The Employment Creation Impact of the Addis Ababa Integrated Housing Program — Draft

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Abstract

Severe housing shortages and high unemployment prevail in Addis Ababa. The Addis Ababa Integrated Housing Programme (AAIHDP) is an active labour market programme that attempts to tackle these problems simultaneously by creating and supporting Small and Medium Enterprises to construct housing using low-cost technologies novel for Ethiopia. This paper analyses the employment creation impact of the program and shows it to be negligible. Program participants do benefit from higher earnings and the program premium is highest for those at the bottom of the income distribution. Paradoxically, the program premium is due to the fact that the AAIHDP has created firms which are larger than pre-existing construction firms, though selection on unobservables cannot be ruled out. More generally, this paper, which is (one of the) first to analyse the impact of SME support programs on employment and earnings in a developing country context, challenges the case for promoting SMEs to generate employment.

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1 Introduction[1]

Severe housing shortages and high unemployment prevail in Addis Ababa. The Addis Ababa Integrated Housing Program (AAIHDP) aims to tackle the housing shortage and unemployment simultaneously by deploying labour-intensive Small and Medium Enterprises (SMEs)[2] to construct low-cost condominium housing using technologies novel for Ethiopia. As an active labour market program potentially capable of killing two birds with one stone, the AAIHDP features prominently in Ethiopia's draft Plan for Accelerated and Sustained Development to End Poverty (PASDEP)[3]; it is intended to be scaled up to 20 major Ethiopian cities.

In line with other advocates of SME[4] support programs that target labour demand and poverty, the architects of the IHDP have argued that SMEs create more jobs per unit of investment by virtue of being more labour-intensive. In addition, since the majority of the poor work in the small scale sector it is believed that SME employment disproportionately accrues to low-skilled workers, and would therefore constitute an effective means of targetting the poor. Finally, the IHDP is believed to foster the creation of firms which use technologies which are more cost-efficient.

To date, no analysis of the employment creation impact of the IHDP has been made. More generally, the empirical evidence on the effectiveness of SME promotion programs across the globe is weak, and virtually inexistent for developing countries, despite the increasing policy prominence of, and financial support for such programs.

This paper responds to the lacunae in literature by analysing the employment creation impact of AAIHDP, focussing on employment and earnings,

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[1] **Disclaimer:** The findings, interpretations, and conclusions expressed in this paper are entirely my own. They do not necessarily represent the view of the World Bank, its Executive Directors, or the countries they represent.

[1] Notes: I have benefitted tremendously from the guidance and support of my supervisor, Francis Teal. I would also like to thank the staff of the EEA, in particular Ato Kibre Moges, Ato Daniel Aklilu, and Ato Assefa Massi, and all the supervisors and enumerators for their efforts to collect good quality data. I also owe gratitude to Caterina Ruggeri Laderchi, Emily Kallaur and Rumana Huque at the World Bank.

All errors are my own.

[2] To be clear; our definition of a SME is an enterprise comprising less thant 50 workers. A micro enterprise is an enterprise comprising fewer than 10 workers.

[3] MoFED (2005)

[4] In this paper, an SME is defined as an enterprise comprising 50 workers or less.

relying on the potential outcomes framework pioneered by Roy (1956) and Rubin (1974). Using a unique matched worker-firms dataset, collected specifically for the purpose of analysing the program, it is shown that the program has been been succesful in increasing earnings for the poorest workers, but has had a negligible impact on the volume of labour demand, though it has biased its composition in favour of skilled labourers.

Previewing the findings of the paper, program firms are shown to employ a different technology than non-program firms, but are neither more efficient nor generate more jobs per unit of investment than non-program firms.A review of the literature, presented in section 3, suggest that an inverse correlation between firm-size and labour-intensity exists in certain sectors such as manufacturing, but that SMEs are certainly not uniformly more labour-intensive than large firms. Consistent with the latter claim, this paper does not find evidence of a positive association between firm-size and capital-, and inputs intensity in the construction sector are Addis.

In contrast with the belief that SMEs would create more jobs for low-skilled workers, better educated individuals who have experienced unemployment spells in the past are shown to be more likely to benefit from IHDP participation, perhaps because program participation is partly decided on the basis of a test. This finding is consistent with the sparse available evidence on SME *support* programs which indicates that educated unemployed workers are most likely to benefit from such programs (Betcherman et al., 2004). Unfortunately, the higher human capital of workers in program firms does not translate into higher productivity.

On the bright side, workers in program firms earn more than workers in non-program firms and this effect is strongest for workers at the bottom of the earnings distribution. While it cannot be ruled out that this result is driven by differences in unobservable characteristics between program participants and non-participants, it seems that the effect is due to a positive association between firm-size and wages. Program firms are on average larger than non-program firms and consequently pay more. Once firm-size is controlled for, the program premium disappears on average, though it remains positive for the poorest workers. Paradoxically then, the AAIHDP has benefitted workers by setting up larger scale firms.

Judging the overall impact of the program is difficult because of the possibility of general equilibrium effects and path dependency. Almost 1 in every 5 firms in construction in Addis participated in the program and these firms used inputs which otherwise would have been used by different firms. Nevertheless, the program has certainly not stifled all entry. In fact, the vast majority of entry remains accounted for by non-program firms. The program has reduced the availability of inputs for non-program participants and may have had exerted mild upward pressure on the demand for skilled labour.

Program firms are heavily reliant on support and 22% of program firms have perished already. Combined with the massive underutilization of existing construction capacity, this suggests that the case for supporting further entry may be questioned. The fact that SME support in the form of access to credit, land and inputs turned out to be most effective attests to the pernicious impact that heavy market distortions in land, credit and inputs markets have on the performance of the construction sector as a whole. This paper takes the view that rectification of such distortions should be an key policy priority and provides a potentially more potent vehicle for employment creation than the IHDP, which is set up to remedy the consequences of market failure, but not its causes. More generally, the results of this paper suggests that caution is warranted when advocating SME support as an effective vehicle for employment creation and poverty reduction.

The paper is organized as follows. The next section elaborates on the motivation for this paper in more detail. Section 3 examines the case for promoting SMEs and finds it to be rather weak, arguing for the importance of a good private sector development strategy instead. Section 4 describes the program and the context in which it operates. The data and descriptive statistics are presented in Section 5, while the methodology is discussed in section 6. Results are presented and discussed in section 7, while section 8 concludes. Additional information can be found in the Appendix.

2 Aim & Scope

2.1 Research Goal

Research Goal The primary objective of this paper is to establish whether the employment creation impact of the Addis Ababa Integrated Housing Programme can be identified. The specific focus of the paper is on earnings and employment; identifying the determinants of these is key to answerring the question. More generally, this paper hopes to make a contribution to the ongoing discussion on the efficacy of active labour market programmes and SME support. The motivation for these goals is explained in the next subsection. It should be stressed at the outset that this research is incapable of evaluating the long-run effects of the IHDP, though some tentative evidence suggestive of the likely sustainability of the program and its effects will be presented.

Theoretical Framework To assess the impact of the program, it is necessary to make assumptions about the volume, type and remuneration of employment that would have prevailed, should the program not have occurred. The potential outcomes approach, due to Roy (1951) and Rubin (1974), provides a framework to tackle this problem and constitutes the basis for the burgeoning literature on econometric program evaluation, part of which is surveyed in section6.1.1.

Counterfactual Housing construction by the "conventional" private sector is the general counterfactual to assessing the impact of the program. Given the magnitude of the housing shortage and the existence of very severe input constraints, it is almost certain that the inputs now taken up by the program would have been used by other firms to construct housing, had the program not been introduced. The question is therefore not whether to build houses, but how. Although housing can be constructed in many different ways, it seems natural to take the existing *modus operandi* as our main counterfactual. The potential outcomes under consideration are thus both conditional on housing construction. This conditionality is motivated by our interest in establishing the net impact of the AAIHDP.

The effect of the program on participants can be identified The effect of the program can be decomposed into the effect of the program on participants, and its impact on non-participants. Starting with the former, to establish the impact of the program on labour demand and earnings, one needs to know (1) whether or not AAIHDP firms create more jobs than non-AAIHDP firms (would have), (2) whether proportionally more jobs in program firms are accruing to the poor than jobs in non-program firms (would have) and (3) how much workers benefit from being employed in an AAIHDP firm (i.e. how much more they earn than they otherwise would have). If these three sub-questions can be addressed satisfactorily, then both the impact of the program on participants, and the determinants of earnings and employment will have been identified.

The effect on non-participants is much more difficult to estimate. Unfortunately, identifying the impact of the program on participants does not suffice to establish the overall impact of the program since one also needs to consider the impact on non-program firms and potential entrants, and the workers in those firms. One can side-step this issue by ruling out general equilibrium effects and path dependency, *de facto* assuming the impact on non-participants away, yet such stringent assumptions are clearly fallacious. Unfortunately, the armage at our disposal for testing for general equilibrium effects is limited; highly analytical quantitative work is not possible with available data. Nevertheless, this paper makes an effort to analyse whether and, if so, how the IHDP has affected non-participants.

In addition, merely establishing the impact of the program on participants will provide us with a good estimate of the first-order effects of the program, and thereby a reasonable approximation of the overall impact of the program, as the effects on non-program participants are likely to be second-order.

2.2 Motivation

In spite of the burgeoning interest in the effectiveness of SME support programs from both academics and policymakers, there is a lacuna in the literature on the performance of Active Labour Market Programs in developing countries in general, and on SME support in particular: In a recent review of the literature Betcherman et al. (2004, p51) conclude:

> *The evaluation literature on the labor market impacts of ALMPs is thinnest in the case of micro-enterprise development and self-employment assistance programs. There are relatively few studies and of those that do exist, many are concerned with the program's effect on business development rather than on the future employment and earnings of participants*

In fact, they found a total of 13 studies evaluating the impact of SME support programs,[5] none of which operated in a developing country context. Furthermore, only 5 studies focussed on the impact of such programs on earnings and employment prospects. Starting to fill the observed lacuna is thus a major contribution of this research.

[5] They exclude studies which do not use a control group.

From a more immediate policy perspective, the existence of plans to roll out the IHDP to 20 other major cities and the importance of the IHDP as a role model in Ethiopia's draft Plan for Accelerated and Sustained Development to End Poverty (PASDEP) for 2006-2010, call for a critical evaluation of the employment creation impact of the program.

In addition, while the construction sector has recently experienced rapid and significant changes, described in section 4.1.1, up to date information on earnings, employment and the overall investment climate in the construction sector in Ethiopia is unavailable. Such information is notoriously difficult to collect.[6] Obtaining a representative sample of matched firms and workers in the construction sector in Addis was a challenge (see section 5 and the Appendix), but has yielded a lot of information that was previously unavailable. For example, the data permit analysis of the determinants of wages and productivity jointly. Moreover, they contain information on firm-specific prices, allowing discrimination between productivity and profitability. It will be shown that using revenue data as indicators of productivity would have yielded seriously misleading results[7].

The data and findings from this survey also provide information on the impact of the investment climate on the performance of construction firms, and complement recent and ongoing work in Ethiopia on urban labour markets, particularly on labour market transitions and unemployment. The construction sector in Ethiopia is, in our view, particularly interesting for a number of reasons. To start with, the heavy market distortions operating in the sector warrant investigation. Furthermore, the construction sector plays an important role in the rural-urban transformation, by absorbing a lot of migrants and facilitating urbanization, and is critical to unlocking Ethiopia' economic potential (GDS, 2004). In addition, the fact that the construction sector produces outputs which are non-tradable makes for an interesting comparison with the available evidence on employment creation, productiv-

[6] Although a value chain analysis of the construction sector is available, the ongoing existing investment climates Rural and Urban investment climate assessments do not focus on construction. Moreover, a recent effort by Ethiopia Central Statistical Agency to collect a statistically representative sample of construction firms was forfeited when it proved difficult to trace all firms. The most recent successful data collection effort on the construction sector dates from 1998.

[7] It should be noted that collection of firm-specific prices was also necessitated by the existence of systematic price differentials between program and non-progrma firms, an issue which is explained in section 6.

ity growth and innovation in manufacturing firms in Ethiopia in particular, and Africa in general.[8]

3 Links to the Literature

The desirability of SME support is a hotly contested issue. Some believe that SME support is one of the most effective means of assisting and targetting the poor, while others regard tampering with the size distribution of firms as distortionary and wasteful. The case for SME promotion often relies crucially on the argument that SMEs create more and better income-generating opportunities for the poor. Other alleged advantages of SMEs include their contributions to competition, entrepreneurship and innovation, creation of products which are more suitable for the poor, as well as political and social dividends. These contributions of SMES are beyond the scope of this paper, which is concerned with poverty alleviation through employment creation.

In terms of employment creation, the benefits of SME promotion can be categorised into static, distributional and dynamic benefits. These benefits are discussed sequentially, in the context of a production function framework, which provides a means of linking labour demand, productivity and efficiency. Yet, this section starts by providing a brief overview of the results from manufacturing surveys, which provide a useful benchmark to compare the findings of this paper against.

3.1 Lessons from Manufacturing Surveys

Manufacturing surveys across Sub-Saharan Africa yield four stylized facts: large firms i) are more capital intensive but have lower returns to capital ii) have higher labour productivity iii) hire workers which are better educated on average, and iv) pay higher wages for workers with similar observable characteristics (Teal, 2007) Note, however, that the higher labour productivity can at least in part be attributed to higher capital intensity and that

[8] The IHDPs efforts to induce technological innovation are also interesting not only because the non-tradable nature of outputs in the construction sector implies that the scope for technological progress through learning by exporting, argued by some to be the main channel of technological progress in developing countries (see e.g. Rosenberg (1976)) at early stages of development, is limited, but also because there is a concern that SMEs have special problems in upgrading their capabilities (see e.g. Biggs (2002) and the references therein).

the evidence for increasing returns to scale is modest at best. If the pattern prevaling in the manufacturing sector generalizes to other sectors, then one expects small firms to exhibit higher returns to capital, to create more jobs per unit of investment, to hire more low-skilled workers and to pay lower wages.

3.2 SMEs as vehicles for employment creation: Static Arguments

Ignoring the distributional and dynamic benefits of promoting employment in small enterprises for the moment, the case for promoting small-scale enterprises to stimulate labour demand has to rely on the arguments that 1a) small firms are relatively more labour-intensive and 1b) have relatively high capital-productivity compared to larger firms and that 2) small firms use labour and capital more efficiently (or at least not less efficiently), i.e. that higher labour intensity is not achieved at the expense of productivity.

Factor proportion and technology choices jointly determine both productivity and labour intensity, and are consequently at the heart of the issue. Differences in relative factor prices in turn affect factor and technology choices and thus warrant attention.

Factor Proportions Capital intensity and labour productivity typically rise with firm size, yet this pattern is not an iron law. For example, Little (1987) and Little et al. (1987) observe that the more disaggregated the data, the more likely this pattern this pattern is violated. They furthermore argue that technology is a better guide to capital intensity than firm size. Consistent with the latter argument is the finding that between-industry variation in capital intensity is much smaller than within-industry variation in capital intensity, which has led some to advocate promoting labour-intensive industries rather than small scale firms (see e.g. Halberg (2001), and the references therein).

Technology The maximum efficient scale of production will on the technology used, but from a theoretical point of view it is not clear why large firms should necessarily be more efficient. While it is true that fixed costs, such as for example for business licensing, give rise to de facto economies of scale, coordination costs also rise with firm size, since organizational complexity tends to grow with firm size. Turning to the empirical evidence, Mead and Liedholm (1998) find that microenterprises consisting of one worker are typically the least productive firms in developing countries (see e.g. Mead and Liedholm, 1998), yet argue that firm size is not generally a reliable indicator of either technology choice or efficiency.[9] It is telling that studies on manufacturing data generally cannot generally reject the hypothesis of constant returns to scale, nor that of homotheticity (see e.g. Teal and Soderbom, 2004 [**?**]).[10] Similarly, Tybout (2000) argues that there are no large unexploited scale economies in the manufacturing sectors of most developed countries and, moreover, that markets in developing countries are not less competitive than those in developed countries, as evidenced by turnover rates and cross-plant productivity dispersion similar to that in developed countries. The latter is an important point as one might be concerned that industries dominated by a small proportion of large firms, as is the case in most developing countries, might tolerate more inefficiency than the more egalitarian size distributions observed in developed countries.

Factor Prices Differentials

Capital The relative price of factors may vary with firmsize, and almost surely drives differences in factor proportion choices. Large firms typically have better access to formal credit than small firms because of economies of scale in monitoring lending and because they are more likely to be able to put up collateral. Market failure in the provision of credit to small firms has often been used to justify interventions in credit markets, and spawned the recent micro-credit revolution.

Labour On the other hand, as observed above, large firms typically pay higher wages for workers with the same observable characteristics. This finding may be due an efficiency wage story, the fact that large firms tend to be more capital intensive and thus require a stable and effort exerting workforce, sorting, job ladders, or institutional factors such as unionization

[9] In interpreting the finding that small firms are generally the least productive firms, one should be aware of the tremendous heterogeneity that characterizes the small scale sector; many microenterprises in developing countries fulfill an insurance function and provide a repository for excess labour in times of economic downturns.

[10] Homotheticity and profit maximization imply that observed differences in factor proportion choices can only be attributed to factor price differentials.

rates (see e.g. Fafchamps & Soderbom, 2004). In addition, it may be the case that small firms are capable of drawing more heavily on cheap, family, labour. Whatever the reason, larger firms seem to face a higher price for labour.

The Business and Regulatory Environment The business environment impacts on the performance of all firms, yet may affect firms of different size in a different manner. Fixed costs associated with doing business, information gathering and conducting transactions will typically weigh heavier on small firms than on large firms. Furthermore, government procurement procedures, tax structures and (export) subsidy schemes may discriminate against different types of firms. It has also been argued that SMEs are more likely to suffer from market failures in the provision of training and the introduction of new technologies as a result of the fixed costs associated with such productivity enhancing investments.[11]

3.3 Dynamic Benefits of Small Firms

Turning to dynamics, if SMEs expand employment or productivity more rapidly then large firms, a case for their promotion could be made even when they are not more labour-intensive than larger size firms. The empirical evidence for this proposition is disappointingly thin. While it is certainly true that small firms are disproportionate creators of new jobs, they are also disproportionate destructors of jobs. The net employment creation impact of small firms in developed countries does not vary systematically with firm-size; while small firms grow faster conditional on survival, they are also less likely to survive (Scarpetta et al., 2004).

In contrast to the proposition that small firms would generate more new jobs than large firms, Biggs and Shah (1998) find that in the manufacturing sector in Africa, large firms were the main source of net job creation in countries where net job additions took place. Only in Zambia, which experienced an overall net job loss, did small firms create more jobs than large firms.[12]

[11] See e.g. Biggs(2002) and the references therein.

[12] In interpreting this finding it is important to keep in mind a distinction made between enterprises that fulfil a safety net function, and ones that are genuinely "dynamic"; small enterprise creation in economic downturns tends to create proportionally more enterprises fulfilling an insurance function, while during prosperous times business tend to be more "dynamic" (Mead and Liedholm (1998))

Similarly, Van Biesebroeck (2004) shows that unconditionally, firm size is negatively related to employment growth, but conditional on firm characteristics, including age and productivity level 6 years ago, larger firms grow faster.[13]

3.4 Distributional Benefits of Small Firms

Even if small firms do not generate more jobs than larger firms and are not expanding employment faster than larger firms, their promotion may be justified on the basis of the distributional benefits of having more small-scale enterprises; small firms on average hire workers which are not as well educated as their counterparts in large firms. Indeed, the majority of the poor in developing countries are employed in the small scale sector. Yet, large firms offer better benefits and more stable employment than small firms. The fact that poor people are more likely to be employed in the small scale sector does thus not imply that they are better off being employed in small firms.

3.5 Empirical Evidence

Evidence on the effectiveness of SME support While the idiosyncratic nature of the IHDP makes it difficult to compare to other Active Labour Market programs, reviews of the literature on Active Labour Market Programs typically find that (see e.g. Betcherman et al, (2004) and Fretwell et al. (1999)) that the evidence on the effects of such programs is mixed, but that well targetted and well-designed programs may have positive effects. Although the evidence on SME support programs is extremely thin, it suggests that SME support programs can be beneficial for certain subsamples of the unemployed, particularly better educated males. In addition, programs which package access to credit with other forms of business support seem to lead to better results than programs which facilitate access to credit alone.

[13] As Van Biesebroeck himself points out, the latter is a regression to the mean effect; surprisingly, more productive firms expand their workforce and value added more slowly, while unproductive firms do the opposite. His findings generate suprising predictions, since they imply that the variance of the size distribution of firms will explode in the long run as larger firms, which are more productive, should grow faster. Sample attrition may be an issue; for example, only the previously unproductive firms that have managed to catch up are expected to be in business today.

Cross-Country Evidence Ayyagari et al. (2004) construct a cross-sectional cross-country database of the size and productivity of the SME sector, and find that SMEs account for a larger proportion of GDP and employment in countries with a higher GDP per capita. At the same time, however, the size and importance of the informal sector is inversely related to the level of GDP per capita, so that the *aggregate contribution of small enterprises to GDP and manufacturing varies little if at all with the level of economic development* (Ayyagari et al. (2005), p9) . The fact that the database is cross-sectional limits the extent to which causal inferences can be drawn from these results. Nevertheless, exploiting cross-country variation in the importance of SMEs, the authors show that that a competitive business environment stimulates SMEs; low entry costs, property rights protection and efficient credit information sharing are all associated with a stronger SME sector.

3.6 Taking Stock

Overall, the case for promoting SMEs seems weak. Small enterprises are neither uniformly more labour-intensive than large-scale firms, nor are they more productive; if anything the literature suggests they are less productive. Furthermore, SMEs do not increase productivity or labour demand more rapidly than large firms. The limited evidence that is available for developing countries suggests that large firms expand employment and productivity faster. As far as distributional benefits of promoting SMEs are concerned, it is true that small firms tend to employ proportionately more unskilled workers, yet they also pay such workers lower wages than large firms do.

At the same time however, it is important to recognize that small firms may suffer disproportionately from policy distortions and market failures. Remedying such distortions is desirable for firms of any size. The consensus (see e.g. Levy, (1994), Hallberg (2001), Ayyagari et al. (2004), Biggs (2002)) seems to be that a good private sector development strategy is the key to a thriving SME sector. Such a strategy would have to attempt to create a level playing field for firms of all sizes, rather than focus on size as such.

The next section describes the consraints operating in the construction sector in Addis, and what the IHDP does to remedy market failure.

4 The AAIHDP

This section describes the IHDP and the context in which it operates.

4.1 Context: Construction, Housing and Employment in Addis

4.1.1 The housing shortage: a supply side problem

Estimates of the housing shortage in Addis vary between 250,000 and 300,000 housing units (IHDP, 2004), and the shortage is increasing by approximately 40,000 units each year (Construction Ahead, 2005).[14] The housing shortage has arisen because housing supply has not kept up with increasing housing demand due to population growth, immigration, dilapidation of the existing housing, progressively increasing diaspora demand for housing, a lack of alternative investment opportunities and speculation. The supply of housing has been constrained inter alia by restrictive land policies, a legacy of marginalizing the private sector in housing development, and, more recently, by severe shortages of key construction inputs such as cement, which have triggered price escalations and delays in the delivery of buildings. As an example, the demand for cement is almost 60% higher than the available supply[15].

Recent reforms in the areas of customs, business regulation, and registration have helped stimulate housing supply, by relaxing financing constraints, alleviating the burden of bureaucratic procedures, and marginally increasing the availability of land. Yet many challenges for the construction sector persist, including difficulties in obtaining inputs, finance, and accessing land, poor regulation, the absence of quality insurance, lack of technological know-how and adequate equipment, unpredictability of tax liabilities, and also bidding- and tender procedures vulnerable to corruption.

[14] In addition, at least a third of the estimated total housing stock of 640,000 units are of very poor quality (World Bank, 2004).

[15] According to the abovementioned Value Chain Analysis by Global Development Solutions (2006), local producers can supply 1,7 million tons of cement per year, while the demand is 2.7 million tons. More recently, an article in Addis Fortune of 26 December 2006, suggested the demand for cement had risen to 3 million quintiles. (Addis Fortune, 2006)

Types of Firms Firms operating in the construction sector can be divided into contractors and non-contractors. Contractors typically engage in a variety of activities and are capable of constructing entire buildings, or parts of buildings, by themselves, although they can sub-contract specific tasks to third parties. Contractors have a license grade between 12 and 1; the lower the license grade, the bigger the projects the contractor is allowed to undertake. Non-contractors are typically responsible for the provision of inputs or the completion of certain specific parts of buildings and do not have a contracting license grade.

Types of Workers Many jobs in the construction sector often require very little supervision, and are thus suited for casual workers. For the same reason, the construction sector offers many employment opportunities for migrants. Nevertheless, social connections are very important for finding employment. Casual workers are frequently under-, if not un-, employed and the income of casual labourers is consequently both lower and more volatile than that of permanent workers. Casual workers are also more likely to operate informally.

4.1.2 Unemployment in Addis[16]

Urban labour markets in Ethiopia are characterized by high unemployment and informality. Urban activity and employment rates are low; in 2004, 58.4% of people older than 15 years of age were economically active and 48.4% were employed. The unemployment rate is high at 16.5%, unemployment spells are long, and underemployment is an important problem, particularly for those in the informal sector, which is substantial.

The labour market position of youth, women and uneducated people is particularly disadvantaged. In urban areas, only a third of people between 15 and 25 years of age are working. The unemployment rate for this group is 26.5% despite a low activity rate of 41.7%. In Addis, the picture is even more dramatic: only 23% of youngsters between 15 and 20 years old were employed in 2004, while the comparable figure for smaller towns is 36%. The labour market position of women reflects their disadvantaged position in society: they carry most of the burden of domestic work, have lower educational achievements, are less likely to obtain a job, have longer unemployment spells,

[16] All statistics in this section were taken from "Urban Labour Markets in Ethiopia: Challenges and Prospects" (World Bank, 2006)

and suffer pay discrimination when they do manage to get a job. As in other countries, uneducated people are less likely to be employed and even when they are, wages are low.

4.2 The AAIHDP

4.2.1 Objectives

The overarching objective of the AAIHDP is to improve the living standards of Addis residents, especially low-income citizens, through the creation of employment opportunities and the provision of affordable housing. The specific objectives of the programme include '*promotion of micro and small-scale enterprises, which can absorb more labour force and operate at a lower overhead cost'* and '*promotion of cost efficient housing construction technology*' (Project Profile, 2004, p1).

To achieve these objectives, the AAIHDP aims to construct 192,500 houses, generate 80,000 job opportunities, support 1300 existing SMEs and create another 1000 new ones. In addition, it attempts to strengthen the construction sector by developing 1,200 ha of land, promoting low-cost technologies, changing training systems, ensuring minimum construction standards and developing the institutional capacity required to construct 50,000 houses each year. It should be noted that the employment creation target is ill defined as the administration's definition of a "job opportunity" is not very informative.[17]

4.2.2 Rationale

The program manual ascertains the need for an integrated programme and provides the rationale for deploying SMEs and introducing new technologies

> The market cannot deliver low-cost housing at the required quantity and reasonable price. The currently available industrial technology does not allow the construction of low-cost houses,

[17] The housing construction, employment creation and SME promotion targets are all taken from an AAIHDP presentation at the World Bank in October 2006, while the targets for Slum upgrading, land development and the non-quantifiable targets are taken from "Addis Ababa Integrated Housing Development Program" (no author), Addis Ababa, March 2004.

> and hence it is important to involve as many micro and small-scale enterprises as possible in the program so as to promote low-cost technologies. Moreover, it is important to develop a housing technology that requires lower level of skill and that can be implemented extensively in a short period of time. As such, the role of the government is of great importance. (Project Profile, 2004, p1).

The argument for promoting SMEs thus relies on the assumptions that the market is not capable of constructing low-cost housing by itself and that reliance on SMES, which are labour-intensive and have low overhead costs, would generate extra employment opportunities and also reduce construction costs. A further crucial, yet implicit, assumption underlying the program design is the necessity and desirability of supporting SMEs. Such support is essential for capacity creation as well as for the introduction of new technologies.

4.2.3 Low-Cost Technology

To produce housing affordable by low-income dwellers, the IHDP has developed a production process that deviates from the one conventionally used in the private sector. The low-cost aspect of the program consists in building a different, less luxurious, homogeneous type of housing, using novel low-cost construction technologies, cheaper inputs, fixed-price contracts and a standardized production procedure permitting greater specialization. Particularly important are the introduction of new technologies, such as pre-cast beams and ribslabs, reducing the needs for material inputs and formwork, and the fixed price system, which forces firms to sell their outputs below the market price in exchange for the support they receive.[18]

4.3 Activities

The main task of the AAIHDP program office, which is in charge of administering the IHDP, is to is to promote micro and small-scale enterprises to produce the inputs and to build the low cost housing that the City Administration wants to make available to low income residents in Addis. To this

[18] For more details, see World Bank (2007).

end i) it creates new SMEs ii) awards contracts to such newly created SMEs, as well as other firms and iii) it provides support for firms in the program. This subsection describes these activities in turn.

4.3.1 Creation of MSEs

The process of creating an MSE occurs in three steps, i) registration of interested and eligible individuals, ii) testing to verify such individuals have the necessary skills and iii) enterprise formation. Anybody with a valid identity card who has either graduated from a TVET college or can show proof of having experience in the construction sector, can register for participating in the program at the Kebele level.[19] Following registration, all eligible applicants have to sit a test which serves to verify their skills meet the minimum standard. The test has a theoretical component which accounts for 30% of the score and a practical component accounting for 70% of the score, thus placing most emphasis on practical skills. Individuals who pass the test can establish an enterprise, either by themselves or together with other successful applicants. Individuals who failed the test are allowed to resit the test at a later date and may attempt to upgrade their skills by joining successful candidates in the project as assistants. Most individuals choose to form cooperatives.[20]

4.3.2 Awarding of contracts

SMEs Depending on their date of formation and the availability of work in their area, SMEs will be awarded with contracts and work under the supervision of engineers and foremen from the Project Office. Only SMEs formed through the process described in the previous section (i.e., registration, testing and SME formation) are awarded IHDP jobs. However, when newly organized SMEs are unwilling or unable to complete certain works,

[19] A kebele is the smallest administrative unit in Ethiopia

[20] A cooperative is a firm of at least 10 people who jointly own the firm and share the profits. Cooperatives are an attractive organizational structure since they are exempt from profit tax. Furthermore, by pooling resources together, cooperatives enable individuals who would otherwise have been incapable to raise sufficient funds, to meet the initial investment requirements associated with starting a business. A downside of forming a cooperative is the potential risk of free-riding due to proftit sharing.

preexisting licensed SMEs are invited to take up the job.[21] Anecdotal evidence suggests it is not very common for preexisting SMEs to re-register in order to obtain IHDP work.

Contractors While the IHDP is aimed at sustaining and bringing into production the SMEs it has created, other firms can also benefit from its contracts, most notably contractors. Large contractors (grade 6 and above) are hired for foundational and structural works, though many have eschewed participation because of the fixed price system, which is described later. The selection of contractors occurs on the basis of the temporal order in which they registered with the Project Office, unless the number of registered contractors exceeds the number of available jobs, in which case a draw is held.[22]

4.3.3 Support

The IHDP provides wide-ranging support to SMEs by providing and, in certain cases, subsidizing a place to work, facilitating access to credit, providing training and access to inputs (on credit) and subsidizing machinery for firms producing rebars (reinforcement bars) or hollow blocks:

- **Land Grant** By i) enabling access to land and ii) subsidizing certain types of land and sheds[23], the IHDP tries to ensure SMEs have a place to work.

- **Access to credit without collateral (through a joint bank account).** The IHDP does not extend credit itself, but rather connects

[21] SMEs which have not been created by the program are not allowed to compete for IHDP jobs, unless they have registered their members as fresh candidates, passed the test and regrouped as a new SMEs.

[22] In theory, this draw would have been ideal for the purposes of constructing a control group. In practice, it was tremendously difficult to check which firms in the sample had tried to participate but did not succeed; the answers to the corresponding question in the survey seemed noisy.

[23] The program provides certain sites, such as TVET compounds, for free if the shed on it is built with wood (80% of cases), while it charges the full amount of rent if the shed on the site was constructed with metal (approximately 20% of all cases).

program firms with existing Micro-Finance Institutions and enables them to obtain credit without having to put up collateral[24].

- **Input provision on Credit.** The IHDP provides cement, rebars and iron on credit. Firms do not have to pay when these inputs are provided to them. Instead, the costs of inputs are deducted from their payment upon completion of the contract.

- **Subsidized Machinery-Lease**. Program firms producing rebars and/or hollowblocks can purchase subsidized machinery on lease; the IHDP instructs the MFIs which machineries to buy and to which companies to lease them out. Again, payments are not made upfront but deducted from payments for completed contracts.

- **Training**. Firms which engage in pre-cast beam and/or hollow block production are trained before deployment.

- **Demand.** Awarding work to new firms and shielding them from competition by non-program firms is perhaps the most important support the IHDP provides to program SMEs. Note that firms which have been created by the program are free to carry out jobs for clients other than the IHDP.

5 Data & Descriptives

The data are from a recent survey of matched firms and workers in the construction sector in Addis Ababa Ethiopia, conducted by the Ethiopian Economic Association, in collaboration with the World Bank, in December 2006. The survey was designed specifically for the purpose of analysing the employment creation impact of the program. It covered 240 firms and 971 workers, 241 of whom were casual workers. Alongside the quantitative data-collection, qualitative evidence was collected by means of in-depth interviews

[24] Firms in the program can open a bank-account together with the IHDP without having to provide the collateral which is normally required. The program office's signature provides the required collateral, even though the program office is not responsible for repayment in the case of default. In all other respects (interest rates, repayment periods and grace-period) program firms face the same lending conditions. Yet, every time program firms make a withdrawal, they have to ask for permission from the Program Office.

with respondents.[25]

The workers data contain detailed information on workers' earnings, their employment history, experience, skills, educational background, program participation, jobsatisfaction, motivation for choosing their current activity and on a number of socio-demographic characteristics including household characteristics and parental background. In addition, a module on household assets was collected in order to be able to create a predicted wellfare-indicator, see Appendix .3. Permanent and casual workers were administered slightly different questionnaires. The casual worker questionnaire contained additional questions pertaining to casual employment but excluded the questions from the permanent workers questionnaire which were irrelevant for casual labourers.

The firm-level data provide information on a rich set of firm characteristics, including their activities, age, size, capital stock, inputs, outputs, expenditure, revenues, organizational and occupational structure, program participation and support, access to finance, inputs and skilled personnel, constraints, expectations, the number and type of workers they employ, the wages they paid to these workers and employment dynamics. A lot of energy went into collecting data on the volume and total costs of inputs and outputs, as well as on expenditure and revenues. Input- and output prices provide natural instruments for production function estimation and could potentially help decompose differences in profitability into differences in productivity and differences in market power. Unfortunately, the data on inputs and outputs and the price data are quite noisy. One problem was that only 35% of firms in our sample keep complete books of account, while 32% do not keep any books at all.[26] While contractors and large firms are typically better at keeping books of account, they were less capable of providing accurate information on the precise amounts of different inputs used, because of the scale of their operation and because they operate in different locations.

[25] Transcriptions of interviews and a short document summarising the main conclusions from that work are avaible from the author upon request.

[26] Program firms kept much better books of accounts.

5.1 Sampling Strategy

Firms[27] The Addis Ababa City Administration keeps a registry of firms in which all firms in the construction sector are registered and thus documents the underlying population of interest. This registry is not flawless, however. To start with, the requirement that any firm should have license to legally operate is not always strictly enforced for many small and young firms. Secondly, though the registry is updated annually, it also lists a number of firms which are no longer active. Thirdly, some firms have been entered several times, for example under (slightly) different names, with a different addresses or merely with a different phone number. One firm was registered no less than 7 (!) times. Fourthly, it turned out to be very difficult to trace individual firms on the basis of the information given in the registry; contact details were often conflicting, outdated, or missing altogether. Moreover, the registry does not indicate whether a firm participates in the AAIHDP or not. It was not possible to trace program firms in the registry. Fortunately, the IHDP program office keeps a record of all the firms that have registered for participation in the program. Armed with this list, tracing program firms was still not easy because some of the firms on the list had exited, while other had moved. In fact, more (18% of the firms that could be contacted) were not operating.

To get an estimate of the reliability of the registry and to obtain further information on firm entry and firm exit, a sub-sample of 600 firms in the registry was selected randomly and contacted.[28] 31 entries turned out to be duplicates of other firms in the list and 303 of these firms could be contacted by phone in 4 attempts or less. Of the firms that could be contacted, 25% had exited. 17% the firms which are alive today are working for the AAIHDP. The registry is thus not perfectly reliablie, but seems to provide a reasonably

[27] We would like to thank Ato Mulugeta from the City Administration and his team, in particular Ato Daniel for their indispensable efforts in delivering the list of firms in the construction sector in Addis, as well as Ato Solomon from the program office for providing us with a list of AAIHDP firms and going through this list to see which ones were definitely active.

[28] The only criterion for seleciton into the subsample was having a phonenumber. The total number of firms in the registry is 2958; phonenumbers are available for 1249 observations. One might worry that firms providing a contact phone-number differ from those that do not, yet one should consider that different subcity collect different details, so that contact phonenumbers are documented in some subcities, but not in others. The response rate for this interview as high (83% for the firms that we managed to contact.)

good approximation to the true underlying population of firms.

Given the heterogeneous nature of activities in the construction sector, one key concern was making sure that the sub-samples of program and non-program firms were both comparable and representative. To achieve this, program firms were sampled on the basis of their activities, with the sample frequency corresponding roughly to the population frequency as derived from the AAIHDP list, with slight oversampling of firms in activities whose population frequency is low, and excluding marginal activities. Details can be found in the appendix. Secondly, the population of non-program firms engaging in comparable activities was identified, by excluding firms engaging in activities in which program firms do not (e.g. road construction). Again the sample frequency of firms engaging in different types of activities roughly corresponds to the observed population frequency. Thirdly, a distinction was made between contractors and non-contractors. Contractors with a license grade between 1 and 6 are considered large, as they are allowed to execute structural works. Large contractors were oversampled since there are relatively fewer of them. The sampling for non-contractors did not take the size of the firms into account.

The external validity of our sample is good, to the extent that the registry of firms and the program lists provide approximate representations of the true underlying population of firms. Judging to what extent the findings for the construction Addis generalize to other countries, and to a lesser extent to other cities in Ethiopia, is much more difficult in view of the idiosyncratic aspects of the construction sector in Addis, such as the severity of input constraints, restrictive land policies and the legacy of marginalizing the private sector.

Workers Up to four workers per firm were interviewed, stratified by occupational category in order to ensure occupational heterogeneity in our sample and to obtain a better picture of the workforce in different types of firms. The sample of workers contains disproportionately many wokrers form higher occupational echelons (see Appendix).[29] In addition, women, particularly those not employed as casual workers, were oversampled in order to docu-

[29] One should keep in mind, however, that in the worker sample the definition is based on workers's own categorization; it could be that some workers categorize themselves as skilled labourers, while being categorised as unskilled by the firm. Unfortunately, it is not possible to check this since only 4 workers per firm were sampled.

ment their disadvantaged position in the labour market. Enumerators were also instructed to attempt to sample one casual worker per firm.

One potential caveat with our sample arises from having had to request the approval of the manager in order to interview workers. Given the manager's control over whom we could interview, the sampling of workers within occupational categories may not be entirely random.[30] In addition, response bias cannot be ruled out a priori. Yet, most of our questions are factual in nature and most managers were cooperative. In our impression, the assignment of workers for interviews was more due to chance, i.e. being around at the time of our interview, than to strategic selection on the manager's part.

5.2 Descriptives

5.2.1 Firms

A skewed size distribution The size distribution of firms is highly skewed; in our sample of 240 firms, 45% of firms have fewer than 10 workers, the cutoff conventionally used to define a micro-firm, 75% have fewer than 20 workers, 10% have more than 50 workers (and are thus large) and only 3 firms have more than 500 employees, yet these 3 firms account for more than 35% of the total employment in our sample, while firms larger than 50 employees account for 2/3 of all employment[31]. In contrast, firms with fewer than 10 employees account for less than 10% of employment. The distribution of capital is similarly skewed, with 3 firms reporting capital in excess of 10 million Birr, while roughly 2/3rds of the firms has capital worth 100000 Birr or less. The mean capital stock is 499962.3 Birr, while the median capital stock is 45700 Birr.

Types of Firms The sample is equally divided between program- and non-program firms, containing data on 92 program non-contractors, 89 program non-contractors, 28 non-program contractors and 31 program contractors. A program firm is defined as a firm which fulfils at least one of three criteria; i) having been created by the program ii) having received support

[30]The decision to oversample women was taken after pilotting of the survey as the pilotting revealed that women were the least likely to be assigned to give an interview.

[31]Of course, this is partly because large contractors are overrepresented in our sample.

Table 1: **Firms Main Descriptives**

Firmtype	**Revenues**	**Expenses**	**Capital**	**Workers**	**Inputs**	**Age**
Non-Program Non-Contractor	411493.5	491422.1	139055	7.78	241726.5	3.89
	100000	72800	19550	5	61790	2.75
	87	85	84	89	85	91
Program Non-Contractor	261650	220947.9	148498.3	18.10	371979.7	2.26
	120000	117286	40000	13	79952	1.67
	85	84	87	86	77	88
Non-Program Contractor	1810213	1505461	911048.3	50.56	785597.1	5.464286
	756779.5	558484	409350	16	328890	5.5
	24	25	26	27	14	28
Program Contractor	5862614	5020353	2313023	128.78	2282393	6.26
	1628899	1461756	605905	24	1619038	5.75
	30	30	31	31	15	31
Total	1227273	1109722	526276.1	32.64	494363.6	3.78
	150712.5	135270	53125	11	87500	2.04
	226	224	228	233	191	238

From top to bottom: means, medians and observations

from the program or iii) having worked for the program[32]. In addition, a distinction is drawn between contractors and non-contractors, where contractors are defined as firms which have a contracting licensegrade. The differences between contractors and non-contractors are considerable (see Table 1): contractors tend to hire more workers, be more capital-intensive, use more inputs and have higher labour productivity.[33] We are primarily interested in exploring the differences between program non-contractors and non-program non-contractors as this category of firms contains most of the SMEs and has benefitted more from AAIHDP support.

Program firms are larger than non-program firms Program firms employ significantly more workers than non-program firms[34] and the differ-

[32] In addition, there was one firm engaging in pre-cast beam production which did not meet any of these criteria but was classified as a program firm since pre-cast beam technology is not applied by non-program firms as yet.

[33] Note that the program has also set up (small) contracting firms, engaging in activities such as wall construction. This classification does not distinguish such firms from those that were already operating in the construction sector but joined the program.

[34] It should be noted that this effect is particularly pronounced for non-contractors.

ence is significant both for contractors and non-contractors.[35] According to imputed measures of age of the workforce and education, workers in program firms are also slightly younger, though this effect is not statistically significant, and significantly better educated than their counterparts in non-program firms. Focusing on non-contractors, non-program firms have both higher revenues per worker and higher expenses per worker, so that value added per worker is not significantly different in program firms. In addition, non-program non-contractors seem to be slightly more input intensive, but are not more capital intensive than program non-contractors. In contrast, contractors working for the program seemingly use more inputs per workers than contractors not in the program, despite having a similar capital intensity.[36]

Capacity Underutilization & Input Constraints[37] Existing construction capacity is massively underutilized. The average firm operates 9 months per year and at 51% of its capacity. Only 7 firms report operating at 100% of their capacity. Program firms operate at slightly higher capacity than non-program firms (53% vs 50% respectively). Input constraints are the most cited reason for capacity underutilization. Indeed, 80% of non-contractors and 98% of contractors report facing difficulties accessing inputs and 44% of firms indicate they have had to refuse contracts because they lacked inputs. Cement is the scarcest input. Surprisingly, a larger proportion of program firms reports facing input constraints than non-program firms.

Credit Credit is the most important obstacle to operating successfully on a day-to-day basis according to the majority of firm-managers. Program firms are as likely as non-program firms to cite credit constraints as their major problem, even though a much larger proportion of them managed to obtain formal credit; 39% of non-program firms took out a loan, while 72% of program firms took out a loan. The difference can be attributed entirely to the better access program firms have to formal credit. While interest rates

[35] Significant in the sense that equality of means is rejected at the 5% level by a simple T-test.

[36] T-tests for equality of means in input-intensity significantly reject the null at the 10% level, but not at the 5% level. The t-tests were computed using the logged transformations of the variables. The power of the test is undermined by the fact that standard errors are considerable.

[37] ibid.

for formal loans are similar, program firms are less likely to have to put up collateral for credit. Enabling access to credit is thus major achievement of the program. Yet, 59% of program firms are defaulting on their loans, while only 30% of non-program firms are defaulting on their loan.

Table 2: **Credit**

Firmtype	**Borrower**	**Default**	**FormalCredit**	**Formalinterest**	**Formalcollateral**
Non Program Non-Contractor	35%	10%	14%	9.68%	91%
Program Non-Contractor	76%	53%	65%	10.33%	47%
Non-Program Contractor	50%	21%	43%	9.15%	100%
Program Contractor	61%	19%	42%	8.70	80%
Total	55%	28%	40%	9.91%	65%

Comparing sources of start-up capital confirms the importance of access to credit. For 26% of program firms an MFI constituted the major source of initial capital, whereas the corresponding percentage for firms outside the program is less than 1%. Own saving was the most important source of initial capital for 80% of the firms outside of the program and for 57% of firms in the program. The importance of access to credit is further evidenced by the fact that 75% of the program firms that exited claimed it was because they could not obtain access to credit.

Entry Better access to credit not only helps prevent exit, but also facilitates entry. The differences in median and mean firmage between program and non-program firms are substantial, but not dramatic, suggesting that the program has not stifled all entry (see also section 7.5.1). The average age of a program non-contracting firm is 2 years, while the average age of non-program non-contracting firm is 4 years. In contrast, program contractors are older than non-program contractors, probably reflecting the fact that only large contractors, which tend to be old, can conduct structural works. Firm-size is indeed correlated with the age of the firm ($\rho = 0.24$).

Prices Price deflators were constructed to enable productivity comparison. These price-deflators, whose construction is discussed in the appendix, give a good summary of the relative prices faced by different firms and are consistent with prior knowledge; program and non-program firms face similar prices for inputs, but different prices for outputs. At the mean, firms receive some 10 to 20% less for their produce for the program (see Table 3). These price differences are both a between- and a within-firm phenomenon; the program simply pays less for outputs.

Table 3: **Prices**

Firmtype	**Outputprice**	**InputPrice**	**Program-Outputprice**	**Program-InputPrice**
Non-Program Non-Contractor	1.13	1.02		
	1.08	1.00		
	46	56		
Program Non-Contractor	0.96	1.08	0.95	1.10
	0.97	1.00	0.97	1.02
	71	67.00	58	51
Non-Program Contractor	0.94	0.92		
	0.82	0.74		
	9.00	13		
Program Contractor	1.08	0.88	1.35	1.14
	1.13	0.89	1.44	1.06
	5	13	4	10
Total	1.02	1.02	0.96	1.09
	1.00	1.00	0.97	1.02
	131	149	66	68

means, medians and number of observations

Regressing the price estimators on capital intensity, firm-size, inputs, program participation and firmtype confirms our finding that program firms receive lower output prices,[38] although this effect is no longer significant if the number of workers is included. The coefficient on the size of the labour force is negative. Since it is known that program firms on average larger than non-program firms, we can be confident that program firms indeed face lower output prices.

Turning to input prices, there is some weak evidence suggesting that contractors face lower input-prices. Yet, neither program participation nor firm size are related to the price of inputs.

Program Support Not all program firms received all types of support. 87% of program non-contractors received some support. Of this group, 69%

[38] Results are presented in the appendix.

received a piece of land, 36% received buildings on their land, 31% received machinery and 51% received access to credit via the program. In the group of contractors, 55% of firms received support, reflecting the fact that many such contractors were not created by the program; 41% received land, 29% received buildings as well, 12% of firms received machinery and 29% received credit. The provision of land is the most important support from partaking in the AAIHDP according to 45% of the participants, while access to inputs, demand and training were considered to be the most important form of support by 13%, 12% and 13% of the respondents respectively. The importance of land support is not surprising in view of the notoriously restrictive land policies in Ethiopia and the heavy subsidization of land.[39]

It should also be noted that the targetting of support could be improved upon since 22% of firms supported by the program perished. 59% of such firms received land and 51% received training, yet only 6% received credit via the IHDP, while 75% claimed the lack of access to credit was the most important reason to stop operating. Support to exiting firms is effectively wasted, as it has no long-run impact.

Occupational Structure & Human Capital Contractors pay much higher wages than non-contractors, particularly for supervisory personnel, e.g. managers, engineers, foremen. This finding hints at efficiency wages, a possibility investigated in section 7.4. Program firms pay higher wages than non-program firms, yet workers in non-contracting firms are not as well educated as those in non-contracting program firms. Non-contracting program firms hire more skilled workers than non-contracting non-program firms. For contractors, no such difference seems to exist.

Taking Stock A restrictive land market, shortages of inputs and credit constraints are the key problems faced by the construction sector, which is characterised by a highly skewed size distribution of firms. Large firms are responsible for the largest share of labour demand. The IHPD seems to have achieved some success in alleviating credit constraints and has also managed

[39] 27% of program firms do not pay anything for their land. Once these are excluded, the median and mean payments for land by program and non-program firms are similar. Note that while many program firms have complained about the quality of land provided to them, non-program firms are equally likely to complain about such issues, which seem to be pervasive as 42% of firms complain about problems with utilites and 45% about problems with market access.

to provide program firms land, though not all program participants have received support in equal measure. While program firms are distinctly larger than non-program firms, the descriptive statistics do not suggest that they are more productive or more capital- or labour-intensive, though there is some evidence that program non-contractors are less inputs-intensive and program contractors are more inputs-intensive than their non-program counterparts. Program firms also seem to pay higher wages and hire more skilled workers. We will return to these issues in section 6.1.1, when we continue the analysis of the firm-level data.

5.2.2 Workers[40]

General descriptives Estimating the impact of program participation on earnings is a key objective of this paper. To ensure comparability between the sample of program participants and non program participants, students, whom constituted a substantial part of our sample, and casual workers not attached to any firm are dropped from the sample, leaving us with 703 observations.

Simple descriptive statistics are presented in tables 4 and 5. The workers in the construction sector tend to be very young with an average age of 28,6. The sector is dominated by men, as only 18,5% of our sample are women, despite oversampling them, as well as by migrants from rural areas; no less than 54% of the workers in our sample were not born in Addis, and 75% of such workers (42% of the total sample) arrived when they were adults. Educational attainment varies considerably but is rather low on average; while the average worker has gone to school for almost 10 years, only 1% of workers have a college degree, 10% have a degree higher than secondary school and 60% enjoyed some form of secondary education.

Wages in the construction are very low. The median wage is 390 Birr a month, or less than $2 a day, yet this statistic hides heterogeneity across occupations; engineers, the best educated workers, earn the most with a median wage of 2000 Birr per month, while unskilled labourers, the least educated workers, are the worst off with a median monthly income of 311 Birr. Large firms, defined as those firms having 50 employees or more, pay higher wages than small firms for almost all occupations. Overall, the distribution of earn-

[40] Note that our sample overrepresents skilled workers, yet I present the unweighted sample averages. Weighting on the basis of the frequency of occupational categories does not change the results markedly.

Table 4: Non-Program Participants-Descriptive Statistics

	Permanent			Casual		
Variable	**Mean**	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**	**N**
Age	29.607	8.893	275	26.651	6.632	83
Sex	0.804	0.397	281	0.843	0.366	83
Casualdummie	0	0	281	1	0	83
Monthlypay	636.163	1396.347	252	309.518	322.429	81
Dayspermonth	23.588	3.012	279	17.843	9.083	83
Apprenticepast	0.274	0.447	281	0.241	0.43	83
Highest Educational Attainment						
Primary Incomplete	0.189	0.392	281	0.253	0.437	83
Primary Complete	0.185	0.389	281	0.229	0.423	83
grade10	0.164	0.371	281	0.181	0.387	83
grade12	0.285	0.452	281	0.133	0.341	83
grade12plus2	0.096	0.295	281	0.036	0.188	83
college	0.018	0.132	281	0	0	83
TVETcomplete	0.192	0.395	281	0.048	0.215	83
Prior Activity						
-unemployed	0.146	0.354	281	0.337	0.476	83
-private sector employee	0.313	0.465	281	0.133	0.341	83
-self employed	0.078	0.269	281	0.024	0.154	83
-casual worker	0.153	0.361	281	0.145	0.354	83
-student	0.235	0.425	281	0.169	0.377	83
-public sector employee	0.046	0.21	281	0.024	0.154	83
-inactive	0.007	0.084	281	0.012	0.11	83
Employment History						
-government employee	0.135	0.343	281	0.072	0.261	83
-private sector employee	0.441	0.497	281	0.398	0.492	83
-domestic	0.053	0.225	281	0.096	0.297	83
-familywork	0.028	0.167	281	0.012	0.11	83
-self employed	0.089	0.285	281	0.036	0.188	83
-working for a cooperative	0.025	0.156	281	0.012	0.11	83
-unemployed	0.473	0.5	281	0.735	0.444	83

Table 5: Program Participants-Descriptive Statistics

	Permanent			Casual		
Variable	**Mean**	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**	**N**
Age	29.139	7.031	230	25.959	5.261	73
Sex	0.842	0.366	234	0.74	0.441	77
Casualdummie	0	0	234	1	0	77
Monthlypay	1114.189	2586.259	221	344.433	217.35	77
Dayspermonth	23.667	2.549	234	20.649	7.828	77
Apprenticepast	0.449	0.498	234	0.429	0.498	77
Highest Educational Attainment						
Primary Incomplete	0.12	0.325	234	0.299	0.461	77
Primary Complete	0.137	0.344	234	0.234	0.426	77
grade10	0.239	0.428	234	0.169	0.377	77
grade12	0.35	0.478	234	0.195	0.399	77
grade12plus2	0.128	0.335	234	0.013	0.114	77
college	0.013	0.113	234	0	0	77
TVETcomplete	0.44	0.497	234	0.104	0.307	77
Prior Activity						
-unemployed	0.167	0.373	234	0.221	0.417	77
-private sector employee	0.359	0.481	234	0.195	0.399	77
-self employed	0.068	0.253	234	0.026	0.16	77
-casual worker	0.184	0.388	234	0.299	0.461	77
-student	0.162	0.37	234	0.143	0.352	77
-government employee	0.047	0.212	234	0.039	0.195	77
-inactive	0	0	234	0	0	77
Employment History						
-government employee	0.205	0.405	234	0.104	0.307	77
-private sector employee	0.44	0.497	234	0.455	0.501	77
-domestic worker	0.103	0.304	234	0.104	0.307	77
-family worker	0.034	0.182	234	0.026	0.16	77
-self employed	0.051	0.221	234	0.065	0.248	77
-working in a cooperative	0.145	0.353	234	0.065	0.248	77
-unemployed	0.684	0.466	23	0.805	0.399	77

ings is well-behaved; the log of monthly income is approximately normally distributed.

Casual vs Permanent Workers Comparison of permanent and casual workers reveals that permanent workers are older and better educated than casual workers, work more days per month and earn more. Indeed, when asked why they choose to become casual workers, 91% of the casual workers answered they lacked alternative opportunities. In contrast, only 5% indicated they liked the activity. Casual unemployment is thus largely involuntary. Contrary to popular wisdom but in line with the argument made by Garrett (2003), there is no evidence of casual workers disproportionately diversifying their incomes and taking on a multitude of activities to make a living; only 6% of casual workers indicate to have another income generating opportunity, while 12% of permanent workers indicate to have other income generating opportunities. In terms of employment history, a much larger proportion of casual employees documents having experienced an unemployment spell, and a smaller proportion has experience working as a government employee than permanent workers do.

Program Participants vs Non-Participants Workers in program firms earn significantly more than workers in non-program firms, yet have lower household expenditure.[41] Household expenditure was predicted on the basis of asset holdings, see the appendix for details. Permanent workers in program firms are also much better educated than permanent workers in non-program firms. Casual workers in program firms work more days than casual workers outside program firms.[42]

Workers permanently employed in program firms are much more likely to have a TVET degree than workers in non-program firms and are also slightly younger on average. Furthermore, program firms seem to hire more female casual workers, though there is no such difference for female permanent workers.

Moving on to a comparison between the activities workers were undertaking immediately prior to their current job, see table 4 and 5, it emerges that the differences between workers in different types of firms are far from dra-

[41] Expenditure was predicted on the basis of asset holdings. See the appendix for information on the construction of this variable.

[42] T-tests reject equality of means for all three propositions.

matic in this respect. Judged on sample proportions, program firms do not draw disproportionately on the unemployed, students and inactive workers.

Looking not just at the activity immediately prior to the current one, but to the entire employment history of workers, as documented in table 4 and 5, reveals that larger proportions of workers in program firms report having experienced an unemployment spell at some point in the past, having worked as a domestic employee, having been employed by the government, and having worked in a cooperative. Surprisingly, a lower proportion of such workers indicated to have been self-employed or active as a casual worker.

It should be noted that th differences between program participants and non-participants are most pronounced for permanent workers; the differences between casual workers in and outside of the program are marginal.

Taking Stock These simple descriptives suggest that the program has neither been very successful at providing more jobs for low-skilled workers nor in providing opportunities for the unemployed, the inactive or workers in marginal jobs. Yet, workers in program firms do earn significantly more and have lower predicted household expenditures. If one is willing to believe that predicted household expenditure estimated on the basis of asset holdings is a good measure of long-term wealth, then the program might be argued to be reasonably successful at the targeting the poorest educated workers. Note, however, that the program seems to do very little for unskilled workers.

6 Methodology & Empirical Strategy

This section lays out the empirical strategy for assessing the impact of the program on labour demand and wages. It also explains how we attempt to gauge the general equilibrium effects of the program.

6.1 Labour Demand

6.1.1 Do Program Firms generate more jobs?

For any given output level, technology and factor proportions choices determine labour demand. Documenting and explaining the technology and factor proportion choices of both program and non-program firms is thus a necessary step towards assessing whether AAIHDP firms generate more jobs than non-AAIHDP would have. Recall that housing construction by the private sector is the counterfactual.

The production function Under the assumptions of profit maximization and perfect competition, labour demand can be derived from the production function. Our starting point is the translog production function, which encompasses the conventional Cobb-Douglas specification and can be interpreted as a second order approximation to a more general specification. After verifying that Cobb-Douglas restrictions are valid we proceed with estimating the more parsimonious Cobb-Douglas production function with physical capital K, human capital, H and inputs, I as factors of production and Y as a measure of total output or value added: $Y = AK^{\alpha}H^{\beta}I^{\gamma}$

Making the assumption that human capital can be proxied by the size of the labour force, L, and taking logs yields the conventional estimable equation;[43]

$$\ln Y = \ln A + \alpha \ln K + \beta \ln L + \gamma \ln I + v \tag{1}$$

To test whether technologies of differ between firms, this equation is augmented with interaction terms between the different factors of production and technology, yielding: .

$$\ln Y = \ln A + \alpha \ln K + \beta_u \ln L_u + \rho_A PA + \rho_L PL + \rho_K PK + \rho_I PI + \gamma \ln I + z \tag{2}$$

For example, if one is testing for differences in technology between contractors and non-contractors, PA is a program participation dummy, PL is the interaction between program participation and labour, PK, the interaction between program participation and capital, PI, the interaction between program participation and inputs. Under the null hypothesis of a common technology, none of such interaction terms should be significantly different from zero: $\rho_K = \rho_L = \rho_I = \rho_A = 0$.

[43] This equation can also be written to yield productivity per worker.
$\ln \frac{Y}{L} = \ln A + \alpha \ln \frac{K}{L} + \gamma \ln \frac{I}{L} + +(\beta + \alpha + \gamma - 1) \ln L + v$
Assuming CRS, this equation reduces to:
$\ln \frac{Y}{L} = \ln A + \alpha \ln \frac{K}{L} + \gamma \ln \frac{I}{L} + w$
Note that assuming CRS reduces the number of parameters to be estimated from 8 to 6.

Endogeneity Possible endogeneity of the error term with the observable covariates, arising either from simultaneous determination of outputs and inputs and/or from correlation of observed inputs with omitted unobserved inputs, constitutes the fundamental econometric problem in production function estimation (Ackerberg, 2003). Fortunately, our research design allows us the luxury of being able to use input- and output prices as natural instruments. Information on prices also helps us to discriminate between productivity and profitability. Assuming that outputs are homogeneous enables the construction of firm-specific input and outputprice deflators, which in turn can be used to yield more appropriate measure of output.

Further instruments are initial size, capital stock at startup, and the age of the firm. These are expected to be related to correlated to the current size and factor proportion choices of firms. The validity of these instruments hinges on the absence of a correlation with the error term of the production function. If, for example, learning by doing is important, or if older firms have better networks, enabling them to acquire jobs and inputs more easily, such a correlation might well arise and the instruments would be invalid. Fortunately, Sargan tests of overidentifying restrictions typically comfortably accept the exogeneity of the instruments, as demonstrated in section 7.

Allowing for Heterogeneous Labour The basic specification presented in equation 2 is somewhat unsatisfactory, since it implicitly assumes that labour is a homogeneous input, while it is known that workers in program firms are on average betted educated and therefore likely to be more productive. Moreover, our interest lies in documenting the demand for different types of labour.

One possibility to deal with the heterogeneity of labour is to assume that human capital, H, is a function of the educational attainment of the workforce, E, and the number of workers L. Assuming that $H = Lexp^{h(E)}$ allows us to control for both the quantity of labour, proxied by the number of workers, and heterogeneity in the quality of labour, proxied by some function $h(E)$ of the average educational attainment of workers. Taking logs and imposing a linear functional form for $h(E) = \frac{\delta}{\beta} * E$ provides us with an estimable equation of the human capital augmented production function:

$$\ln Y = \ln A + \alpha \ln K + \beta \ln L + \delta \ln E + \gamma \ln I + u \tag{3}$$

The "quality " of the labour force matters if $\delta \neq 0$.

A Caveat Merely establishing differences in factor proportions and technologies does not suffice to assess the additionality of employment creation by the program, as it does not tell us what would have happened should the program not have been implemented. The current choices made by non-program firms provide some indication as to what factor proportion and technology choices incumbents and potential entrants would have made, had the program not been introduced. Yet, they are not perfect proxies for such choices since the program may have affected (the evolution of) i) the prices and availability of inputs ii) the prices of and demand for outputs iii) the available technologies. Even if this were not the case, one has to keep an open mind about path dependencies stemming from adjustment costs, learning by doing or the evolution of the size distribution of firms more generally, see section 6.3.

6.1.2 Do AAIHDP Firms Hire Proportionally More Poor Workers?

Under the assumption of profit maximization and perfect competition, equation 3 tells us what type of labour is demanded by different types of firms, thus also telling us something about the relative position of low-skilled workers. Such an analysis is complemented by estimating probit selection models of passing the test and being hired by a program firm using worker level data.

6.2 Wages: How much do workers benefit from being employed in a program firm?

6.2.1 The Framework

The Treatment Effect of Interest Establishing the impact of the program on earnings requires us to estimate how much more, or less, workers in program firms earn than they would have earned outside of the program. In the terminology of the potential outcomes approach, we are interested in estimating the Average Treatment Effect on the Treated=$ATT(X) = E[Y_{1-}Y_0|X, D=1]$, the impact of the program on those who actually participated. Unfortunately, the counterfactual earnings outside of the program are unobserved and individual-specific, forcing us to make strong assumptions to identify the treatment effect.[44]

The Basic Selection Problem Conditional on observable covarariates, X, a comparison of earnings of program and non-program participants with similar observable characteristics will yield an unbiased estimate of the impact of the program as long as $E[Y_1|X, D = 0] = E[Y_0|X, D = 0]$,[45] that, is as long as the earnings outside the program are a reliable indicator of what program participants would have earned, should they not have participated. Yet, it is well possible that the earnings of participants and non-participants would have differed even if participants had not taken part in the program. Due to self-selection, participants may differ from non-participants in terms of unobserved characteristics, U, which may affect both earnings and the probability of being employed in a program firm. For example, if the test for program participation discriminates against less able workers, then the average treatment effect might be biased upwards, as able workers would have been likely to obtain a good paying position elsewhere. In contrast, if people with fewer opportunities (for example because they lack the social network that is instrumental in searching for a job in Ethiopia) are more likely to apply for program participation, then the ATT could be downward biased, since the program participants would have been less likely to land a well paying job than non-program participants. A priori it is thus difficult to sign the bias.

[44] As the treatment effect is essentially an individual-specific random variable which is unobserved, the best one can do is estimate population averages (see e.g. Caliendo and Hujer (2005)). For population averages to be informative about the treatment effect, it must be assumed that the non-treated and treated are drawn from the same underlying population. Furthermore, it must be assumed that those who were not treated were not affected by the treatment of others. In statistical terms, this is known as the Stable Unitary Treatment Assumption (SUTVA). In economic terms, this means that general equilibrium effects, such as spillovers, are assumed away.This is clearly a very stringent assumption. For example, if the IHDP has increased labour demand, it will most probably have exerted upward pressure on wages for all workers in the construction sector. Similarly, the allocation of inputs is zero-sum; the inputs allocated to IHDP firms are no longer available to other firms, thus restricting the amount of output they can produce and indirectly their demand for labour.

[45] Note that unobservables are allowed to affect Y_1.

The Toolkit: Estimating Treatment Effects This paper employs switching regressions, matching estimators, control functions and instrumental variable methods to estimate the treatment effect. Switching regressions and matching rely on "selection on observables" or" ignorability of treatment", the assumption that once observable attributes are appropriately controlled for, the distribution of the counterfactual outcome Y_0 in the treated group is the same as the distribution of the observed outcome Y_0 in the control group $(Y_{1,}Y_0|X||D)$. In contrast, control function approaches and instrumental variableallow for selection on unobservables.

Switching Regressions Assuming a (log-)linear Mincerian relationship between earnings Y, observable characteristics, X, such as age, education and firm-characteristics, and a linearly additive impact of program participation D,

$$Y = \alpha + \beta_X X + \beta_T * D + \epsilon \tag{4}$$

In this scenario, the OLS estimator of β_T is an unbiased estimate of the ATT, which coincides with the ATE, as long as ϵ is not correlated with X or D, as implied by selection on observables. If there is selection on unobservables, the residual, ϵ, will be correlated with the decision to join the program D and β_T will be biased.[46]

Propensity Score Matching Matching estimators are less restrictive than switching regressions as they do not make assumptions on functional form.[47] The idea behind matching is to ex-post mimic experimental conditions by selecting a comparison group that is as similar as possible to the treatment group in terms of observable characteristics that might affect the outcome. However, just as switching regressions, matching relies crucially on the assumption that controlling for observables suffices to create conditional independence between treatment and outcomes and consequently assumes away

[46] Note that the specification is restrictive: the program has a linearly additive impact on earnings. Of course, it is well possible that the benefits of program participation interact with the observables or unobservables, in which case a more appropriate specification would be (see e.g. Blundell and Costas Dias (2003))

$Y = \alpha + \beta_X X + \beta_{dX} DX + \beta_T * D + \epsilon_0 + D * (\epsilon_1 - \epsilon_0)$

The possibility of interactions between treatment and covariates was controlled for but not found to be important.

[47] Matching estimators belong to the class of non-parametric estimators.

the problem of unobservables. Furthermore, it is customary to invoke the assumption that treated individuals have a counterpart over the set of observable covariates X over which a comparison is made: $P(D = 1|X)$ <1 for all $X \in C$. This assumption is know as the common support or "overlap" assumption and essentially rules out the possibility of having no (positive probability of) variation in the treatment assignment[48]. If no such overlap exists for all observations, comparison of restricted and non-restricted individuals ought to be restricted to the common support region.[49]

Matching on a large number of covariates is difficult because of the high-dimensionality of the problem. Rosenbaum & Rubin (1983) proposed to match on the propensity score, p(x), the probability of being treated given the full set of observable characteristics instead, and showed that selection on observables $(Y_{1,}Y_0|X||D)$, also implies that conditioning on the propensity score suffices to create conditional independence between treatment assignment[50] and earnings $(Y_{1,}Y_0|p(X)||D)$. The idea behind propensity score matching is thus that individuals with the same propensity score but different treatment assignments can be used as controls for one another.

A major drawback of propensity matching is that there are a multitude of choices to make in implementing propensity score matching, such as choosing the matching algorithm, and that different matching procedures produce radically different results. Furthermore, there is no consensus as to how to best assess whether covariate balance has been achieved (Ho et al., 2007), that is, whether matching has been successful.[51] Given the strong assumptions underlying matching and the divergence in effects estimated with different matching estimators, the matching approach is our least preferred method to estimate impact of the AAIHDP[52].

[48]The resulting estimate of the treatment effect is then an estimate of the effect on the treated in the support region, but not the sample as a whole. As a result, matching estimators refrain from making out of sample predictions, yet by presenting a partial picture they may fail to provide a reliable estimate of the overall effect of the program.

[49]NB If one is also interested in etimating the ATE, one has tomake the further that restriction $P(D = 1|X) > 0$ for all $X \in C$.

[50]Of course not treatment itself.

[51]Ho et al. (2007) point out that the goal of matching is not necessarily to produce perfect matches, but rather to reduce bias. He notes that matching is rarely followed by analyses more sophisticated than mere t-test of means, while, in principle any estimation method that would control for the remaining biases would be justifiable.

[52]Heckman (2003) shows that control function methods are more robust to misspecification of the control variables.

Nevertheless, radius and kernel-based matching estimates of the ATT will be presented, using different bandwidths and calipers. Throughout, the common support restriction is imposed to ensure we are comparing the comparable. Consequently, our matching estimate should be interpreted as the effect on the treated for whom comparable observations exist. Radius matching is a generalization of nearest neighbour and caliper matching[53], but avoids bad matches by imposing a tolerance level, a caliper, on the maximum difference in propensity scores between treated observations and controls and as such attempts to minimize bias due to comparing dissimilar individuals. In addition, it uses all observations within the caliper for comparison, thereby minimizing the variance of the estimator. In caliper matching only a few controls are used for each treated individual. Kernel matching estimators use all available controls in the sample, assigning a different weight to each, and consequently have lower variance, yet risk introducing bias by comparing dissimilar individuals. The choice of bandwidths and calipers both involve a trade-off between bias and variance, with higher bandwidths and calipers leading to lower variance but at the risk of introducing bias.

Control function approach We now turn to methods that tackle selection on unobservables. Returning to the framework provided by switching regressions, one possible solution to the potential endogeneity problem is to purge the residual ε of its bias due to correlation with program participation, by modelling the selection on unobservables explicitly. A popular control function is the Heckman selection model. If the selection equation is $D = E\gamma + u$, where u is a zero mean error term and E is a vector of covariates at least one of which is not part of M, and ε and u are jointly normal, then the ATT, β_T, can be retrieved from an OLS regression on observable covariates X, program participation D, and the inverse Mills ratio.

$$Y = \alpha + \beta_X X + \beta_T * D + \lambda \frac{\phi(E\gamma)}{\Theta(E\gamma)} + \epsilon \tag{5}$$

The disadvantage of this approach is that it requires us to make rather strong assumptions on the distribution of the error term. Furthermore, it is highly desirable, though not stricly necessary, to have an exclusion restriction, Z, a variable associated with selection, but not with the earnings

[53] Nearest Neighbour matching estimators can produce inconsistent results, see Heckman (2003).

equations, for otherwise the control function approach is identified solely on distributional assumption[54].

Instrumental Variables Instrumental variable methods are a standard solution to the problem of endogeneity. We need a variable Z, correlated with program participation i) $Corr(Z, D) \neq 0$, but not with the residual in the earnings equation ii) $Corr(Z, \epsilon) = 0$.[55]. Note that Z is analogous to the exclusion restriction in the control function approach, but IV-methods do not require us to make distributional assumptions. If valid instruments can be found, and if these instruments are binary, then the ATT can be defined as:

$$ATT_{iv} = \frac{E[Y|X, Z=1] - E[Y|X, Z=0]}{P[D=1|Z=1] - P[D=1|Z=0]} \tag{6}$$

The ATT can be estimated by a 2SLS regression, where the coefficient on the instrumented participation dummy will give the ATT. However, if the treatment gain is not the same for all individuals, this coefficient cannot be interpreted as a standard ATT or ATE, but must instead be interpreted as a LATE, a local average treatment effect, that is the effect of treatment on the treated who would actually change their treatment status in response to a change in the instrument.

The major drawback to the instrumental variables approach is the difficulty of finding convincing instruments. Even if instruments orthogonal to the residual, they may be "weak", that is not sufficiently strongly correlated to participation, causing inefficiency and possibly, inconsistency of the IV estimates (see e.g. Bound et al. (1995), Staiger and Stock (1997)).

Having experienced an unemployment spell in the past and having experience working in a cooperative are the instruments for program participation used in this paper. These variables are strongly correlated with program participation, but not with earnings. The idea behind these instruments is that they may reflect preferences for certain types of employment, for example a dislike of casual work and/or a desire to be employed in a firm. The

[54]Aachen (1986) points out that the Heckman model may in fact worsen selectivity bias if the selection model does not perfectly predict participation, since in such case the amount of unexplained variation in the outcome equation has reduced, thus effectively increasing the impact of remaining endogeneity due to selectivity not controlled for.

[55]In addition, Z may not be determined by X.

variables are not correlated with earnings once the standard explanatory variables have been controlled for. I acknowledge that the conceptual validity of these instruments is open to the criticism that these dummies capture part of a worker 'human capital, since on the job learning is one mechanism by which human capital is imparted. In order to assess the strength of this criticism, I ran regressions on earnings including the duration of the last significant unemployment spell, as well as the duration of having worked for a cooperative as explanatory variables, but they did not have a significant impact. Of course, there are other ways in which these dummies could be correlated with earnings potential not captured by the control variables included in the regressions. Unfortunately, it is difficult to test for such assumptionss.

6.3 Identifying the impact on non-participants

Identifying the impact of the program on non-participants is very difficult. Nevertheless, data on entry and exit were collected by means of contacting 600 firms listed in the registry of firms. In addition, the questionnaire contained subjective questions on the impact of the program. In view of data-constraints the scope for making quantitative estimates of the impact of the program is limited since longitudinal data are lacking. Instead, we will largely confines ourselves to suggestive descriptives. In doing so, this paper compares favorably to other papers on the impact of SME support which typically remain silent on the issue, though they generally acknowlege its importance.

7 Results

7.1 Which type of firms participate in the program?

Table 6 presents results of probit regressions of program participation on firm selection. In interpreting these regressions, we should be careful not to draw causal inferences; rather, the probit regressions ought to be interpreted as a descriptive tool to analyse the characteristics of participants and non-participants. Indeed, the results of the probit estimations confirm the descriptive statistics presented in section 5.2.

Perhaps the key distinguishing feature of program firms is that they are on average larger than non-program firms. Column 1 presents the results of a

Table 6: Selection Into the Program- Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Workers(log)	.597*** (.136)		.630*** (.176)	.998*** (.289)	2.041 (2.378)	.830** (.364)	5.404* (3.138)
Capital(log)	.048 (.057)		.022 (.076)	-.006 (.093)	1.508 (1.080)	.160 (.168)	1.060 (.769)
Inputs(log)	-.048 (.066)		-.028 (.086)	-.079 (.106)	1.135 (.915)	-.219 (.252)	-.882 (.785)
Firmage		-.036 (.025)	-.033 (.034)	-.045 (.044)	-.355 (.281)	.010 (.112)	-.047 (.135)
Startcapital		-.004 (.036)	-.016 (.049)	.038 (.057)	-1.736 (1.503)	.117 (.117)	-.927* (.512)
Laboratstartup		.035*** (.008)	.010 (.012)	.017 (.022)	-.031 (.031)	-.0002 (.008)	-.027 (.022)
Landconstrainted		-.190 (.199)	-.107 (.254)	.148 (.332)		-.393 (.659)	
Creditconstrained		-.268 (.209)	-.070 (.258)	-.240 (.319)		1.461* (.852)	
Inputconstrained		.446 (.279)	.695* (.383)	.892** (.427)		1.232 (.902)	
Skillsdifficulty		-.328 (.222)	-.210 (.284)	.027 (.362)	-3.648 (2.864)	-.085 (.617)	-3.773* (2.127)
Locationproblem		.195 (.201)	.501* (.263)	.344 (.315)	6.328 (4.871)	.278 (.543)	5.755* (3.196)
Formalloan		.956*** (.214)	1.015*** (.267)	1.323*** (.326)	-3.907 (3.608)	.207 (.579)	1.253 (1.047)
Informalloan		.249 (.244)	.235 (.303)	.035 (.362)		1.030 (.682)	
Agewworkforce		-.045** (.022)	-.013 (.026)	.002 (.030)	-.297 (.254)	.004 (.057)	-.559* (.324)
Educworkforce		.096*** (.033)	.155*** (.046)	.163*** (.054)	1.606 (1.634)	.266** (.121)	1.328 (.852)
Constant	-1.357* (.700)	.082 (.882)	-3.196** (1.489)	-4.629** (1.954)	-18.487 (22.606)	-8.406** (3.357)	.120 (5.146)
N	175	220	163	139	24	93	34
Pseudo-R2	.121	.257	.345	.468	.579	.435	.68
Chi2	29.353	78.38	77.851	90.162	18.865	31.149	30.748
Log-likelihood	-106.621	-113.221	-74.054	-51.234	-6.868	-20.188	-7.243

probit regression of participation on all factors of production. Participation is only correlated with the size of the labour force, not with the other factors of production. This result is further elaborated upon in the next section.

Column 2 presents the results of a probit model using size at startup, firm age, different types of constraints, and characteristics of the workforce as explanatory variables. The results show that program firms not only tend to be larger now, but also employed signficantly more workers at startup. Furthermore, program firms have a workforce which is better educated and younger than the workforce of non-program firms, though the negative coefficient on age becomes insignificant once factor proportions are controlled for. In addition, program participants have significantly better access to formal credit. Quite suprisingly, the age of the firm is not associated with the probability of participating once initial size variables are controlled for. None of the constraints-dummies are significant, suggesting that program and non-program firms are equally likely to report facing, credit-, and land-constraints, as well as difficulties with their location. On the other hand, program firms are more likely to indicate suffering from input-problems, an effect which is significant at the 10% level.

Once controls for factors of production are added (see column 3), the coefficient on the size of the labourforce at startup loses significance. In addition, the age of the workforce is no longer a significant predictor of program participation. Yet, the results continue to show that program firms employ signifcantly more workers, which are typically better educated, and that program firms have better access to formal credit.

The specification in Column 3 is our preferred specification[56] and is estimated separately for the subsamples of non-contractors (column 4) and contractors (column 5) to see whether the differences vary with the type of firm.[57] The results for non-contractors are much more pronounced; all the coefficients which were significant before have increased. In addition, the coefficient on reporting to face input constraints is now significant at the 5% level. In contrast, the predictive power of the specification is much lower for contractors. While this may be partly due to the fact that the number of

[56] I also tried including subcity- and activity- dummies. These had no predictive power, apart from the activity-dummies which captured activiteis almost exclusively conduced by program firms (such as pre–cast beam production).

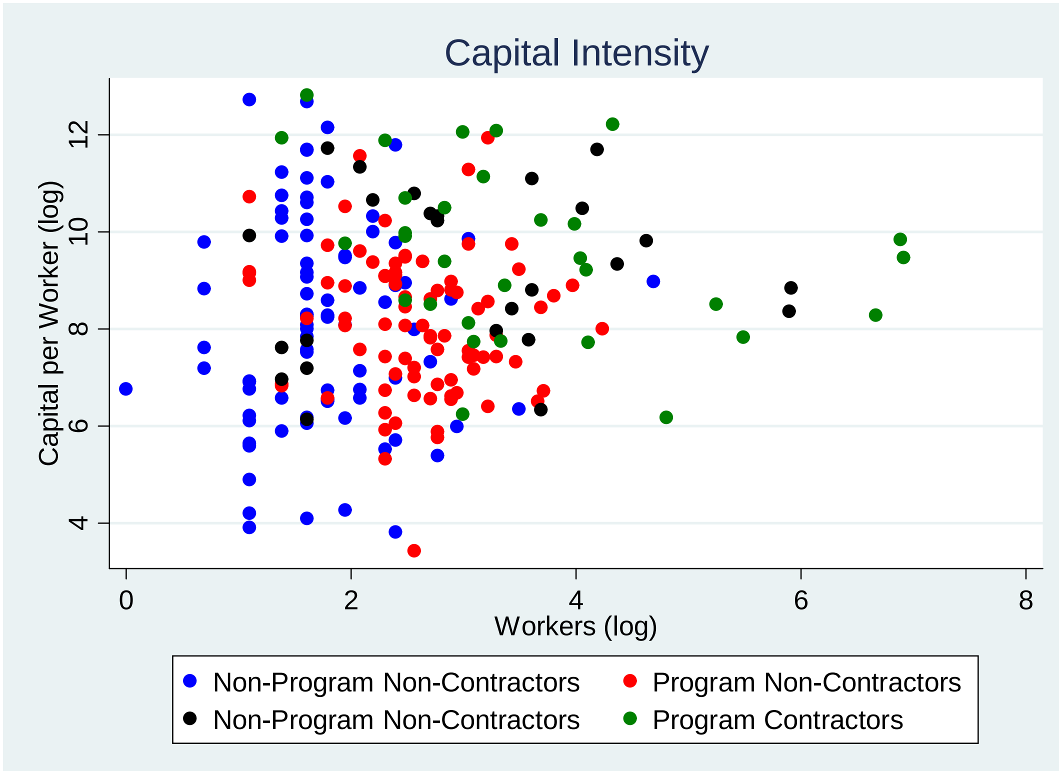
[57] Note that the constraints-dummies and the dummy on having obtained informal credit were dropped when these exhibitted little or no variation over the subsample on which the preferred specification was estimated (e.g. columns 5 and 7), typically small subsamples.

observations for contractors is low, it is not surprising that contractors in and outside of the program seem to be very similar, since contractors were typically created prior to the inception of the program.

Thus far, the results have only provided insight into the characteristics of firms in and- outside of the program. If one is interested in selection into the program it may be sensible to exclude those firms that received support from the program, since their characteristics may have been affected by this support. Coimparing the characteristics of firms that did not receive support but still joined the program with those that did not join the program arguably provides a better idea of the differences between firms that chose to join the program and those that decided not to. Column 4 presents the results of estimating the preferred specification on the subsample of non-support recipients; the dummy on acess to formal credit loses significance, but the coefficients on the size and average educational attainment of the labour force are still significantly positive.

In an attempt to obtain an idea about the participation decision of pre-existings firms via a different route, column 6 excludes firms created in the last 3 years. Unfortunately, this leaves us with a rather small number of observations. Yet, again program firms are larger, had less capital at startup, employ a younger workforce and are less likely to indicate facing skills-difficulties. Perhaps the very high coefficient on the size of the firm is due to the fact that contractors tend to be older and that this sample consequently overrepresents contractors.

Summing up the results of this section; program firms are larger, have a better educated labour force and have better access to formal credit. Once firm-size is controlled for, program firms do not differ from program firms in terms of capital stock and use of inputs. Program firms and non-program firms are equally constrained in terms of land, credit and location, yet program non-contractors are more likely to report facing input constraints. In addition, we find that program firms were larger at startup, a finding which will be exploited in instrumental variable regressions. The differences between program and non-program firms are most pronounced for non-contractors, while they are likely to be small for contractors. The differences in observable characteristics between firms that were operating prior to the introduction of the program and decided to join the IHDP and those that did are small but suggest that program firms are larger and employ better educated workers. Looking at firms older than 3 years, we find that program firms are on average larger, started with less capital, employ younger workers



and are less likely to face a shortage of skilled workers.

7.2 Do Program firms use more labour?

7.2.1 Factor Proportions

Capital Intensity Graph **??**, which plots the amount of capital per worker against the total number of workers for different types of firms, shows that capital intensity, the amount of capital per worker, does not differ between program and non-program firms and also does not vary systematically with firm size, though contractors are significantly more capital intensive then non-contractors, a result which is robust to controlling for size. SMEs thus do not seem to be less capital intensive than larger firms.

These findings are backed up by regressions of capital intensity on firm-size (column1), firm-size and program-participation (column2), and firm-size, program-participation, and being a contractor (column3) presented in table 7. Neither the program dummy, nor the interactions of the program dummy with firm-size or being a contractor[58] are significant, confirming that program firms are not less or more capital intensive than non-program firms. The coefficient on the log of the labour force is not significant in either specification, demonstrating the absence of a relationship between firm-size and capital intensity. Contractors are significantly more capital-intensive than non-contractors, but the capital-intensity of contractors also does not vary with firm size.[59] The higher capital intensity of contractors is due to the fact that they have been around for longer, and therefore have been able to accumulate capital, and to the activities they engage in; once controls for firm age and firm-activities are included, the coefficient on being a contractor loses significance (see column 4). On the other hand, the coefficient on log of the size of the labour force now becomes significantly negative, most probably because firmage and size are postively correlated..

Input Intensity Graph **??** presents a similar picture for input-intensity using deflated inputs. Input-intensity also does not seem to depend on program participation or on firm-size. On average, contractors seem to use more inputs per workers.

Regressions presented in Table **??** reveal that input intensity[60] is strongly correlated with capital-intensity and that contractors use more inputs per workers, even after controlling for firm-size and capital intensity (see column 4). Size variables and program participation indeed have no impact [61], confirming the intuition from the graphs. Difference in firm age and activities

[58] As a robustness check, controls for different activities were included, to analyse whether the composition of the sample could be driving the results. While firms in differen activites differ in their capital requirements, with electrical installation requiring the least capital and firms engaging in structural works and wall construction possessing the most capital, controlling for activities did not break the result, nor did controlling for the interaction between activities and the size of the firms. Results ommitted to conserve space.

[59] Results omitted to conserve space.

[60] Regressions using the non-deflated input measure can be found in the appendix.

[61] Recall that the mean input-intensity of program non-contractors is significantly different from that of non-program non-contractors at the 10% level, and that the capital intensity of program contractors is significantly different from that of non-program contractors at the 10% level. Yet, the significance of differences between program and non-program firms vanishes completely once other factors, such as capital-intensity, are controlled for.

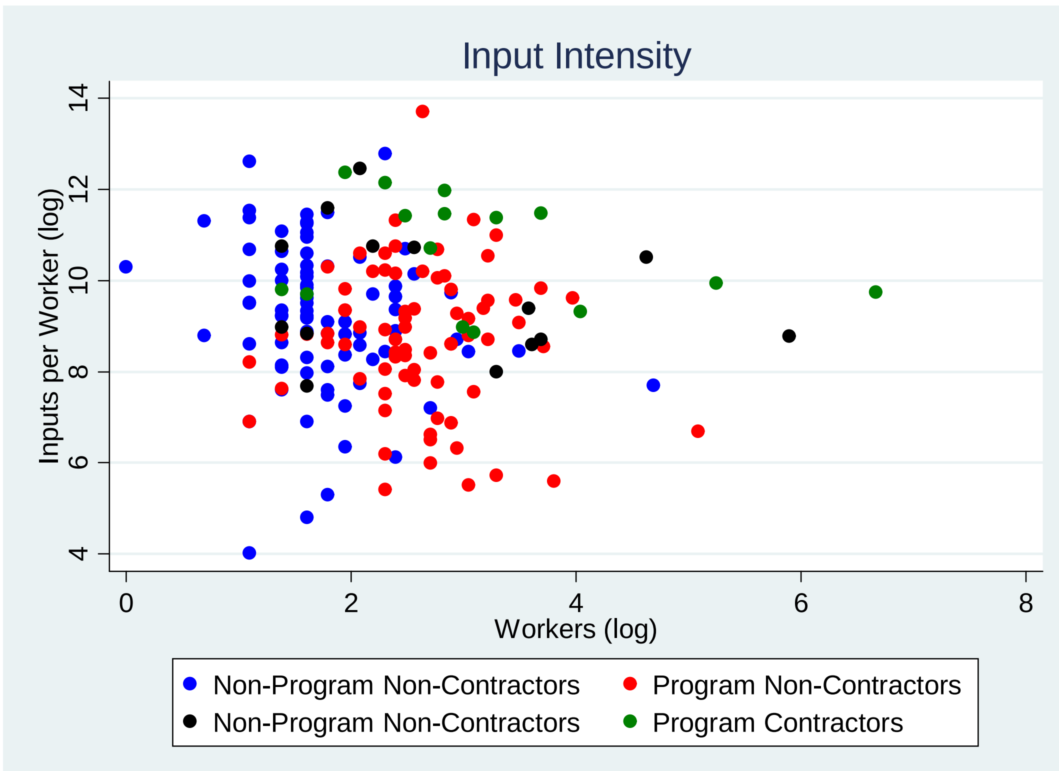

Input Intensity
Inputs per Worker (log)
4
6
8
10
12
14
0
2
4
6
8
Workers (log)
Non-Program Non-Contractors
Program Non-Contractors
Non-Program Non-Contractors
Program Contractors

Table 7: Capital-Intensity

	(1)	(2)	(3)	(4)
Workers(log)	.141	.140	-.159	-.393***
	(.115)	(.124)	(.135)	(.145)
Program-dummie		.006	.191	.195
		(.272)	(.263)	(.285)
Contractor			1.481***	.798
			(.317)	(.498)
Firmage				.096***
				(.036)
Constant	8.084***	8.083***	8.353***	8.323***
	(.312)	(.314)	(.306)	(.406)
N	222	222	222	220
R2	.007	.007	.097	.206
Adjusted R2	.002	-.002	.085	.14

Note: Activity Dummies Surpressed in Column 4

do not account for the difference in input intensity between contractors and non-contractors. One obvious explanation is that contractors face lower input prices, yet their better access to inputs might also be due to their legal status[62] or having better social capital.

Summing up In sum, factor proportions do not seem to vary with firmsize, suggesting that small firms are not more labour intensive then large firms. Moreover, program firms are not more labour intensive than non-program firms. Yet, contractors tend to be much less labour intensive. The absence of a correlation between firm-size and labour intensity contrasts starkly with the pattern prevailing in manufacturing sectors across Africa, as discussed in section 3.

[62] By law, projects of a certain size can only be carried out by contractors with a certain license; in allocating inputs, such contractors may be favoured.

Table 8: Input-Intensity (Deflated Inputs)

	(1)	(2)	(3)	(4)	(5)
Workers(log)	.022	-.184	-.163	-.187	-.058
	(.148)	(.167)	(.164)	(.165)	(.176)
Program-Contractor		-.282	-.336		
		(.312)	(.310)		
Program-Non-Contractor		1.053*	.856		
		(.615)	(.607)		
Contractor		.805	.629	1.245***	1.487***
		(.514)	(.509)	(.378)	(.540)
Capital-Labour-Ratio			.226***	.237***	.216***
			(.075)	(.075)	(.076)
Program-dummie				-.105	.270
				(.284)	(.300)
Firmage					-.014
					(.036)
Constant	9.132***	9.511***	7.647***	7.488***	6.817***
	(.379)	(.367)	(.716)	(.716)	(.783)
N	147	147	144	144	143
R2	.0001	.13	.185	.166	.355
Adjusted R2	-.007	.106	.156	.142	.262

Note: Activity Dummies Surpressed in Column 5

7.2.2 The Production Function[63]

Revenue-Based Production Functions Table 9 presents the results obtained from estimating the basic Cobb-Douglas specification using revenues as output variable and capital, number of workers and inputs as explanatory variables. Unfortunately full data are not available for 70 firms, forcing us to reduce our sample to 170 observations. Starting with the full sample, the parsimonious Cobb-Douglas model fits the data rather well, judged by the Adjusted R2 of .73. Parameter exstimates are consistent with CRS. The significance of the inputs-variable is striking and attests to the

[63] Results for the translog model are ommitted to conserve space, but are available upon request. The translog model accepted Cobb-Dougals restrictions, as evidenced by a Likelihood Ratio test-statistic of 8.27 (Prob>Chi2=0.22). Overall, parameter estimates were reasonably well-behaved, satisfying homotheticity, concavity and constant returns to scale. Unfortunately, the positiveness of the predicted cost share of inputs is violated for 14% of the observations and the parameter estimates are all insignificant, apart from the coefficient on capital.

	All	All	Non-contractors	Contractors
	(1)	(2)	(3)	(4)
Workers(log)	.248***	.196*	.178	.312
	(.083)	(.102)	(.155)	(.208)
Capital(log)	.099***	.052	.042	.052
	(.035)	(.037)	(.044)	(.270)
Inputs(log)	.611***	.574***	.593***	.828**
	(.042)	(.044)	(.054)	(.363)
Contractor		-2.435*		
		(1.373)		
Contractor-Workers		.023		
		(.180)		
Contractor-Capital		.276**		
		(.120)		
Contractor-Inputs		-.037		
		(.163)		
Program-dummie			-.070	-1.961
			(.535)	(2.999)
Program-Workers			.088	-.253
			(.242)	(.294)
Program-Capital			.043	.388
			(.082)	(.303)
Program-Inputs			-.056	-.187
			(.078)	(.412)
Constant	3.404***	4.372***	4.308***	1.369
	(.436)	(.508)	(.578)	(1.923)
N	170	170	143	27
R2	.735	.757	.637	.875
Adjusted R2	.731	.747	.619	.828

Table 9: Production Function - Revenue Based Output Measure

existence of severe input constraints.

Contractors use a technology that is different from the one non-contractors use. Adding interaction terms between each factor of production and being a contractor and adding a dummy for being a contractor to test whether technology differs between contractors and non-contractors (see Column 2), reveals that the share of capital in value added is higher for contractors at the 5% level, while the dummy on being a contractor is negative at the 10% level. The null hypothesis that contractors and non-contractors have the same production function is thus rejected when using revenue as a measure of output.

To test for differences in technology between firms in- and outside of the program, interactions between program participation and all the parameters of the production function are added to the baseline specification. The sample is split up into non-contractors(Columns 3) and contractors (column 4), since the hypothesis that contractors and non-contractors share the same production function was rejected by the data, using revenue as a measure of output. The data do not reject the hypothesis that firms in the program, whether they are contractors or non-contractors, are using a different technology then firms outside the program are using revenue as a measure of output.

Taken together, the results from using non-deflated revenue as a measure of output would imply that labour productivity does not differ between program and non-program firms and also does not vary with firm size. Nevertheless, the capital coefficient for contractors is significantly different from that of non-contractors, suggesting contracting firms use a different technology. Note that to interpret revenue as a measure of output, the law of one price should hold. Yet, the data flatly reject this law (see section 5.2). The revenue measure is thus potentially misleading. We now proceed to show that accounting for price-differences is indeed crucial.

Using Deflated Measures of Outputs and Inputs By deflating both revenues and inputs by the output- and input-price deflators respectively, better measures of inputs and outputs are obtained. Unfortunately, such measures are not available for all observations. The difference between the results obtained using deflated and non-deflated measures is not due to decreased sample size, except in the case of contractors, for whom only 14 observations are left once deflated measures are used.[64] Consequently, the results for contractors described below should be treated with appropriate skepticism.

Table 10 presents the results of using deflated measures of revenue and inputs. The coefficient on labour becomes uniformly higher as one would expect. Constant Returns to Scale are still a valid description of the data.

The null hypothesis that contractors use the same technology as non-contractors can no longer be rejected, as evidenced by column 1. This is certainly at least in part due to the fact that fewer observations are available; running the corresponding regressions using non-deflated measures of

[64] Results ommitted to save space.

Table 10: Production Functions - Using Deflated Revenues and Inputs

	(1)	(2)	(3)	(4)	(5)
Workers(log)	.516***	.539**	.431**	.573**	.661
	(.153)	(.250)	(.207)	(.291)	(.523)
Capital(log)	.096	.127*	.067	.137	.649
	(.062)	(.077)	(.085)	(.088)	(.460)
Deflated-Inputs(log)	.456***	.344***	.599***	.336***	.009
	(.064)	(.094)	(.081)	(.103)	(.492)
Contractor	-1.691				
	(1.402)				
Contractor-Workers	-.199				
	(.262)				
Contractor-Capital	.232				
	(.148)				
Program-dummie		-2.353*			1.700
		(1.326)			(27.681)
Program-Workers		-.113			-.199
		(.311)			(1.297)
Program-Capital		-.024			.016
		(.092)			(2.532)
Program-Deflated Inputs		.248**			-.130
		(.120)			(4.411)
Constant	4.517***	5.395***	3.363***	5.320***	4.075
	(.727)	(.982)	(1.046)	(1.075)	(2.805)
N	108	94	56	38	14
R2	.715	.623	.652	.496	.875
Adjusted R2	.698	.592	.632	.452	.729

output on the corresponding sample (see column 2), the null hypotheses of similar technology can not be rejected either, so the absence of a significant difference in technology may not be due to using a different revenue measure. Furthermore, the null hypothesis that contractors in- and outside of the program use the same technology is accepted (see column 3). At the risk of belabouring the point, attaching much value to any of the results for contractors would be unwise, given the small sample size.

Moving on to column 4, which presents results for the subsample of non-contractors, a striking contrast with the non-deflated specification is immediately apparent; The null hypothesis that program firms use the same technology as non-program firms is strongly rejected since the interaction term between inputs and being in the program is positive and significant,

Table 11: CRS Production Functions - Using Deflated Revenues and Inputs

	All	All	Non-contractors	Non-contractors
	(1)	(2)	(3)	(4)
Capitalperworker(log)	.127**	.084	.084	.128
	(.059)	(.060)	(.059)	(.082)
Deflated-Inputsperworker(log)	.496***	.486***	.486***	.363***
	(.062)	(.064)	(.063)	(.094)
Contractor		1.099		
		(1.728)		
Contractor-Capitalperworker		.698***		
		(.248)		
Contractor-realinputsperworker		-.674**		
		(.298)		
Program-dummie				-1.803
				(1.361)
Program-Capitalperworker				-.062
				(.119)
Program-Inputsperwoker				.211*
				(.119)
Constant	4.110***	4.495***	4.495***	5.650***
	(.579)	(.625)	(.618)	(.965)
N	108	108	94	94
R2	.484	.535	.456	.475
Adjusted R2	.474	.512	.444	.445

suggesting the share of inputs in value added in program firms is almost twice as large than in non-program firms. In addition, the dummy on program participation is negative and significant at the 10% level, suggesting that program firms are less efficient. In terms of productivity, these effects almost offset each other at mean capital intensity (8.95) the difference in labour productivity between contractors and non-contractors would be very small ceteris paribus.

CRS is comfortably accepted for both program and non-program firms. This restriction reduced the number of parameters to be estimated and thus alleviates the identification requirements, a benefit which will be exploited when using IV-methods. Table 11 presents the results of the deflated revenue production funcion imposing CRS. The data reject the hypotheses that contractors and non-contractors use the same technology, while the difference in technology between program non-contractors and non-program non-contractors is less pronounced in this specification.

Table 12: IV Production Function Estimates

	(1)	(2)	(3)	(4)
Capitalperworker(log)	.409	.157	-.202	-.229
	(.291)	(1.359)	(.439)	(.610)
Deflated-Inputsperworker(log)	.200	-.253	.515	.019
	(.328)	(1.147)	(.438)	(.999)
Program-dummie		-6.498		-11.939
		(23.636)		(19.093)
Program-Capitalperworker		-.565		
		(1.183)		
Program-Inputsperwoker		1.222	.131*	1.285
		(2.635)	(.069)	(1.848)
Constant	4.385***	11.574	5.993**	12.536
	(1.666)	(16.631)	(2.417)	(10.988)
Instruments	Inputprice Relativepay Laborstart Startcapital firmage workers	Inputprice Relativepay Laborstart Startcapital firmage	Inputprice Relativepay Laborstart outputprice	Inputprice Relativepay Laborstart outputprice
N	89	89	89	89
Uncentered R2	.179	-1.152	-.211	-1.329
Centered R2	.16	-1.282	-.254	-1.439
Anderson-Rubin F	2.266	1.691	2.497	2.497
Anderson-Rubin F-p	.055	.146	.049	.049
Anderson Cannon. Corr.	3.825	.248	2.821	1.126
Cragg-Donald F	.729	.046	.676	.267
Sargan	1.198	.	.752	
Hausman Chi2	1.27	0.92	2.80	1.57
	0.53	0.97	0.42	0.81

Table 13:

Addressing Endogeneity Issues None of the above estimates attempt to control for potential endogeneity of output with factor choices. Such endogeneity, arising for instance because of simulateneous determination of inputs and outputs or correlation of observed factors with ommitted variables or mismeasurement, would cause the OLS estimates of the production function to be biased. Instrumenting factor choices can overcome such a problem.

Imposing CRS enables us to estimate output per worker as a function of the capital-labour ratio and inputs-intensity, thereby reducing the number of parameters to be estimated, thus facilitating identification of the production function and reducing the instrumenting requirements. While conceptually superior, input- and outputprice perform disappointingly as instruments as they turn out to be rather weakly related to factor choices. In addition to price-data, the relative wage-level, the age of the firm, the size and capital-stock of the firm at startup are used as instruments. In one regression we also use the number of workers as an instrument.

The results of our instrumenting regressions are presented in table **??**. Results of first-stage regressions are presented in the appendix. We focus on non-contractors only in order to circumvent having to instrument for differences between contractors and non-contractors. The first column uses initial size variables, the relative wage level, the inputs-price, firmage and the number of workers as instruments. To faciliate identification it assumes that all firms use the same technology, an assumption which is clearly questionable. The resulting parameter estimates are reaonably behaved but imprecise. Moreover, the instruments are weak and at best weakly identify factor choices.

In the second column, differences in technology between contractors and non-contractors are allowed for and we drop the number of workers as an instrument, since Sargan tests of overidentifying restrictions rejected its orthogonality. This instrumenting regression is disappointing; the estimates are all very poorly behaved and the instruments are still very weak.

Allowing only for differences in input-intensity (column 3) yields more promising results; the hypothesis that program and non-program contractors use the same technology is now rejected, as was the case in the conventional OLS specification. The capital stock and startup and the age of the firm were dropped as instruments since they turned out to be weak. The outputprice was used instead.[65] However, when differences in both input-intensity and technology are allowed for (column 4), the null hypothesis that program firms and non-program firms use identical technologies cannot be rejected. Again, the parameter estimates are ill-behaved and the instruments are weak.

Overall, the results of our attempts to identify the production function by means of instrumental variable estimation are disappointing[66]; The main problem seems to be that our instruments are too weak. The regressions consequently yield implausible point estimates, sometimes even negative. Nev-

[65] Output price was not used in other regressions since its exogeneity could not be established by Sargan tests of overidentifying restrictions.

[66] NB: Assuming the capital stock is exogenous does not alter these results.

ertheless, for all estimated equations Hausman tests comfortably accept the null hypothesis of no exogeneity. In the following, we proceed with OLS, because it is more efficient in the absence of endogeneity.

Table 14: The Augmented Human Capital Production Function

	(1)	(2)	(3)	(4)	(5)
Workers(log)	.523**	.585**	.575**	1.238***	.557**
	(.255)	(.262)	(.248)	(.278)	(.246)
Capital(log)	.134*	.116	.100	-.005	.157**
	(.079)	(.079)	(.080)	(.079)	(.077)
Real-Inputs(log)	.344***	.350***	.260***	.197**	.313***
	(.094)	(.095)	(.098)	(.093)	(.093)
Program-Dummie	-2.250*	-2.290*	-3.181**	-1.820	-2.390*
	(1.359)	(1.335)	(1.388)	(1.269)	(1.302)
Program-Labour	-.106	-.141	-.180	-.589*	-.061
	(.313)	(.316)	(.319)	(.320)	(.306)
Program-Inputs	.248**	.239**	.318**	.181*	.273**
	(.120)	(.121)	(.124)	(.110)	(.118)
Program-Capital	-.031	-.016	-.014	.091	-.047
	(.094)	(.093)	(.093)	(.096)	(.091)
Educationworkforce	-.013				
	(.033)				
Ageworkforce		.010			
		(.017)			
RelativePay			.327		
			(.301)		
AvgPay				.468***	
				(.098)	
Proportionsupervisorystaff					1.313**
					(.641)
Constant	5.462***	5.054***	6.359***	3.311***	5.068***
	(1.002)	(1.135)	(1.087)	(.957)	(.977)
N	94	94	89	83	94
R2	.624	.625	.61	.74	.641
Adjusted R2	.588	.589	.572	.712	.607

The Augmented Human Capital Production Function Table 14 presents the results of augmented human capital production functions, estimated over the entire sample of non-contractors, using deflated revenues and deflated prices. The coefficient on the average educational attainment of the workforce is extremely close to zero, suggesting that conditional on capital and inputs, the education of the workforce does not matter. The average age of the workforce is also found to be insignificant (see column 2).[67] This implies that firms which hire workers with more human capital are not necessarily more productive. Of course, the measures used here are very crude and it is not unlikely that using better measures of human capital a significant impact of human capital on productivity could be established.

Column 3 reveals that higher wages for workers are not associated with significantly higher output, where the relative wage variable is defined as the weighted average of firm-specific deviation from the median wages for different occupations, with weights proportional to the proportion of the firm's labour force in different occupations. This implies that firms that pay more are not more productive once the occupational structure of the workforce is accounted for. Yet, the average wage of workers in the firm, presented in column 4, has a strongly significant impact on output. Clearly, the causality underlying this correlation can be bidirectional. Combined with column 3, it suggests that the occupational structure of the workforce matters a great deal. This is confirmed by column 5, which reveals that the proportion of supervisors, where a supervisor is defined as a manager, engineer or foremen, in the workforce is positively related to output at the 10% level.

Program Support Support dummies are added to the production function. to estimate the impact of support. Receiving support has a positive effect on output, as is evident from column 1 of table 15, though inclusion of a dummy for support received does depress the program participation dummie. Looking at the different types of support separately, it emerges that credit seems to be the most effective type of support, since firms which receive credit produce almost 50% more output then program firms which do not. Similarly, conditional on receiving support, program firms which receive a building from the program office produce significantly more output. The impact of training, machinery, land and other types of support is minimal. Self-reported difficulties with hiring skilled workers, obtaining inputs, credit and complaints about the location of the firm have no significant impact

[67] These results are robust to estimating this specification on subsamples of contractors and non-contractors, using a measure of value-added as dependent variable and deflating both input- and output prices.

Table 15: Program Support

	(1)	(2)	(3)	(4)
Workers(log)	.498**	.452*	.427*	.485**
	(.241)	(.236)	(.229)	(.236)
Capital(log)	.116	.103	.107	.089
	(.074)	(.072)	(.070)	(.072)
Real-Inputs(log)	.354***	.364***	.377***	.424***
	(.090)	(.088)	(.086)	(.095)
Program-Dummie	-2.983**	-3.541***	-3.440***	-2.928**
	(1.293)	(1.308)	(1.271)	(1.352)
Program-Labour	-.161	-.282	-.288	-.371
	(.299)	(.299)	(.291)	(.301)
Program-Inputs	.221*	.256**	.258**	.202*
	(.115)	(.114)	(.111)	(.121)
Program-Capital	-.008	.037	.035	.061
	(.088)	(.089)	(.087)	(.090)
Program-Support	1.032***	1.035**	1.122**	1.095**
	(.361)	(.457)	(.445)	(.460)
Program-Land		-.254	-.299	-.223
		(.318)	(.310)	(.323)
Program-Buildings		.547**	.572**	.579**
		(.274)	(.267)	(.272)
Program-Machinery		-.392	-.360	-.318
		(.253)	(.247)	(.252)
Program-Training		-.260	-.260	-.210
		(.287)	(.279)	(.288)
Program-Credit		.521**	.618**	.522*
		(.258)	(.254)	(.268)
Program-Other		.171	-.224	-.295
		(.878)	(.869)	(.881)
Borrower			-.495**	-.498**
			(.208)	(.218)
Inputconstrained				-.006
				(.290)
CreditConstrained				.091
				(.178)
Location-Problem				-.042
				(.176)
Skills-Problem				.336
				(.224)
Constant	5.486***	5.586***	5.696***	5.167***
	(.944)	(.920)	(.896)	(1.016)
N	94	94	94	94
R2	.656	.697	.717	.727
Adjusted R2	.624	.643	.663	.657

on production (see column 4). An alternative explanation, which cannot be ruled out altogether, would be that firms which are more productive are more likely to secure access to credit and land. That is, the association between different types of support and productivity need not be causal.

Taking Stock The results in this section have vindicated the emphasis on collecting price-data in the research design, yet, unfortunately, the price-data were not of sufficient quality to serve as fully satisfactory instruments. The hypothesis that OLS estimates of the production function do not suffer from endogeneity was not rejected, permitting us to proceed with OLS based production functions.

Contractors and non-contractors seem to be using a different technology, with contractors being more capital-intensive. Yet, no conclusive evidence of differences in technology choice between program contractors and non-program contractors was found. In contrast, the difference in technology choice between non-contractors in and outside the program are marked, though they remain obscured when using a revenue-based measure of output. Using deflated inputs and outputs, it can be shown that program firms have a higher inputs-share, though a lower efficiency parameter. At mean input intensity, resulting differences in output would approximately offset each other.

The human capital characteristics of the workforce matter very little for output: the higher human capital stock in program firms does not immediately translated in higher output. Instead, it is the occupational structure of the workforce which seems to matter as firms with a higer proportion of supervisory personnel produce more output. Clearly, supervisory personnel are typically better educated, though it may also be the case that supervision increases effort.

The impact of program support is positive, though not all types of support have been equally effective. Receiving credit and a place to work are the types of support associated with the highest increases in output.

7.3 Selection into the program

This section investigates what drives selection into the program by estimating simple probit selection models of passing the AAIHDP test and being hired by a program firm, investigating the role of human capital and employment history.

Table 16: Passing the Test (Probit)

Primary	-.396
	(.370)
Grade10	.645*
	(.379)
Grade12	.407
	(.358)
TVET	.694**
	(.279)
Sex	.004
	(.271)
Age	.023
	(.015)
Apprentice-Past	.177
	(.218)
Constant	-.586
	(.628)
N	189
Pseudo R2	.165
Chi2	39.51(7)
Log-Likelihood	-99.841

Passing the AAIHDP test. The question as to whether workers took the test or not was only administered to permanent workers. Approximately 1/3 of permanent workers took the test. The probability of taking the test in itself is mainly related to in which subcity one is located and to employment history; those who have experienced unemployment spells in the past are more likely to take the test, while those who have experience as employees were less likely to take the test.[68] Of the workers who took the test, 65% passed. A simple probit estimation, presented in table 16, shows that educational attainment is the best predictor of passing the test. Having completed a TVET degree is a very strong predictor of passing the test, whereas having completed an apprenticeship in the past does not enhance the probability of passing the test. Having completed some secondary education enhances the probability of passing the test. Gender, age, employment history, and activities prior to the current one do not affect the probability of passing the

[68] Estimated by means of a simple probit; I have ommitted the results to conserve space, but they are available upon request.

test (results omitted to save space).[69]

Being Employed in a Program Firm[70] 202 permanent workers, and 62 casual workers indicate to be employed by a firm which is working for the program. Table 17 presents the results of probit regressions on being hired by a program firm. The first column is the baseline specification with educational attainment, human capital characteristics and subcity-dummies as explanatory variables. Column 2 adds dummies for prior activities, column 3 dummies for employment history while column 4 includes both. The first and perhaps most important observation is that more educated workers are much more likely to be employed in program firms. As to be expected, having a TVET degree is strongly positively associated with the probability of being employed in a program firm. The importance of training is further evidenced by the positive and significant coefficient on having completed an apprenticeship in the past, although this may also reflect the fact that those who do not pass the test can join the firm as apprentices. In contrast, age and gender are not correlated with the probability of being hired. There is also strong geographical variation in the probability of participating in the program, as evidenced by the significance of some of the subcity dummies, reflecting that the program has been more active in some areas then in others..

Including dummies for prior activities (see column 2) enables us to see whether program firms draw disproportionately on unemployed workers, casual workers and workers in otherwise marginal jobs. The insignificance of such dummies suggests that this is not the case; Workers in program firms are not more likely to have been unemployed, working as a casual labourer, or active in an otherwise marginal activity immediately prior to obtaining their current jobs in program firms.

Including dummies for the entire employment history of individuals instead (see column 3 and 4) shows that workers who have experienced a significant unemployment spell (e.g. longer than three months) in the past, workers who have experience working in a cooperative, as well as workers who have experience as a domestic employee are much more likely to be employed in program firms. In contrast, workers who have experience being

[69] The test therefore does not discriminate against women.

[70] The determinants for participating in the program did not differ between casual workers and permanent workers, yet that may partly be due to the fact that the determinants of program participation for casual workers were very imprecisely estimated.

Table 17: Being Hired by a Program Firm

	(1)	(2)	(3)	(4)
Primary	.468*	.534**	.438*	.482*
	(.240)	(.246)	(.246)	(.253)
Secondary	.491**	.592**	.470*	.536**
	(.241)	(.249)	(.248)	(.256)
College	.635	.679	.468	.539
	(.550)	(.568)	(.580)	(.597)
TVET	.513***	.576***	.502***	.538***
	(.138)	(.142)	(.145)	(.149)
Sex	.023	.023	.050	.043
	(.134)	(.142)	(.140)	(.146)
Apprenticepast	.404***	.352***	.377***	.352***
	(.110)	(.113)	(.115)	(.117)
Prior-Casualwork		.550*		.701**
		(.298)		(.315)
Prior-student		-.068		.163
		(.296)		(.308)
Prior-unemployed		.379		.337
		(.299)		(.306)
Prior-employeefirm		.306		.370
		(.290)		(.314)
Prior-self-employed		.167		.386
		(.357)		(.426)
Past-Unemployed			.529***	.513***
			(.114)	(.121)
Past-Governmentempl			.316*	.251
			(.162)	(.189)
Past-Domesticempl			-.027	-.137
			(.113)	(.148)
Past-Familyemp			.515***	.417**
			(.198)	(.212)
Past-Selfemployed			.259	.278
			(.321)	(.326)
Past-cooperative			-.132	-.147
			(.226)	(.303)
Past-casualworker			.851***	.824***
			(.242)	(.244)
N	661	658	661	658
Pseudo-R2	.111	.128	.167	.176
Log-Likelihood	-405.115	-395.734	-379.889	-374.116

employed as a casual worker are less likely to be employed in program firms. These effects are all significant at the 5% level.

In sum, program firms did not draw disproportionately on unemployed workers or casual labourers, though having experienced an unemployment spell at some point in the past is associated with an increased likelihood of participating in the program. Instead, program firms are more likely to employ highly educated workers, in particular those with a TVET degree. The finding that more educated workers are more likely to be working in program firms contrasts with the programme's stated ambition of generating employment opportunities for the poorest, since it is plausible that poverty and educational attainment are inversely related. Yet, this finding is probably at least in part due to the imposition of quality standards, which can be considered a very positive aspect of the IHDP.

7.4 Earnings

We now turn to the estimation of the program's impact on earnings, presenting the results of switching regressions, matching estimators, control function approaches and instrumental variable estimation.

Table 18 presents the results from OLS estimations of switching regressions on the log of monthly income, using both worker- and firm-level controls. Column 1 presents the results of regressing monthly income on the worker controls, which include days worked, a casual dummie, gender, age and its square, educational dummies to allow for non-linear returns to schooling, dummies indicating having worked as casual worker and being employed in a program firm immediately prior to their current activity. Returns to schooling are clearly convex, which is consistent with the literature. In addition, having completed an apprenticeship is associated with significantly higher earnings.[71] Also note that the returns to being a casual workers are significantly lower than the returns to being a permanent worker. Interestingly, interactions between education dummies and being a casual worker are not significant, suggesting that the returns to education are similar for both

[71] Furthermore, the returns to completing an apprenticeship are higher for those with less academic training (results not presented here to conserve space but available upon request), suggesting, in line with Kayharara and Teal (2006) that, in contrast to academic training, the return to additional vocational training is a decreasing function of academic education.

Table 18: Switching Regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dayspermonth(log)	.601*** (.085)	.578*** (.085)	.582*** (.086)	.576*** (.086)	.008 (.287)	-.070 (.280)	-.075 (.280)
Casualdummie	-.237*** (.079)	-.266*** (.079)	-.285*** (.080)	-.294*** (.080)	-.321 (.252)	-.384 (.246)	-.385 (.246)
Age	.019 (.021)	.015 (.021)	.017 (.021)	.015 (.021)	.006 (.027)	-.003 (.027)	-.006 (.027)
AgeSquared	-.011 (.029)	-.006 (.029)	-.008 (.029)	-.005 (.029)	.004 (.037)	.016 (.036)	.020 (.036)
Sex	.308*** (.080)	.312*** (.079)	.319*** (.080)	.318*** (.080)	.291*** (.110)	.332*** (.108)	.323*** (.108)
SomePrimary	.218 (.134)	.181 (.134)	.232* (.135)	.212 (.135)	.290 (.205)	.279 (.201)	.254 (.202)
PrimaryComplete	.206 (.138)	.167 (.137)	.207 (.138)	.188 (.138)	.173 (.208)	.167 (.204)	.142 (.205)
Grade10	.348** (.140)	.304** (.140)	.358** (.140)	.338** (.140)	.468** (.209)	.430** (.205)	.390* (.208)
Grade12	.417*** (.136)	.373*** (.136)	.398*** (.136)	.381*** (.137)	.482** (.200)	.440** (.196)	.419** (.197)
Grade12plus2	.809*** (.170)	.788*** (.169)	.816*** (.169)	.809*** (.169)	.823*** (.243)	.824*** (.237)	.798*** (.239)
College	1.766*** (.389)	1.711*** (.386)	1.657*** (.387)	1.651*** (.386)	1.747*** (.485)	1.433*** (.478)	1.441*** (.478)
TVETdegree	.115 (.085)	.074 (.086)	.083 (.085)	.063 (.086)	-.007 (.110)	-.010 (.109)	-.024 (.109)
Prior-Casualwork	.201** (.085)	.180** (.084)	.228*** (.085)	.214** (.085)	.282** (.115)	.294*** (.113)	.286** (.113)
Prior-employeefirm	.280*** (.071)	.267*** (.071)	.273*** (.071)	.268*** (.071)	.186* (.096)	.176* (.094)	.170* (.094)
ApprenticeshipCompleted	.135** (.065)	.113* (.065)	.120* (.065)	.111* (.065)	.148* (.089)	.157* (.088)	.152* (.088)
Program-Dummie		.194*** (.064)		.110 (.070)			.102 (.096)
Workers			.100*** (.028)	.081*** (.030)		.170*** (.047)	.152*** (.050)
Capital-Intensity						.010 (.021)	.010 (.021)
Input-Intensity						.070*** (.026)	.071*** (.026)
Constant	2.573*** (.468)	2.650*** (.465)	2.417*** (.468)	2.476*** (.469)	4.720*** (1.051)	3.974*** (1.042)	4.055*** (1.045)
N	619	619	605	605	357	357	357
R2	.338	.348	.36	.363	.193	.241	.243
Adjusted R2	.31	.32	.331	.333	.132	.176	.176

casual and permanent workers.[72] The age-earnings profile is concave, since the coefficient on age is positive and the coefficient on the square of age is negative, yet both age variables are not significant.

The high coefficient on sex suggest that women earn some 35% less than men, even after controlling for days worked and education. This effect attests to the existence of widespread gender discrimination in Ethiopia. The result is robust to inclusion of program participation dummies and firm-level characteristics. Moreover, it is robust to the inclusion of occupation dummies and interactions of such dummies with gender, which suggests that discrimination takes places within occupations[73]. In addition, women have far less chance of completing schooling. Controlling for educational achievement therefore masks the prevalence of gender discrimination in the labour market.

Column 2 present the results of the same regression, using a dummy for program participation as an additional regressor, thus effectively estimating a switching regression. The impact of program participation on earnings is positive and strongly significant. Not controlling for firm characteristics, workers earn almost 20% more than workers in non-program firms with comparable observable characteristics. This result is robust to augmenting the specification with interactions between program participation and educational attainment, being a casual worker, days worked, having completed vocational training and the duration of the last unemployment spell and gender; none of these interaction terms is significant.[74]

Column 3 drops the participation dummy momentarily and adds a control for firm-size, which turns out to be strongly positively related to earnings. The finding that large firms pay more is important because it suggests that, ceteris paribus, it would be preferable to have a larger share of the labour force being employed in large, rather than small firms. Note that the finding that large firms pay more is consistent with a wealth of evidence on manufacturing firms. Interestingly, the program participation dummie, re-included in column 4, is no longer significant once firm-size is controlled for, suggesting that workers in program firms earn more because program firms tend to be larger than non-program firms. Yet, it will be shown shortly that it cannot be ruled out that the program premium is due to selection on unobservables.

[72] Result not presented here to conserve space, but are available upon request.
[73] Results not presented to conserve space, but available upon request.
[74] Results are not reported here to conserve space, but are available upon request.

In column 6 and 7, further firm-level controls, notably input- and capital-intensity are included. Unfortunately, such firm-level characteristics were available only for 357 individuals forcing us to drop about 40% of our sample. Clearly, this raises some concerns about comparability with the other regressions, which is why column 5 presents the same regression as column 1, but now only using the subsample for whom capital- and input- data are available. Surprisingly, the R2 drops substantially, from .36 to .19. In addition, the coefficient on days worked drops while the dummy for being a casual workers loses significance. The reason is that there are only 11 casual workers left in our sample and that there is relatively little variation in the number of days worked by permanent workers, so that the variation on which the identification of the days worked variable rests is now very small. Fortunately, the other parameter estimates turn out to be relatively stable, though the implied age-earnings profile is somewhat anomalous (even though it is insignificant).

Column 6 and 7 show that workers in firms which use more inputs per worker earn more. Somewhat surprisingly, the amount of capital per worker has no significant impact on earnings after controlling for firm-size and inputs. The correlation between firm-size and wages is certainly not diminished by inclusion of these firm-controls. The program dummie loses significance, though it remains positive, suggesting that the firm-size is the channel by which earnings of program and non-program participants with similar observable characteristics differ.

Quantile Regressions

Quantile regressions enable us to judge the effect of the program on the poorest workers. Columns 1-4 of table 19 present the results of a quantile regressions at the first decile and the three quartiles using worker-level controls only, while columns 4-8 present the same picture, now controlling for firm-size. The IHDP definitely seems to benefit those at the bottom of the earnings distribution most. At the the lowest decile of the earnings distribution, program-participation is associated with a 30% increase in monthly income (column 1). At the first quartile, the program premium has halved to about 16% (column 2), while program participation has no impact for those in higher quantiles of the earnings distribution. Surprisingly, program participation is associated with a 17% increase in earnings for the poorest decile even after controlling for firm-level characteristics (column 5), though pro-

Table 19: Quantile Regression

	10%	25%	50%	75%	10%	25%	50%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dayspermonth(log)	.717***	.873***	.653***	.415***	.283	-.245*	-.065
	(.099)	(.080)	(.082)	(.072)	(.210)	(.145)	(.176)
CasualDummie	-.193**	-.231***	-.219***	-.202***	-.254	-.184	-.254
	(.090)	(.073)	(.077)	(.068)	(.283)	(.175)	(.183)
Age	-.003	.007	.009	.028*	.001	-.0007	-.002
	(.020)	(.017)	(.019)	(.017)	(.021)	(.022)	(.020)
AgeSquared	.015	.0004	-.007	-.028	.013	.001	.009
	(.025)	(.023)	(.026)	(.024)	(.026)	(.030)	(.026)
Sex	.165*	.267***	.269***	.304***	.337***	.293***	.318***
	(.095)	(.073)	(.077)	(.065)	(.092)	(.082)	(.085)
SomePrimary	.370***	.027	.104	.131	.266	.069	.259*
	(.135)	(.122)	(.128)	(.109)	(.184)	(.147)	(.157)
PrimaryComplete	.490***	.109	.122	.134	.545***	-.045	.087
	(.139)	(.122)	(.131)	(.113)	(.174)	(.148)	(.160)
Grade10	.504***	-.004	.093	.242**	.568***	-.041	.141
	(.139)	(.123)	(.133)	(.117)	(.177)	(.151)	(.163)
Grade12	.583***	.130	.272**	.291***	.759***	.124	.270*
	(.131)	(.122)	(.130)	(.112)	(.168)	(.142)	(.154)
Grade12plus2	.772***	.320**	.583***	.683***	.944***	.358**	.394**
	(.181)	(.149)	(.163)	(.149)	(.220)	(.170)	(.186)
College	2.015***	1.749***	1.808***	1.602***	1.958***	1.088***	1.466***
	(.200)	(.348)	(.337)	(.328)	(.240)	(.217)	(.331)
TVETdegree	.085	.110	.170**	.119	-.013	.019	.227***
	(.093)	(.074)	(.084)	(.078)	(.106)	(.076)	(.088)
Prior-Casualwork	.146	-.014	.025	.191***	.123	.013	.159*
	(.090)	(.074)	(.082)	(.071)	(.106)	(.086)	(.090)
Prior-employeefirm	.171**	.120*	.143**	.339***	.056	.093	.064
	(.076)	(.064)	(.069)	(.061)	(.088)	(.067)	(.075)
ApprenticeshipCompleted	.047	.047	.098	.104*	-.032	.071	.102
	(.075)	(.058)	(.063)	(.056)	(.091)	(.067)	(.070)
Program-Dummie	.303***	.160***	.087	.069	.167*	.034	-.101
	(.073)	(.057)	(.062)	(.054)	(.088)	(.066)	(.077)
Workers					.115***	.141***	.182***
					(.043)	(.034)	(.040)
Capital-Intensity					.048***	.028**	.028*
					(.017)	(.014)	(.016)
Input-Intensity					.048**	.072***	.059***
					(.025)	(.019)	(.021)
Constant	1.492***	1.983***	3.107***	3.559***	1.622*	4.676***	4.367***
	(.549)	(.434)	(.438)	(.385)	(.837)	(.630)	(.701)
N	619	619	619	619	357	357	357

gram participation has no significant effect at higher quantiles of the earnings distribution. The table also suggest that the association between earnings and schooling is strongest for the poorest (see column 1), while columns 5-8 suggest that the relation between firm-size and earnings is stronger in the top-half of the earnings distribution than in the bottom half.

Table 20: Matching Estimates

Matching Algorithm	Bandwidth/Caliper	ATT	Standard Error	p	Absolute Bias Reduction
Radius	0.03	0.1266	0.0847	0.135	6.52
Radius	0.05	0.1377	0.0771	0.074	6.98
Radius	0.10	0.1654	0.0740	0.026	7.77
Kernel	0.03	0.1303	0.0786	0.097	7.09
Kernel	0.05	0.1337	0.0805	0.097	6.90
Kernel	0.10	0.1569	0.0898	0.080	7.39

Matching Matching estimators make less stringent assumptions on functional form by allowing for non-parametric estimation of the treatment effect. The propensity score used to match workers was estimated using dummies for educational achievement, a dummy for having completed an apprenticeship, age, sex, employment history dummies and subcity dummies as explanatory variables. Firm-level characteristics and job-characteristics, such as days worked and the dummy for being a casual worker, were omitted from our specification, since these are not characteristics of individuals. A common support restriction was imposed, forcing us to drop 1 untreated and 19 treated individuals, leaving us with 329 untreated and 271 treated individuals.[75]

Density plots of the propensity scores for treated and untreated individuals before and after matching reveal that matching on the propensity score has been successful. Yet, while the absolute bias has been reduced very substantially,[76] perfect covariate balance has certainly not been achieved. T-tests for equality of means reject that treated and untreated individuals in

[75] Consequently, the estimated ATT should be interpreted as the ATT for those individuals for whom a comparison can be drawn. However, since the region of common support encompasses almost the entire sample, the divergence between the estimated ATT for individuals for whom a comparison can be made and the ATT for all treated individulas is likely to be small.

[76] The standardised bias is the difference of the sample means in the treated and non-treated sub-samples as a percentage of the square root of the average of the sample variances:

the matched sample are similar in terms of having completed TVET degrees, being casual workers, having completed an apprenticeship in the past and living in the subcities Kolfe-Keranyo, Yeka and Arada, though workers are similar in other observable characteristics.[77] Moreover, Pseudo-R2s of the probit estimation of the probability of receiving treatment on the unmatched and matched samples are typically very similar, which indicates covariate balance has not improved too much.[78] The differences between the control and the treatment group have been reduced, but have not been eliminated.

Overall, the average treatment effect on the treated is lower than the estimate using OLS. In addition, the estimate of the treatment effect is monotonically increasing in the bandwidth/caliper used, being about 14% at a bandwidth/caliper of 0.05 and around 16% at a bandwidth/caliper of 0.10. The effects are significant at the 10% level, and even at the 5% level for Caliper matching using a bandwidth of .1. At very small bandwidths/callipers the average treatment effect on the treated does not vanish. The matching estimators thus provide mild evidence of positive treatment effects, but also suggest that the switching regression estimate of the average treatment effect on the treated may be too high.

Control Function Approaches Turning to the control function approach (see table 21), the preferred selection model described in the previous section, i.e. using the full set of employment history variables including is used to model selection. To predict earnings, we use the preferred earnings equations, with and without firm-level controls (see columns 2 and 7 of Table 18). Using controls for individual characteristics only, the two-step estimator[79] does not demonstrate that selection is an issue, since Heckman's lambda is insignificant even at the 10% level. The positive sign on the selection term furthermore suggests that bias due to unobservables would inflate, rather than deflate, the estimated treatment effect, since the estimated effect of program participation is now negative and insignificant. Once firm-level

$\frac{\frac{1}{Nt}\sum X_T - \frac{1}{Nt}\sum X_C}{\sqrt{V(Xt)+V(Xc)}}$

(due to Rosenbaum & Rubin, (1985))

[77] Results ommitted here to conserve space.

[78] The better the covariate balance, the smaller the difference between treated and untreated individuals in the matched sample. Consequently, in a perfectly balanced matched sample, the pseudo-R2 should be very close to zero.

[79] Estimation using the MLE estimator yielded unreliable results since the likelihood function was not concave.

Table 21: Control Function Approach (Heckman)

	(1)	(2)
Dayspermonths(log)	.574***	-.073
	(.083)	(.269)
CasualDummie	-.250***	-.377
	(.078)	(.236)
Age	.017	-.005
	(.020)	(.025)
AgeSquared	-.009	.018
	(.028)	(.035)
Sex	.304***	.344***
	(.080)	(.107)
SomePrimary	.241*	.345*
	(.139)	(.206)
PrimaryComplete	.227	.228
	(.142)	(.208)
Grade10	.368**	.502**
	(.145)	(.216)
Grade12	.444***	.537***
	(.143)	(.208)
Grade12plus2	.858***	.927***
	(.173)	(.247)
College	1.802***	1.518***
	(.392)	(.472)
TVETdegree	.134	.031
	(.094)	(.113)
Prior-Casualwork	.180**	.292***
	(.083)	(.108)
Prior-employeefirm	.285***	.197**
	(.071)	(.093)
ApprenticeshipCompleted	.164**	.182**
	(.073)	(.088)
Program-Dummie	-.161	-.264
	(.228)	(.257)
Workers		.152***
		(.048)
Capital-Intensity		.008
		(.020)
Input-Intensity		.069***
		(.025)
Constant	2.756***	4.161***
	(.462)	(1.005)
Heckman's lambda	.231	.246
	(.142)	(.16)
N	619	357

controls are included, the Heckman selection term is even less significant while the impact of the dummy also remains insignificant and negative. In short, the null hypothesis that unobserved heterogeneity does not impact on earnings and program participation simultaneously cannot be rejected, yet the impact of program participation vanishes once selection is controlled for. Consequently, it cannot be ruled out that the program premium is due to selection on unobservables.

Instrumental Variable Estimation The results from our instrumental variable estimation, presented in Table 22 are similar to those obtained by the control function approaches in failing to establish a significant effect of program participation on earnings. Again, the estimated coefficient on program participation is insignificant, suggesting upward, rather than downward selection bias, while the overall pattern of results has not been affected.

Column 1 uses having experienced an unemployment spell in the past as an instrument for program participation. This instrument is certainly relevant, as is evidenced by an Anderson canonical correlation statistic of 23.04 and a Cragg-Donald F-statistic of 22.49.

When program participation is instrumented by having experience working in a cooperative (see column 2), program participation does not have a significant on earnings. Having experience working in a cooperative is not as strong an instrument as having experienced an unemployment spell in the past, yet its performance is still very satisfactory from a statistical point of view. The instrument is certainly relevant, judged by the Cragg-Donald F-Statistic of 11.30 and an Anderson Canonical Correlation of 11.68.

Combining these instruments yields the same result. Moreover, Sargan tests of the validity of overidentifying restrictions cannot reject the validity of either instrument when we test them in turn. Of course, the Sargan test requires at least one of the instruments to be valid, which is difficult to prove. Note that these results are robust to inclusion of firm-level controls (see columns 4-6).

Thes IV estimates of program participation are probably best interpreted as Local Average Treatment Effects; the estimates represent the impact of program participation for those individuals whose program participation was in fact influenced by their employment history, but not the impact on participants in general, unless the gains from participation are homogeneous across the population.

Table 22: Instrumental Variable Estimation

	unemp	coop	unempandcoop	unemp	coop	unempand
	(1)	(2)	(3)	(4)	(5)	(6)
Program-Dummie	-.135	.050	-.066	-.088	.092	-.001
	(.361)	(.453)	(.294)	(.443)	(.455)	(.333)
Dayspermonths(log)	.617***	.595***	.609***	-.065	-.075	-.070
	(.095)	(.099)	(.091)	(.271)	(.269)	(.269)
CasualDummie	-.218**	-.245**	-.228**	-.382	-.385	-.384
	(.095)	(.102)	(.089)	(.237)	(.236)	(.236)
Age	.021	.018	.020	-.0008	-.006	-.003
	(.022)	(.022)	(.021)	(.028)	(.028)	(.027)
AgeSquared	-.015	-.010	-.013	.012	.019	.016
	(.031)	(.031)	(.030)	(.039)	(.039)	(.037)
Sex	.305***	.309***	.306***	.339***	.324***	.332***
	(.080)	(.079)	(.079)	(.111)	(.110)	(.107)
SomePrimary	.244	.208	.231	.301	.256	.279
	(.150)	(.157)	(.144)	(.222)	(.222)	(.209)
PrimaryComplete	.232	.196	.219	.189	.144	.167
	(.154)	(.161)	(.147)	(.224)	(.225)	(.212)
Grade10	.379**	.337**	.363**	.465*	.394	.431*
	(.162)	(.171)	(.153)	(.264)	(.266)	(.237)
Grade12	.448***	.406**	.432***	.459**	.421**	.441**
	(.158)	(.168)	(.150)	(.211)	(.211)	(.201)
Grade12plus2	.825***	.804***	.817***	.846***	.800***	.824***
	(.174)	(.174)	(.171)	(.255)	(.255)	(.243)
College	1.804***	1.752***	1.785***	1.426***	1.440***	1.433**
	(.399)	(.400)	(.392)	(.462)	(.459)	(.459)
TVETdegree	.144	.105	.130	.002	-.023	-.010
	(.115)	(.128)	(.105)	(.121)	(.122)	(.114)
Prior-Casualwork	.216**	.196**	.208**	.301***	.287**	.294***
	(.093)	(.096)	(.089)	(.114)	(.114)	(.111)
Prior-employeefirm	.289***	.277***	.284***	.182*	.171*	.176*
	(.075)	(.076)	(.073)	(.094)	(.094)	(.092)
ApprenticeshipCompleted	.150*	.129	.143**	.161*	.152*	.157*
	(.077)	(.082)	(.072)	(.087)	(.087)	(.086)
Workers				.185**	.153*	.170**
				(.091)	(.093)	(.075)
Capital-Intensity				.011	.010	.010
				(.020)	(.020)	(.020)
Input-Intensity				.070***	.071***	.070***
				(.025)	(.025)	(.025)
Constant	2.520***	2.593***	2.547***	3.905***	4.047***	3.973**
	(.486)	(.490)	(.475)	(1.063)	(1.060)	(1.033)
N	619	619	619	357	357	357
R2	.319	.343	.33	.234	.243	.241
Adjusted R2	.289	.314	.301	.166	.176	.173

Taking Stock The overall conclusions emerging from these regressions are that workers with similar observable characteristics earn more in program firms than in non-program firms and that this effect may at least in part be explained by the fact that program firms are larger than non-program firms, though selection on unobservables cannot be ruled out as an explanation for the program premium, as evidenced by IV and selectivity-corrected estimates. In addition, these methods suggest that bias due to unobservables will lead to inflated, rather than deflated, estimates of program participation.Quantile regressions reveal that program participation particularly benefits those at the bottom of the earnings distribution, a result which is robust to including firm-level controls.

7.5 The Impact of the Program on Non-Participants

To gauge the general equilibrium effects of the program, data on entry and exit were collected. In addition, managers impressions of the impact of the program on wages, inputs and demand was sollicited. Although more objective and longitudinal data would be better suited to estimating the impact of the program on non-participants, such data are unfortunately not available. Consequently, the scope for drawing quantitative conclusions is limited.

7.5.1 Entry and Exit

In general, firm turnover is high, as reflected by the age distribution of firms; the average firm is in our sample is 3.2 years old, while the median firmage is 2 years. However, most firms that exit, do so in the first few years of operation; 44% of firms that have exited have done so after less than a year of existing, while fewer than 10% of firms that exited had been operating for more than 3 years. This is consistent with the pattern of entry and exit in other countries and sectors (see e.g. Scarpetta et al., 2004).

Using statistics from a subsample of firms from the registry, 12% of firms in the registry turned out to have exited in 2006, while 27% of firms in our sample started in 2006, suggesting that net entry rates are positive and substantial A problem with these statistics is that entry rates are more accurately documented than exit rates, since the registry of firms is cleaned periodically, so that estimates of exit rates are biased downwards. Restrospective computation of survival rates of firms in previous years, suggest these have dropped from 96% in 2003 to 86% in 2006, hinting that the program may have contributed to a higher exit rates. Again, these statistics are biased because of the periodical updating of the registry of firms by which firms that have exitted are removed. Moreover, the size of the bias tends to increase as time passes, since eventually all firms which have exitted in any given year are removed.

For program firms themselves getting work from the AAIHDP is the key determinant of survival: 95% of program firms in the sample have ever completed a program assignment, while only 13% of program firms that perished claimed they were working for the program. In total, 22% of the firms supported by the IHDP have exited since the start of the program.

The AAIHDP seems to have had a very limited impact on entry; more than three quarters of entrants into the construction sector over the past three years have not participated in the program. In addition, gross entry of non-participants in 2005 and 2006, the years for which the data are most reliable, are almost identical, despite variation in gross entry of program firms.

Given the benefits conferred by program participation, one may wonder why not more entrants have sought to take part in the program. When asked why they did not join the program, 54% of the managers claimed not to be aware of the program, 21% claimed participation would not have been beneficial, and the rest either claimed they had no opportunity to register, or did not fulfil the criteria.

In short, the program has not stifled entry and obtaining contracts from the AAIHDP is cricital for survival for program firms.

7.5.2 Opinions of firms in the construction sector

To gauge the general equilibrium impacts of the program, firms were asked a rim of question on the impact of the AAIHDP on competition, the labour market, input prices and availability and other effects. While the answers to such questions are inevitably subjective in nature and consequently not perfectly reliable, they are also likely to be informative about the impact of the program.

Competition & Competitiveness Starting with the impact of the AAIHDP on competition, 77% of firms believe competition has increased over the past 3 years, while 11% believe it has decreased. Of those that believe competition increased, 35% (or 27% of all firms) believe it was because of the

AAIHDP. Firms working for the AAIHDP are much more likely to indicate that competition increased because of the AAIHDP than non-participants (in total 15% vs 38%).

Consistent with this finding, only 12% of non-participating firms believe termination of the AAIHDP would improve their business, while 16% of AAIHDP participants indicate that termination of the IHDP would cause their company to go bankrupt, and another 30% fear their business would slow down should the program be terminated. The AAIHDP on average purchases 80% of the output produced by firms parttaking in the program, again pointing towards the importance of obtaining program contracts.

This claim is further evidenced by the finding that only 56% of AAIHDP firms have completed contracts for clients other than the AAIHDP, raising concerns about the competitiveness of the 44% of AAIHDP participants that did not work for other clients. Indeed, firms are on average more concerned about competition by non-program firms than about competition from AAIHDP firms. For example, 36% of all firms indicate to experience competition from AAIHDP SMEs, while 51% of all firms indicate to experience competition from non-program SMEs.

Labour Market Impact of the AAIHDP When asked about the impact of the AAIHDP on the labour market, 54% of firm managers replied that the program had not caused any change or did not know about the program; 12% claimed the program had led to an increase in the shortage of skilled workers; while 9% contended that the program had sparked a general wage rise; 8% of employers took a more positive view and replied that the program had created employment and income. The remaining firm managers provided answers not particularly illuminating about the impact of the program. Not a single manager mentioned or hinted that the program had contributed to a greater pool of skilled labour.

The impact of the AAIHDP on input markets Asked about the impact of the IHDP on input markets, only 18% of managers claimed the program had not made an impact or did not know of the program; 49% claimed that the program had contributed to increasing shortages of raw materials; 23% argued along similar lines that the program had led to rising input prices.

The impact of the AAIHDP on output When asked about the impact of the AAIHDP on output, 54% of firms managers responded the AAIHDP had not affected their output, while 12% claimed that the shortage of inputs resulting from the AAIHDP had forced them to delay certain projects.

Taking Stock To the extent that firm managers' subjective opinions are informative about the objective reality in the construction sector, they suggest that the program may have increased labour demand slightly, particularly for skilled workers, thus leading to an increase in the price of labour. It is not very likely that the program has contributed to a surge in the price of unskilled labour, because program firms are more likely to employ skilled workers. Moreover, unskilled labour is abundant in Addis and the demand for unskilled labour from construction firms only accounts for a tiny fraction of the total demand for unskilled labour. The impact of the IHDP on input markets has been marked; the zero-sum nature of such markets means that the introduction of the IHDP must have crowded out other projects. Through these channels, the IHDP has certainly affected the output of other firms, though perhaps to a limited extent, since the majority of managers claim not to have been affected by the introduction of the IHDP. Unfortunately, the data do not permit quantification of the impact of the IHDP.

8 Conclusion

The results strongly suggest that the IHDP has not had the job creation impact it was designed to have. Program firms are not more labour-intensive than non-program firms and in fact hire more high-skilled workers than non-program firms. In addition, program firms do not draw disproportionately on the low-skilled, the unemployed, youth or women. On the other hand, program participants do have lower predicted wellfare and earn more than non-program participants. In addition, the program premium is strongest for those at the bottom of the earnings distribution, amounting to 30% for the poorest decile of workers. Paradoxically, the program premium is most probably due to a correlation between firm-size and wages; once firm-size is controlled for the program premium disappears, although the possibility that the program premium is driven by differences in unobservable characteristics between program and non-program participants cannot be ruled out entirely.

It has also been shown that program firms use a different technology than non-program firms and that contractors employ technologies that differ from those used by non-contractors. While it is true that the output of IHPD firms is more responsive to increases in inputs, they also tend to be less efficient, so that the average productivity of program firms and non-program firms is very similar. If these patterns can be extrapolated, then the low-cost technology introduced by the program would lead to higher productivity should it be employed in larger firms. In contrast to studies of manufacturing firms across Africa, this study does not find that capital intensity and labour productivity increase with firm-size. We do find that large firms pay more than small firms, although it is poorly understood why this should be the case.

The zero-sum nature of input availability implies that the program undoubtedly has led to some crowding out. Nevertheless, the effect of the program on entry and exit rates of non-program firms seems to have been minimal, a finding which is rather suprising as one would expect rent-seeking entrants to attempt to benefit from program support, unless the obligations that come with program participation offset such gains. The IHDP may also have led to upward pressure on wages for skilled workers. The overall impact on the labour market is likely to be limited because program firms have not generated more jobs and because demand for skilled labour comes from many sources other than the construction sector. The IHDP has contributed to competition for inputs, but its impact on net demand has been limited given the existence of enormous excess demand.

Existing construction capacity is severely underutilized. Such underutilization, in combination with our finding that 1 in 5 IHDP firms have perished already, cast doubts on the desirability of promoting entry and providing new firms with support. Better targetting of support might improve its effectiviness. Moreover, the finding that IHDP firms are highly dependent on the IHDP for work may leads to concerns about their sustainability.

As an active labour market program, the IHDP has not been an unequivocal success. It has not created more jobs, but beneficiaries of IHDP jobs earn more than they would in other firms. In addition, the finding that workers in IHDP firms earn more because such firms are larger than non-program firms cast doubts on the desirability of SME support in general.

Alleviaton of the supply constraints that have caused the housing shortage will lead to job creation and should be a policy priority. The overall business climate faced by construction firms is hostile and should be ameliorated. By increasing the availability of inputs, more buildings can be constructed and more workers will be hired. In fact, increasing the supply of cement is probably a very powerful vehicle for employment creation, given the magnitude of the housing shrotage. Unfortunately, political economy considerations may preclude succesful alleviation of these constraints. In addition, liberalising land markets will stimulate housing demand and entry and could also facilitate access to credit by enabling firms to use land as collateral. Making sure that firms have good access to credit is also of eminent importance. The fact that access to credit, land and inputs are the most effective forms of IHDP corroborates our argument that rectifying these market failures should be the policy priority.

The paper also provides directions for future research. Focussing on the program for the moment, future work documenting the dynamics of entry, exit, and productivity and employment growth would enable more reliable assessment of the impact of the program. In addition, uncovering why large firms pay more than small firms is an important challenge.

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10 Appendix

Table 23: Determinants of Output Prices

Program-dummie	-.137**	-.144**	-.124	-.161**	-.160**
	(.069)	(.070)	(.083)	(.079)	(.079)
Contractor		-.073	-.048	-.141	-.174
		(.113)	(.127)	(.121)	(.132)
Workers(log)			-.028		
			(.050)		
Capital-Intensity				.034	.032
				(.026)	(.027)
Input-Intensity				.017	.016
				(.027)	(.027)
Firmage					.010
					(.016)
Constant	1.101***	1.113***	1.170***	.723***	.729***
N	149	149	147	144	143
R2	.003	.023	.031	.032	.031
Adjusted R2	-.004	.009	.01	.004	-.004

Table 24: Determinants of Input Prices

Program-dummie	.047	.043	.012	.007	.006
	(.068)	(.067)	(.075)	(.077)	(.078)
Contractor		-.151*	-.196**	-.180*	-.173*
		(.089)	(.098)	(.102)	(.105)
Workers(log)			.046	.045	.040
			(.044)	(.045)	(.046)
Capital-Intensity				-.013	-.014
				(.020)	(.021)
Firmage					-.001
					(.010)
Constant	.999***	1.028***	.943***	1.050***	1.074***
	(.050)	(.052)	(.097)	(.193)	(.198)
N	149	149	147	144	143
R2	.003	.023	.031	.032	.031
Adjusted R2	-.004	.009	.01	.004	-.004

Table 25: Input-Intensity

	(1)	(2)	(3)	(4)	(5)
Workers(log)	-.135	-.279*	-.197	-.227	-.113
	(.128)	(.143)	(.147)	(.147)	(.150)
Program-Non-Contractor		-.240	-.271		
		(.275)	(.275)		
Program-Contractor		1.018*	.830		
		(.575)	(.579)		
Contractor		.797*	.555	1.149***	1.374***
		(.470)	(.487)	(.348)	(.496)
Capital-Labour-Ratio			.208***	.219***	.164***
			(.063)	(.063)	(.061)
Program-dummie				-.076	.285
				(.253)	(.263)
Firmage					.007
					(.031)
Constant	9.459***	9.679***	7.808***	7.693***	7.174***
	(.318)	(.305)	(.620)	(.620)	(.647)
N	185	185	175	175	173
R2	.006	.123	.169	.154	.374
Adjusted R2	.0005	.103	.144	.134	.301

Note: Activity Dummies Surpressed in Column 5

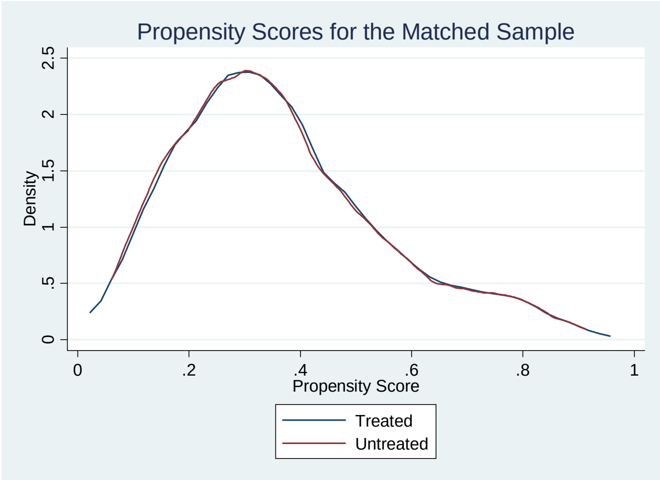
.1 Auxiliary Tables

.1.1 Propensity Score Estimation and Balancing

	Propensity Score Estimation
Primary	.438*
	(.246)
Secondary	.470*
	(.248)
College	.468
	(.580)
TVETcomplete	.502***
	(.145)
Sex	.050
	(.140)
Apprenticepast	.377***
	(.115)
Ever-Unemployed	.529***
	(.114)
Ever-govtemployee	.316*
	(.162)
Ever-PrivateSector	-.027
	(.113)
Ever-Familywork	.515***
	(.198)
Ever-Selfemployed	.259
	(.321)
Ever-Cooperative	-.132
	(.226)
Ever-Casualwork	.851***
	(.242)
N	661
LL	-379.889
Chi2	151.98
Pseudo R2	0.167

Subcity Dummies Not Reported: significant for Yeka, Kolfe-Keranyo and Arada

.1.2 First-Stage Regressions Corresponding to the Instrumental


Propensity Scores for the Matched Sample
Density
0
.5
1
1.5
2
2.5
0
.2
.4
.6
.8
1
Propensity Score
Treated
Untreated

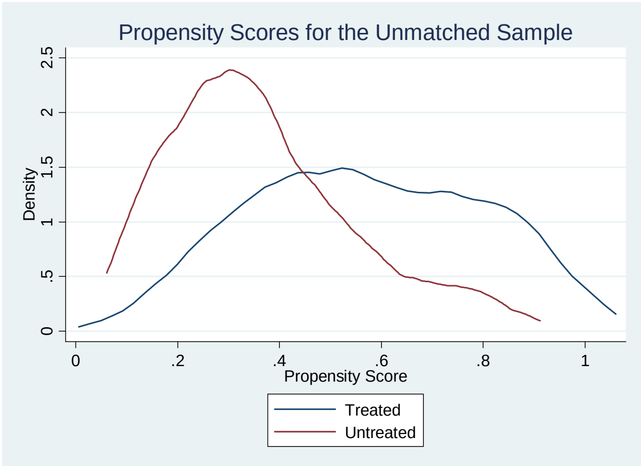

Propensity Scores for the Unmatched Sample
Density
0
.5
1
1.5
2
2.5
0
.2
.4
.6
.8
1
Propensity Score
Treated
Untreated

Table 26: First Stage-Column 1

	Capital per worker	Real-Inputsperworker
	(1)	(2)
Relative-Pay	.535***	.396**
	(.188)	(.178)
Inputsprice	-.256	-.941**
	(.401)	(.380)
Laborstart	.028*	.010
	(.017)	(.016)
Startcapital	.116*	.114*
	(.067)	(.064)
Workers(log)	-.868***	-.386
	(.278)	(.263)
Constant	9.237***	9.795***
	(.739)	(.699)
N	89	89
R2	.175	.156
Adjusted R2	.125	.105

Table 27: First Stage-Column 2

	Capitalperworker	Inputsperworker	Program-dummie	ProgramCapital	ProgramInputs
	(1)	(2)	(3)	(4)	(5)
Startcapital	.075	.099	.021	.253	.273
	(.069)	(.063)	(.022)	(.177)	(.241)
Firmage	.125	.087	.032	.343	.358
	(.091)	(.083)	(.029)	(.236)	(.321)
Laborstart	.015	.006	.015***	.135***	.156***
	(.017)	(.015)	(.005)	(.044)	(.059)
Relative-Pay	.411**	.347**	.049	.465	.723
	(.191)	(.174)	(.060)	(.492)	(.669)
Inputsprice)	-.441	-1.040***	.008	.206	-.280
	(.420)	(.383)	(.132)	(1.086)	(1.476)
Constant	7.578***	8.951***	.146	.019	-.207
	(.720)	(.656)	(.227)	(1.860)	(2.529)
N	89	89	89	89	89
R2)	.098	.146	.131	.165	.124
Adjusted R2	.044	.094	.078	.115	.072

Table 28: First Stage-Column 3

	Capitalperworker	Inputsperworker	ProgramInputs
	(1)	(2)	(3)
laborstart	.022	.010	.131**
	(.016)	(.015)	(.057)
lnrelativepay	.433**	.385**	.753
	(.184)	(.171)	(.649)
ainputspricedeflatormeda	-.307	-.890**	.200
	(.407)	(.378)	(1.433)
outputpricedeflatormeda	.670*	.198	-1.970
	(.388)	(.360)	(1.366)
cons	7.596***	9.584***	4.859**
	(.668)	(.620)	(2.352)
N	89	89	89
R2	.101	.116	.122
Adjusted R2	.058	.073	.081

Table 29: First Stage-Column 4

	Capitalperworker	Inputsperworker	ProgramInputs	Program-dummie
	(1)	(2)	(3)	(4)
laborstart	.022	.010	.131**	.013**
	(.016)	(.015)	(.057)	(.005)
lnrelativepay	.433**	.385**	.753	.049
	(.184)	(.171)	(.649)	(.058)
ainputspricedeflatormeda	-.307	-.890**	.200	.047
	(.407)	(.378)	(1.433)	(.128)
outputpricedeflatormeda	.670*	.198	-1.970	-.190
	(.388)	(.360)	(1.366)	(.122)
cons	7.596***	9.584***	4.859**	.595***
	(.668)	(.620)	(2.352)	(.210)
N	89	89	89	89
R2	.101	.116	.122	.135
Adjusted R2	.058	.073	.081	.094

Variable Estimates of the Production Function

.1.3 First-Stage Regressions Corresponding to the Instrumental Variable Estimates of the Earnings Function

	c-1	c2	c3	c4	c5	c6
	(1)	(2)	(3)	(4)	(5)	(6)
Everunemployed	.169***		.159***	.185***		.169***
	(.039)		(.039)	(.046)		(.045)
Evercooperative		.271***	.248***		.313***	.284***
		(.074)	(.074)		(.080)	(.079)
CasualDummie	.137***	.113**	.130**	.051	-.009	.024
	(.053)	(.053)	(.052)	(.149)	(.149)	(.147)
Dayspermonths(log)	.120**	.151***	.123**	-.020	-.035	-.029
	(.049)	(.049)	(.049)	(.131)	(.131)	(.129)
Age	.014	.016	.012	.023	.022	.019
	(.013)	(.013)	(.013)	(.015)	(.015)	(.014)
AgeSquared	-.019	-.025	-.017	-.032	-.032	-.027
	(.018)	(.018)	(.018)	(.020)	(.020)	(.019)
Sex	.004	-.005	.005	.104*	.101*	.110*
	(.048)	(.049)	(.048)	(.057)	(.058)	(.057)
SomePrimary	.168**	.179**	.157*	.221**	.229**	.202*
	(.084)	(.084)	(.083)	(.112)	(.112)	(.110)
PrimaryComplete	.166*	.172**	.155*	.243**	.228**	.222**
	(.086)	(.086)	(.085)	(.113)	(.113)	(.111)
Grade10	.211**	.192**	.193**	.369***	.320***	.332***
	(.087)	(.087)	(.086)	(.112)	(.113)	(.111)
Grade12	.184**	.194**	.180**	.193*	.185*	.183*
	(.085)	(.085)	(.084)	(.109)	(.109)	(.107)
Grade12plus2	.136	.136	.134	.275**	.246*	.252**
	(.104)	(.105)	(.104)	(.129)	(.129)	(.127)
College	.202	.274	.226	-.110	-.061	-.076
	(.192)	(.192)	(.190)	(.236)	(.236)	(.232)
TVETdegree	.231***	.203***	.213***	.178***	.137**	.155***
	(.052)	(.052)	(.052)	(.059)	(.059)	(.058)
Prior-Casualwork	.105**	.108**	.098*	.054	.075	.049
	(.052)	(.052)	(.051)	(.061)	(.061)	(.060)
Prior-employeefirm	.047	.040	.028	.030	.024	.008
	(.044)	(.044)	(.044)	(.051)	(.051)	(.051)
ApprenticeshipCompleted	.111***	.112***	.110***	.052	.057	.055
	(.040)	(.040)	(.039)	(.047)	(.047)	(.047)
Program-Dummie						
Workers				.176***	.173***	.170***
				(.026)	(.026)	(.025)
Capital-Intensity				.012	.012	.014
				(.011)	(.011)	(.011)
Input-Intensity				-.008	-.007	-.006
				(.014)	(.014)	(.014)

.2 Sampling Strategy

Non-Contractors

Licensegrade	**Number of Firms**	**Population Frequency**	**Number in Sample**	**Sample Frequency**
1			2	6.45
2				
3	2	2.86	3	9.68
4	4	5.71	5	16.13
5	7	10	7	22.58
6	57	81.43	11	35.48
7			2	6.45
8				
9				
10				
11				
12			1	3.23
Total	70	100	31	100

Table 30: **Program Contractors**

Tables present an overview of the proportion of firms in the underlying population of interest, sorted by activity, and the corresponding proportion of firms in our sample. To ensure our sample was representative, we made sure that the proportion of firms in our sample roughly corresponds to the proportion of firms in the population. If only very few firms engaged in a particular activity, such as carpentry and electrical installation, that activity was typically not included in the sample, while firms in other relatively minor activites were marginally oversampled, at the expense of slightly undersampling firms engaging in popular activities. One big problem was that firms can engage in different activities at the same time. Since it was difficult to establish which firms engaged in multiple activities, this was not taken into account when sampling firms. It turned out that 15 program non-contractors and 32 non-program non-contractors engaged in multiple activities.[80]. Also

[80] Contractors also engage in multiple activites, yet this is less of an issue since the license grade is a good indicator of comparability.

Table 31: **Non-Program Contractors**

Licensegrade	**Number of Firms**	**Population Frequency**	**Number in Sample**	**Sample Frequency**
1				
2				
3	36	6.87	2	7.41
4	60	11.45	7	25.93
5	141	26.91	4	14.81
6	107	20.42	4	14.81
7	82	15.65	4	14.81
8	140	26.72	3	11.11
9	40	7.63	1	3.70
10	4	0.76	2	7.41
11				
12				
Total	524	100	27	100

note: 1 program contractor did not indicate his licensegrade

Table 32: **Program Non-Contractors**

Activity	**Number of Firms**	**Population Frequency**	**Number in Sample**	**Sample Frequency**
Pre-Cast Beam	186	13.12	25	15.63
Hollow/concrete Blocks	440	31.03	39	24.38
Wood and Metalwork	403	28.42	51	31.88
Gravel	92	6.49	8	5
wall	162	11.43	10	6.25
Electrical Installation	80	5.64	8	5
Sanitary Installation	32	2.26	6	3.75
Sitework and Finishing	23	1.62	9	5.63
Other	0	0	4	2.5
tot	1418	100	160	100

Table 33: **Non-Program Non-Contractors**

Licensegrade	**Number of Firms**	**Population Frequency**	**Number in Sample**	**Sample Frequency**
Pre-Cast Beam				
Hollow/concrete Blocks	297	12.40	1	1.03
Wood and Metalwork	461	19.25	30	30.93
Gravel	2	.084	24	24.74
Wall		2	2.06	
Electrical Installation	3	0.13	1	1.02
Sanitary Installation			2	2.06
Sitework and Finishing			4	4.12
Other	61	2.55	33	35.02
Fabricated Building Materials	1450	60.54		
Unfabricated Building Materials	120	5.01		
Total	2395	100	97	100

note that for pre-cast beam production, and to a lesser extent for hollow block fabrications, there are no suitable counterparts for program firms, since firms engaging in these production activities use methods not previously applied in Ethiopia. In addition, electrical and sanitary installation were not practiced separately prior to the introduction of the program. Consequently, it was difficult to trace firms engaging in such activities amongst non-program participants.

One big difficulty in sampling firms was that many were registered under the rather general headings of "Production & Sales of Unfabricated Building Materials" and "Production & Sales of Fabricated Building Materials"; while clearly relevant for the purposes of comparison, it was difficult to guess a priori to what extent these firms engaged in activities comparable to program firms.

Contractors Contractors in Addis fall in two type; building contractors, which only focus on the construction and general contractors, firms which engage in a multitude of activities which may include building construction but also road construction and infrastructure development. The sample was confined to building contractors, since we wished to avoid comparing the incomparable. As can be seen in tables , large contractors were oversampled to ensure they were represented in our sample. The tables also reveal that the sampling frame provided by the list of program firms was not fully accurate as a number of firms had a licensegrade which was either higher or lower than the licensegrade listed in the list of program firms and the general registry of firms. In addition, the program contractor with licensegrade 12 seems an anomaly.

.3 Construction of Variables

Contractor All firms which have a contracting licensegrade were labelled as contractors.

Capital Stock The capital stock variable includes the sum of the value of all capital, including buildings, machineries, vehicles, tools, and other assets, at replacement cost. The measure does not account for rental capital. Only 7 firms in our sample indicated to rent capital. Morevoer, imputing the value of such rental capital and using the sum of rental capital and owned capital did not affect the results.[81]

Inputs Data on total inputs were obtained by adding up the expenses for individual inputs.

Construction of the welfare-indicator The predicted welfare indicator is constructed by using information on asset holdings to predict household expenditure, using the CWIQ methodology. The model was calibrated using data from the 1999/2000 HECIS; household expenditure in that survey was regressed on asset holdings for the subsample of Addis residents. The resulting parameter estimates were used to predict household expenditure using the information on asset holdings in the EEA/World Bank survey. Estimates of the predicted wellfare expenditure turned out to be rather high, perhaps because ownership of a radio and a TV were lumped into a single category, or because the price of goods, particularly electronics, has changed over the years.

[81]Imputing the value of such capital was difficult since the replacement cost of these items was not documented in the questionnaire; instead items were valued at the median value for each item, multiplied by the percetage deviation of the rental price from the median rental price.

Imputing the average educational attainment of the workforce Taking a weighted averages of ducational attainments of workers in different occupational categories yields and estimate of the average educational attainment of the workforce. The weights corresponded to the relative proportion of the workforce within a certain occupation, while firm managers' responses to questions about the educational attainment of workers in different occupational categories; for managers, engineers, foremen, skilled labourer, unskilled labourers and apprentices, were used to impute the educational attainment of workers in different occupations. Unfortunately, firm managers only had to provide a coarse estimate, forcing us to impute more specific numbers. The following imputations were used

1) Never been to school - 0 years of schooling
2) Primary school incomplete - 3 years of schooling
3) Primary School complete - 6 years of schooling
4) Grade 10 complete - 10 years of schooling
5) Grade 12 complete - 12 years of schooling
6) College diploma - 14 years of schooling
7) College degree - 16 years of schooling

Such imputations are inherently arbitrary and the measure of educational attainment constructed is noisy. Ideally, one would have observed the educational attainment of all workers in the firm, yet this was impossible since only 4 workers per firm were interviewed, which is why we rely on the managers' responses instead.

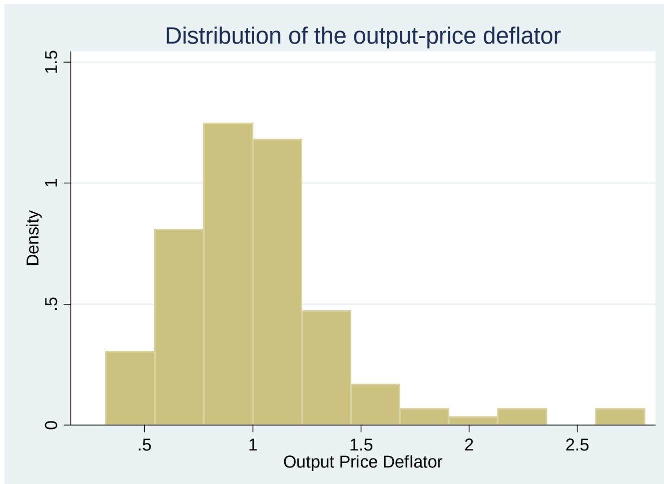
Imputing the average age of the workforce The procedure for constructing a variable representing the average age of the workforce is similar to that for imputing the average educational attainment of the workforce; a weighted average of the manager's responses to questions about the typical age of workers in different occupations, with weights proportional to the share of the workforce within any given occupation, were used to construct to construct a measure of the average age of the workforce. Again, coarseness in the response options for managers forces us to make arbitrary choices. The following imputations were used;

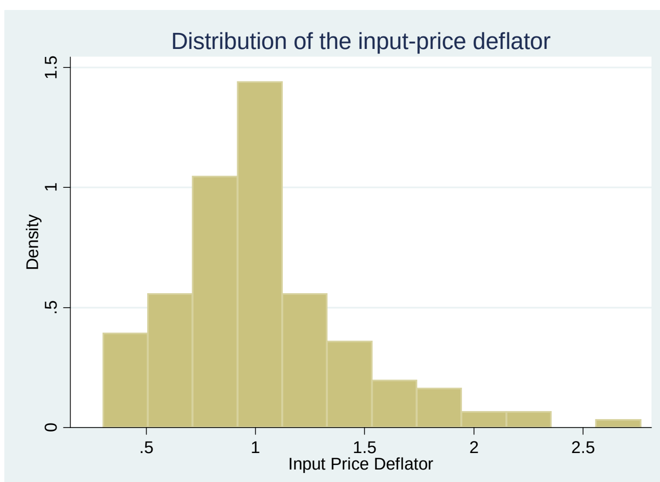
1) 15-20 - say 17.5
2) 20-25 - say 22.5
3) 26-35 - say 30
4) 36-50 - say 43

5) Above 50 - say 56

Price-deflators The data on the amount and costs of different inputs used and the amount and prices of different outputs produced allowed us to construct firm-specific input- and output prices. Collecting these input- and output data was arduous because firms, particularly large ones, simply do not keep track of the precise amounts and exact costs of (all of) the different inputs they use. The data on inputs and outputs consequently seem to contain a lot of measurement error and some severe outliers, which were dealt with with by scaling down reported prices to appropriate orders of magnitude, that is to the same order of magnitude as the median price. Recall that inputs- and output data were missing for 30 contractors and for 49 firms in total.

To enable a price-comparision between firms using different inputs, input- and output price deflators were created as follows. Firstly, firm-specific inputs and output-prices were created for each in- and output by dividing quantity of input (output) used by the total amount of money paid (received) for the particular input (output). Secondly, these prices were divided by the median price for that input (output) for all firms, to arrive at relative price. The inputprice-deflator is then constructing by taking the weighted average of these relative prices, with weights corresponding to the relative cost-share of the respective input in the total costs of inputs (outputs). Similar deflators were constructed for program and non-program inputs and outputs separately, enabling us to compare the cost of program inputs with the costs of non-program inputs. The resulting price-deflators are reasonably well-behaved as they are symmetrically distributed around 1, although there are some implausible outliers, which are excluded by trimming values greater than 2 and smaller than .5.


Distribution of the output-price deflator
Density
0
.5
1
1.5
.5
1
1.5
2
2.5
Output Price Deflator


Distribution of the input-price deflator
Density
0
.5
1
1.5
.5
1
1.5
2
2.5
Input Price Deflator