison observations are used as matches, so that for some observations the number of matches could be more than two (In that case the weight attached to each match is less than 0.5 as the weights for all matches used to construct the counterfactual outcome trajectory of a given treatment group observation sum up to one).

²⁴The minimum and maximum propensity score of the treatment group are 0.0044 and 0.7581, respectively, and the corresponding values for the comparison group are 0.0050 and 0.7017.

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significant differences between means in the comparison and treatment group covariates used to estimate the propensity score, and standardized percentage bias is reduced to acceptable levels²⁵. Table 4.F.2 further shows that the success of the chosen matching procedure is also confirmed by other indicators that can be used to assess its quality.

Figure 4.F.1: Distribution of propensity scores before and after matching



²⁵Caliendo and Kopeinig (2008) consider standardized percentage biases below 3-5 percent to indicate matching success.

4.F Estimation of the propensity score and balance of covariates

Table 4.F.2: Covariate balance diagnostics

	Mean		Standardized percent- age bias	Percentage reduction in abs(bias)	Variance ratio of T over C
	Treatment	Comparison			
Head is indigenous = 1	0.43	0.44	-0.7	95.8	
Head of household is male = 1	0.87	0.88	-2.6	-99.6	
Age of head	45.23	45.38	-1.0	88.1	1.02
(Age of head) ²	2274.90	2283.20	-0.5	94.3	1.03
Children aged 0 to 7 = 1	0.60	0.58	2.5	78.9	
Women in childbearing age = 1	0.90	0.90	-0.1	99.0	
Dirt floor = 1	0.53	0.53	-0.1	99.2	
Total monthly household income per capita	472.37	477.54	-1.1	93.4	0.99
Household receives oportunidades= 1	0.59	0.60	-1.3	95.2	
Head has more than primary school education = 1	0.11	0.12	-1.5	60.8	
Head completed primary school = 1	0.21	0.20	3.5	36.2	
Operates an agricultural microenterprise = 1	0.36	0.34	3.0	-10.1	
Operates a non-farm microenterprise = 1	0.07	0.06	0.8	38.1	
Minimum wage	50.85	50.39	0.9	96.2	0.70 [†]
Average distance to health care institution is less than 50km = 1	0.28	0.33	-8.9	50.2	
Draft animals, monetary value (in pesos)	744.93	699.89	1.7	85.6	
Production animals, monetary value (in pesos)	1667.50	1681.90	-0.3	97.4	
Number of hectares used	2.45	2.62	-2.7	-85.0	
Exposure to <i>Seguro Popular</i> in 2007 (in quarters)	10.27	10.45	-4.7	89.3	
	Pseudo R ²	Prob > χ^2	Rubin's B	Rubin's R	
	0.003	0.258	13.0	0.83	

Note: Monetary values are expressed in baseline (2003) prices.

*, **, and *** indicate statistically significant differences in means between comparison and treatment group at the 10%, 5%, and 1% levels respectively.

[†] indicates variance ratio outside [0.93; 1.08].

[‡] indicates Rubin's B > 25 Rubin's R outside [0.5; 2].

4. THE IMPACT OF SOCIAL HEALTH INSURANCE ON PRODUCTIVE ACTIVITIES IN RURAL MEXICO

Appendix 4.G Robustness checks for the impact of health shocks prior to the introduction of *Seguro Popular*

Table 4.G.1: Impact of health shocks on health care expenditures, productive assets and activities prior to 2003, intensive margin and poisson fixed effects results

	(1)	(2)
Medication, amount spent	-0.589*** (0.168) [7,236]	-1.403*** (0.113) [16,431]
Med. consultations, amount spent	-0.742** (0.365) [3,799]	-1.612*** (0.180) [9,018]
Total health care exp., amount spent	-0.754*** (0.187) [7,666]	-1.459*** (0.115) [17,266]
Draft animals: Quantity	0.012 (0.024) [19,562]	0.018 (0.124) [27,695]
Production animals: Quantity	0.098*** (0.027) [47,766]	0.117** (0.033) [57,072]
Land: Number of hectares used	-0.005 (0.027) [52,630]	0.007 (0.128) [70,501]

Note: Column (1) shows results for log transformed outcomes coming from a fixed effects regression specification with covariates as specified in equation (4.1). These represent changes at the intensive margin. Column (2) shows results from fixed effects poisson regression, on the health status of household heads and time-varying covariates as in equation (4.1), but without community-specific fixed effects. Monetary values are expressed in March 1999 prices. Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Appendix 4.H Alternative matching results for the impact of *Seguro Popular* on health care expenditures and investment in productive activities

Table 4.H.1 shows results from nearest neighbor propensity score matching on first differenced outcomes, using one and three nearest neighbors (plus potential ties) with replacement.

4.H Alternative matching results for the impact of *Seguro Popular* on health care expenditures and investment in productive activities

Table 4.H.1: Impact of *Seguro Popular* on health care expenditures and investment in productive activities, propensity score matched difference-in-differences results with 1 and 3 nearest neighbors

	(1)	(2)
Nearest Neighbors	1	3
<i>A. Health care expenditures</i>		
Medication, probability of spending	-0.015 (0.018)	-0.018 (0.016)
Medication, amount spent	-3.615 (7.225)	-4.030 (6.010)
Med. consultations, probability of spending	-0.017 (0.015)	-0.022* (0.012)
Med. consultations, amount spent	-3.711 (5.028)	-5.284 (4.073)
Total health care exp., probability of spending	0.003 (0.019)	0.001 (0.016)
Total health care exp., amount spent	3.323 (9.688)	-0.780 (8.362)
<i>B. Investment in productive activities</i>		
Operates a microenterprise = 1	-0.030* (0.015)	-0.006 (0.011)
Operates an agricultural microenterprise = 1	-0.018 (0.013)	-0.001 (0.009)
Operates a non-farm microenterprise = 1	-0.007 (0.009)	-0.003 (0.007)
Agricultural machinery and equipment: Ownership = 1	-0.023*** (0.008)	-0.016** (0.007)
Agricultural machinery and equipment: Monetary value (in pesos)	57.796 (210.809)	-32.170 (163.493)
Draft animals: Ownership = 1	-0.036*** (0.014)	-0.027** (0.012)
Draft animals: Quantity	-0.046* (0.025)	-0.039* (0.021)
Draft animals: Monetary value (in pesos)	-27.071 (66.888)	-76.113 (53.407)
Production animals: Ownership = 1	0.035* (0.020)	0.030* (0.017)
Production animals: Quantity	0.099 (0.305)	-0.082 (0.254)
Production animals: Monetary value (in pesos)	49.729 (128.763)	29.773 (107.457)
Machinery and equipment used in non-farm business: Ownership = 1	0.005 (0.007)	0.005 (0.006)
Machinery and equipment used in non-farm business: Monetary value (in pesos)	60.606 (50.806)	55.100 (48.960)
Land: Usage = 1	0.011 (0.019)	0.003 (0.016)
Land: Number of hectares used	0.457 (0.471)	0.115 (0.395)
Number of Obs.	8,229	8,229

Note: Monetary values are expressed in baseline (2003) prices.

Abadie-Imbens standard errors are given in parentheses.

The number of observations varies depending on the outcome suggested. Number of observations are given as the maximum number of observations over all outcomes for a given specification.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Appendix 4.I Adjustment of p-values to account for multiple hypotheses testing

In panel B of table 4.5 I consider a range of different outcomes and categories that are all meant to capture investment in productive activities. This can be seen as essentially testing the same hypothesis fourteen times for each of the three estimation specifications chosen. Since the probability of a type I error, i. e. rejecting the null-hypothesis when it is actually true, increases with the number of hypotheses tested, I control for multiple hypotheses testing, using a two-stage procedure developed by Benjamini et al. (2006) and described in Anderson (2008) to control the false discovery rate (see Anderson, 2008, for a detailed description of the approach used here). The false discovery rate is the proportion of rejections that are type I errors and is well suited to exploratory analysis because it maintains greater power than other such methods, like family wise error control for example (Anderson, 2008). Table 4.I.1 reports original p-values for each of the three specifications used for estimation and corresponding adjusted p-values, called sharpened q-values. For calculation of these sharpened q-values, I relied on the Stata code provided online by Michael L. Anderson and pooled p-values of the three estimation specifications.

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Table 4.I.1: P-values and sharpened q-values for Table 4.5, Panel B

Specification	(1)		(2)		(3)	
	p-value	sharpened q-value	p-value	sharpened q-value	p-value	sharpened q-value
Operates a microenterprise = 1	0.186	1.000	0.938	1.000	0.176	1.000
Operates an agricultural microenterprise = 1	0.327	1.000	0.470	1.000	0.411	1.000
Operates a non-farm microenterprise = 1	0.885	1.000	0.795	1.000	0.483	1.000
Agricultural machinery and equipment: Ownership=1	0.256	1.000	0.699	1.000	0.040	0.381
Agricultural machinery and equipment: Monetary value (in pesos)	0.450	1.000	0.391	1.000	0.992	1.000
Draft animals: Ownership=1	0.737	1.000	0.844	1.000	0.034	0.381
Draft animals: Quantity	0.215	1.000	0.294	1.000	0.107	0.863
Draft animals: Monetary value (in pesos)	0.004	1.000	0.068	1.000	0.489	1.000
Production animals: Ownership=1	0.611	1.000	0.269	1.000	0.089	0.771
Production animals: Quantity	0.220	1.000	0.075	1.000	0.924	1.000
Production animals: Monetary value (in pesos)	0.000	1.000	0.001	1.000	0.620	1.000
Machinery and equipment used in non-farm business: Ownership=1	0.512	1.000	0.389	1.000	0.433	1.000
Machinery and equipment used in non-farm business: Monetary value (in pesos)	0.643	1.000	0.591	1.000	0.290	1.000
Land: Usage=1	0.334	1.000	0.790	1.000	0.625	1.000
Land: Number of hectares used	0.489	1.000	0.961	1.000	0.359	1.000
Covariates Matching						

²⁶The Stata code is available at [https://are.berkeley.edu/~sim\\$mlanderson/ARE_Website/Research.html](https://are.berkeley.edu/~sim$mlanderson/ARE_Website/Research.html)

Appendix 4.J A model of health shocks, income risk, and investment

This section lays out the conceptual framework that links health shocks, income risk, and investment in productive activities. The framework will guide the analysis of the impact of insuring households against health risks on investment in productive activities in the subsequent sections. It combines a 2 period model of consumption and investment, formulated by Eswaran and Kotwal (1989), with elements of a simple model set up by Pradhan and Prescott (2002) that combines consumption, health risk, and prices for health care treatment. While Eswaran and Kotwal (1989) show how income variability and investment in productive assets are inversely related, without explaining the source of the variability in household income, Pradhan and Prescott (2002) link income fluctuations and health shocks through reduced earnings capacities and costs for the treatment of illnesses. Their model, however, is not an agricultural-household-type model, as the one developed by Eswaran and Kotwal (1989), since it analyzes consumption decisions independently of production decisions, as opposed to treating the unit of observation as both, consumer and producer, and allowing for interdependence of consumption and production decisions.

I focus on the financial risk implied by health shocks, through reduced earnings capacities and costs for health care treatment, and show how, and under which circumstances, this risk leads to lower investment in productive assets. In the model, households are subject to health shocks. They have the possibility of using their assets as precautionary savings to cope with health induced income fluctuations or investing them in illiquid productive assets. In the event a household is hit by a health shock, the household can decide whether to pay for treatment or to remain ill. Treatment restores health status and thereby full earnings capacity. The decision will depend on household income on one side, and prices for health care treatment on the other. Low income households will less often seek care than households with higher income, and only at low prices, because their income will not be enough to pay for expensive treatments. On the other hand, households earning a low labor income have less to lose if a health shock prevents them from working and are hence subject to a lower income variability than households earning a higher income, assuming that both types of households have the same probability of experiencing a health shock. As a result, in the absence of any insurance mechanisms, households with higher income have a higher incentive to hold precautionary savings in case one or more household members fall ill. If the price of health care treatment is taken as an indicator of the severeness of the health shock,

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this implies that a health insurance scheme that provides free health care, like *Seguro Popular* in Mexico, will have the biggest impact on investment if it insures expensive and hence complicated treatments. In addition, free health insurance will have a larger impact on investment, the higher the income at stake for the beneficiary household.

Specifically, households derive utility from both consumption, c , and health, h . They are assumed to be risk-averse, which is represented by a constant absolute risk aversion (CARA) type utility function of the form:

$$u(c, h) = a - b \exp^{-\mu c} + h, \text{ with } b > 0, \mu > 0. \quad (4.1)$$

Their two-period utility function is additively separable and has the form:

$$U(c^1, h^1, c^2, h^2) = u(c^1, h^1) + \beta u(c^2, h^2), \text{ with } 0 < \beta < 1. \quad (4.2)$$

$$(4.3)$$

In the first period all household members can either be healthy or the household is hit by a health shock ε . For simplification I assume that $\varepsilon = 1$ if a household experiences a health shock and zero if all household members are healthy, so that $h^1 = 1 - \varepsilon$. In the presence of a health shock, household utility is decreased, since utility depends on health. Furthermore, illness reduces the earnings capacity of the household. First period income is thus subject to health risk, and has the form $z(1-\varepsilon)$.²⁷ Following Pradhan and Prescott (2002), I assume that all illnesses are treatable and that treatment always restores the household's health status. Households can thus choose to restore their health status, which reinstates their full earnings capacity z . But treatment has a cost, which will be represented by p . In the second period, all household members are healthy ($h^2 = 1$) and household income is z , that is, it is certain.

At the beginning of period 1 households are endowed with initial assets A , which they can divide between investment I and precautionary savings S :

$$A = S + I. \quad (4.4)$$

The decision on how much of the initial asset to allocate to precautionary savings and on how much of it to allocate to investment has to be taken before the realization of period 1 income. Precautionary savings can be used to stabilize consumption in

²⁷The assumption that earnings capacity of the whole household is reduced to zero if $\varepsilon = 1$ does not necessarily mean that all household members have to be ill, but allows for the possibility that healthy household members cannot earn income because they have to care for sick relatives.

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case of a negative income shock triggered by illness in the first period. Investment in productive activities has a positive and certain return r , which is only realized in period 2. This means that households only have the option to invest in illiquid productive assets. Households cannot borrow to cope with the financial consequences of a health shock. Hence, households face a tradeoff between increasing income and consumption through investment in a productive asset, and insuring consumption in period 1 against potential, health induced, income downfalls. The exact magnitude of the income downfall depends on whether the household decides to treat the illness and restore full earnings capacity, or not. The household will favor treatment to no treatment in case of illness, if:

$$u(z - p + A - I, 1) > u(z(1 - \varepsilon) + A - I, 0). \quad (4.5)$$

The utility function given in (4.1) implies state independence of household preferences. Therefore, and because of the decreasing marginal utility of consumption, seeking treatment at the cost p to restore the health status of the household will be preferred by high income households for a wider range of possible prices for health care treatment. Households with a low household income z only find it optimal to pay for treatment if prices for are low.

The household's consumption in period 1 and 2 will be:

$$c^1 \leq \tilde{y}^1 + A - I \quad (4.6)$$

$$c^2 = y^2 + rI + \tilde{y}^1 + A - c^1 \quad (4.7)$$

with $\tilde{y}^1 = z$ if all household members are healthy ($\varepsilon = 0$), $\tilde{y}^1 = z - p$ if the household is hit by a health shock and opts for treatment ($\varepsilon = 1, \textit{treat}$), $\tilde{y}^1 = z(1 - \varepsilon)$ if the household is hit by a health shock and opts against treatment ($\varepsilon = 1, \textit{notreat}$), and $y^2 = z$.

Depending on which health state a household realizes in the first period, and whether the household chooses to seek treatment or not if it experiences a health shock, it will solve the following maximization problem:

$$\max_{c_i^1, c_i^2} u(c_i^1, h_i^1) + \beta u(c_i^2, h_i^2), \quad (4.8)$$

(with $i = \varepsilon = 0; \varepsilon = 1, \textit{treat}; \varepsilon = 1, \textit{notreat}$), and subject to the respective budget constraint given by equations (4.6) and (4.7), which yields solutions for consumption in each state i and period t (with $t = 1, 2$) of the form $c_i^t(r, I)$.

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The households expected utility is then given by:

$$EU(c^1, h^1, c^2, h^2) = p_{\varepsilon=1}\{u[c_{\varepsilon=1,treat}^1(r, I), h_{\varepsilon=1,treat}^1] + \beta u[c_{\varepsilon=1,treat}^2(r, I), h^2]\} + p_{\varepsilon=0}\{u[c_{\varepsilon=0}^1(r, I), h_{\varepsilon=0}^1] + \beta u[c_{\varepsilon=0}^2(r, I), h^2]\}, \quad (4.9)$$

if (4.5) holds, and

$$EU(c^1, h^1, c^2, h^2) = p_{\varepsilon=1}\{u[c_{\varepsilon=1,notreat}^1(r, I), h_{\varepsilon=1,notreat}^1] + \beta u[c_{\varepsilon=1,notreat}^2(r, I), h^2]\} + p_{\varepsilon=0}\{u[c_{\varepsilon=0}^1(r, I), h_{\varepsilon=0}^1] + \beta u[c_{\varepsilon=0}^2(r, I), h^2]\}, \quad (4.10)$$

otherwise; with $p_{\varepsilon=1}$ and $p_{\varepsilon=0}$ being the probability of the realization of health shock $\varepsilon = 1$ and $\varepsilon = 0$, respectively, and both $p_{\varepsilon=1} = 0.5$ and $p_{\varepsilon=0} = 0.5$.

Households will then choose the level of investment in the productive, illiquid asset I that maximizes expected utility EU :

$$\max_I EU(c^1, h^1, c^2, h^2). \quad (4.11)$$

Appendix 4.J.1 shows that the first period budget constraint (4.6) is binding, so that first and second period consumption are independent of each other, simplifying to:

$$c_i^1 = y_i^1 + A - I \quad (4.12)$$

$$c_i^2 = y_i^2 + (1 + r)I \quad (4.13)$$

which are the solutions for consumption in each state i of the form $c_i^t(r, I)$ entering (4.11).

The optimal level of investment I^* is then given by the investment level that solves:

$$\max_I EU(c^1, h^1, c^2, h^2) = p_{\varepsilon=1}\{u[z - p + A - I, h_{\varepsilon=1,treat}^1] + \beta u[z + (1 + r)I, h^2]\} + p_{\varepsilon=0}\{u[z + A - I, h_{\varepsilon=0}^1] + \beta u[z + (1 + r)I, h^2]\} \quad (4.14)$$

if (4.5) holds, and

$$\max_I EU(c^1, h^1, c^2, h^2) = p_{\varepsilon=1}\{u[z(1 - \varepsilon) + A - I, h_{\varepsilon=1,notreat}^1] + \beta u[z + (1 + r)I, h^2]\} + p_{\varepsilon=0}\{u[z + A - I, h_{\varepsilon=0}^1] + \beta u[z + (1 + r)I, h^2]\} \quad (4.15)$$

otherwise.

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With the utility function given in (4.2), the corresponding first order condition is:

$$\frac{\partial EU}{\partial I} = -0.5\mu b \exp^{-\mu(z+A-I)}(1 + \exp^{\mu p}) + (1+r)\mu\beta b \exp^{-\mu(z+(1+r)I)} = 0 \quad (4.16)$$

if (4.5) holds, and

$$\frac{\partial EU}{\partial I} = -0.5\mu b \exp^{-\mu(z+A-I)}(1 + \exp^{\mu z \varepsilon}) + (1+r)\mu\beta b \exp^{-\mu(z+(1+r)I)} = 0 \quad (4.17)$$

otherwise.

Solving (4.16) and (4.17) for I , gives the optimal investment level for the range of prices at which a household decides to undergo treatment (I_{treat}^*), and the range of prices for which treatment becomes too expensive ($I_{notreat}^*$):

$$I_{treat}^* = \frac{\ln((1+r)\mu\beta b) - \ln(0.5\mu b) - \ln(1 + \exp^{\mu p})}{\mu(2+r)} + \frac{A}{(2+r)} \quad (4.18)$$

$$I_{notreat}^* = \frac{\ln((1+r)\mu\beta b) - \ln(0.5\mu b) - \ln(1 + \exp^{\mu z \varepsilon})}{\mu(2+r)} + \frac{A}{(2+r)} \quad (4.19)$$

otherwise.

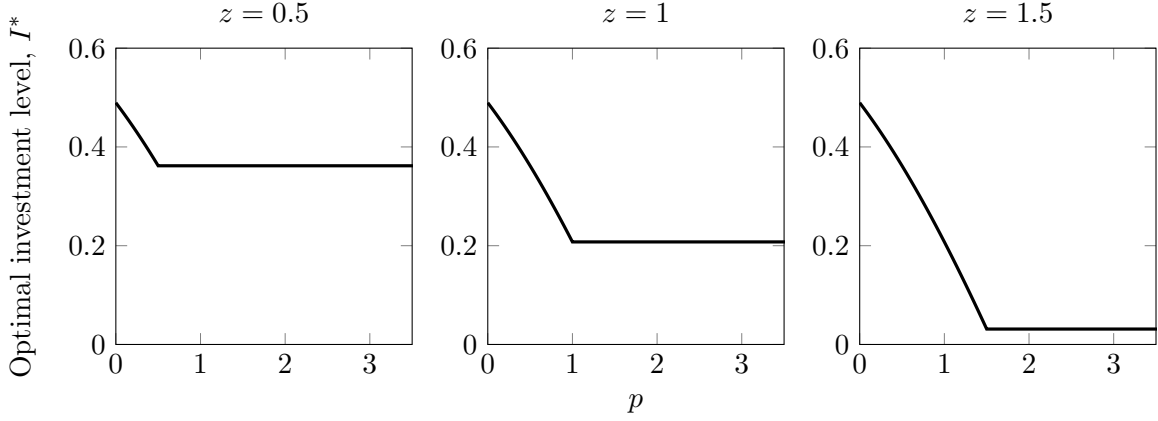
The solutions for the optimal investment level given by equations (4.18) and (4.19) show that, at prices low enough to make treatment an option, the optimal investment level is a function of the price of the treatment. At prices that are so high that the household decides to forego treatment, optimal investment is independent of treatment prices, but depends on household income z . Whether a household decides to forego treatment at given prices, depends on the income of the household, assuming all other parameters of the model remain the same. The lower the income, the earlier a household will decide not to buy health care treatment. Hence, for small prices, investment of low and high income households will depend on treatment prices in the same way.

For illustrative purposes, I further assess the comparative static properties of the optimal investment choice I^* with respect to varying prices for treatment, p , and different income levels, z , graphically. Figure 4.J.1 shows the optimal investment level I^* that solves equations (4.16) and (4.17) for different treatment prices, p , and income levels, z . As can be seen in figure 4.J.1, the optimal investment level in illiquid assets, I^* , has its maximum at a price of zero, which implies zero income variability. The higher the price for health care treatment, the lower the optimal level of investment in the productive asset. At the point where it becomes optimal for the household to forego treatment, the curve of the optimal investment level becomes flat. At this point the price for health care treatment becomes irrelevant for the household and any further

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increase in prices does not translate into higher potential income fluctuations. There is hence no need to increase precautionary savings any further. What then matters for the magnitude of precautionary savings, and consequently investment in the productive asset, is the income variability stemming from the potential reduction in the earnings capacity of the household. This will be higher for households earning a higher income. And figure 4.J.1 shows that the optimal investment level at prices prohibiting treatment is lower, the higher the household income at stake.

Figure 4.J.1: Optimal investment as a function of prices for health care, at different income levels



(Parameter values: $a = b = \mu = 1, \beta = 0.9, A = 1, r = 0.2$)

4.J.1 Independence of consumption across periods

For a given investment choice I and depending on which state i (with $i = B, G$) a household realizes in the first period, it will solve the following maximization problem:

$$\max_{c_i^1, c_i^2} u(c_i^1) + \beta u(c_i^2), \quad (4.1)$$

subject to the budget constraint given by:

$$c_i^1 \leq y_i^1 + A - I \quad (4.2)$$

$$c_i^2 = y^2 + (1+r)I + y_i^1 + A - I - c_i^1 \quad (4.3)$$

The corresponding Lagrangian function is:

$$\mathcal{L}(c_i^1, c_i^2, \lambda_1, \lambda_2) = u(c_i^1) + \beta u(c_i^2) + \lambda_1(y_2 + rI + y_i^1 + A - c_i^1 - c_i^2) + \lambda_2(y_i^1 + A - I - c_i^1) \quad (4.4)$$

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with λ_1 and λ_2 being the Lagrange multipliers on the constraints for period 1 and period 2 consumption, respectively.

The first order conditions are:

$$u'(c_i^1) - \lambda_1 - \lambda_2 = 0 \quad (4.5)$$

$$\beta u'(c_i^2) - \lambda_1 = 0 \quad (4.6)$$

$$c_i^2 = y^2 + rI + y_i^1 + A - c_i^1 \quad (4.7)$$

$$\lambda_2(y_i^1 + A - I - c_i^1) = 0 \quad (4.8)$$

$$\lambda_2 \geq 0 \quad (4.9)$$

$$y_i^1 + A - I - c_i^1 \geq 0 \quad (4.10)$$

Equation (8) is satisfied if either:

$$\lambda_2 = 0, \text{ or} \quad (4.11)$$

$$y_i^1 + A - I - c_i^1 = 0 \quad (4.12)$$

If the latter is the case, then the first period constraint is binding, and period 1 consumption and period 2 consumption are independent of each other, and the maximization problem has a corner solution. In that case, optimal consumption in each period only depends on the investment level I :

$$c_i^{1*} = y_i^1 + A - I \quad (4.13)$$

$$c_i^{2*} = y^2 + (1+r)I \quad (4.14)$$

I now check whether there are solutions satisfying the first order conditions of the maximization problem (4.1) for $c_i^{t*}(r, I)$ in the case that $\lambda_2 = 0$.

The utility function is given as:

$$u(c) = a - b \exp^{-\mu c}, \quad (4.15)$$

From the first order conditions (4.5) and (4.6) it follows that:

$$\mu b \exp^{-\mu c_i^1} - \mu \beta b \exp^{-\mu c_i^2} = \lambda_2, \text{ which is equivalent to:} \quad (4.16)$$

$$\mu b \exp^{-\mu} (\exp^{c_i^1} - \beta \exp^{c_i^2}) = \lambda_2 \quad (4.17)$$

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If $\lambda_2 = 0$, expression (4.17) requires that either

$$\mu b \exp^{-\mu} = 0, \text{ or} \quad (4.18)$$

$$\exp^{c_i^1} - \beta \exp^{c_i^2} = 0 \quad (4.19)$$

Since $b > 0$ and $\mu > 0$, $\mu b \exp^{-\mu}$ cannot be zero.

Now consider equation (4.19).

$$\text{If } \exp^{c_i^1} - \beta \exp^{c_i^2} = 0, \quad (4.20)$$

$$\text{then } \exp^{c_i^1} = \beta \exp^{c_i^2}, \text{ which is equivalent to} \quad (4.21)$$

$$c_i^2 = c_i^1 - \ln \beta \quad (4.22)$$

Equation (4.22), however, violates (4.7).

Hence, $\lambda_2 = 0$ does not satisfy the first order conditions of the maximization problem, and the only solution to the maximization problem (4.1) is given at the point where the first period budget constraint is binding, with:

$$c_i^{1*} = \tilde{y}_1 + A - I \quad (4.23)$$

$$c_i^{2*} = y^2 + (1 + r)I \quad (4.24)$$

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