Search frictions and Urban Youth Unemployment: Evidence from an Experiment in Subsidized Transport

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Abstract

Are unemployed youth constrained in their ability to find work by the high costs of gathering information about job vacancies? I estimate the impacts of reduced job search costs on labour market outcomes for unemployed youth in spatially dislocated areas of urban Addis Ababa, Ethiopia. Some job seekers were randomly assigned to receive a transport subsidy, twice a week for up to 11 weeks, and covering costs to travel from the outskirts of the city to a central area where information on vacancies can be consulted. I find that receiving this transport money increases the likelihood of finding permanent employment by 7 percentage points (over a control mean of 19%), and otherwise improves the quality of jobs found by treated respondents. I use an innovative phone survey to follow the sample on a weekly basis to identify the mechanisms by which these jobs are found by looking at trajectories of job search intensity and labour supply over all weeks of the study. Job seekers are less likely to have become discouraged, they are more likely to be searching for a job and to have visited the vacancy boards while the subsidies were being paid. There is some evidence that job seekers were less likely to have taken temporary jobs in the early weeks. These results are consistent with a story of respondents waiting and searching more intensively for good quality jobs, instead of doing temporary work or becoming discouraged. These search impacts are persistent, treated individuals are far more likely to still be searching many weeks after the transport program came to an end. In addition, I test for the impact of the phone calls by randomly assigning respondents neither calls nor phone subsidies: I find no effect of this high frequency data collection on labour market outcomes.

1 Introduction

Financial constraints related to the high costs of transport may make it challenging, and at times impossible, for poor, cash and credit-constrained job seekers to search for jobs. These

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job seekers may underinvest in job search activities, fail to find their desired employment, or may be forced to take inferior forms of employment in order to survive. If these constraints fall particularly heavily on the poor, new urban migrants, or those without the requisite experience or social connections to find jobs, then these individuals could be systematically excluded from the labour market.

This paper uses an RCT to test directly for this connection between the costs of job search and equality of employment outcomes, by looking at whether financial support, in the form of transport subsidies, causes better employment outcomes for those who might otherwise be excluded from jobs. These constraints have implications for equality of access to opportunity for young people in the context of a growing economy with expanding employment opportunities, but significant informational frictions in distribution and access to information about job vacancies. In this sense, the costs of job search could be preventing economic growth from being "pro-poor".

Many economies in Sub-Saharan Africa have achieved high and sustained growth in the last decade. However, economic expansion has rarely been followed by marked improvements in the labour market outcomes of the poor and the young (International Laour Organization, 2012). There is thus growing awareness that search frictions, arising in particular from the spatial mismatch between low-income residential areas and job hubs, may be preventing economic growth to be truly pro-poor in urban settings. This is true of Ethiopia (World Bank, 2007), where youth unemployment in Addis Ababa is well over 25%. However, there is insufficient descriptive data on the search process used by young people, and a lack of rigorous evaluation of job search assistance programs in the context of Sub-Saharan Africa.

My randomized control trial distributes money to cover the costs of transport for young unemployed job seekers living on the outskirts of Addis Ababa to travel to the center of the city. Participants could redeem the costs of that trip if they arrived a designated spot located at the major transport hub in the city center, which also happened to be the location of the main job vacancy boards, which are the main source of information about new opportunities for many Ethiopians. At $2 per week per respondent (to cover a $1 return trip to the center) the intervention was designed to be a low cost program, which could be easily scalable to larger transport subsidy program, or job search assistance grant.

I evaluate these transport subsidizes using detailed baseline and endline surveys 4 months apart, as well as a novel phone call survey, for which respondents were phoned every week for 3 months, and asked a series of short questions about labour market outcomes. This regular phone call data allows me to track the trajectories of search intensity over the weeks, and look at where individuals found jobs, or gave up searching entirely.

The questions for this phone call survey were decided upon before the randomized trial began, and are limited in number. Throughout the paper I investigate the impact of the transport subsidies with specific focus on that limited set of outcomes from the phone survey. I look at additional more detailed job market outcomes included in the paper endline surveys only as a robustness check, and to further investigate the mechanisms of the main impacts. This strategy should in part allay concerns about multiple hypothesis testing across a range of different measures.

Furthermore, I use a two sample approach, to compare and validate the results of the trial across two very different populations of job seekers in Addis Ababa. One sample was taken

from unemployed people found at home in randomly selected slum areas of Addis Ababa, where most of the respondents were unskilled and had limited employment opportunities in professional or white-collar work. The second sample was taken from individuals found at the main vacancies boards in Ethiopia, who were mainly well-educated, active job seekers, who still aspired to sought-after professional jobs. Surprisingly however, these highly educated respondents were not significantly wealthier than the other sample, owed in part to the massive expansion of free higher education in Ethiopia during recent years.

1.1 Main Findings

I find that the individuals receiving the treatment have improved labour market outcomes at endline, suggesting a causal link between the costs of transport and success in the labour market. Treated respondents were more likely to have better jobs: permanent (stable) employment, in white collar work, that they set out to find. In addition, treated respondents were less likely to have become discouraged from job search: they were less likely to have given up searching while not having employment. They were also less likely to have given up searching while being engaged in casual or temporary labour.

Among those sampled at the boards (referred to as the *board* sample), treated respondents were more likely to have found permanent jobs (usually requiring high educational attainment) although not more likely to be working on average. I estimate the impact on finding permanent employment to be 7% percentage points over a the control mean of 19%, in this sample. This result is robust to different specifications including the inclusion of a full set of covariances, baseline blocking categories, first difference estimates, and logistic regression.

The results exhibit considerable heterogeneity across the two separate samples. For the sample found at home in parts of the city (referred to as the *city* sample throughout) treated respondents were more likely to be working on average. While the treatment had no effect on finding permanent employment for this sample (the chances of them finding this kind of work is considerably lower for those without education in general), it increase the probability of being satisfied with a job, having found employment through formal means, and working in an office job.

Using the high frequency phone call data, I look at the impact of the transport subsidies over the entire duration of the study, and its impact on the trajectory of job search activity over time. In all, and for both samples, the treatment shifted the distribution in the labour market status of the sample, away from discouragement, into both more active job search, and (by the later weeks, including the last week) into more and better employment.

The percentage of respondents searching for jobs declines significantly over the course of the study, as respondents give up looking for jobs, either settling for unemployment, or taking temporary jobs to make ends meet. The treatment seems to be kept this type of discouragement at bay for the treated respondents. In most weeks, treated individuals are more likely to be searching for employment, both in general, and specifically travelling to the jobs boards to look for formal positions. The impacts do seem to be at the extensive margins: conditional on searching for a job, treated individuals do not search more often, they are just far more likely to have not given up. The impacts kick in earlier for the *city* sample, who

were less likely to be searching for jobs in the first place, who seem to have been temporarily encouraged to start searching, while for the results are evident for later weeks among *board* sample, once these more active job seekers begin to give up.

These results suggests very real financial constraints which cause job seekers in Addis Ababa to give up searching for jobs when they start running out of money, perhaps in conjunction with becoming discouraged at the prospects on the job market. Indeed the results seem to *persist* beyond the end of the transport subsidy period, so that treated individuals are still searching for jobs more intensively, even after the money is no longer being delivered. This suggests "safety net" role for the transport cash transfer, which prevents respondents from slipping into discouragement, rather than just a temporary substitution effect from leisure to job search.

1.2 Theory and Literature

The growing urban inequality, unemployment and urban sprawl of Addis Ababa, in conjunction with evidence of considerable barriers to access information and jobs for young people, provides motivation for studying the role transport costs in the lives of young job seekers in this setting (see section 2 for a detailed discussion of unemployment and job search in Addis Ababa). An extensive literature on the role of search costs, the constraints placed on the poor in their ability to find good jobs, and job search generally, also motivates the first study that I know of to look at the casual impacts of high transport costs on labour market outcomes in an African context. It is to this literature that I contribute.

The results in this paper are consistent with a very simply model where job search costly in monetary terms. Credit (and possibly cash) constrained job seekers decide their optimal search intensity to equate the marginal cost of foregone consumption due to the costs of search with the marginal gain of increased probability of having a good job in the next week. In this setting, poorer individuals find it harder to search intensively because the marginal cost to searching is much greater for them. They might like to borrow against their future earnings from employment, but they are credit constrained. In the extreme case, they may not even have enough money to search at all and still have money for consumption.

This set up relates to a well established literature on job search intensity and labour supply. Standard search theoretical models predict that job search intensity and unemployment durations can be influenced by liquidity constraints (Chetty, 2008; Card et al., 2007). In these models individuals with cash constraints are forced to find jobs *too quickly* because of their inability to smooth consumption across their employed and unemployed states. However the Ethiopian job search context differs from the usual job search model since job search isn't costly just in terms of foregone wages while in unemployment, or a simple separable utility cost of job search. Rather search is costly in monetary terms, and it is possible that unemployment benefits could actually increase job search intensity by increasing liquidity. In the Ethiopian Labour market that I study, young individuals are often forced to choose between taking low quality temporary work in order to increase current consumption on the one hand, or searching intensively for good (permanent) employment on the other.

In this sense the model relates to the literature on rural-urban migration, in which the decision to look for a job in the city is costly and has risky outcomes when individuals are

close to subsistence levels and cash and credit constrained. Bryan et al. (2011) randomly assign subsidies to cover the costs of out-migration to rural households, and this increases employment outcomes in urban areas. These migrating individuals foregone production in agriculture or local jobs, to take the risk of finding better (urban) employment. Indeed, individuals in my sample have the option of temporary or family enterprise work, while they are searching for permanent wage employment, but many seem able to do this work while still searching. But working on temporary work is time consuming, and could constrain job seekers' ability to search for work. So while individuals could easily mitigate the risk associated with search by working, this is likely to inhibit their ability to find work by reducing the time they have available to search, regardless of how much they might be spending.

The empirical literature suggests that financial constraints can indeed lead to sub-optimal job search and labour supply in developing country contexts. Ardington et al. (2009) argue that South Africa's pension grant frees up resources in house which allow prime age household members to make the required investment to migrate to find work in urban areas. Franklin (2012) shows that government housing in South Africa frees female household members to enter the labour force, perhaps because the physical burdens of slum living were relieved. Dinkelman (2011) finds that rural electrification increased labour supply of females, for similar reasons. Field (2007) shows how improved property rights in Peruvian slums seems to have caused a shift away from work in household and increased labour market hours.

However, with a simple partial equilibrium model of this kind, reducing search costs only improves labour market outcomes for those those receiving the subsidies, at the expense of other job seekers. In this sense the treatment effects estimated in this paper could be *displaced* in general equilibrium. This was shown to be the case for job search assistance programs in France Crepon et al. (2013).

Yet theories of urban labour markets suggests that high transport costs due to living location increase the costs of search for anyone, and could thus be bad for all individuals living far from jobs, and for the economy as a whole (Pissarides, 2000; Zenou, 2009). If search frictions caused by the costs of acquiring information increase with distance (Ihlanfeldt, 1997), particularly in markets where other channels and institutions for the dissemination of information are poorly developed (as is the case for Ethiopia, where respondents have to travel to physical job boards just to find out about vacancies). As the city grows, the costs of congestion and transport, are likely to increase these frictions further in the absence of other labour market fixes. Search and matching frictions have been shown to be a significant cause of unemployment, lengthier unemployment spells, and even lower wages and productivity (Pissarides, 2000).

However, this paper focuses on, and tests for, the implications of search costs for equity of outcomes, rather the inefficiency of the labour market in general.[1] The constraints on search activity due to search costs and cash constraints are likely to fall particularly hard on those who are poor, and those who live further away from the center of the city[2] for a number of possible reasons:

[1]This paper cannot test explicitly for general equilibrium effects, such as increased employment or productivity, of lowering search costs. But I can test for the existence of a search costs as a significant friction in the labour market, which the theory strongly suggests plays a role in overall levels of employment and productivity

[2]These are often, and increasingly, the same people in growing African cites

(1) Poor respondents might be particularly likely to be living far away from jobs and the city center, as rents are driven up in the center, and as city beautification drives forces high density slum type settlements out of the city[3]. (2) The costs of relocation closer to jobs might be particularly burdensome for the poor (Wasmer and Zenoub, 2006) (3) For the poor the costs of job search might simply exceed the benefits of finding employment, particularly if the more desirable jobs do not pay significantly more Holzer et al. (1994).[4] (4) The poor may be particularly constrained in their ability to access credit, either from formal sources, or in the case of the young from their parents or family, in order to borrow now against expected future earnings in better work. (5) Furthermore the opportunity costs of spending money on job search are greater for the poor, for whom that this relatively small amount of money may have large returns to other uses. (6) If search is risky and unpredictable the young and poor, with limited savings or buffer stocks, may find job search too risky, as it could threaten their livelihoods if it does not pay off.

Indeed these are the arguments made for the existence of the spatial mismatch hypothesis (Zenou, 2009), where the increased costs of job search fall disproportionately on minority, low skilled workers in the United Sates (Ihlanfeldt and Sjoquist, 1998). An experiment perhaps most similar to the one conducted in this study, Phillips (2012) estimates the impacts of transport subsidies in the city of Washington, DC, where free metro train tickets induce increased job search, and lead to treated individuals finding employment slightly faster.

Further, certain individuals may be at a particular disadvantage with regards to information about job opportunities. The transport subsidy tested here acts a way of improving access to information for those at such a disadvantage, by bringing them closer to information about jobs. A growing body of literature looks at interventions that overcome small informational asymmetries and barriers. Jensen (2012) looks at a program giving information to young woman about good quality white-collar jobs available in nearby urban areas, and shows that this information alone increases rates of work, not just at the specific jobs, about which information was given, but employment generally. Beam (2012) looks at an intervention to bring unemployed individuals in Philippines to job fairs, ostensibly to find out information about opportunities to work overseas, but finds that individuals attending the job fairs seem to pick up information that leads them to be more likely to work in urban areas, actually in the Philippines.[5]

Finally, I contribute to a growing literature dealing more generally with youth labour market attachment and school-to-work transitions. Notably, my study evaluates a relatively low costs intervention that seems to play a role in speeding up youth entry into employment. Given that other studies (Betcherman, 2004) find relatively weak impacts of relatively expensive Active Labour Market Policies, wage subsides and employment training, this study suggests that relatively cheap interventions aimed at removing labour market frictions could be considered as complements to other labour market fixes.

This paper presents some of the only evidence, to my knowledge, of the impacts of phys-

[3]These drives are visibly in effect all over Addis Ababa and have an an impact on the lives of many of the poor (Yntiso, 2008)

[4]At least in the short run, some of the better jobs in the market do not pay more initially, but often opportunities for promotion or other long term benefits, by virtue of their more permanent nature

[5]On a related topic, Pallais (2013) finds that information asymmetries about worker quality can be overcome easily by writing more detailed job references in order to correct for an under-supply of publicly available information about worker quality.

ical disconnection from jobs, in an urban context, on inequality of employment outcomes. In this sense, it constitutes evidence for a version of the spatial mismatch hypothesis, according to which, the young and poor, with lower labour market attachment are poorer access to jobs as a result of high transport costs.

1.3 Implications

This paper uses the first randomized experiment in urban transport costs used in an African context, to my knowledge, coupled with a unique high frequency data set on respondents' labour outcomes, which allows me to observe a range of volatile employment outcomes over time, and track the trajectories of job search intensity and routes into employment among my sample. In addition, I can look at how the treatment effects measured in the paper change with time, and remain persistent after treatment ended.

The results found in this paper seem clear and convincing evidence that respondents were constrained by the costs of search, and searched more intensely. However, there are some caveats. One is the short time frame of the study, which was run from start to finish in just 4 months, and observed respondents for the last time, just a little more (and sometimes less) than a month after the intervention ended. While treated respondents were more likely have found jobs at endline, the impacts could be short lived if the control group catch up and find the same good jobs, just slightly later. This could be the case if treated respondents just brought their job search intensity forward while they were being subsidized, instead of just procrastinating, or spreading their search efforts over time. However, given that the control group were more likely to be discouraged (and less likely to be searching) at endline, this seems unlikely to be the case. An additional round of surveying has been commissioned, and the results will be added to this paper in due time.

Secondly, this study is a small sample study. While many of the results are reasonably large and statistically significant, and sustained over a number of weeks, a larger sample would provide reassurance that they hold, especially at higher levels of significance. This is particularly true the heterogeneous treatment effects, which are often estimated for treatment groups of less than 100 individuals, and are thus have limited power (less than 80%) to detect large treatment effects of even 0.4sd (standard deviations) at 5% significance levels. Thus treatment effect estimates of large magnitude are only significant at the 10% level.

Finally, the impacts are seen most noticeably among highly educated, active job seekers. While this sample is certainly representative of the average educated job seeker in Addis Ababa, the treatment effects may not be apply to the youth population as a whole. Scaling up the intervention to their entire population may not have the same benefits to individuals who do not fit this profile.

2 Setting and Experiment

Addis Ababa, like many cities in Africa, is suffering from high unemployment, even during a time of enormous economic growth and urbanization. While youth unemployment appears to have fallen marginally over the last few years, it is a alarmingly high, at 28% (Broussard and Teklesellasie, 2012). The data collected for this study searching for jobs in a market where

information about jobs is hard to find, and expensive to seek out, especially as the city grows, and new migrants and recent school and university graduates live further and further from the city center, which good jobs are most easily available.

The story of youth unemployment in Ethiopia, is partly a story of education (Krishnan et al., 1998; Serneels, 2007). While in the past highly educated youth were on average wealthier than those without education, this is no longer such a strong relationship. The enormous expansion of opportunity in University education, and the low costs of attending university have led to a large population of poor youth with university degrees, diplomas, or vocational training. For these individuals, long term unemployment is not often an option, they have to find temporary or casual employment wile searching for the jobs that they want.[6]

In 1999 only about 4% of urban Ethiopia had any kind of higher education, this rose to 27% by 2011.[7] When in the past education was a guaranteer of a good job, and is still viewed as the route to a middle class life (Mains, 2012), among the youth aged 20-24 the rate of unemployment is in fact the highest among those with some kind of post-secondary education (Broussard and Teklesellasie, 2012). Because many young educated people give up job search after failing to find work in their preferred profession (entering a state of discouragement), the standard definition of unemployment used in this context is the broad one; anyone available and willing to work is considered unemployed, instead of just those who are searching for work (Broussard and Teklesellasie, 2012). It is individuals in the broad category of unemployment that this study focuses on.

Yet a standard queueing story often applied to Ethiopia, according to which highly skilled individuals *wait* for civil service jobs may be a useful framing of the unemployment problem, is something of a misnomer. While most individuals do aspire to this sort of white-collar or public sector work, getting a job in Ethiopia is hardly a matter of waiting: finding employment is time consuming, expensive and, for lucky and well qualified individuals, rewarded by sustained work and application. Jobs are not easy to find out about and to apply for.

Mains (2012) describes the youth unemployment rate as *voluntary* phenomenon for the highly skilled, whereby unemployed youth are unwilling to engage that they consider shameful, or not worth their time. My work offers an alternative story for modern day Addis Ababa. Many highly educated individuals are now taking work outside of their chosen profession, often in fields that they would previously not worked in, on short term contracts, in order to survive, while all the time searching for work better fits they aspirations and educational background. My surveys ask about all work opportunities available to respondents and attitudes to shame[8] and find little evidence of individuals turning down jobs or refusing to do certain types of work. Job seekers want permanent or white-collar work because these jobs are more secure, provide more regular hours and therefore higher monthly earnings. They are also have to invest heavily in searching for this jobs, and have to balance opportunity costs of their time and money constantly.

The majority of white-collar jobs are found on job boards, located in the center of the

[6] I will describe the nature of this kind of temporary in the section describing my sample, but 40% of individuals who did not have work by the end of the study, did some kind of short term work during the previous 15 week period. Only 12% of jobs done by all respondents during the course of the study were considered to be permanent

[7] Author's calculations from the CSA Urban Employment/Unemployment Surveys)

[8] The Amharic term for the feeling associated with work or other activities below one's class or dignity is *yilunta*

Addis Ababa. Many of the jobs on these boards are cross-posted in newspapers available for rent near and around the job boards. Most individuals find out about these types of jobs by travelling to the boards. Since are many sources of information and new vacancies are released sporadically on different days, it pays to travel to boards regularly and survey all of the available information sources.[9] The job boards are most common method of job search, more than 50% of all unemployed youth across Ethiopia use the boards, or newspapers available next to them, as their primary method of job search (Broussard and Teklesellasie, 2012).[10]

This market for jobs is one plagued by frictions. For employers, the lack of a centralized places to recruit and advertise jobs, mean that they publicize vacancies at a number of boards and newspapers, all at some cost (and for low productivity, low paying jobs, the costs of more intensive recruitment are likely to be prohibitively high). Employers complain about being swamped with applications, often from unqualified applicants, and now some firms charge application costs, presumably to separate out the qualified applicants, thus further increasing the costs of job search for the unemployed. There is the widespread belief that many employers have also switched to hiring through referrals or social networks, rather than making jobs publicly available.

For job seekers the costs of gathering information about jobs are high. While the vacancies on boards are freely accessible, the newspapers, which often contain different or more up to date jobs, cost money to rent (very few people pay the prohibitively high cost of actually buying the newspapers). Applications often come with a fee, and some privately run labour brokers charge money to put job seekers in touch with work. The main cost for job seekers, however, are the costs of transport to travel to the centrally located job boards. This costs are unevenly distributed, however. In a city of over 4 million people (in the greater urban area), and with continuous settlement for up to 10km in any direction from the city center, some individuals have enormous distances to travel to have access to this job information, and the costs of transport are high and have been growing. A detailed discussion of the costs of transport, relative to the money available to the respondents, is a given in the next section 2.1. A map of Addis Ababa and it's physical size is given in the appendix, figure B.2. The central four sub-cities (not shaded grey) are probably a good guide to the size of the city as little as a 30 years ago.

The city of Addis Ababa has been growing rapidly, "its population has nearly doubled every decade. In 1984 the population was [1.4 million], in 1994 it was [2.1 million] and it is currently thought to be 4 million. UNHABITAT estimates that this number will continue to rise, reaching 12 million in 2024." (UNHABITAT, 2003). A good discussion of recent

[9] It is puzzling why there isn't a market for job vacancy information to be delivered to areas other than the center. The information on the boards is not centralized in any way, with different boards and newspapers being run independently. Job seekers want to get access to the full set of available vacancies, and thus are willing to travel to look it all. Since new information is released almost every day, and applying quickly increases the chance of getting jobs, it wold seem that there are returns to visiting the boards regularly. The costs of collecting this decentralized information and disseminating it, on a regular basis, seems to high to be a profitable service for job seekers who have very little money to spend on job search and for whom the marginal benefit of finding out about one more vacancy, is very low. Indeed I spoke to one entrepreneurial service trying to do exactly this, by allowing paying subscribers a small fee to be able to phone in and et information about job vacancies, but respondents using his service complained that the information was incomplete and not up to date enough to be worthwhile to them. The phone service was understaffed and hard to get through to.

[10] The second most common method is asking friends about jobs, followed by going directly to work sites to ask for work.

urban developments in Addis Ababa is given in Yntiso (2008). Addis Ababa has been the major arrival city for rural-urban migration in Ethiopia, and is one of the most growing rapidly cities in Africa as a result (UN-HABITAT, 2005). Many of the new migrants can no longer access well positioned land in the centre of the city, and long term residents are also increasing being forced out of the inner city slums to make way for new development (UN-HABITAT, 2005).[11]

So for those living far away from the city center, finding ad job for the first time or after a spell of unemployment, tends to be difficult, especially for those without savings or financial support. Many of the job seekers in my sample were individuals that had a graduated the previous summer, 10 months before the baseline survey, and had still failed to find a first job, while claiming to have searching intensively, on and off, for that whole time. Individuals who had never had work before had been without for work since they graduated on average for one whole year. Others had been searching for well over a year. Among those who had done at least some work before, they had found themselves unable to find work for, on average, 30 weeks since they last worked.

These features of high youth unemployment, difficulty of acquiring employment for young graduates, long periods of involuntary unemployment, high inequality, and growing low density urban form, suggest Addis Ababa as an excellent setting to study the role of search costs as a barrier to entry into employment. This is particularly true for the very poor, living in peripheral areas of the growing city, and facing transports costs comprising a high proportion of their weekly expenditures. It is to these costs of transport and the experimental intervention designed to reduce them, that I now turn.

2.1 Transport costs in Addis Ababa and the intervention

Individuals were given money to cover the costs of the transport if they arrived to collect it at designated spot in the center of the city. The amount given out was enough to cover the costs of return trip to the center by the proffered type of mass public transport available. Addis Ababa is serviced by a fleet of large orange buses, run and subsidized by the government. While these are buses are very cheap, they are uncomfortable, overcrowded, and arrive less frequently than the other main form of transport available, the mini-bus taxi. These mini-buses are similar to those used in many African countries, an overview of the industry can be found (Kumar and Barrett, 2008). Given that most young people prefer to use mini-buses instead of buses, we budgeted enough money to make the trip by mini-bus.[12] The modal amount given was 15 birr (no one received less than 12 or more than 20 birr) for a return trip, or just less than $1 per day.[13]

The amount was chosen to not exceed the costs of the travel by enough to entice indi-

[11]Compensation is usually poor and many of those displaced are suffering from having to move to dislocated areas where they no longer have access to their social networks and business links in the center of the city (Yntiso, 2008). Existing research documents the loss of income, and transport related problems of those living in worse locations within the city (Yntiso, 2008)

[12]At the endline survey, when the subsidies were no longer being handed out, 58% of respondents said their main mode of travel was in a mini-bus, 38% said that they used a bus, while a negligible number used other modes of transport such as walking or getting lifts with acquaintances with cars. Travel on buses, on average, took 10 minutes longer than a mini-bus trip, which had an average one way trip time of 33 minutes.

[13]The US dollar - Birr Exchange stood at around 18 Birr to $1 at the beginning of the experiment.

viduals to collect the money and return home with no other purpose to the trip. Indeed a few individuals, who made no effort to search for a job during the course of the study, did initially collect the money perhaps hoping to receive more than they were offered, but soon stopped coming for the money when they realised that there was little profit to be made on each trip. Thus the intervention was designed to impact individuals who had reasons to travel, in most cases to search for work, and were constrained by the costs of travel at the margin.

The amount did have a small surplus built in in order to cover the additional costs of searching and applying for jobs. At the endline survey, respondents report that, on average, a return trip by mini-bus cost 9.50 Birr, compared to 5.50 Birr by bus.[14]

However, in focus groups at the end of the project, most respondents reported that public transport fares only made up about half of the total cost of search. There are other costs, such as buying for food for sustenance while away for home or renting the newspapers that had information about the jobs available. The extra 5 or so birr built into the subsidy were designed to *partially* cover these costs, without inducing obvious wealth effects. In my sample at the endline survey, 75% of respondents without jobs, who had travelled to the center of Addis Ababa, reported that their main reason for travelling was to "search for a job", as opposed to recreational or other activities. This should be enough to allay concerns about wealth impacts of the intervention, as there would rarely be additional money left over after making the trips and searching.[15]

The costs of transport seem small, but for the unemployed and poor of Addis Ababa, they do present a significant barrier to searching for work. Average weekly expenditure per respondent in the sample was 127 birr at the baseline survey, but this masks considerable heterogeneity. Median Expenditure was 80 birr, 70 birr among those without some kind of employment (usually temporary). Some of those without employment drew this expenditure either from meager savings (only 22% of the sample had any savings at all), but in the most cases survive on money from their parents, transfers which averaged 154 birr per week, among the 50% of the sample who reported getting money from parents).[16] Even those with some kind of employment earned, at the median, only 138 birr per week.

Thus, a single trip by mini-buses costing 9.50 birr represents, in my sample, 12% of median weekly expenditure. At baseline, respondents were travelling to the center twice a week. Two return trips by bus would cost 13% of median expenditure, or around 23% by mini-bus. Indeed, in the baseline survey, transport costs were on average 25 birr, or 20% of total expenditure. The transfer provided by the intervention, of up to 30 Birr per week, thus provides a significant transfer for many of the respondents, but one that was non-fungible. Individuals who were offered the full 11 weeks of the program, had the option collect up to

[14] Although is likely to be an underestimate of the costs as it was clear that some respondents were reporting on the costs of shorter trips in their neighbourhood, not a trip to the center.

[15] Some regular job seekers who got the treatment reported that they would have spent money to travel to the boards anyway and that the money that they were given did not induce them to search more intensively, but said that they had used the additional cash on other aspects of job search instead: they had used the money to rent more of the job newspapers for longer, for the application fees some job brokers and even firms charged prospective employees, which allowed them to apply for more jobs, and for the costs of transport to travel on to employers where applications needed to be made. While it was possible to walk to collect the money, all respondents were living at least 5 km from the collection point at the baseline, and very few reported ever walking into the center.

[16] The section describing the sample following this provides a more detailed description of the lives and budgets of these job seekers.

330 Birr, or $17 over the course of the study.

2.2 Experimental design

The sample was assigned to treatment and control groups randomly, with the sample split into three groups: the pure control group, a control group who did not receive the transport program but did get weekly phone calls, and the treatment group who received both phone calls and the transport treatment.[17] Immediately after the completion of the baseline data collection on April 4, 2013 (the baseline took 16 days from March 19 to April 4), the sample was assigned to treatment and control groups for the purposes of the experiment. Randomization was done by stratifying the sample by a number of different baseline covariates, including gender, education and baseline covariates. I followed the standard blocking procedure as suggested by Bruhn and McKenzie (2009). Then within these strata, 30% of each strata were assigned to both the transport and the *calls only* groups. The remaining 40% were designated as pure controls. A more detailed discussion of the variables used to block randomize is given in the data description section. Figure B.1 in the appendix gives an overview of the randomization design and timeline. 551 respondents received the phone calls, of them a further 255 were offered the transport treatment. 326 were not contacted again until the endline.[18]

This facilitated the roll out of the treatment to the first half of the treatment group in the week beginning 8 April 2013, which will be referred to as *Week 1* throughout the rest of the paper. Phone call surveys, which are described in more detail in section 3.3 were also begun in that week (1). Respondents were phoned on the preceding weekend, informing them that they had randomly been selected to receive a transport subsidy program, that would completely cover their transport costs for two days per week. They were told for how long this transport money would be handed out. They would receive the treatment by arriving at the center of Addis Ababa, to a bus terminal and hub, near where the main job boards in the city are located, showing their identification,[19] signing for the amount of money that they could collect, and then receiving the specified money. The amount specified was tailored to the distance an individual travelled; using the transport costs for a trip from each respondents place of living, using the current rates in Addis Ababa, as surveyed by the enumeration team. They could sign for the money twice a week, and no more, and needed to collect the money before midday.[20]

A makeshift kiosk was set up in a public recreational space next to the central bus terminal, and the transport money was handed out to the selected respondents by a single individual, from the first week right up until the 11th week of the study. Respondents were

[17] No one was assigned to just the transport treatment without getting the phone calls

[18] Practically, however, it should be noted that the blocking had to be done on a pre-entry data set. The full baseline (paper) survey could not be entered in time for the randomization to be done for treatment to begin timeously. Thus a few key blocking variables were entered by hand in order for the blocking to be done. There were a few mistakes in the pre-entry of the data, which lead to some individuals being assigned to the wrong strata, but since these errors appear to have happened at random, this has not upset the balance on baseline variables

[19] Almost everyone in Addis Ababa has some kind of identity card, provided to them by their kebele or woreda (the lowest local government administrative unit) either where they live or in the place that they are from

[20] This was designed to limit the use of the transport subsidy for recreational use, to make it useful for job seekers who would come in early to see the new job postings, and apply for jobs in the afternoon, but not individuals who wanted to take advantage of evening entertainment in the city center

randomly divided into those receiving the treatment for 8 weeks and those receiving it for 11 weeks, and they were informed about how long they would get money in advance. In the week before the intervention ended they were told, either when they collected the money or by phone, that they would no longer be receiving the transport in the next week. The last respondents received their money in week 11 of the study. The last phone calls of the study were completed in week 12. In total respondents could collect the money *up to* 22 times.

3 Description of the Sample

The project timeline, figure B.1 in the appendix also provides an overview of the data collection and sampling of the study, starting in week 0 with the basline survey. The first section of this chapter (3.1) describes this baseline survey after which the sample randomly allocated to treatment and control. Regular phone calls to a subset of respondents were conducted from week 1 until week 11, as described in section 3.3. Three Weeks after the end of the phone call and transport treatments, the endline survey began, with the majority of respondents re-interviewed in detail 16 weeks after the baseline survey was conducted. Because of the short time frame of the project, and a rapidly changing job market outcomes in our sample, respondents were reinterviewed in a *random* order to prevent bias from recall as a result of being interviewed in different weeks, because a purely random amount of time would have passed since the end of the treatment for all respondents.[21] Section 3.5 describes in detail the endline survey and issues of attrition.

3.1 Sampling Strategy

This study is comprised of complementary representative samples of *two* different populations from Addis Ababa. The distinction between the two samples is central to the analysis of this paper and will be used throughout. From here on, I refer to the one sample as the *city* sample and the other as the *board* sample for reasons that will become clear shortly. The two samples full sample, taken together without dividing respondents in this way, will be referred to as the *pooled* sample.

Screening: Both samples comprise of individuals age 18-30, made of men and women, who were available for work, and would be able to start a new job in Addis Ababa in the next 2 weeks if one was offered to them. Individuals who had some kind of work were not excluded, but the screening process was designed to exclude all respondents with permanent employment, those with jobs that they were simply not working at in the week of surveying and those who were only interested in working outside of Ethiopia. It also excluded anyone engaged in full time education, work in the home, or with disabilities making them unable to work. In addition, all individuals in both samples were screened on their place of living: only individuals living in neighbourhoods and small satellite towns at least 5km away from the center of Addis Ababa were sampled. See the map in figure B.2 in the appendix for an idea of the layout of Addis and the radius outside of which the sample was drawn. Individuals in the sample live, on average, 6.8km as the crow flies (sometimes considerably further by road)

[21] Although, as outlined in 2.2, randomized variation in the week in which the intervention ended was intentionally introduced in order evaluated the persistence of treatment effects.

from the city center where the transport money was collected.

The individuals making up the two samples, both screened for eligibility, were found in the following ways:

***city* sample** This sample was randomly drawn by going door-to-door in 7 small enumeration areas around Addis Ababa. These enumeration areas were stratified by sub-city (10 large administrative units in Addis Ababa). The 4 central subcities of Addis Ababa, all completely contained in a 5km radius from the center point of the city, were removed from the sample. The 6 more distant subcities were all sampled, with one Kebele (local government unit) chosen from each subcity. The most populous, *Kolfe Keraniyo* was over-sampled by selecting two Kebeles from that subcity.[22] Two enumerator teams then moved outward in different directions from the center of the chosen Kebeles, surveying about 60 individuals per Kebele. The survey sites are marked in figure B.2.

***board* sample** The board sample was drawn by randomly approaching individuals who were gathered in the areas around the job boards in the center of Addis Ababa. Although they were all interviewed in the center of the city, these respondents were screened on their place of living, all of them lived in same subcities used in the sampled for the city sample, ensuring that they lived on average 5km away from the center. Since they were all by definition looking at the job boards, they would all fit the screening criteria above, since they would be job seekers.

The two sample approach was used to ensure the impacts of the two treatments could be tested and compared in the two samples to see for whom the intervention was most appropriate. For instance the *board* sample who were active seekers might need the money to continue their job search while the rest had no need for it. Alternatively, the *city* sample might have been less active job seekers precisely because of their monetary constraints, and would be most effected at the margin.

During the piloting of the questionnaire, it was revealed that the standard approach of door-to-door sampling, given time and budget constraints which prevented us returning on multiple days to find individuals, meant that we were undersampling highly educated individuals and individuals who were seeking work through formal channels into the permanent employment at the job boards.[23] While some individuals found at home were searching for work, many were doing so informally in their local areas. Discouragement and idleness were common among the individuals sampled in this way. By sampling at the job boards we were easily able to find individuals who were actively engaged in formal job search, althouh, as will become clear soon, not all of them did so every week, or could afford to continue doing so as the study progressed.

3.2 Representativeness

As a result of two sample approach, and the predominance of highly educated respondents found at the board, my sample does over-sample individuals who have some kind of the

[22] with a population of over half a million, this subcity is more populous than the next biggest subscity by more than 50%.

[23] It appeared that well educated job seekers who were interested in visiting the job boards were rarely at home when their households were approached for interviews.

tertiary education. In total my sample comprises 43% who have a diploma or a degree, and a full 23% of the sample with a degree. 80% of my sample has a grade 10 or above.

By comparison survey data from the Ethiopian Statistics Agency, suggests that the population of the same cohort of unemployed individuals living in Addis Ababa, has 22% with some kind of post-secondary education, with just less than 10% having some kind of degree, but a further 9.6% of that that total age cohort (including those not available for work) still identifying as students.[24]

Surveying at the job boards meant that we over-sampled highly skilled and educated individuals, who had already made the effort to visit the boards, relative to the total population of Addis Ababa. Although they were still many individuals without high levels of education at the boards, they were under-represented. Still, the sample gathered from the board can be considered to be representative of the average active young job seeker in Addis Ababa.

However, highly educated young job seekers are a non-neglible contingent of the Addis Ababa youth population, a contingent that is growing in size every year. The *boards* sample provides a representative sample of educated and motivated job seekers, who are possibly the group most likely to respondent to job search assistance programs. The *city* sample represents a better random sample of the young unemployed people of Addis, albeit one that under-represents the highly educated that dominate the other sample.

In terms of other important youth demographics the sample is roughly representative of the population of Addis. 48% of the sample is ethnically Amaharic compared to 49% of the population of Addis Ababa (UNHABITAT, 2003). The sample slightly overrepresents the Oromifa ethnic group (29% of the sample compared to 18% of the population), probably because of the sampling in the outskirts of Addis, which are closer to the Oromia province which surrounds the city.

In this sense, average treatment effects estimated in here, are not argued to be Average Treatment Effects of an intervention of this type, when applied to any person in Addis Ababa. Rather I seek to estimate and compare treatment effects and descriptive outcomes across two representative samples, both of relevant and dominant populations of the city.

3.3 Phone Survey

In order to measure trajectories and test for changes in job market outcomes and job search behaviour during the weeks during which the treatment was being implemented, a phone survey was conducted to gather high-frequency data on job seekers immediately after the baseline survey was complete, and up until 3 weeks before the endline survey was conducted. To test for motivational or Hawthorne effects of regular phone calls about job search, we restrict the phone call program to a sub-sample of individuals. In all 551 individuals were assigned to the phone call survey, and were to be reached by the phone numbers that were recorded during the baseline survey.

The phone survey was conducted by two skilled enumerators who were provided with cell phones and airtime and attempted to call each of the chosen respondents on a weekly basis. They were told to phone the same individuals on the *same day* of the week each week.

[24]Own calculations, from the CSA Urban Employment/Unemployment Survey

Since the questions asked focused on activities in the last 7 days (since the last phone), so that for an individual who was reached by phone call every week, there should be a complete record of their weekly activities for the entire 11 weeks. The phone calls took on average 4-5 minutes in the first weeks of the survey, with familiarity bringing down that time to about 2 minutes. Still there were weeks when not every individual could be reached, and there were some respondents who could not be reached by phone, because they had given the wrong number. In all 4,510 interviews were conducted over 11 weeks, an average of just over 400 individuals contacted each week, with 465 individuals contacted at least once during the survey. Contacted individuals were contacted on average 10.4 times during the study. About 100 individuals who were assigned to the phone survey were never reached by phone, although some were later found in the endline survey.

Again, figure in the appendix provides a useful overview of the design of the trial, including a visualization of the phone call surveys, the sample involved and the weeks during which the phone calls were conducted.

Importantly, costs of mobile credit, time constraints and patience of our respondents all limited the length of the interview that could be conducted in the phone call surveys. As a result only 8-12 questions were administered during this survey, giving only a handful of measures that can be used in the analysis of the phone call data. While this restricts the detail of the investigation that can be conducted, it has the advantaged of pre-committing me to testing the significance of just a few major outcomes. These are the outcomes that I analyse in detail throughout the paper, using more detailed endline surveys to investigate further where necessary. In addition, most of the measures are binary, which prevents someone analysing this data from handpicking variable definitions that produce significant treatment effect estimates.

Most notably, the key variable to be measured in the phone call survey was the "Permanent Job" outcome, which was chosen as the primary question about job quality to be the focus of the study. This was because it was the variable that was clearly the most sought after property of a job among job seekers when this was discussed in focus group discussions at the baseline survey, and from basic data in the baseline survey. A permanent job, may not imply better wages or hours, but promises long term security and less risk of loosing employment in the future.[25] Figure C.1 presents a one page version of the questionnaire used for this survey.

3.4 Test of Balance

Table 1 presents test for balance on variety of job market outcomes, focusing initially on the main employment *outcomes* from the phone call survey. This is followed by balance tests for the main respondent characteristics, and other labour market outcomes.

Importantly, because I am working with two separate samples, with different characteristics on average, and the extent to which I look at treatment effects in each sample separately. Furthermore randomization was stratified, as outlined in previous section, on baseline characteristics, but this was done *separately* for each sample.[26].

[25]The nature of permanent work is discussed in more detail in A.

[26]This was because the distribution of baseline variables was so different across the two samples that it made little

The following variables were used to stratify the randomization, in each of the two samples:

boards Gender, Diploma, Degree, Currently Employed, Work Experience, Age

cities Gender, Completed Grade 10, Currently Employed, Currently Searching for a job, Age

To show that attrition did not have differential impacts on the composition of the treatment and control groups I present balance tests for the sample that were resurveyed at the follow up survey. I discuss the attrition problems of my sample in more detail in the next section, but this table gives at least a first check that attrition did not significantly impact on the balance of the characteristics between random and control. This is suggestive evidence, that at least on observables, those that attritted from the treatment group were not significantly different from those that did not. I check for balance on observables among the group that were reached for the phone call survey, to show that attrition from the phone call survey is not effecting balance either. These are presented in additional balance tables presented in the appendix, table **??**.

The first variable on which I test for balance is the sample dummy variable, for being the *board* sample. Randomization was done separately for each sample but in such a way that the treatment and control group are made up of an almost identical proportions of the two different samples. This makes it possible to test for average treatment effects with the *pooled* sample, although in all specifications, I control for *sample* as a robustness check. I then proceed to test for balance on the main labour market outcomes at baseline, and find no significant differences.

There is balance across a wide range of measures, in the pooled sample, and the two samples separately. Very few measures, and none of the blocking variables or major outcome variables, are statistically different across groups. In fact only one variable is statistically significant at the 10% level, out of 30 variables tested: individuals in the control group are more likely to be recent grads (individuals who finished school, university or vocational training in the last 15 months). This is a group that may not have been searching for work for quite as long. This heterogeneity is only evident, however, among the Boards sample.[27]

This balance holds after attrition to the final endline survey, in Panel B and attrition for the phone call surveys, in Panel C. This gives assurance that the actual samples used for estimating treatment effects (both at endline (B) and weekly (C)) are broadly balanced on covariances. There are a handful of notable exceptions, which are discussed in the more detail in the section on attrition. All variables that do exhibit notably differences at baseline, are used as covariates in estimating regressions as robustness checks.

sense to stratify by the same variables. For example, nearly half of the *boards* sample had degrees, making it a very useful variable on which to block for that sample. However only three individuals in the other sample had degrees, meaning that using this status to stratify would serve no purpose

[27]This lack of balance might be expected to bias estimates of treatment effects *downwards* since my descriptive statistics suggest that recent grads are more likely to still be searching for work at endline, and they are more likely to find permanent employment in the endline survey.

Table 1: Test for Balance in Full Sample and within Board and City Samples

Panel A: Entire Sample at Baseline

	Full Sample				*Boards Sample*				*City Sample*			
	treat	cont	diff	p-val	treat	cont	diff	p-val	treat	cont	diff	p-val
Sample	.539	.54	-8.2e-04	.982	1	1	0		0	0	0	
Work	.256	.258	-.0027	.934	.201	.201	8.4e-04	.983	.319	.326	-.007	.892
Permanent Work	.0039	.0065	-.0026	.643	0	0	0		.0084	.014	-.0056	.642
Searching	.829	.829	7.0e-04	.98	.971	.973	-.0018	.912	.664	.66	.0042	.935
Visisted Boards	.624	.628	-.0044	.902	.964	.958	.0059	.765	.227	.242	-.0152	.744
Discouraged	.12	.129	-.0091	.713	.0216	.018	.0036	.794	.235	.26	-.0244	.609
Hours Worked	7.38	6.06	1.32	.197	6.89	5.15	1.74	.207	7.95	7.13	.82	.588
Construction	.0891	.0905	-.0013	.95	.0935	.0749	.0187	.497	.084	.109	-.0247	.454
Female	.217	.223	-.0059	.848	.129	.132	-.0022	.948	.319	.33	-.0105	.838
Diploma	.205	.183	.0229	.431	.302	.287	.0147	.749	.0924	.0596	.0328	.238
Degree	.236	.242	-.0059	.853	.432	.44	-.0085	.866	.0084	.0105	-.0021	.845
Finish Gr 10	.783	.788	-.0054	.858	.928	.955	-.027	.232	.613	.593	.0205	.703
Age	23.7	23.4	.312	.162	23.9	23.6	.302	.27	23.5	23.2	.324	.371
Household Size	3.52	3.48	.038	.8	2.76	2.89	-.134	.414	4.41	4.18	.236	.321
Head of HH	.225	.223	.0019	.952	.302	.263	.0387	.392	.134	.175	-.041	.311
Amhara	.453	.496	-.0425	.252	.446	.494	-.048	.343	.462	.498	-.0361	.51
Oromo	.318	.3	.0173	.612	.388	.356	.0322	.509	.235	.235	2.1e-04	.996
Orthodox	.705	.721	-.0151	.652	.712	.698	.0146	.752	.697	.747	-.0499	.303
Muslim	.101	.113	-.0123	.595	.0432	.0719	-.0287	.244	.168	.161	.0067	.869
Lives with Family	.256	.268	-.0124	.706	.367	.383	-.0163	.739	.126	.133	-.0073	.844
Born out of Addis	.612	.612	1.3e-04	.997	.813	.814	-.0014	.971	.378	.375	.0027	.959
Recent Grad	.345	.401	-.0557	.123	.468	.551	-.0833	.0989	.202	.225	-.0229	.613
Work Experience	.523	.499	.0241	.517	.417	.389	.028	.571	.647	.628	.019	.719
Weeks w/o Work	37.6	40.4	-2.75	.409	37.3	34.4	2.93	.43	38	47.4	-9.4	.1
HH Wealth index	-.0149	.0143	-.0292	.695	-.112	-.0166	-.0953	.382	.0985	.0506	.0479	.628
Own Room	.229	.223	.0057	.853	.23	.201	.0296	.472	.227	.249	-.0222	.636
Kms from center	6.15	6.33	-.181	.467	6.4	6.86	-.456	.282	5.85	5.71	.142	.481
Weekly expenditure	179	152	26.9	.0352	202	174	28.8	.115	152	128	24.8	.152
Money from fam	84.9	75.1	9.83	.395	113	105	7.69	.657	52	39.6	12.4	.371
Reservation Wage	1224	1282	-58.7	.341	1325	1398	-73	.37	1106	1147	-41.5	.653
N	(258)	(619)			(139)	(334)			(119)	(285)		

Panels B and C for balance after attrition, presented over the page

3.5 Attrition

Attrition was high, for a survey of such a short duration. This was in large part due to the methods used to recontact respondents, which was done primarily via phone, during a time when the Ethiopian mobile network was highly unreliable.[28] Table 2 shows the rates of attrition at varies points of survey: 14% of the total sample could not be found at all after the baseline survey, and about 25% were not found at the endline survey.

A large proportion (just less than half) of the total attrition took place between the first phone call surveys, which is measured for the phone call respondents. This sort of attrition may have implications for the representativeness of the sample (as we might imagine that highly mobile youth, or those without good access to mobile technology, would be more likely to leave the sample), but it is very unlikely to be correlated with the transport treatment,

[28] We were careful to list 2, sometimes 3 phone numbers per respondent, including a number of a next of kin, but there were still mistakes in the phone numbers given, and the turnover of phone contracts made numbers subject to change. In addition, budgetary constraints limited the amount of time and money I could spend tracking down respondents at the endline outcomes, especially for the *board* sample for whom I didn't have detailed information about place of living (having just surveyed them at the boards)

since treatment did not start until *after* the first phone calls.

Table 2: Attrition by Treatment Status

		Calls		
	Control	No Transport	Transport	Total
Never found	81	22	22	125
	24.85%	7.43%	8.63%	14.25%
Contacted by phone, not Endline	0	35	31	66
	0%	11.82%	12.16%	7.53%
Refused at Endline	9	12	7	28
	2.76%	4.05%	2.75%	3.19%
Found at Endline	236	227	195	658
	72.39%	76.69%	76.47%	75.03%
Total	326	296	255	877
	100%	100%	100%	100%

Indeed the transport treatment does not seem to have had any impact on attrition rates. Attrition is a problem for the estimation of treatment effects only if it effects the probability of the attrition, and if attrition is correlated with key outcomes measures. In this case it seems that attrition was different for the treatment group.

The phone call survey seems to have marginally improved the probability of finding a respondent, because we were more closely tracking these individuals, and some would inform the phone call enumerators if they were likely to move town or change phone numbers, allowing us to stay in touch for longer. For the paper endline follow up survey, the rates of making contact are only marginally higher than the pure control group. 76.5% versus 72.3%. This difference in rates of attrition is not statistically significant.

Among the group of individuals receiving the phone calls, the group getting the transport looks uncannily similar to the group receiving phone calls but not transport money, in terms of rates of attrition. Since so much of the analysis will be performed just looking at this group (getting phone calls) it is assuring that the treatment did effect the rates of attrition.

Furthermore, the balance tables presented for the sample reached at the endline (see table 1: Panel B) and the sample reached at least once during the phone calls (see table 1: Panel C), showing very few deviations from balance after attrition, suggesting that the type of individuals that couldn't be recontracted, did not differ along observable characteristics from those that did, between the treatment and control groups. One notable exception to this is the *Kms for the center* variable, which was balanced at baseline, but not among those at endline, among the *board* sample. It seems that treated individuals living further from the center were *more* likely to attrit.*On average, those living far away were more likely to attrit.* This seems counter-intuitive, since one would expect the transport subsidies to encourage youth living far away to come to the center more, making the more likely to be present near the city for the endline survey.

4 Estimation Strategy

I estimate the Intention to Treat (ITT) effect of the transport subsidy program, both at the time of the follow up survey, and during the weeks during which the phone calls and transport subsidies were being implemented simultaneously. Due to problems with implementation of the program and the imperfect rate of take up, we expect these estimates to be downward estimates of the true treatment effects (ATE). In later sections attempt to recover of local average treatment effects (LATE) for those who took up the treatment.[29]

Since there were many periods when treated and control individuals are observed with and without the treatment (for the sample that were included in the phone call survey), there are multiple specifications, and periods in which treatment effects can be estimated. Initially I estimate the treatment effects using only the main follow up paper survey, where all individuals are observed. This is 16 weeks after the baseline survey, between 5 and 7 weeks after the last week that treated individuals could collect the transport subsidies. I then turn to looking at the treatment effects during the multiple weeks during which a phone call survey was conducted, and treatment was being implemented simultaneously. The length of the phone calls limits me to analysing just a few outcome variables.

The estimation focuses on just a few key binary labour market outcomes. I am restricted in the number of variables available for analysis in the high-frequency data due to the very short nature of the questionnaire. In that questionnaire, there are only two questions about the quality of work an individual has undertaken- one, whether that job is a *permanent* job, and secondly, a subjective response question about whether the respondent is satisfied with the job, or would be interested in taking other work if offered in the short run. On job search, the phone calls asked only if any steps had been taken to *search* for work, and if the *job boards* were visited. There are also questions about the hours worked, and the wages paid, for those in work. And for job searchers, questions about the number days they spent searching for work, and how many times they travelled to the city center. But in terms of looking at changes in labour market status outcomes, I am restricted to looking only at work, permanent work, job search, and visiting the job boards, and relevant intersections of these: discouragement (not search or working), and job search on the job.

I focus on these main binary outcomes[30] for both the high-frequency analysis and the detailed endline survey, using other measures from the endline to look at more detailed at job quality, including wages and hours, only once the main results are established. In this way, the high-frequency questionnaire provides a form of an informal pre-analysis plan, hopefully assuring the reader that the labour market outcomes chosen weren't selected from many, for the purposes of finding statistical significance.

In all regressions, standard errors are clustered at the **kebele** level, the smallest geographic unit of administration in Addis Ababa. Each individual is recorded as living in one of the Addis Ababa's kebele, apart from other individuals (clustered together) who commute in from small satellite towns just beyond the boundary of the city. All of these kebeles were located outside of a 4 km radius from the center of city, and my sample includes individuals

[29]Problems with implementation make measurement of take up problematic, which is why these results are not presented in the conjunction with the ITT estimates

[30]The subjective question about job types, quality and subjective perceptions of jobs is presented in section 5.2.1 (on job quality) and section 5.4 (which looks at the main mechanisms of the treatment effects)

from 70 different clusters.

4.1 Treatment Effects at follow up survey

The primary aim of the paper is to evaluate the impact of the transport intervention. The main results presented here compare respondents who received the transport program to a control group composed of everyone who did not get the transport, including those that did, and did not, get the phone calls. I return later to the phone calls, to test whether they had any impact of labour outcomes, which would diminish the validity of the control group, defined in this way.[31]

Basic POST Specifications: In the specifications below I estimate the difference (λ) in treatment and control groups at the single follow up paper survey for a set of important job market outcomes y. I estimate a basic difference in means (BAS), and a version with a vector of control variables at baseline X_{i0} (COV). Here T_i is a dummy variable indicating that individual i was in the group assigned to recieve the transport subsidies.

$$y_i = \alpha + T_i\lambda + \epsilon_i \tag{BAS}$$
$$y_i = \alpha + T_i\lambda + X_{i0}\beta + \epsilon_i \tag{COV}$$

In the results presented in the next section, I also implement a logistic regression specification (LOG) and estimate a specificition which includes the blocking variable dummy variables used in the randomization (BLC) as additional robustness checks.

Heterogenous Treatment effects: I estimate the same specifications on different subgroups in the sample separately. The key source of heterogeneity in the sample is by how individuals were originally sampled: the *boards* and *city* samples. The *boards* sample includes those more likely to be motivated. By interacting dummy variables for being in either the B_i (boards) or C_i (city) samples, we can estimates group specific treatment effects λ_1 and λ_2, respectively. Following that I specify a general form, which could be used to test for heterogeneity across any number of subgroups s. α_s is the group specific intercept, S_{si} the dummy variable for being in sample category s, and λ_s the group specific treatment effect[32]. This more general form will be used to denote all heterogenous treatment effect estimators from here onward.

$$y_i = \alpha_1 B_i + \alpha_2 C_i + T_i B_i \lambda_1 + T_i C_i \lambda_2 + X_{i0}\beta + \epsilon_i$$

[31] Using the larger pooled control group (which increases power) requires the assumption that individuals surveyed at the baseline who had received the phone calls to be equally valid controls as the group who were not contacted all in the intervening 16 weeks. In a later section I will return to this assumption and show it to be entirely justifiable, showing no significant difference between these two control groups.

[32] These co-efficients measure the size of the treatment effect for each categoery separately. Simple t-test can be used to test the difference in the size of the coefficients

general case:

$$y_{is} = \alpha_s + \sum_s T_i S_{si} \lambda_s + X_{i0}\beta + \epsilon_i \tag{HET}$$

Difference in Difference (DD): While randomization appears to have been conducted and implemented correctly and the sample is balanced across treatment and control groups, a difference in difference (DD) specification controls for difference in baseline outcomes. We use these specifications as a robustness check, and to improve estimation for measures with small imbalances at baseline.[33] In this specifiction, H_{it} is a dummy variable indicating that the treatment is switched on for individual i at time t.[34] As before, a second specification provides estimates for heterogenous treatment effects across groups s, but requires the inclusion of a full set of time-group interaction intercepts α_{st}.[35] Now the relevant treatment effect is estimated by λ.

$$\begin{aligned} y_{it} &= \alpha_t + T_i\gamma + H_{it}\lambda + X_{i0}\beta + \epsilon_{it} \\ y_{ist} &= \alpha_{st} + \sum_s T_i S_{si}\gamma_s + \sum_s H_i S_{si}\lambda_s + X_{i0}\beta + \epsilon_{it} \end{aligned} \tag{DD}$$

Fixed effects, ANCOVA and difference specifications: As with the DD estimates, including fixed effects is less efficient in randomized control trials with weekly autocorrelated outcomes, but are estimated here as a robustness check. I use standard fixed effects estimate (FE) with individual intercepts μ_i, but also a second specification which allows for the individual characteristics to influence change over time, by regressing individual change in the dependent variable ($\Delta y_{i16} = y_{i16} - y_{i0}$) between the baseline in week 16 and endline on treatment and other baseline characteristics (FD).

$$y_{it} = \alpha_t + T_{it}\lambda + \mu_i + \epsilon_{it} \tag{FE}$$

$$y_{i16} - y_{i0} = \alpha + T_i\lambda + X_{i0}\beta + \epsilon_i(t = 16) \tag{FD}$$

A similar estimator to the difference in difference estimator is ANCOVA estimator, which regresses the outcome at endline, or at any other (set of) points, on the baseline dependent variable. The ANCOVA estimator is more efficient that either difference in difference estimator or the standard POST estimator which ignores baseline outcomes (Frison and Pocock, 1992).

[33] McKenzie (2012) shows that when autocorrelation in outcomes are high (greater than 0.5 in the two period case), this DD specification provides greater power to detect treatment errors. For most of our treatment variables autocorrelations are lower, or just lower than 0.5. Indeed, our esimation results are less efficient using the DD specification (standard errors are larger).

[34] Treatment does not switch off in this specification, once someone has been receiving the subsidy $T\_on_{it} = 1$, even after they stop getting the treatment. In a later section I compare these estimates to a dummy that does switch off, to check for persistent treatment effects

[35] That is, $\alpha_{st} = \sum_s \sum_t week_{it} S_{si}$

$$y_{i16} = \alpha + y_{i0}\rho + T_i\lambda + X_{i0}\beta + \epsilon_i \tag{ANC}$$

4.2 Treatment effects on multiple follow survey rounds

Pooled treatment effects

In addition to estimating the treatment effects at follow up, I estimate treatment effects for interceding weeks, to look at how behaviour responded over the course of the study, both immediately after it was begun, and the trajectory of the impacts over time. This allows me to look into the mechanisms by which I find impacts on labour market outcomes at the endline survey. In addition, pooling data on the same individuals over multiple time periods provides additional power to detect impacts in my relatively small sample. Averaging outcomes over multiple weeks, especially for volatile outcomes experienced for job-seekers, improves the efficiency of these impacts (Bruhn and McKenzie, 2009). Initially, I estimate the average treatment effect (POOL) over all weeks for all treated individuals *for all the weeks after treatment began*. In the case of binary outcomes, this is in the average increase in the probability of a treated individual in each post-treatment week that they observed. Heterogenous treatment effects can also be estimated this way, by including group specific time intercepts.

$$\begin{aligned} y_{it} &= \alpha_t + T_i\lambda + X_i\beta + \epsilon_{it} && \forall t \neq 0 \\ y_{ist} &= \alpha_{ts} + \sum_s T_i S_{si}\lambda_s + X_{i0}\beta + \epsilon_{it} && \forall t \neq 0 \end{aligned} \tag{POOL}$$

Trajectories: Using multiple observation and treatment rounds I estimate week-specific, and even week-group-specific treatment effects, which allow for the analysis of trajectories of the treatment effects over time. W_{it} is a dummy variable equal to one at time (week) t, such that λ_t estimates the difference between the treatment and control group at time t. I use only the treatment group dummy T_i such that λ_0 estimates the difference between treatment and control at baseline, before treatment begins; equivalent to a check for balance at basline.[36]

$$\begin{aligned} y_{it} &= \alpha_t + \sum_t T_i W_{it}\lambda_t + X_i\beta + \epsilon_{it} \\ y_{ist} &= \alpha_{st} + \sum_s \sum_t T_i W_{it} S_{is}\lambda_{st} + X_i\beta + \epsilon_{it} \end{aligned} \tag{WEEK}$$

The second equation, when applied to two heterogenous groups s and the full 14 times periods available, estimates 28 different treatment effects, which I present in two adjacent columns in regression tables. However, the small size of the sample in the phone call survey (400 individuals) gives me limited power to detect differences in binary outcomes for multiple groups. Furthermore, if the treatment takes some time to have an effect (as is strongly

[36]Note that in this specification he coefficient estimate λ_t is equivalent to a basic POST specification above esimated for the sample restricted to week t

suggested in the main results from this data), the POOL estimator, which averages across weeks, may fail to reject the null of no effects because of negligible differences in the early weeks of observation. I also estimate the trend in the treatment effect over time. Using standard non-parametric regressions of search or work probability over time, for both treatment and control, I compare the trajectories of the two groups. Equations below estimate the trajectory to the linear weekly treatment effects over time, using a linear, quadratic, possibly higher order functions of the time (week) variable.

$$
\begin{aligned}
y_{it} &= \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + X_i\beta + \epsilon_{it} \\
y_{it} &= \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + T_i w^2\lambda_2 + X_i\beta + \epsilon_{it} \qquad \text{(TRD)}
\end{aligned}
$$

By dividing the weeks naturally into months, I can test the average treatment weekly treatment effect across months, to see if the treatment and control groups are significantly different over a collection of weeks after treatment has ended. I continue to include all week, and week-sample, specific fixed effects, which means that month fixed effects are not needed.

$$
\begin{aligned}
y_{imt} &= \alpha_t + \sum_m T_i M_{mi}\lambda_m + X_{i0}\beta + \epsilon_{it} \\
y_{imt} &= \alpha_{st} + \sum_s \sum_m T_i M_{mi} S_{is}\lambda_{sm} + X_{i0}\beta + \epsilon_{it} \qquad \text{(MON)}
\end{aligned}
$$

Persistence of treatment effects Finally, all of the analysis above estimates treatment effects by counting someone as treated if they have received the treatment in the past even after he/she has stopped receiving the subsidies. For behavioural outcomes, I want to know if the estimated treatment effects are driven by changes in behaviour only when the treatment was actually being received, or if the effects persist after treatment has ended. To do this I estimate the following equation (PERS), focus the analysis on the time periods after week 7, at which point half of the sample stopped receiving the treatment, in order to compare those individuals to those are still collecting the subsidies:

$$
y_{it} = \alpha_t + T_i\lambda + P_{it}\delta + X_{i0}\beta + \epsilon_{it} \qquad \forall t \geq 8 \qquad \text{(PERS)}
$$

Here, P_{it} is a dummy variable indicating that the treatment was actually being delivered to individual i at time t, while T_i continues to denote just that individual i was treated during the study. The estimate of the coefficient δ represents the *additional* impact of having receiving the money at a given time, over and above the effect of being in the treatment group at all (possibly without receiving the subsidies at time t). If the impacts are treatment are completely transient, and dissipate immediate after treatment ends, δ should be similar to coefficients on T in similar equations, while the coefficient on T, λ should be indistinguishable from zero. However, if the impacts are wholey persistent (they do not dissipate at all after treatment ends) the reverse should hold; δ will indistinguishable from zero.

4.3 Labour Market Status categories

Much of the analysis in this paper looks at the probability of an individual having a binary certain labour market outcome, such as having a permament job, or having searched for work in the last 7 days. A finding that one treatment group is, for instance, more likely to be searching for a job, would be far less interesting if I was unable to say what the control group are doing instead. Finding that they were more likely to be working and thus not searching is very different to finding that they'd become discouraged and were doing nothing.

In order to fully account for the changing composition of labour market outcomes at the follow up survey I categories in I estimate ordered logistic models, where the probability that individual i has labour market outcome j is given by

$$P(j|X_{0i}, T_i) = P(\eta_j \leq y_i^\star \leq \eta_{j+1}) \tag{OLOG}$$

where

$$y_i^\star = T_i\lambda + X_{0i}\beta + \epsilon_{it}$$

using maximum likelihood techniques. I define the ordinal categorization of different labour market outcomes in the appropriate section 5.1.

5 Main Results

I estimate the impacts of the transport subsidies on the labour market outcomes of job seekers, both at the endline survey, and over time in the phone call surveys. For all estimators I estimate treatment effects for the two samples separately, and for the two samples pooled together. I also investigate heterogeneity of treatment effects by different individual characteristics.

I start in section 5.1 by giving an overview of the *distribution* of treated and control respondents across different labour market categories, by treatment and control individuals. The treatment seems to have shifted the distribution, at each margin, the direction of better jobs among those with jobs, more search activity (among those with temporary jobs and those without jobs), and consequently away from discouragement, or dropping out of the labour force entirely. I then turn to looking at each of these job market categories in turn, first focusing on the outcomes of those in the full sample followed up at endline paper survey, in section 5.2. Section 5.3 looks at the smaller sample from the phone call survey to estimate the effect of treatment on search intensity and labour supply while the subsidies were still being given out, and to assess the *trajectory* of the impacts over time.

For the most part I restrict the analysis to the binary labour market outcomes that were asked about during the phone call survey, to prevent concerns data mining and the problems associated with it. Section 5.2 briefly investigates some additional job quality determinants from the endline survey, but these are presented separately from the other results. Section 5.4 starts to investigate some of the other more detailed questions to look into some of the mechanisms through which the treatment might be having an effect, by looking at the impact

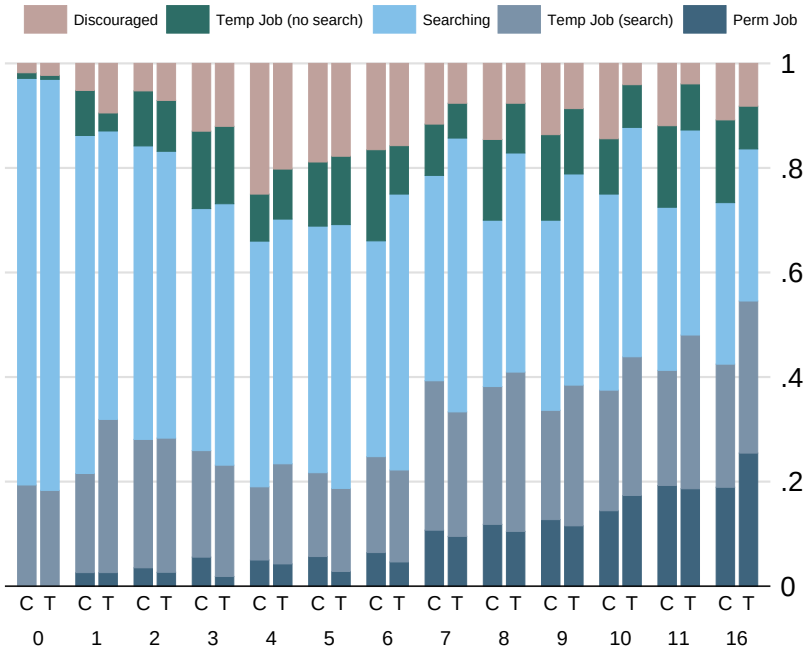
of treatment on attitudes aspirations and financial outcomes.

All regression results have standard errors clustered at the kebele level, which is the smallest relevant administrative unit in the city of Addis Ababa. Since respondents in the sample live across 70 different kebeles, I do not have problems associated with too few cluters (**?**).

5.1 Overview: Composition Effects

This section presents graphically changes in labour market status categories graphically to give an overview of how the composition of the control and treatment samples changed over time. I do this separately for the *board* (5.3) and city (5.3) samples. For each week (1-16), stacked bar graphs show the percentage of both the control (C)and treatment(T) group separately, classified into one of five *distinct* categories, starting from the top of each bar: 1- Discouraged (not working or searching); 2- Temporary work (not searching) 3- Searching (but no work) 4 - Temporary work (but also searching)[37], 5 - Permanent work.

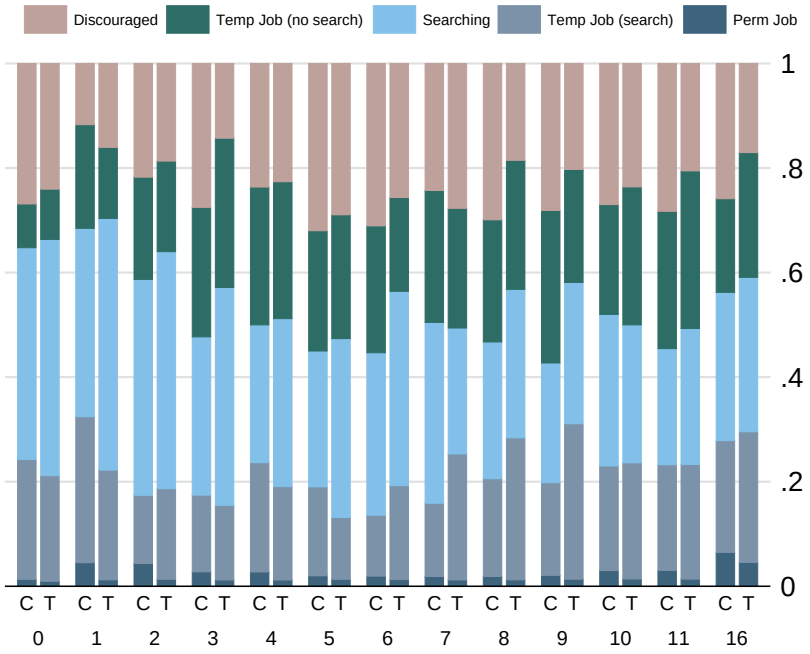
Figure 1: Composition of the sample for each week by treatment and control: *Board Sample*



Some basic trends in the composition of the the sample are evident. Among the boards sample almost everyone was searching for work at the baseline, even among this with tem-

[37]The distinction between Searching or not searching among temporary workers is important, as on the job search is extremely important, especially for individuals how do not consider their work to satisfactory or long term. If much temporary work is used as a means to short run subsistence, and perhaps to make money to search for other work, it is as interesting to look at job search in this group as those without work.

Figure 2: Composition of the sample for each week by treatment and control: ***City Sample***



porary work. However, as the study goes on, more of the unemployed are likely to become discouraged (stop searching for work), while more of those with work are likely to give up looking for better work. These trends hold for both samples, and rate at which people give up job search seems similar among those with to those without jobs. Discouragement sets in as some individuals give up searching, but then falls as more individuals find more employment.

I discuss the trends in the main variables over time in more detail in section 5.3. For now it seems evident that the transport treatment, at each margin, pushes respondents away from discouragement, both into work, more permanent work, and into increased job search intensity (regardless of employment status). This is true for most of the later periods of the study, after which the treatment had been running for a while and had time to take effect. A key focus of these results is to look at how long it took for the treatments to take effect, and how they took effect sooner among the *city* sample, than the *boards*.

One notable exception is the *board* sample, who seem to be less likely to be working for the middle weeks of the study. This is not clearly visible in the figures above, but is made clear in Figure B.3 in the Appendix, where all the employment categories are stacked together so that the top of the blue bar represents the employment rate. As I discuss in 5.3, this is consistent with a theory of respondents substituting low quality temporary work in favour of more intensive job search that they are actually interested in. In addition, as also discussed in greater detail shortly, treated individuals are less likely to be engaged in temporary work, while also *not* searching for a job at the same time. If taking temporary work in a profession that is not one's own, and then giving up trying to find a better job, can be considered as a

Table 3: Ordered Logistic Regression: Effect of treatment on labour market status

	(1) All Weeks	(2) After Week 7	(3) Week 16 Only
Panel A: Effects across samples			
Effect for *boards*	0.20	0.42**	0.53***
	(0.14)	(0.18)	(0.19)
Effect for *city*	0.21	0.32*	0.30*
	(0.17)	(0.17)	(0.16)
Panel B: Effects in pooled Sample			
Pooled Effect	0.20*	0.37***	0.43***
	(0.11)	(0.12)	(0.13)
Obs (both panels)	5,011	2,202	658

[1] Dep Var is a categorical variable: 1- Discouraged; 2- Temp work (no searching) 3- Searching (no work) 4 - Temp work (and searching); 5 - Permanent work

[2] Log-odds coefficients are reported

[3] All regressions include a full set of control variables.

[4] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

form of discouragement, the treatment seems to have also prevented discouragement in this way.

Among the *boards* sample, the number of people in permanent work gradually grows over the weeks. The total percentage of people working is given by the top of the blue *Temp Work (No search) bar*, and shows relatively little difference between treatment and control in the *boards* sample, aside from the weeks 5-7, as already discussed. Importantly, for the *board* sample, week 16 shows an increased probability of having a permanent job, a difference that I investigate in more detail in the next section.

I attempt to assess the impact of the transport treatment on the *distribution* of individuals across employment categories. I assume an ordinal ranking of job market outcomes equivalent to the one outlined for the tables above, where workers transition away from discouragement towards job search, employment and eventually permanent work. I estimate an ordered logistic model, specified by equation OLOG for the two samples separately, the pooled samples, and for both the final week of the study, and all of the later weeks combined. The results clearly a show statistically significant impact of the treatment on ordered categorical variable, in the positive direction: of more job search, and better jobs, for both samples. The effect seems bigger, however, for the *boards* respondents.

5.2 Endline Outcomes

I test first for an impact of treatment on having a permanent job. I estimate the following series of regressions (all specified in section , with the appropriate equation labels used in that section as column headers in the tables): The basic POST estimator both with (BASE) and without controls (COV), blocking variables (BLK), and an ANCOVA estimator with lagged dependent variable (ANC) and in column (6), (FD) gives an impact of the treatment on a first difference of the dependent variables between the first and last weeks, including baseline controls. I also include a logistic regression (LOG) as a robustness check in column (2),

reporting the marginal effects at the median.[38] In each Table I estimate the treatment effects in different sub-samples in separate panels, according the specification given by equation HET. For each set of regressions I also present the (unconditional) mean of the dependent variable for the relevant control group(s), to give an indication of the scale of the impacts.

Table 4: Effects of transport subsidies on having Permanent Employment at endline

	(1) BAS	(2) LOG	(3) COV	(4) ANC	(5) BLK	(6) FD
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)						
					Control mean : 0.130	
TE Pooled	0.028	0.027	0.042	0.043*	0.032	0.045*
	(0.027)	(0.024)	(0.026)	(0.025)	(0.026)	(0.025)
Panel B: Treatment Effects At Follow Up by Sample						
					Control mean (board) : 0.190	
					Control mean (city) : 0.065	
TE board	0.068*	0.046*	0.080**	0.080**	0.073*	0.080**
	(0.038)	(0.028)	(0.037)	(0.037)	(0.040)	(0.037)
TE city	-0.019	-0.036	-0.005	-0.003	-0.020	-0.000
	(0.032)	(0.054)	(0.033)	(0.031)	(0.026)	(0.031)
Panel C: Treatment Effects At Follow Up by Degree Status						
					Control mean (deg) : 0.220	
					Control mean (nodeg) : 0.100	
TE deg	0.15**	0.098*	0.17**	0.17**	0.15*	0.15*
	(0.074)	(0.056)	(0.075)	(0.075)	(0.076)	(0.076)
TE nodeg	-0.011	-0.012	-0.002	-0.001	-0.004	-0.004
	(0.028)	(0.034)	(0.028)	(0.028)	(0.026)	(0.026)
Observations	652	652	652	652	652	652

[1] Dep Var is a dummy variable equal to one if the individual reported having worked at a permanent job in the last 7 days, measured at endline (week 16). Results are from OLS regressions on endline outcomes, details of the specifications titled are in the REF

[2] Panel A gives average ITT effect for everyone. Panels B and CS estimate effects for different groups. (B): The two different samples- "board" and "city" (C): Those with and those without a degree

[3] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

The central finding is that the sample from the boards (the active job seekers) and individuals who have university degrees are more likely to have found permanent jobs, but there are no effects for those from the *city*, who have relatively low chances of finding employment with or without the transport subsidies. Furthermore, the effect seems to be driven by those with university degrees. I find that *boards* respondents who received the treatment are about $6.8 - 8$ ppt more likely to find permanent employment. Amon those with degrees the effect is even bigger (between 15 and 18 ppt); in fact the results are driven entirely by those with degrees.

These results are robust and consistent across all specifications, including the logistics regression, and the first difference regressions, which removes any possible differences in outcomes at the baseline. There is no notable heterogeneity by gender (the results have not been presented here).

I find an insignificant average treatment effect on permanent work for the *pooled* sample in Panel (A). Here, the treatment effect of those in the *boards* is diluted by the effect on the

[38] I estimate a basic diff-in-diff speciation in addition, which yields similar point estimates, but larger standard errors due to the relatively low auto-correlation in many of the volatile employment outcomes

city sample, which is close to zero and not at all significant. This is most likely because the probability of such responds finding permanent work is very low (around 6%), and it is hard to help these respondents to find employment.

If individuals are more likely to have good, permanent jobs, is this because they have found more work on average? Or have they found these permanent jobs instead of taking temporary jobs? In Table 5 the impact on having done any work in the last seven days is only just statistically insignificant in the *pooled* sample, but reasonably large in magnitude at around 6 percentage points, across all six specifications. I find that these results are driven mostly by those in the *city* sample who have found jobs; there is no significant impact on the *boards* sample.

Table 5: Effects of transport subsidies on having employment at endline

	(1) BAS	(2) LOG	(3) COV	(4) ANC	(5) BLK	(6) FD
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)						
						Control mean : 0.530
TE Pooled	0.058*	0.059*	0.062*	0.065*	0.057*	0.081*
	(0.034)	(0.035)	(0.035)	(0.034)	(0.034)	(0.043)
Panel B: Treatment Effects At Follow Up by Sample						
						Control mean (board) : 0.580
						Control mean (city) : 0.460
TE board	0.044	0.046	0.043	0.046	0.049	0.067
	(0.051)	(0.052)	(0.051)	(0.051)	(0.051)	(0.062)
TE city	0.076	0.075*	0.087*	0.089**	0.068	0.100*
	(0.046)	(0.044)	(0.044)	(0.041)	(0.041)	(0.058)
Panel C: Treatment Effects At Follow Up by Degree Status						
						Control mean (deg) : 0.480
						Control mean (nodeg) : 0.540
TE deg	0.23***	0.22***	0.26***	0.26***	0.23***	0.23***
	(0.073)	(0.066)	(0.075)	(0.075)	(0.074)	(0.074)
TE nodeg	0.000	0.001	-0.004	-0.001	-0.002	-0.002
	(0.041)	(0.043)	(0.041)	(0.039)	(0.040)	(0.040)
Observations	652	652	652	652	652	652

[1] Dep Var is a dummy variable equal to one if the individual reported having done work in the last 7 days, measured at endline (week 16). Results are from OLS regressions on endline outcomes, details of the specifications titled are in the REF

[2] Panel A gives average ITT effect for everyone. Panels B and CS estimate effects for different groups. (B): The two different samples- "board" and "city" (C): Those with and those without a degree

[3] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

In all, it seems that the *city* sample respondents have been aided in finding employment faster by the treatment. The *boards* respondents have on average many to find more permanent employment that has somewhat displaced other, temporary employment. They are not more likely to have found work on average. Those with university degrees seem to have been helped the most, being significantly more likely to have found jobs, most of which seem to have been permanent jobs.

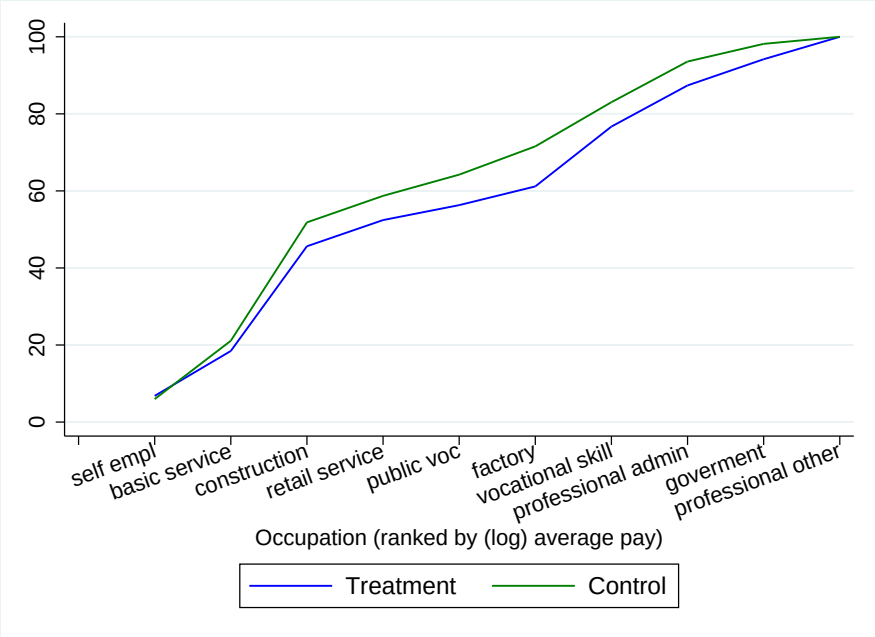
I relegate results showing the impact of treatment on search activities at endline to the appendix, and for discussion in section 5.3.2 on the persistence of the effects of subsidies on search intensity.

5.2.1 Impacts on Types of Work and Job Quality

I now turn to look at whether the different jobs found by these two groups differ in their quality and type.

In Figure 3 below, I classify the jobs of all respondents working at follow up into similar occupational groups, rank those groups by average weekly salary earned at follow up, and plot the cumulative distribution among these occupations by treatment and control groups. The results clearly show a positive shift in the quality of jobs among the treated group.

Figure 3: Impact of treatment on distribution of occupations



I find that on average the wage of jobs found by treated job seekers look no different to those in the control group. I am unable to reject the Kolmogorov-Smirnov test of equality in distribution of wages (in levels or logs) between treatment and control groups. This is perhaps not surprising, given the results presented in section A on the nature of work and permanent work, which indicated that permanent jobs were not desirable because of a pay differential, but because of the security and nature of the work.[39]

However, other job quality outcomes were significantly impacted by the transport subsidies, as shown in Table 6. I look at a series of dummy variables indicating that a respondent has a job with a certain quality, all of which are in some ways proxies for the permanence, formality and desirability of work. The key variables are described in the notes to Table 6. For instance, they are 14 ppts more likely to be working in office, as opposed to some kind of other work site, and 4.3 ppts more likely to have found the job through formal means (application and proper interview).

[39]Certainly, during the course of fieldwork, I met and spoke to many unemployed men who were engaged in sometimes hazardous or stressful casual labourer, but often at considerably higher salaries than were available in more formal work.

Table 6: Effects of treatment on Job Quality and Type at Endline (BAS)

	(1) work	(2) casual	(3) ln wage	(4) hours	(5) degree	(6) in office	(7) pay monthly	(8) satisfied	(9) formally
Panel A: Impacts on work outcomes at week 16									
TE Pooled	0.062*	-0.022	0.062	3.74**	0.047**	0.070*	0.069*	0.061**	0.054*
	(0.035)	(0.024)	(0.085)	(1.71)	(0.018)	(0.037)	(0.037)	(0.028)	(0.029)
Heterogeneity by Sample									
TE board	0.043	0.0026	0.094	2.53	0.075**	0.020	0.032	0.015	0.064
	(0.051)	(0.025)	(0.10)	(2.34)	(0.033)	(0.052)	(0.053)	(0.045)	(0.049)
TE city	0.087*	-0.050	0.013	5.27**	0.014	0.13**	0.11**	0.11***	0.042*
	(0.044)	(0.042)	(0.15)	(2.34)	(0.011)	(0.050)	(0.049)	(0.029)	(0.023)
Obs	658	596	355	656	596	596	596	596	596

[1] Results are from Difference OLS regressions on endline outcomes, details of the specifications can be found in the section on heterogeneity. Column (1) presents average ITT effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] Unusual Dependent variables: (5) *Degree*: Respondent has a job that required a degree as minimum qualification (6) *In Office:* Job is performed in an office, or formal business house- proxy for "white collar" work (7) *Pay Monthly:* Respondent is paid every month, usually according to set a contract (9) *Formally:* The job was acquired through an official application and interview process (this excludes referral from a friend or family, or jobs given after just a conversation with the employer

[3] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

The results indicate that the *city* respondents are more likely to find jobs of better quality, indicating that they are not simply taking work faster and accepting inferior work. If I restrict the sample to just individuals who had work, and run the same regression, I confirm that, conditional on having a job, *city* respondents are more likely to have better jobs. This suggests that the treatment helped respondents to find better jobs.

Boards respondents, who are already likely to have jobs in office, or be paid by the month, do not see significant treatment effects on these variables. However, they are more likely to be have found jobs that require at least a degree as a qualification. Given that most respondents prioritized (during focus group discussions) finding employment in the occupation for which they studied or trained, this seems like a positive outcome.

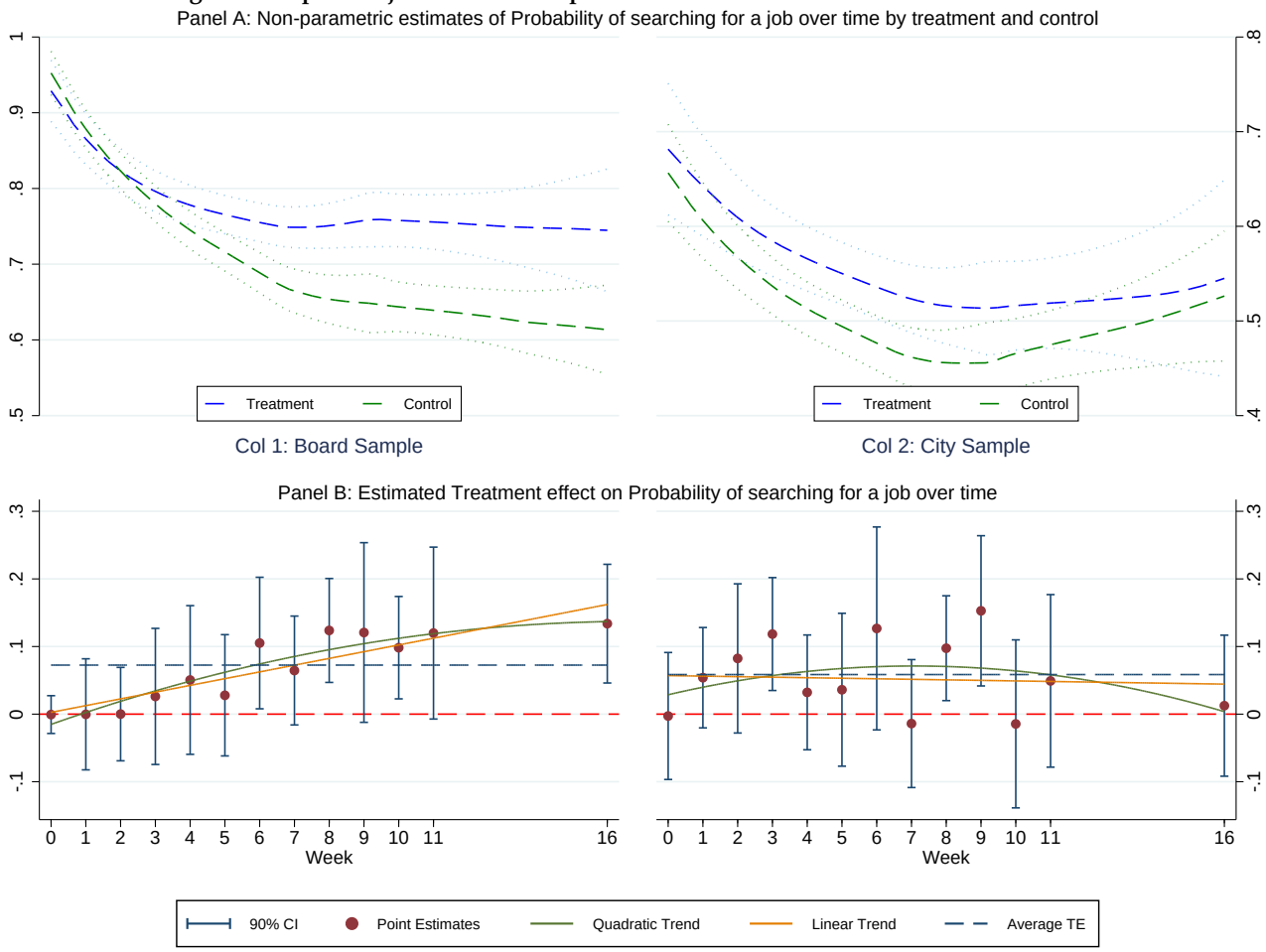
5.3 Impact Trajectories

How did the treatment impact the job market activity of recipients during the weeks that they were receiving it, and how did these impacts change over the course of the study? Did search intensity change change over time, and how and when did treated individuals diverge from the control group over time?

I begin by presenting estimates of the treatment effect on the propensity to search for work over time, looking at the 12 post-baseline surveys, 11 phone call surveys (denoted as week 1-11), and the final paper survey (week 16). As with each key job market outcome variable, I estimate the average impact on the probability of searching for a job across all 12 weeks combined. Using equation WEEK outlined in the specifications section, I then estimate the treatment effect in each week separately. I estimate the trend over a time, estimating an intercept term, linear, quadratic and cubic trend terms, as in equation TRD.[40]. In what

[40]Cubic function estimates are largely not presented here, since they added little explanatory power to the trajectory estimates

Figure 4: Impact on job search: Non-parametric trends and treatment effects over time



follows, I define "treatment" as having received the transport subsidies as any point in the past, the treatment switches on in week 1, and does not "switch off"[41]

Figure 4 summarizes all of these results for both the *board* sample (Column 1) and the *city* sample (Column 2). In Panel A, non-parametric estimates of the probability of searching for employment as a function of time, are presented, showing how search behaviour declined over time, as individuals either found employment or became discouraged and stopped searching for work. However, for both samples, the treated group clearly shows a different. Table 7 estimates (parametrically) these treatment effects over time, presenting both the control means (CM) of the dependent variable over all the weeks, and the linearly estimated treatment different between treatment and control from equation WEEK. This shows how the proportion of individuals searching for a job declined over time, but by considerably less for the treatment group, who were as much as 10% more likely to be searching in particularly weeks during the study. I show results for the two samples pooled together, and

[41] In later analysis, I exploit variation in when the subsidy treatment was ended for different individuals, and the fact that the treatments ended by at least week 11 for everyone (5 weeks before the follow up paper survey) to estimate the persistence of the treatment effects

Table 7: Effects of treatment on Job Search in each week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.820	0.004	0.970	0.640	-0.000	0.009
		(0.028)			(0.017)	(0.057)
week 1	0.750	0.024	0.840	0.660	-0.000	0.054
		(0.033)			(0.050)	(0.045)
week 2	0.700	0.039	0.820	0.550	0.000	0.083
		(0.037)			(0.042)	(0.067)
week 3	0.570	0.073*	0.690	0.450	0.026	0.12**
		(0.041)			(0.061)	(0.051)
week 4	0.550	0.043	0.630	0.470	0.051	0.032
		(0.042)			(0.067)	(0.052)
week 5	0.540	0.034	0.650	0.430	0.028	0.036
		(0.043)			(0.055)	(0.069)
week 6	0.520	0.12**	0.610	0.430	0.11*	0.130
		(0.053)			(0.059)	(0.091)
week 7	0.620	0.033	0.740	0.500	0.065	-0.014
		(0.039)			(0.049)	(0.058)
week 8	0.560	0.11***	0.650	0.460	0.12***	0.098**
		(0.033)			(0.047)	(0.047)
week 9	0.520	0.14**	0.610	0.420	0.120	0.15**
		(0.055)			(0.081)	(0.067)
week 10	0.590	0.051	0.670	0.500	0.098**	-0.015
		(0.043)			(0.046)	(0.075)
week 11	0.530	0.092	0.620	0.430	0.120	0.049
		(0.055)			(0.077)	(0.078)
week 16	0.580	0.079*	0.610	0.530	0.13**	0.012
		(0.041)			(0.053)	(0.063)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is a dummy variable equal to one if the individual reported having take some step to look for work in the last 7 weeks. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

for each separately.[42]

These weekly point estimates of the impact of the treatment in each week are plotted in the Panel B of the 4, showing, for the board sample separately, a clear and persistent upward trend in the treatment effect over time. For the *board* sample, these effects seem to increasing linearly with time, whereas for the *city* sample (in Column 2), these effects seem to have an effect more immediately, at the beginning of the study period, and then remain at similarly high levels, with a decline towards the end (it is negligible in week 16) [43]. Panel B also overlays the linear and quadratic estimates of the trend in the treatment effect over time, confirming a mostly upward linear, and constant (flat), trajectory for the *board* and *city* sample, respectively. The quadratic term for the *city* sample is negative due, but is not statistically significant.

The estimates of the coefficients on these trajectory parameters are presented in Table , and show for the *board* sample a statistically insignificant quadratic term, but a highly significant upward linear term, whereas for the *city* sample, I identify a (significantly) negative quadratic term. I also plot the average treatment effect across all 15 weekly observations, which is estimated and presented in the first row of Table , and is estimated using the POOL

[42] Power is low for weekly-sample specific treatment effect estimates, so the pooled estimates more often statistically significant, but hide some heterogeneity between the two groups

[43] I show, shortly, that this decline in search activity may be driven by these individuals finding better work

specification with the usual set of covariates [44].

Table 8: Trends in the of treatment on Job Search over all weeks

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	0.066***	0.029	0.007	0.073***	0.003	-0.015	0.059*	0.061**	0.035
	(0.021)	(0.021)	(0.025)	(0.027)	(0.028)	(0.026)	(0.034)	(0.030)	(0.044)
Treat X Time		0.0050*	0.015		0.0100***	0.018**		-0.001	0.010
		(0.0026)	(0.0089)		(0.0037)	(0.0086)		(0.0035)	(0.017)
Treat X TimeSq			-0.001			-0.001			-0.001
			(0.00054)			(0.00055)			(0.00099)
CM	0.590			0.680			0.490		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.652	0.686	0.686	0.652	0.686	0.686	0.652	0.686	0.686

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms

[2] Dependent Variable is a dummy variable equal to one if the individual reported having take some step to look for work in the last 7 weeks.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

The results suggest unambiguously that individuals in both samples were more likely to search for jobs while, and after, receiving the transport subsidies, but the trajectory of these impacts differ slightly between the samples. For the boards sample, who were initially more likely to be searching for employment, the impacts took some time to kick in, doing so only as more and more individuals become discouraged. For the *city* respondents, the effect seems to have been more immediate, but less consistent or persistent during the following weeks.

Indeed, it does look as if some individuals in the *city* sample were induced to begin job search, when they were not engaged in search at the time of the first interview. For the boards individuals, who were all searching for jobs to begin with, the treatment prevented the onset of discouragement, or encouraged the resumption of search activity after short periods of discouragement.

In the appendix similar tables and figures estimate similar impact trajectories for other search outcome variables from the phone call surveys. I find that the treatment had similar impacts on the probability of respondents searching for employment at the vacancy boards (see Figure B.4, Table C.1, Table C.2), with the difference between the two samples even more marked: among the boards sample the treatment effect is significant at the 10% for almost all individuals week after week, despite the small sample. The effect is not significant for the *city* sample, perhaps the boards were never likely to be their preferred method of search[45].

Search at the intensive or extensive margin: While selection effects make it hard to estimate whether job seekers searched more *intensively* after getting the transport subsidies (since the treatment induced respondents to search more, evidence suggests that it did not. The results presented in the appendix as tables C.3 and C.4 show a positive impact of treatment on the average number of days spent searching for work (and days visiting the boards in Table B.6), but are not so large that they could not be accounted for by the increase in the proportion of individuals searching for work. Indeed, estimates not presented here, show that there was

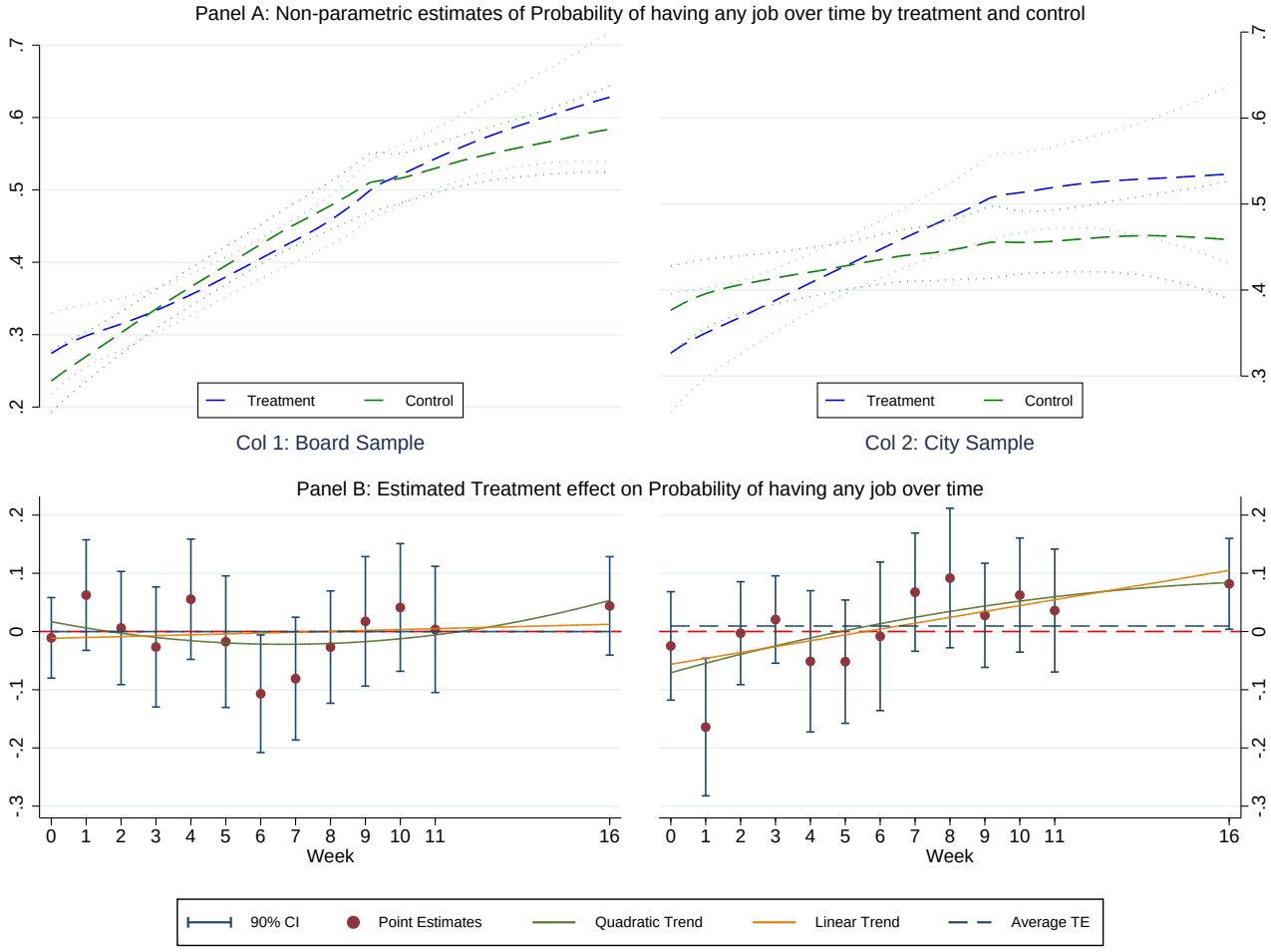
[44] The results are similar for the ANCOVA, DD and FD estimators as well

[45] Although there are individual weeks, early in the study, where the effect is significant, suggesting that the intervention "nudged" or at least encouraged respondents to try to check the boards, possibly with little tangible reward

no significant impact in the number of days searched, per individual that was searching[46].

Effects on employment over all weeks: Of course, the impacts of treatment on search intensity would be interpreted differently depending on whether they were driven by lower levels of discouragement (if the control group were not working or searching), or if they were driven by treated individuals putting off taking a job, such that the control group were more likely to be working, and thus likely to be searching. More importantly I am interested in whether the transport treatment allowed job seekers to find employment faster, or if they were able to avoid taking unwanted and demanding casual labour to support due the treatment.

Figure 5: Impact on having a job: Non-parametric trends and treatment effects over time



I find that for most weeks, the employment rates of the treated group are statistically indistinguishable from the control group for the boards group. These results are plotted in Figure 5 and presenting in tables in the appendix. However, there is evidence of a small dip in the middle of the study period, around the time when many individuals stopped searching

[46] Although, again, this could be because the individuals who were motivated to begin searching for work where ones that were not naturally inclined to search, and thus searched less when they were searching

for work, when the treatment group were less likely to be working. This was a time when most workers still only had casual or temporary work, and while only one of the coefficients is significant, there seems to be a clear and then rise again in the employment rate, hinting that treated individuals were able to avoid taking a temporary work, which might otherwise forced them to give up job search.

For the *city* sample, there also seems to be evidence of an initial fall in employment rates, with a strong upward linear trend, although the treatment group is only more likely to be working in the final week (16).

I find that the treatment does indeed prevent discouragement (see Table 9, and Figure B.7 and Table 9 in the appendix). For the *city* respondents these large and significant results seem driven both by an increased rate of employment, and higher rates of search among the unemployed, whereas for the *boards* the result is driven mostly by respondents.

Table 9: Effects of treatment on Discouragement in each week

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	-0.040**	-0.008	0.007	-0.030	0.005	0.029*	-0.051	-0.024	-0.021
	(0.019)	(0.021)	(0.017)	(0.024)	(0.024)	(0.017)	(0.032)	(0.037)	(0.030)
Treat X Time		-0.0047**	-0.011*		-0.0051**	-0.016*		-0.004	-0.005
		(0.0018)	(0.0067)		(0.0022)	(0.0082)		(0.0031)	(0.011)
Treat X TimeSq			0.000			0.001			0.000
			(0.00045)			(0.00054)			(0.00074)
CM	0.190			0.130			0.260		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.237	0.241	0.241	0.237	0.241	0.241	0.237	0.241	0.241

[1] Dependent Variable is a dummy variable equal to one if the individual reported having take some step to look for work in the last 7 weeks. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

The results in this section confirm the picture presented in figures and at the beginning of the results section showing the impact of treatment on the distribution of employment status. Treatment caused a gradual shift into better quality jobs (as presented in the previous section on endline outcomes), shifted the unemployed towards for jobs, as well as the employed who still wanted to find better employment, and lead to higher employment rates among the *city* respondents (in jobs that were of higher quality).

5.3.1 Impacts on search activities by Months

The tables and plots of weekly treatment effects presented in this section provide an insight into the trajectories that treated and untreated job seekers take. However, in small samples (often only 300-400 individuals were contacted by phone each week), large coefficients are often not statistically significant and subject to some random error which conceals evidence of an upward trend in the treatment effects. Yet the presentence of these effects over a number of weeks suggest that they are unlikely to be purely due to random error. To confirm this, I pool observations into sets of consecutive four weeks, creating 3 successive *months*, to show clearly the increasing size and significance of the coefficients over time, for some of the outcome measures. Using these monthly treatment effects allows me to confirm the

trajectories of the treatment effects, with considerably more power.

These results are presented, just focusing on the core labour market outcomes, in Table 10. The Results emphasis the trajectories illustrated in the figures above, with the treatment effects on search activity taking some time to take effect, and growing over time for the *boards* sample, whereas the impacts are seen as early as the first month for the *city* sample, but seem to have diminished by the third month. However, for the final month, both samples are significantly less likely to be discouraged. For the *boards* this is driven largely by increased search activity among the unemployed, for the *city* sample is a combination of increased search, and increased employment rates. The pooled results show many more significant results, simply due to the added power for pooling the samples together.

To allay fears that these months were chosen strategically to boost significance, I present complementary results were I *restrict* the sample weeks to groups of four weeks, starting with the first four weeks of the study, and iteratively move this window forward by one month and re-estimate the treatment effects using the basic COV estimator used before estimator. This provides a type of moving average monthly treatment effect, and shows the trajectory of treatment effects. These estimates are shown for the pooled sample in Table C.13, but the sample specific estimates are presented in the appendix. These results confirm that the treatment starts to work slowly, and is strong and significant for all the later groups of weeks. The coefficients are very similar to those presented for the corresponding monthly groupings of weeks, although not identical because of the inclusion of covariates which are estimated slightly differently in the restricted samples.

5.3.2 Persistence of Search Impacts

Thus far the empirical results suggest that the transport treatment induced young individuals to search more intensively for employment during the weeks when the treatment was being rolled out, and that these results translate into increased probability of finding permanent employment at the end the endline survey.[47] Because treatment durations differed from individual to individual randomly (some ended treatment in week 8, others in week 11), this endline survey was between 5 and 8 weeks after the treatment ended for treated individuals. One might expect any impact on employment to be persistent some time after the treatment as ended (someone getting a job to keep it for at least a few weeks, while the control may still not have found treatment), but for other behavioural outcomes such as job search effort it is not clear whether these impacts would persist after the treatment ended. If reducing transport costs simply increases the marginal benefit of search relative to other uses of time, such as leisure, then one would expect the impacts to end of the subsidies end. However, if the impacts are persistent, a different theory of change would be needed. In this section, I show that these impacts on search behaviour are persistent, at least until the end of the study.

Tables presented in previous sections, particularly on endline (week 16) outcomes, have already provided some evidence that the impacts of the transport program on job search are persistent. In the weekly treatment effect tables, the coefficient on week 16 (the endline) survey showed a significant effect on the endline probability of searching for a job (7), or searching at the job boards (C.1), mainly among the *board* sample. The *city* sample is

[47] This endline survey was conducted during week 16 after the baseline (with some individuals interviewed in weeks 15 and 17).

Table 10: Monthly Impacts of treatment on main Job Market outcomes

	(1) work	(2) work perm	(3) searchnow	(4) searchboards	(5) discouraged	(6) days search
Panel A: Average Impacts By Month						
month 1	-0.022	-0.016	0.044*	0.035	-0.014	0.17*
	(0.024)	(0.012)	(0.024)	(0.023)	(0.018)	(0.097)
month 2	-0.020	-0.007	0.075***	0.088***	-0.036**	0.090
	(0.024)	(0.012)	(0.024)	(0.023)	(0.018)	(0.098)
month 3	0.027	0.006	0.092***	0.090***	-0.073***	0.36***
	(0.029)	(0.014)	(0.028)	(0.028)	(0.022)	(0.12)
Panel B: Average Impacts By Month and Sample						
board month 1	0.011	-0.010	0.011	0.025	0.006	0.100
	(0.033)	(0.016)	(0.032)	(0.031)	(0.025)	(0.13)
board month 2	-0.059*	-0.013	0.077**	0.085***	-0.026	0.066
	(0.033)	(0.016)	(0.032)	(0.031)	(0.025)	(0.13)
board month 3	0.016	0.011	0.10***	0.11***	-0.073**	0.50***
	(0.039)	(0.019)	(0.038)	(0.036)	(0.029)	(0.15)
city month 1	-0.048	-0.018	0.072**	0.014	-0.038	0.220
	(0.036)	(0.018)	(0.035)	(0.033)	(0.027)	(0.14)
city month 2	0.029	-0.001	0.061*	0.074**	-0.044	0.070
	(0.036)	(0.018)	(0.035)	(0.033)	(0.027)	(0.14)
city month 3	0.041	-0.011	0.064	0.039	-0.065**	0.150
	(0.043)	(0.021)	(0.042)	(0.040)	(0.033)	(0.17)
Obs	5,011	5,011	5,011	5,011	5,011	4,949

[1] Dependent Variables are listed at the top of each column. Results are from POST-OLS regressions on endline outcomes,

[2] Analysis excludes the follow up survey, just restricting analysis to the sample contacted in the phone surveys, with Month 1 defined as weeks 1-4, Month 2 as weeks 5-8 and Month 3 as weeks 9-12.

[3] Panel A gives average ITT effects across the full sample. Panel B estimates different coefficients for the two subsamples.

[4] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

marginally, but not significantly, more likely to be searching for work.

Specifications used until this point have considered individual i as treated (T_{it}) in week t if the person was offer the treatment at any time $\leq t$ (currently *or* in the past). I now estimate a new specification defined in section 4 as PERS (persistence where I estimate the additional impact of having the treatment in that specific week, over and above the effect of being in the treatment group at all. The dummy variable P_{it} is equal to one only if participant i was eligible to receive the treatment in the week t. Once the treatment period ended for an individual, this treatment variable "switches off", while T_{it} stays on. In estimates presented here I estimate the impact on T_{it} as the treatment effect of "on", compared to the treatment effect of having treatment "here" for P_{it}.

In this way, I exploit the randomized variation in when treatment ended, with some individuals stopping the program in week 8, three weeks before the others ended it. Thus in each week 9-11 I can compare those who were still receiving treatment to those who had finished it. I test whether the treatment effects estimated thus far are dominated by the periods in which individuals were actually receiving treatment, or if the treatment effects are similar (or in fact, even greater) in weeks after the treatment has ended, to when it was going on.

In the Table 11, I present the week 16 (endline) specific treatment effect in Panel, to show the persistence some weeks after the end of the programme when no one was still being treated, followed in Panel B with the PERS estimates for the later weeks of the survey (weeks

Table 11: Persistence of Treatment Effects after subsidies have ended

	(1) work	(2) work perm	(3) searchnow	(4) searchboards	(5) discouraged	(6) days search
Panel A: Average Impacts at Follow up Survey (Week 16 only)						
TE on	0.061*	0.040	0.076*	0.068	-0.051*	0.042
	(0.034)	(0.026)	(0.041)	(0.044)	(0.029)	(0.14)
Panel A: Average Impacts over weeks 8 to 12						
TE here	-0.050	0.002	-0.022	-0.006	0.024	-0.200
	(0.047)	(0.027)	(0.047)	(0.040)	(0.029)	(0.20)
TE on	0.061	0.006	0.10***	0.085**	-0.082***	0.37**
	(0.040)	(0.025)	(0.037)	(0.040)	(0.023)	(0.17)
Panel C: Heterogenous Impacts at Follow up Survey (Week 16 only)						
TE on board	0.043	0.080**	0.12**	0.094	-0.019	0.200
	(0.051)	(0.037)	(0.054)	(0.069)	(0.033)	(0.17)
TE on city	0.087*	-0.005	0.023	0.045	-0.096*	-0.140
	(0.044)	(0.033)	(0.064)	(0.046)	(0.050)	(0.22)
Panel D: Heterogenous Impacts over weeks 8 to 12						
TE here board	-0.022	-0.011	-0.066	-0.017	0.021	-0.410
	(0.063)	(0.046)	(0.051)	(0.062)	(0.028)	(0.30)
TE here city	-0.076	0.022	0.030	-0.010	0.029	0.038
	(0.076)	(0.018)	(0.087)	(0.051)	(0.052)	(0.24)
TE on board	0.023	0.028	0.15***	0.13**	-0.066***	0.66***
	(0.054)	(0.040)	(0.048)	(0.059)	(0.023)	(0.24)
TE on city	0.11*	-0.023	0.039	0.034	-0.100**	0.010
	(0.058)	(0.024)	(0.056)	(0.053)	(0.043)	(0.21)
Obs	2202	2202	2202	2202	2202	2202

[1] Dependent Variables are listed at the top of each column. Results are from OLS regressions on phone survey outcomes, with different treatment effects estimated as the average of groups of 4 weeks.

[2] Each coefficient gives the estimate for the treatment effect of *transport* with the sample restricted to the weeks denoted in the first column. The total number of observation used all regressions in each *row* is given in the last column (N)

[3] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

[4] Two types of treatment effects are presented: "on" denotes having received the treatment at any time in the past or currently. "here" indicates the impact of the treatment being available in that specific week.

8 onwards). In Panel B, the coefficient given by *TE on* estimates the average difference in the dependent variable between the treatment and control group in the later weeks of the survey. The coefficient on *TE here* tests for an additional impact among those who are currently receiving the treatment (P_{it}). If the treatment effects are not persistent at all (they fall back to zero as soon as the treatment ends) the coefficient on *TE here* should be large and significant, accounting for all of the difference between treatment and control estimated thus far.

Instead, the opposite seems to be true. For the primary search variables on which I an impact of the treatment has already been hold the coefficient is large and significant 16 weeks after treatment ended (in Panel A) and in Panel B, the coefficients on *TE here* are consistently close to zero and not significant. This suggests no drop off in the increased search activity after the treatment ended for some individuals in weeks 9, 10 and 11. Further, treated individuals are considerably less likely to be discouraged after the treatment ends.

Panel *C* and *D* provide the same estimates of persistence. The standard impacts of the transport (*TE on*) in Panel C are familiar: more permanent jobs for the boards sample, more employment generally for the city sample, and more search activity among the boards individuals.[48] Among the boards sample, the impacts on increased search activity are persistent and strong. The impact of the treatment on reducing discouragement is persistent among

[48]Recall that we did not find more search activity in the last month of the intervention in the city sample, the treatments were significant only in the middle of the study.

the *city* sample, but this is less surprising since this is likely due to an increase in overall employment, rather than increases in search behaviour among the unemployed. Once again, the results seem to be driven from *having received at any time* the treatment, rather than currently being able to collect it.

5.4 Discussion: Attitudes, Aspirations and Reservation Wages

The results presented thus far, would be consistent with a story of credit constraints preventing poor job seekers from being able to invest in job search at an individually optimal level, with the change in the costs of search lowering that barrier to entry into the labour market, and making the returns to search higher relative to the outside option of temporary employment or doing nothing.

However, the persistence of these effects suggests a more nuanced story. For the credit constraint story to explain the persistence of the impacts, the treatment would have had to increase youth capital stocks, such as savings, allowing them to go on searching for longer.

Alternatively, the treatment may have induced some behavioural or learning impacts, whereby discouragement "scars" the unemployment; the transport subsidies prevent job seekers from slipping into dejection and pessimism, which means that they are less likely to be discouraged some time later. Another theory would be that the period of high search intensity, or at least as the results suggest, a *longer* period of sustained job search, teachers job seekers something about the nature of the job market, wages, and how to find find employment, which makes them in turn more likely to keep searching, particularly at the job boards, if that information is positive

Another story could also explain the persistence of the search intensity. The decision to search for a permanent job is one not taken lightly. It is time consuming, and involves a certain fixed cost in getting acquainted with the market, preparing a CV and applications, and keeping up with vacancies, possibly while freeing oneself up from other work obligations, such as in temporary employment. I have already argued temporary work was *reduced* by the treatment in the early weeks of the study. Indeed, many respondents, in focus group discussions, reported searching very intensely in bursts, but then becoming discouraged and ending their job search indefinitely. If the transport subsidy changed the calculus, at the margin, of entering into one of these phases of intensive job search, the treated individuals would be more likely to still be engaged in one of these phases at the endline.

This section will attempt to investigate possible *behavioural and financial* channels through which the treatment could be operating, hopefully to explain some of the main results found thus far.

A more simplistic theory would be that the estimated impacts are due simply to Hawthorne effects: the regular phone calls and attention given to the treated job seekers makes them feel the need to either falsely report increased search intensity, or to actually search more intensely because they are being observed. It is to this hypothesis that I turn to first.

5.4.1 Hawthorne Effects

The possible existence of Hawthorne Effects presents a challenge to the results presented here so far. The phone call surveys may have induced some kind of behavioural change in the sample who were given them, causing those called to search harder for jobs, since they were being called so regularly. If a control group including individuals who did not receive phone calls is used to estimate the effect of the transport treatment, the significant impacts found thus far might be driven by the phone call Hawthorne effects, more than the transport treatment itself. In addition, the treatment effects presented thus far

Luckily the experiment variation in the sample selected to participate in the phone call study allows me to test if the phone calls had a significant impact on the results presented thus far. I test for an impact on the same work outcomes of having the phone calls, among the control group who were not receiving the transport treatment. I am, unfortunately, unable to test for different search trajectories between the two groups, precisely because, by definition, the control group were not observed during the phone call study. Thus I test for differences in the major job market outcomes at the endline survey, in Table 12.

Table 12: Impact of the Phone Call survey on outcomes at endline

	(1) searchnow	(2) searchboards	(3) discouraged	(4) work	(5) work perm
Panel A: Average Impacts at Endline					
TE trans	0.096**	0.081	-0.059*	0.053	0.034
	(0.048)	(0.055)	(0.030)	(0.045)	(0.033)
TE call	-0.029	0.00085	0.011	0.011	-0.010
	(0.049)	(0.047)	(0.044)	(0.053)	(0.035)
Panel B: Average Impacts at Endline by Sample					
TE trans boards	0.13*	0.10	-0.050	0.060	0.10**
	(0.072)	(0.093)	(0.044)	(0.059)	(0.046)
TE trans city	0.050	0.053	-0.073*	0.048	-0.045
	(0.061)	(0.053)	(0.042)	(0.067)	(0.037)
TE call boards	-0.0037	0.0072	0.043	-0.028	-0.067
	(0.069)	(0.075)	(0.044)	(0.064)	(0.055)
TE call city	-0.071	-0.012	-0.035	0.064	0.057*
	(0.072)	(0.052)	(0.087)	(0.087)	(0.029)
Obs	658	658	658	658	657

[1] Dependent Variables are listed at the top of each column. Results are from OLS regressions on phone survey outcomes, with different treatment effects estimated as the average of groups of 4 weeks.

[2] Each coefficient gives the estimate for the treatment effect of *transport* with the sample restricted to the weeks denoted in the first column. The total number of observation used all regressions in each *row* is given in the last column (N)

[3] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

I find that, at endline, there are few if any statistically significant difference between those with phone calls to those who did not receive them, across a range of specifications. I estimate the effect of the phone calls, while simultaneously controlling for assignment to the transport treatment to confirm the treatment effect of the transport *relative to the other individuals who received the phone calls but not the transport*.

Thus, after controlling for the effect of transport, the phone call respondents do not look significantly different to those that didn't get the calls. This improves confidence that estimating the treatment effects of the transport at endline by pooling all of the controls (with and without calls) was legitimate, and the results found there are not driven by any effects of the phone calls.

One competing hypothesis could still explain these results; which is that phone calls, in combination with the transport subsidies, together induced the transport group to search more intensively, but without the phone calls, the transport treatment alone could induce increased search effort. I cannot reject this outright, since budgetary and sample constraints prevented me from assigning some individuals to a transport treatment group, without the phone call.

5.4.2 Savings and Money

In order to investigate whether the president impacts on job search are due to long term wealth effects of the transport money, I test for any impacts of the treatment on endline financial variables. I find no impacts. I look at current weekly expenditure on all goods, expenditure on transport (not presented here), money received in financial support (as a measure of dependence), and money in savings, in total and formal savings (in the bank).[49] I find no evidence that the transport subsidies improved respondents financial positions at endline, suggesting that this is not the cause of the persistent job search intensity.

Table 13: Impact of the Phone Call survey on Finances and Aspirations at endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	log of:								
	savings tot	savings form	money total	expenditure	fair wage	market wage	job prospects	kept occ pref	offers exp
Panel A: Impacts on Aspirations at week 16									
TE Ave	0.029	-0.120	-0.068	0.064	-0.048	-0.036	-0.054	0.085*	-0.061
	(0.20)	(0.23)	(0.12)	(0.062)	(0.043)	(0.048)	(0.041)	(0.049)	(0.32)
Heterogeneity by Sample									
TE board	0.160	-0.340	-0.064	0.087	0.030	0.025	-0.056	0.065	0.150
	(0.28)	(0.28)	(0.17)	(0.087)	(0.057)	(0.064)	(0.054)	(0.063)	(0.31)
TE city	-0.130	0.200	-0.073	0.038	-0.14**	-0.110	-0.050	0.110	-0.300
	(0.28)	(0.29)	(0.17)	(0.093)	(0.058)	(0.070)	(0.063)	(0.076)	(0.59)
Panel B: Heterogenous Impacts on Aspirations by work status week 16									
TE work	0.260	-0.150	0.160	-0.043	-0.040	-0.016	-0.095*	0.090	-0.300
	(0.23)	(0.26)	(0.24)	(0.086)	(0.056)	(0.063)	(0.052)	(0.065)	(0.34)
TE no work	-0.370	-0.037	-0.180	0.150	-0.065	-0.070	-0.010	0.084	0.210
	(0.30)	(0.43)	(0.15)	(0.10)	(0.077)	(0.078)	(0.072)	(0.080)	(0.55)
Heterogeneity by Sample									
TE work-board	0.360	-0.350	0.037	0.003	0.087	0.092	-0.064	0.077	0.067
	(0.32)	(0.31)	(0.32)	(0.11)	(0.084)	(0.089)	(0.072)	(0.078)	(0.41)
TE no work-board	-0.250	-0.360	-0.130	0.200	-0.057	-0.079	-0.047	0.065	0.270
	(0.41)	(0.66)	(0.22)	(0.16)	(0.095)	(0.088)	(0.089)	(0.12)	(0.52)
TE work-city	0.110	0.220	0.390	-0.120	-0.22***	-0.16*	-0.15**	0.120	-0.840
	(0.31)	(0.40)	(0.27)	(0.16)	(0.060)	(0.082)	(0.064)	(0.12)	(0.55)
TE no work-city	-0.500	0.330	-0.240	0.110	-0.073	-0.061	0.026	0.100	0.150
	(0.44)	(0.46)	(0.18)	(0.13)	(0.12)	(0.13)	(0.11)	(0.11)	(0.95)
N	440	225	286	590	594	594	658	450	571

1 Dependent Variables are listed at the top of each column. Results are from OLS regressions on phone survey outcomes, with different treatment effects estimated as the average of groups of 4 weeks.
2 Each coefficient gives the estimate for the treatment effect with the sample restricted to the weeks denoted in the first column. The total number of observation used all regressions in each *row* is given in the column (N)
3 Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

[49] One might expect respondents who received the transport subsidies to save them. But if the transport subisdies allowed respondents to take less temporary work and thus had less income (which seems to be the case among the *boards* respondents, then this would not be the case.

Of course, many of these outcomes are undoubtedly effected by work status, which is an endogenous outcome that may have been impacted by job search. These these results must be viewed with caution, but I look at the different impacts on finanicial outcomes between those that did, and those did not have work at endline.

Furthermore I look at expenditure during the weeks of the transport subsidy. This results are presented in the appendix in Table 13, and suggest that there was no consistent impact of subsidies on expenditure in those weeks. So if there were no changes in long term financial status due to the treatment, one might imagine the respondents spending all of the money that they were given at the time, on more trips to the center, and increased search intensity, without saving any of it. Indeed Figure **??** in the appendix suggests that respondents did increase the number of trips they made to the city center as a result of the intervention.

5.4.3 Reservation Wages

Standard search theory suggests that reduced search costs ought to have an ambiguous impact on employment, or rates of finding jobs, because reduced costs both increase search intensity, but could also increase reservation wages, and thus delay entry to the job market. I test the assumption of this argument by estimating the impact of the treatment on reservation wages. I find little evidence of changes in reservation wages induced by the treatment. Figure B.10, in the appendix shows no significant change in reservation wages throughout the survey, nor at the endline survey. There is an increase in reservation wages for the treated *boards* individuals of about 6 percentage points at the endline (week 16), but the increase is not statistically significant. No other coefficients in other weeks are significant either.

However, I have already argued that the reservation wage model of job search is not entirely applicable to the Ethiopian context, particularly for first time job seekers. There is a relatively small wage premium for higher education, and across different types of jobs. Certain types of jobs and occupations may come with them a promise of higher pay in the future, but job seekers are still not searching for first time jobs on the basis of pay per se. So while respondents may indeed be receiving job offers and rejecting them, they are likely not to be doing so on the basis of the wage offer, but rather on the type of employment being offered; namely whether the job is permanent, secure and/or respectable or white-collar.

Indeed the estimated increase in the reservation wage induced by treatment is a about 220 birr on average, not too different from the difference between the wages offered in permanent and temporary jobs. This results is consistent with the treated individuals preferring to search more intensively for permanent jobs, and thus adjusting their wage expectations up to reflect that preference.

However, I do some see impact of the treatment on perceptions of market wages among those in the *city* sample, perhaps reflecting some learning about the distribution from increased job search. So while, reservation wages stayed mostly constant for these individuals, their views of what they thought the average wage in the current market was, and what they thought was a *fair wage* for the available work would be, fell significantly as a result of the treatment. I hypothesize that this is the result of learning about the prevailing market wage among a group who searched more intensively. Indeed the greatest impacts of the treatment are among those who did find find work. Expectations about the prevailing market wages were simply too high, with most respondents from the *city* sample saying that they could

expect to earn just over 1500 Birr per month in their chosen professions at baseline, when in reality those that found jobs were usually earning little over 1000 Birr. On the other hand

6 Conclusion

This paper looks at the impact of high search costs on labour market outcomes for cash constrained youth in Addis Ababa, Ethiopoia. The job market in this city is characterized by high levels of unemployment, and a growing supply of labour wanting to work in those professions, due to the enormous expansion of the secondary and tertiary education system in Ethiopia.

It is also a market plagued by serious search frictions, in which gathering information about job vacancies and applying for those vacancies is time consuming and expensive. But the costs are particularly high for finding the highly sought after jobs that are in short supply. These are the permanent (often white collar jobs) that are found predominantly at the job boards near the center of town. These jobs pay more and are more secure. Job seekers therefore have to decide between looking for and/or taking temporary work, often in their local areas, often found through social contacts and which are easier to find, or spending a lot of time and effort searching for the jobs they really aspire to.

I test whether these high costs of job search cause poor labour market outcomes for disadvantaged youth living in particularly dislocated parts of the city. A randomly selected group of individuals were given a weekly transport subsidy covering the costs of two return trips from their place of living in around Addis Ababa to the center of town where the vacancy information boards are located. These transport subsidies were offered from between 8 to 11 weeks, also chosen randomly.

I take a split sample approach, surveying two different types of unemployed youth in Addis Ababa, and thus allowing me to compare how job seekers with different backgrounds, looking for different types of jobs, respond differently to reduced job search costs. The *board sample* is comprised of active job seekers, often of high educational attainment, surveyed in areas around the vacancy boards where they were searching for work. The *city sample* is made up of individuals often of lower educational attainment, that were taking fewer active steps to find work, generally relying on less formal methods of job search.

Four months after participants were first surveyed, individuals in *both* samples receiving the transport money are positively impacted in their labour market outcomes, but these impacts differ across the samples, in line with the types of work available to different types of job seekers. I show that *board* sample participants were more likely to find permanent work, particularly in the professions they want to work in, while those in *city* sample are more likely to be working generally, and the work they are doing tends to more formal and less likely to be part time, or casual. Furthermore the transport subsidies are found to increase job search intensity, for those with and without work, throughout the study. These impacts are persistent some time after the program was ended. Some results on the impacts of the treatment suggest that these persistent effects are not due to wealth effects (participants do not have more savings or expenditure at any point in the study), nor does the treatment seem to have effected aspirations or perceptions in sustained away. Although these results should be treated with caution.

The results found here support the hypothesis that labour market frictions are constraining the ability of the young and unemployed to enter the labour market. "Flattening" spatial distance seems to have improved their access to employment opportunities that might otherwise have been denied them, as a direct result of their place of living and financial constraints. This suggests the idea of a spatial mismatch story in the large African capital, of Addis Ababa.

This suggests that labour markets could be made more efficient and equitable to the growing and aspirant urban population by reducing the costs of finding work, either through improved and subsidized transport for the poor, or more direct measures to make access to information about vacancies and employers more readily accessible.

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A What are jobs like, and who finds them?

Over the 16 weeks that I follow my survey of job seekers I observe enormous changes in their lives and job market outcomes.

Of the 658 individuals interviewed in the endline survey, 359 of them were working, compared to only 168 of those individuals at the baseline survey just 16 weeks earlier. Only 183 of the individuals working at the endline had ever held a any kind of job before in their lives, only 111 of them had been working when interviewed at the baseline, and only 55 had kept the sample jobs that had been working at at baseline (of the 186 working at baseline). 57 individuals had been working at baseline, but where no longer at the endline survey. Half of those who weren't initially working, had found work by the endline. And these transitions over 16 weeks miss the considerable changes that occurred during the intervening weeks.

Some individuals found very similar to work to work that they had done in past; they moved from one construction site to another, or found another short term contract in a factory near Addis Ababa. Others found completely new jobs in occupations that they had not worked in before, either because they had finally had a successful application for a job they had always wanted, or because they gave up searching for one type of work and tried something new. In some other cases, they had to settle for low pay, low skilled work,

My sample represents a picture of the jobs available in Addis Ababa. This is not meant a representative sample of the labour market in Addis Ababa, rather it provides a picture of the types of first, or entry level jobs. found by young people. Yet it still gives an overview of what jobs are like, what attitudes are to different types of labour, and who gets what types of jobs. The job market outcomes for different types of respondents, as well as the characteristics of different types of jobs found by respondents in the sample are described in detail. The first two descriptive statistics *job* and *Perm Job* simply give the percentage of a certain time of respondent have jobs or permanent jobs, respectively, whereas the later columns give the average statistics for respondents of a certain type who *have employment*. So for individuals working in construction, of course everyone has a job, but only 6.45% of these jobs are permanent, and 38.2% were found via a referral. For individuals born in Addis Ababa (Born AA) 49.3% had jobs, and 18.3% of the jobs found by these individuals were found at the job boards.

A few notable statistics are facts mentioning: Boards individuals with jobs are far more likely to have found them at the vacancy boards, or got them by applying for through formal channels (getting the job with an interview). Many still find out about their jobs through social networks, but far fewer than those in the *city sample*. But as the panel describing jobs by the method that was used to find them shows, the jobs found at the boards look a lot better; they are more likely to be permanent, pay more, and often require formal applications. These jobs just seem to be hard to actually get.

The panel on education levels show the returns to education in Addis Ababa. Surprisingly the better individuals in my sample, at least at these first jobs, do not seem to earn considerably more than those without higher education. While those with degrees do earn more, the difference is not especially large. However, those with degrees are far more likely to have permanent employment, and to have found their jobs formally or at the job boards. Indeed, jobs that have holding a degree as requirement for employment are overwhelmingly

Table A.1: Descriptions of job market outcomes and characteristics by individual and job types

	N	Job	Perm job	Casual job	Wage	Hourly wage	Hours work	Firm Size	Dissatisfied	Formally	Referral	Board
textbfPanel A: Labour Market outcomes for different respondent characteristics												
By Sample												
City	273	.48	.0586	.15	1107	10.6	36.8	51.8	.344	.072	.344	.064
Board	322	.587	.18	.0714	1306	10.1	42.1	86.2	.522	.337	.202	.455
By Gender												
Male	463	.577	.123	.13	1306	11.1	39.5	74.4	.482	.211	.287	.299
Female	132	.402	.129	.0303	830	6.52	41.9	60.6	.288	.308	.135	.269
By Period of Migration to Addis Ababa (or born in Addis Ababa)												
Born AA	226	.487	.0841	.102	1174	11.1	36.3	64.9	.308	.183	.346	.183
Since Birth	120	.525	.0917	.133	1370	9.36	42.4	49.6	.458	.136	.237	.136
Last 5 Yrs	139	.612	.137	.122	1098	8.64	43.2	67.2	.531	.272	.235	.358
Last 1 Yr	110	.564	.227	.0727	1340	12.2	39.5	113	.576	.339	.169	.559
By Education Level												
No Education	5	.8	.4	.2	610	2.69	49.5	23.3	0	.25	0	.25
Grades 0-9	111	.477	.0541	.144	1233	9.56	40.5	39.8	.34	.06	.28	.1
Secondary Complete	137	.511	.0876	.109	1200	11	39.4	54.2	.5	.0909	.394	.121
Vocational	59	.559	.102	.0847	1126	9.72	41.7	38.8	.5	.125	.344	.219
Certificate	19	.474	0	.105	901	7.89	32.2	6	.333	.222	.222	.333
Diploma	112	.607	.134	.152	1232	8.36	40.9	94.5	.424	.318	.258	.364
Degree	141	.532	.227	.0567	1354	12.4	40.2	126	.515	.441	.103	.559
By Year of last Education attendence												
Last 1 Yr	221	.525	.154	.086	1212	12.2	39	105	.514	.33	.183	.404
13-36 Months	162	.58	.13	.13	1201	8.95	42	75.3	.427	.213	.258	.303
+ 3 Years	211	.517	.0853	.114	1257	9.65	39.2	35.3	.404	.125	.346	.163
Panel B: Labour Market outcomes for different jobs characteristics												
By Job type												
Construction	93	1	.0645	.398	1399	12.2	37	25.1	.551	.0449	.382	.157
o/ Daily Labour	18	1	0	.667	783	7.62	27.3	41.5	.588	.0588	.235	0
Factory Work	18	1	.222	.111	898	5.33	48.3	246	.444	.167	.167	.278
Vocational	32	1	.156	.0313	1475	12.1	39.9	10.5	.29	.0968	.419	.161
Civil Service	17	1	.941	0	1430	7.81	42.4	206	.438	.563	0	.938
Other Gov	33	1	.515	.0606	1092	7.16	43.9	250	.3	.8	0	.8
o/ Skilled	38	1	.421	.105	1454	14.1	40.5	25.3	.333	.485	.182	.394
By Job status												
Permament	74	1	1	0	1339	9.73	44.9	139	.328	.531	.0938	.594
Temporary	155	1	0	0	1236	9.01	40.9	63.5	.47	.208	.295	.309
Casual	64	1	0	1	1138	10.8	34.3	50.4	.556	.0476	.333	.0317
Self Empl	26	1	0	0	1131	12.9	37.9	9	.36	.04	.32	.12
By Method job was found												
At Boards	98	1	.48	.0204	1445	8.62	44.7	140	.36	.607	.0112	1
Networks	164	1	.122	.256	1184	10.5	38.7	40.9	.49	.0573	.465	0
By Job Hiring Method												
Formally	77	1	.532	.039	1315	7.76	45.1	107	.275	1	0	.783
Referral	81	1	.0864	.259	1300	10.6	38.6	30.6	.506	0	1	.0127
By Job Education Requirement												
None	142	1	.0845	.317	1229	11.8	36	38.1	.565	.0072	.348	.058
Secondary	91	1	.253	.176	1134	7.91	43.5	86.4	.393	.371	.169	.404
Degree	44	1	.591	0	1642	10.4	41.5	139	.359	.641	.0513	.872

87.2% advertised at the job boards, and require formal applications.

Permanent Jobs: Individuals who found permanent jobs clearly earn a little more than other types of jobs, but the differences is small, particularly when looking at hourly wages instead of total monthly wages. Permanent jobs afford more hours per work,[50] and are undoubtedly less volatile in terms of the work being available from week to week: looking at the high frequency data, very few individuals (11%) holding down a permanent job had spells of unemployment (weeks when they worked one week, but then not the next) whereas 50% of those among those holding temporary jobs had spells of unemployment. Overall, individuals finding permanent worked on average more weeks over the 16 weeks of the study than those in temporary jobs or having to find casual work on a regular basis.

[50]In an economy where many young workers consider themselves under-employment, in the sense of wanting more hours of work (Broussard and Teklesellasie, 2012), this is a sought after characteristic of a job

Construction work: One of the most striking and perhaps surprising findings of the survey data is the dominance of construction jobs as a means to make a living for young people in Addis Ababa. In the baseline survey about 25% of respondents and 60% of young men who had work were working in construction[51]. Almost half of these jobs were casual labour jobs (individuals were paid daily, or piece rate salaries) and none of them were considered permanent jobs.

At the endline survey, things have not changed significantly. Table A.1 shows that 72 individuals had construction jobs, making 29% of all of the jobs. In addition, a further 6% of the sample were in other daily labour jobs, which often involved similar low skilled, hard labour. Only 8% of these construction jobs were considered permanent[52], and very people were recruited formally. Interestingly, very few of these jobs were found on the job boards, they tended to be found by going to visit worksites, or hearing about them through social networks.

And most interestingly, the wages paid in construction tend to be surprisingly high. On average, these wages were hardly lower than much sought after civil service jobs, with only Other Skilled (non-government jobs usually in specialized occupations such as lawyers or teachers) paying higher hourly wages on average. This may reflect the high premium paid for the kind of difficult labour done in construction, and the enormous demand for this kind of work in the middle of the construction boom currently happening in Addis Ababa. Yet individuals working on construction sites were more likely (by 15pp) to be dissatisfied with their work, and more likely to be searching (by 12pp) for work while working, when compared to all other jobs. When asked what job they expected to work at, in 6 months time, less than half of all construction workers anticipated still working in construction.

The government sector: Government jobs, and the civil service, are sought after by the youth in Addis Ababa. For a detailed history of the civil service in Ethiopia see (Mains, 2012). I distinguish between civil service jobs, usually office and administrative jobs, which are (or perhaps, used to be) highly prestigious, and routes to a middle class life (Mains, 2012), from any other kind of government employment, which may not be quite as sought after. In my baseline survey one third of all individuals with degrees expected to find work in government civil service jobs in the next six months. However, work in this sector is hard to find, and by the follow up survey only 15% of those with degrees were still expecting this type of work, and only 4% had found a civil service position. Discouragement set in quickly. Civil service jobs are almost all permanent positions in large government departments, and are almost exclusively found at the job boards. They are far more likely to be given after a formal job interview, and none were given on the basis of referral alone. However, claims of highly inflated civil service wages appear to be vastly overstated. In fact

Yet, while not everyone is satisfied with civil service jobs, but they are far less likely to be dissatisfied with these jobs than other permanent jobs on average. However, this satisfaction seems to be drive forces other than the wages paid by these jobs: government employees are more less likely to be dissatisfied with their jobs, but *more* likely to be dissatisfied with wages they are paid in these jobs.

I would speculate, based on the characteristics of these jobs, and from discussions with numerous individuals in my sample, some of whom were working in civil service jobs, that the preference for government jobs in due in large part from the permanence of these jobs.

[51] almost no women were working in construction

[52] These permanent construction jobs were usually jobs for managers or highly skilled machine operators, who kept the same job one construction company for some time

B Appendix A: Charts and Images

Figure B.1: Trajectory of Treatment effects across weeks in each sample

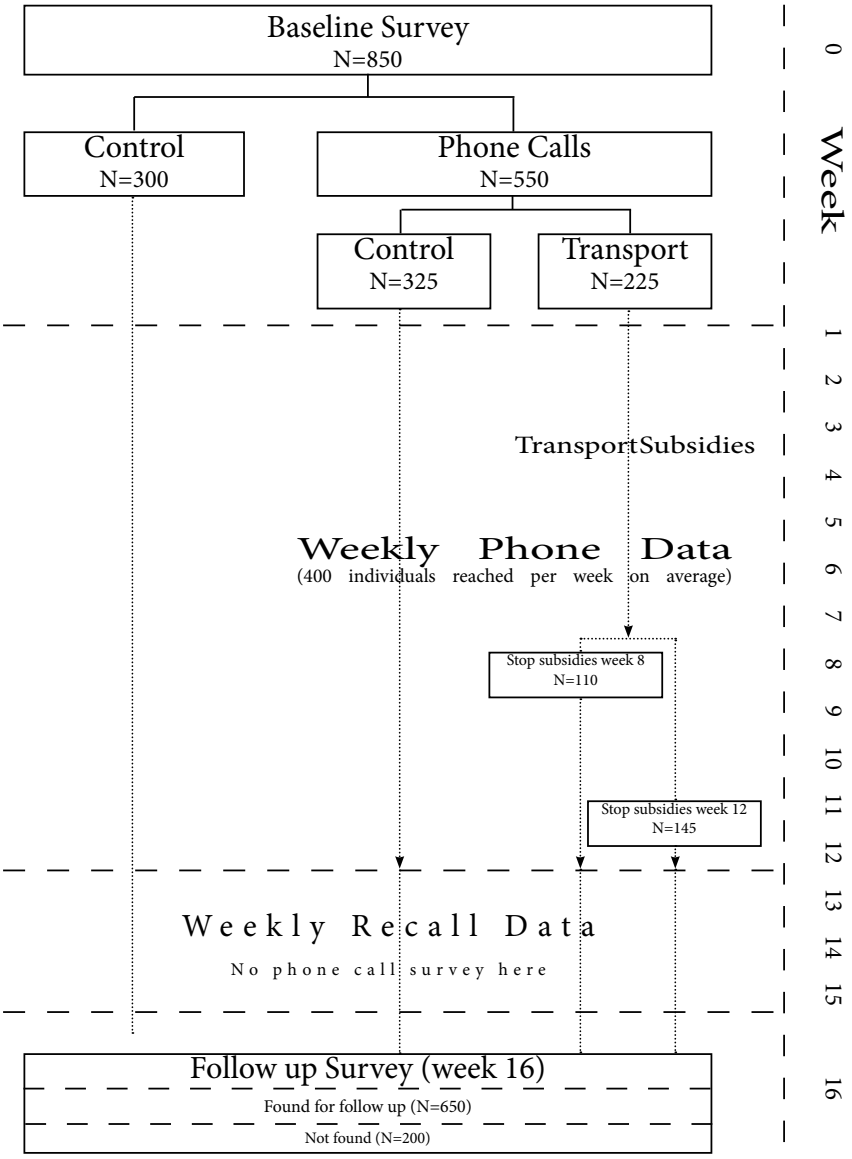


Figure B.2: Map of Addis Ababa showing sampling frame and selected EAs

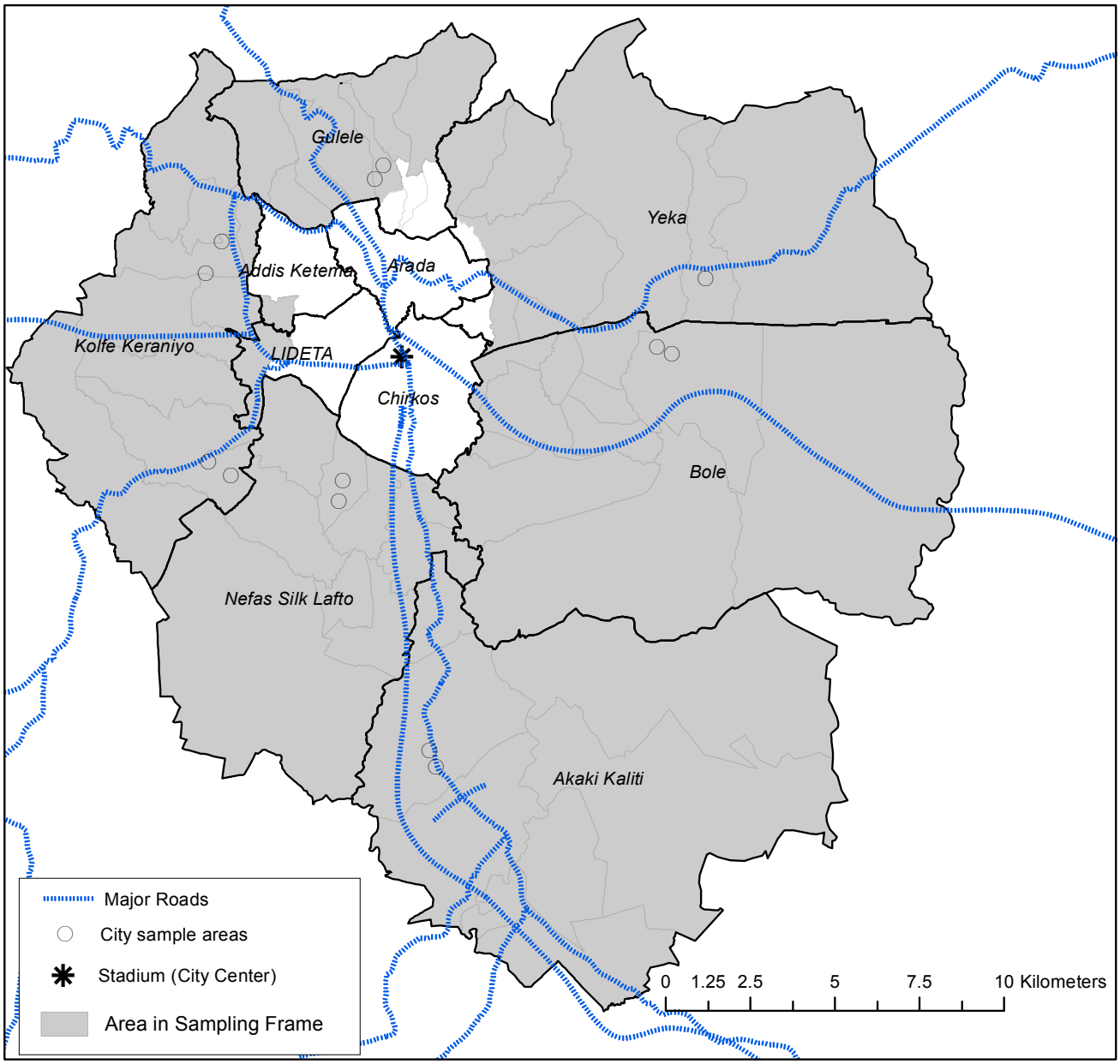


Figure B.3: Composition of the sample over weeks for the treatment & control groups (alternative categorical ordering)

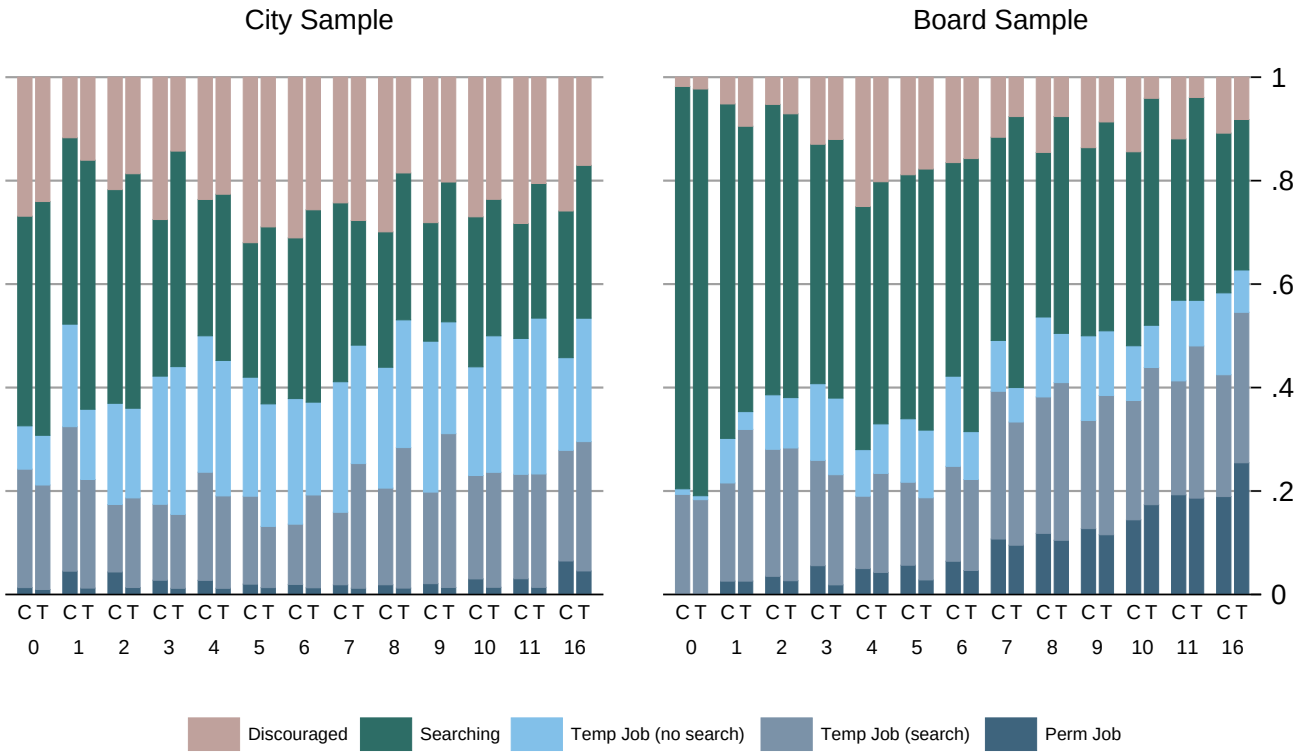


Figure B.4: Impact on visiting the job boards: Non-parametric trends & treatment effects over time

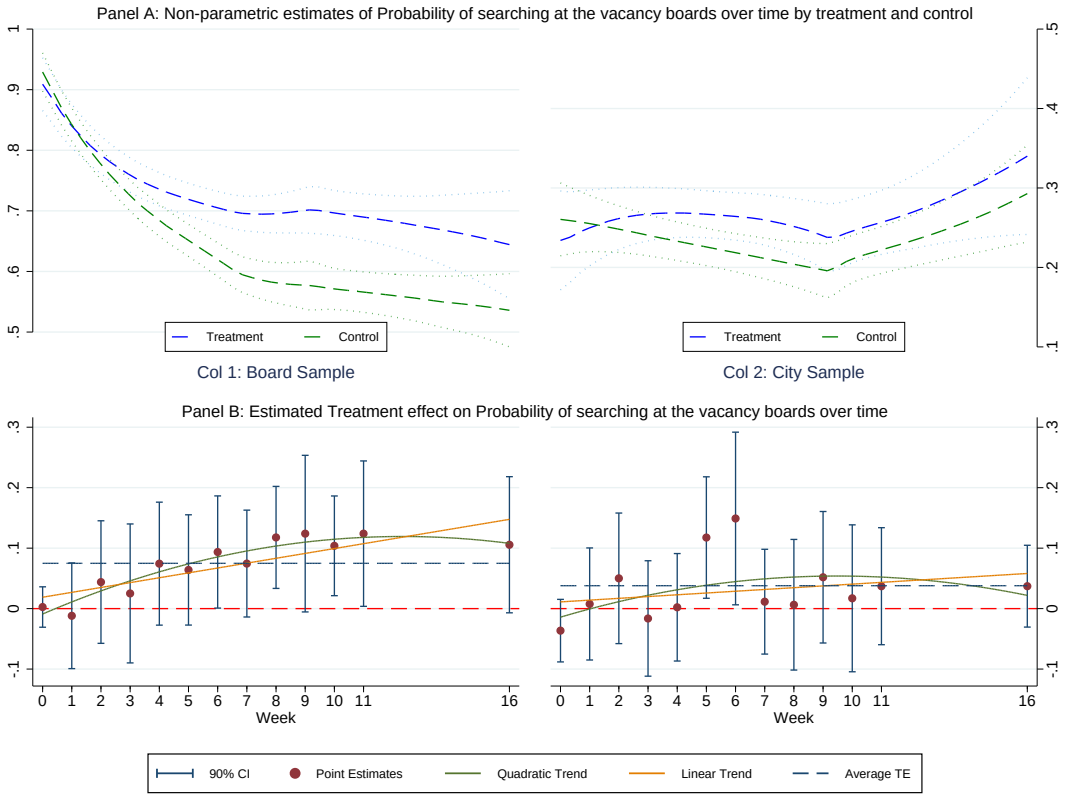


Figure B.5: Impact on days searching for work: Non-parametric trends & treatment effects over time

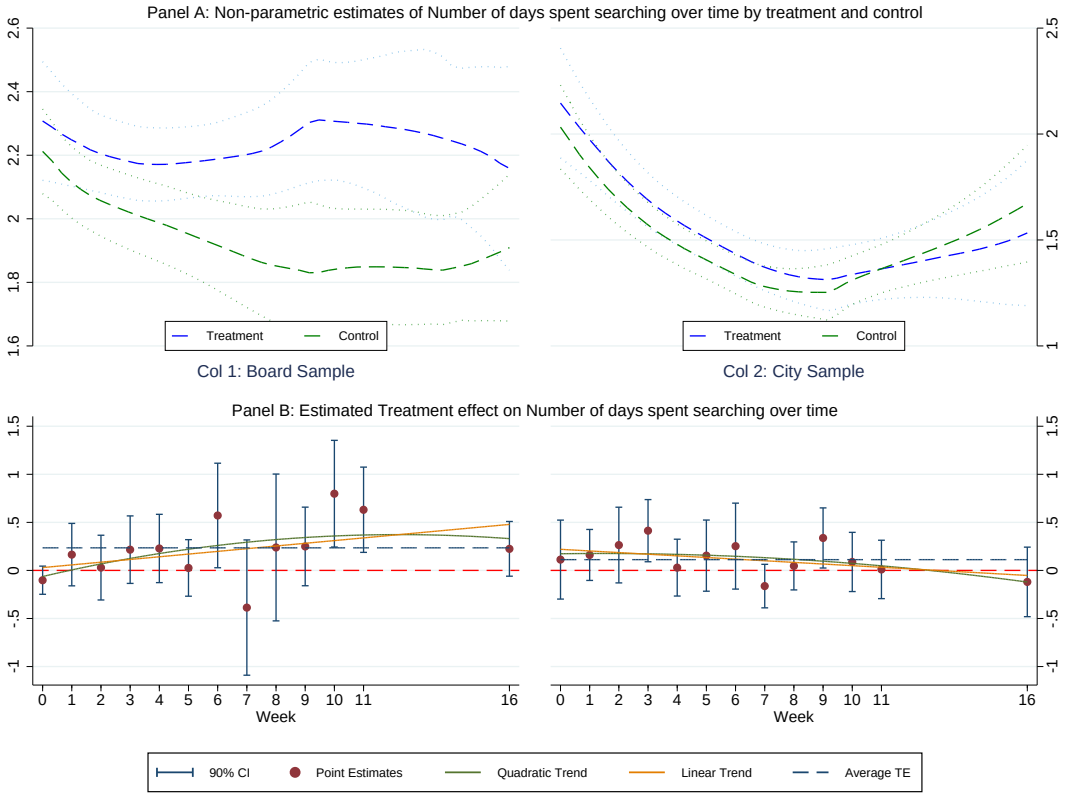


Figure B.6: Impact on Days visiting the vacancy boards: Non-parametric trends & treatment effects over time

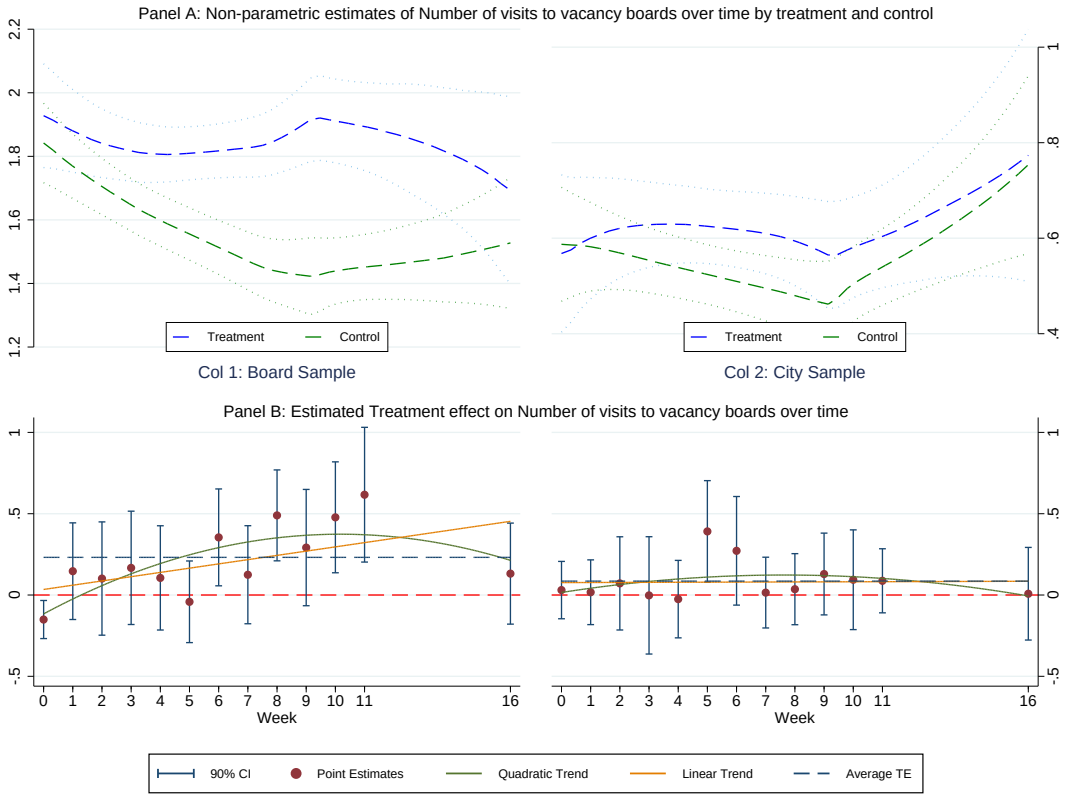


Figure B.7: Impact on Days visiting the vacancy boards: Non-parametric trends & treatment effects over time

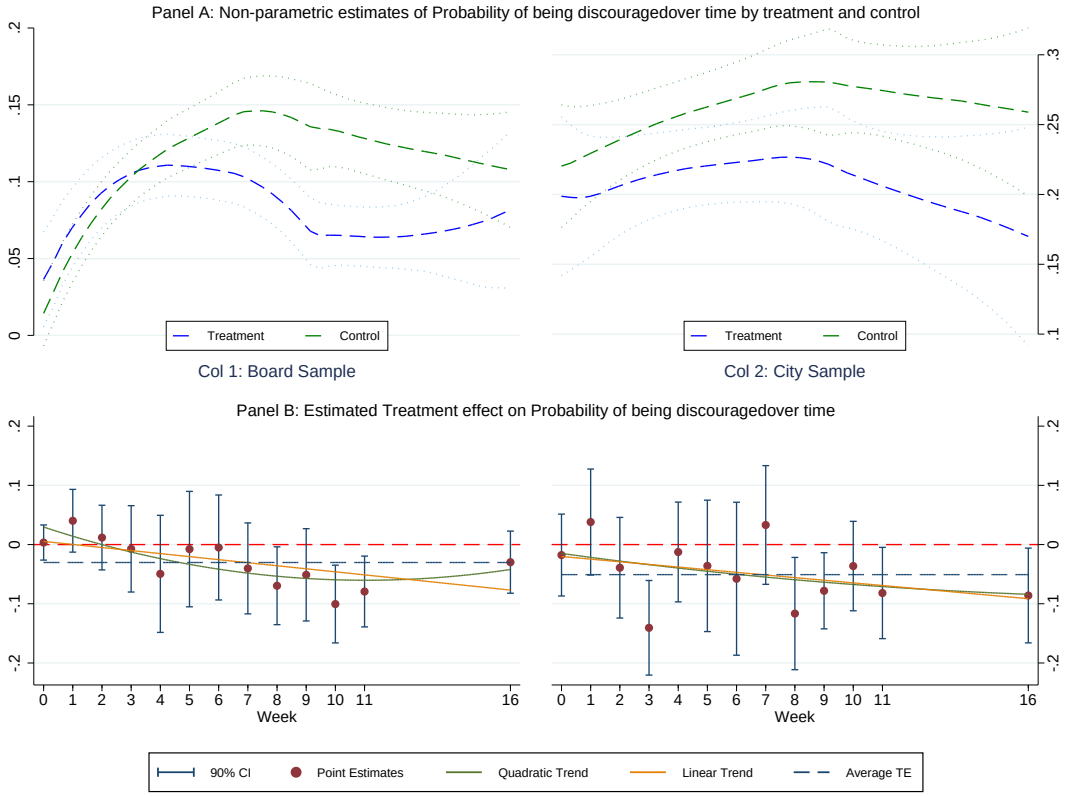


Figure B.8: Impact on Permanent Employment: Non-parametric trends & treatment effects over time

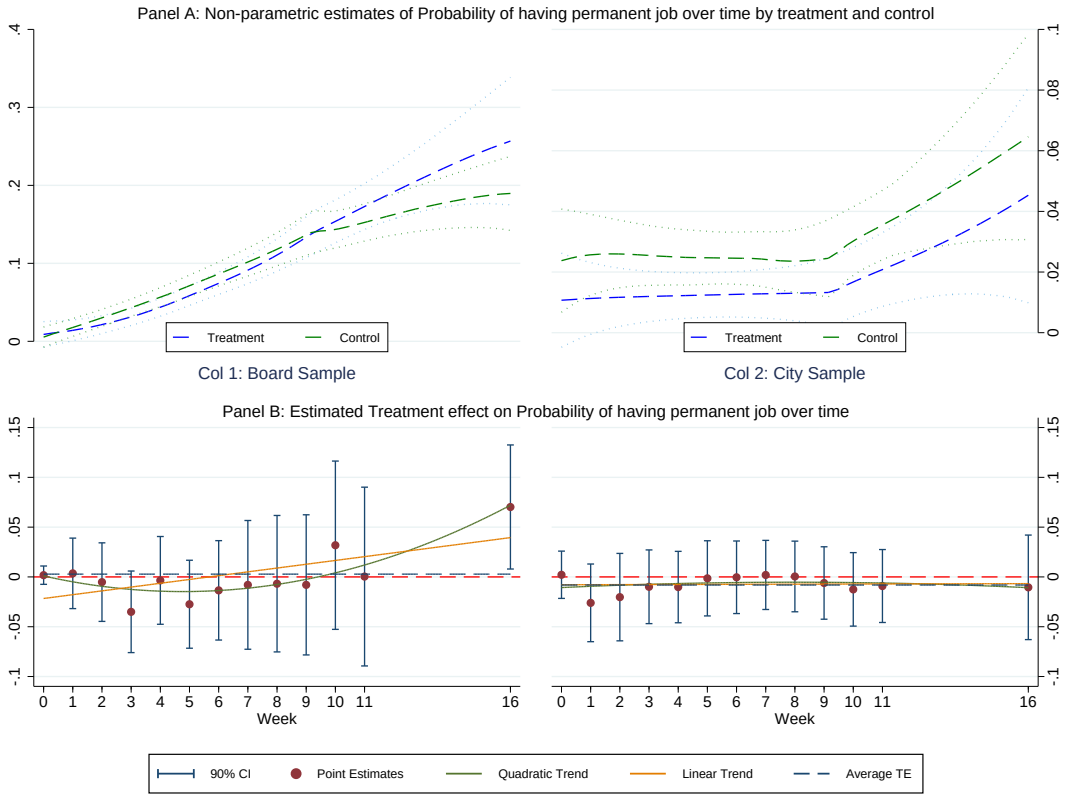


Figure B.9: Impact on Trips to central Addis: Non-parametric trends & treatment effects over time

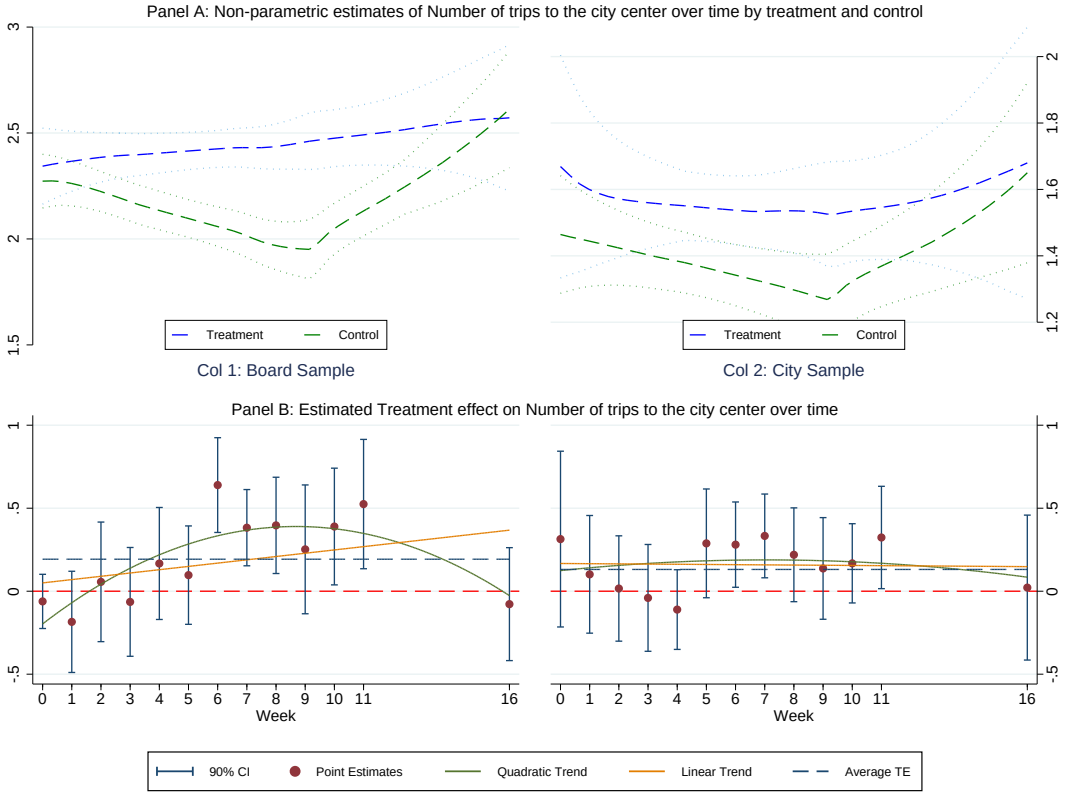
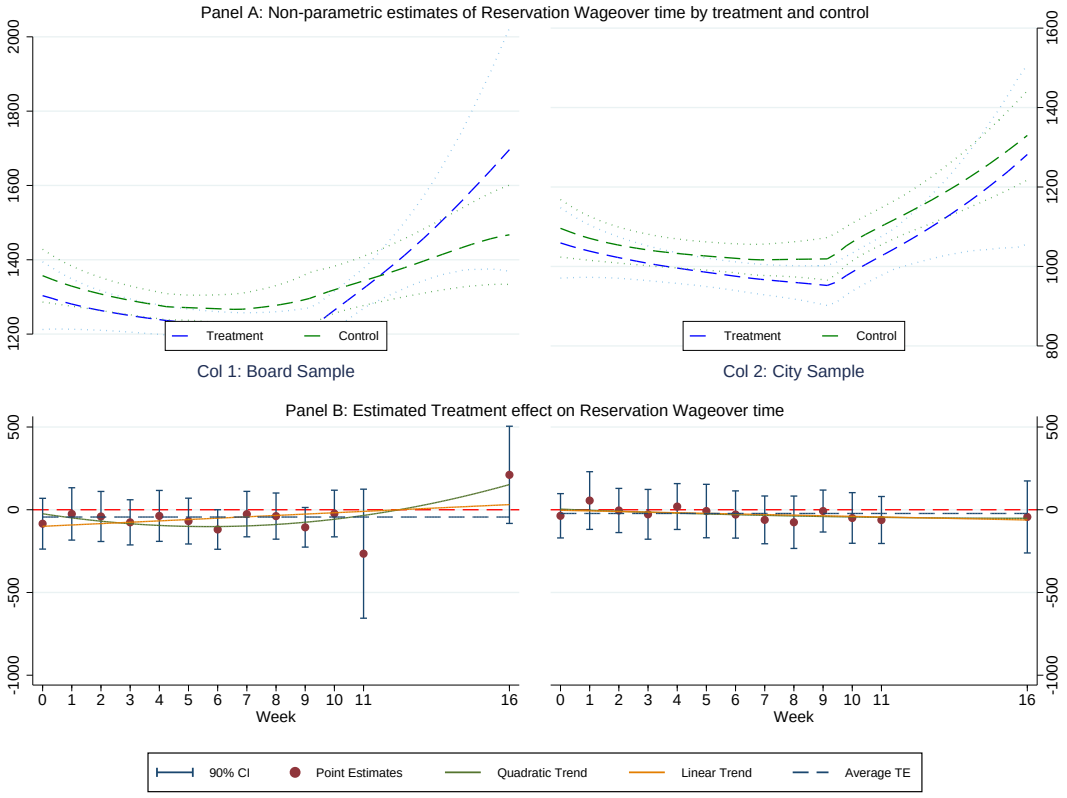


Figure B.10: Impact on Reservation Wage: Non-parametric trends & treatment effects over time



C Appendix B: Regression Tables

Figure C.1: Phone Survey Questionnaire

	Phone Survey Questions	Codes
p1	**During the last 7 days were you engaged in any kind of productive activity such as work for payment, family gain or production for own consumption?**	01 = Yes 02 = No - - > Skip to Question p5
p2	**Was this work short term/temporary or casual work?**	01 = Yes, it is short-term work 02 = No - - > Skip to Question p3
p2_1	*Check if the respondent was engaged in short term work the last time they were surveyed (jobj1_1= 1 if first phone interview, p2_1=1 if is a later interview)* *Ask If **YES**:* **Is this the same work that you were engaged in last week/the last time we interviewed you (was it with the same employer)?**	01 = Yes, I have not changed jobs 02 = No, it is not the same, I have taken work with a different employer
p3	**Was this work full-time, permanent paid employment?**	01 = Yes, it is full-time permanent work 02 = No
p3_1	*If yes, check if the respondent was engaged in permanent employment the last time they were phone surveyed (using existing data spreadsheets).* *If yes:* **Is this the same permanent employment you had last week/the last time we interviewed you (was it with the same employer)?**	01 = Yes, I have not changed jobs 02 = No, it is not the same, I have taken work with a different employer
p4	**Are you satisfied with this job, or are you still hoping for another job?**	01 = Yes, I am satisfied 02 = No, I want other work
p4_1	**How many hours did you work in the last 7 days?**	*Write down the number of days worked*
p4_2	**How much do you think you have earned in the last 7 days?** *If they are being paid on a monthly basis calculate the effective weekly rate by dividing by four*	*Write down the estimated amount earned*
p5	**Have you been searching for work in the last 7 days?** *Ask even if they have already found a full time job.*	01 = Yes 02 = No - - > Skip to Question p6
p5_1	**How many days of the last 7 days have you searched for work?**	*Write down the number of days searched*
p5_2	**How many of hours of the last working day (***ask about Friday if it's the weekend),* ***did you spend searching for work?***	*Write down the number of hours searched*
p5_3	**On how many days of the 7 days did you visit a (or any) job board?**	*Write down the number of days at job boards*
p6	**When we last spoke to you said you expected to find employment working as** _____ (*use most recent existing data on res oppucation),* **are you still interested in this kind of work?**	01 = Yes, I still expect to take this kind of work 02 = No, I am now expecting to take different work work
p6_1	*Ask about the most recent job type they have mentioned, if they just changed the job they are interested in, ask about that:* **You said you would take a job at the monthly wage of** _____ (*use the most recent reservation wage number of the existing data).* **Would you still work at this rate?**	01 = Yes, I would still take that wage -- > skip to p6_3 02 = No, that wage is now too low
p6_2	**If this wage is too low, what is the lowest wage you would now accept for this work?**	*Write down the lowest wage they would work for now (then skip to question p7)*
p6_3	**If you would still work for this amount, would you work for 100 birr less? 200 birr less?** *Enumerator: keep going down until the person says NO and then write down the amount at which the last said YES*	*Write down the amount where they said YES, hen skip to question p7)*
p7	**How much do you think you have spent in cash during the last 7 days?**	*Write down the total amount of cash spent*
p8	**How many times have you travelled into central Addis Ababa in the last 7 days (including this trip)?**	*Write down the number of trips to Addis*

Table 1 (cont): Test for Balance across Samples and after Attrition

Panel B: Sample resurveyed at Follow Up

	Full Sample				*Boards Sample*				*City Sample*			
	treat	cont	diff	p-val	treat	cont	diff	p-val	treat	cont	diff	p-val
Sample	.556	.563	-.0075	.859	1	1	0		0	0	0	
Work	.242	.263	-.0206	.579	.182	.205	-.0228	.616	.318	.338	-.0201	.739
Permanent Work	.0051	.0065	-.0015	.824	0	0	0		.0114	.0149	-.0036	.812
Searching	.828	.826	.0022	.946	.964	.969	-.0055	.787	.659	.642	.0173	.778
Visisted Boards	.631	.65	-.0187	.647	.955	.954	8.8e-04	.971	.227	.259	-.0314	.571
Discouraged	.116	.126	-.0099	.723	.0273	.0193	.008	.632	.227	.264	-.0364	.514
Hours Worked	6.84	6.45	.392	.741	6	5.45	.55	.724	7.9	7.74	.161	.93
Construction	.0859	.087	-.0011	.963	.0818	.0656	.0162	.58	.0909	.114	-.0235	.554
Female	.207	.224	-.0168	.632	.127	.135	-.0079	.839	.307	.338	-.0315	.601
Diploma	.202	.185	.0172	.606	.282	.278	.0038	.94	.102	.0647	.0376	.269
Degree	.247	.252	-.0047	.899	.436	.44	-.0038	.947	.0114	.01	.0014	.914
Finish Gr 10	.818	.807	.0117	.727	.927	.961	-.0341	.165	.682	.607	.0749	.227
Age	23.8	23.6	.255	.301	23.8	23.7	.13	.653	23.9	23.4	.416	.326
Household Size	3.45	3.43	.0285	.869	2.68	2.88	-.202	.275	4.42	4.12	.296	.299
Head of HH	.258	.25	.0076	.838	.336	.282	.0545	.296	.159	.209	-.0499	.325
Amhara	.449	.509	-.0592	.164	.445	.498	-.0526	.356	.455	.522	-.0678	.29
Oromo	.348	.302	.0463	.242	.409	.34	.0693	.206	.273	.254	.019	.736
Orthodox	.717	.737	-.0198	.6	.709	.71	-.0013	.979	.727	.771	-.0439	.425
Muslim	.0859	.102	-.0163	.518	.0455	.0734	-.0279	.321	.136	.139	-.0029	.947
Lives with Family	.242	.261	-.0184	.619	.345	.363	-.0175	.749	.114	.129	-.0157	.711
Born out of Addis	.616	.622	-.0056	.893	.791	.803	-.0122	.79	.398	.388	.0097	.877
Recent Grad	.328	.389	-.0608	.139	.455	.541	-.086	.131	.17	.194	-.0236	.637
Work Experience	.495	.511	-.0159	.708	.391	.409	-.0184	.743	.625	.642	-.0168	.786
Weeks w/o Work	39	40	-1.04	.788	37.7	35.3	2.45	.564	40.6	46.1	-5.58	.417
HH Wealth index	-.0276	.0254	-.053	.547	-.171	.0025	-.174	.165	.152	.0549	.097	.422
Own Room	.247	.224	.0236	.511	.264	.208	.0551	.247	.227	.244	-.0165	.763
Kms from center	5.98	6.45	-.463	.106	6.09	6.94	-.854	.0709	5.85	5.8	.0442	.852
Weekly expenditure	183	155	28.5	.0422	206	166	39.4	.0327	156	140	15.2	.476
Money from fam	96.2	77.9	18.3	.197	123	107	16.4	.42	62.7	40.8	21.9	.236
Reservation Wage	1226	1290	-63.3	.362	1321	1400	-78.8	.424	1108	1147	-39.7	.671
N	(198)	(460)			(110)	(259)			(88)	(201)		

Panel C: Sample Recontacted (at least once) in the Phone Surveys

	Full Sample				*Boards Sample*				*City Sample*			
	treat	cont	diff	p-val	treat	cont	diff	p-val	treat	cont	diff	p-val
Sample	.559	.5	.0587	.207	1	1	0		0	0	0	
Work	.23	.27	-.0398	.326	.185	.23	-.0453	.385	.287	.31	-.0223	.723
Permanent Work	.0047	.0079	-.0032	.664	0	0	0		.0106	.0159	-.0052	.742
Searching	.831	.802	.0294	.417	.975	.968	.0065	.76	.649	.635	.014	.831
Visisted Boards	.629	.607	.022	.628	.966	.968	-.0019	.935	.202	.246	-.0439	.444
Discouraged	.113	.147	-.0341	.278	.0168	.0079	.0089	.53	.234	.286	-.0517	.392
Hours Worked	6.63	6.12	.514	.685	6.28	5.23	1.04	.561	7.09	6.99	.093	.959
Construction	.0939	.0913	.0026	.923	.101	.0794	.0215	.559	.0851	.103	-.0181	.654
Female	.235	.246	-.0113	.777	.143	.159	-.0159	.73	.351	.333	.0177	.785
Diploma	.211	.167	.0446	.22	.303	.262	.0406	.482	.0957	.0714	.0243	.517
Degree	.254	.234	.0194	.628	.445	.452	-.007	.913	.0106	.0159	-.0052	.742
Finish Gr 10	.808	.774	.0337	.376	.941	.952	-.0112	.697	.638	.595	.0431	.519
Age	23.6	23.4	.267	.342	23.8	23.5	.312	.369	23.4	23.3	.185	.685
Household Size	3.53	3.56	-.0337	.86	2.76	2.89	-.124	.549	4.49	4.23	.259	.387
Head of HH	.23	.194	.0356	.349	.319	.238	.0812	.157	.117	.151	-.0338	.473
Amhara	.446	.528	-.0818	.0792	.445	.587	-.142	.0263	.447	.468	-.0214	.754
Oromo	.352	.282	.0704	.104	.429	.317	.111	.0725	.255	.246	.0093	.876
Orthodox	.709	.75	-.0411	.321	.714	.73	-.0159	.783	.702	.77	-.0677	.259
Muslim	.0939	.123	-.0291	.318	.0252	.0873	-.0621	.0365	.181	.159	.0221	.666
Lives with Family	.258	.286	-.0275	.508	.345	.437	-.092	.142	.149	.135	.014	.769
Born out of Addis	.61	.599	.0111	.807	.798	.841	-.043	.383	.372	.357	.0152	.818
Recent Grad	.366	.381	-.0148	.744	.487	.54	-.0523	.415	.213	.222	-.0095	.867
Work Experience	.507	.532	-.0247	.596	.412	.413	-9.3e-04	.988	.628	.651	-.0231	.725
Weeks w/o Work	36.9	38.7	-1.82	.651	36.3	30	6.34	.147	37.7	47.5	-9.82	.156
HH Wealth index	.0038	.0944	-.0906	.354	-.13	.0121	-.142	.301	.173	.177	-.0037	.979
Own Room	.235	.206	.0284	.462	.235	.19	.0448	.393	.234	.222	.0118	.837
Kms from center	6.12	6.46	-.341	.278	6.27	7.19	-.915	.0938	5.93	5.73	.194	.432
Weekly expenditure	183	142	41.1	.0067	211	168	43.7	.0491	146	115	30.8	.117
Money from fam	97.4	68.7	28.6	.0489	125	105	19.7	.373	62.2	32	30.1	.0791
Reservation Wage	1213	1270	-56.6	.427	1326	1406	-80	.422	1070	1133	-63.2	.526
N	(213)	(252)			(119)	(126)			(94)	(126)		

Table C.1: Effects of treatment on visiting the vacancy boards

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.650	-0.016	0.960	0.260	0.003	-0.037
		(0.024)			(0.021)	(0.033)
week 1	0.550	0.015	0.820	0.260	-0.012	0.008
		(0.037)			(0.053)	(0.056)
week 2	0.500	0.056	0.720	0.240	0.044	0.050
		(0.046)			(0.062)	(0.066)
week 3	0.420	0.015	0.630	0.220	0.025	-0.016
		(0.047)			(0.070)	(0.058)
week 4	0.410	0.041	0.550	0.280	0.074	0.002
		(0.042)			(0.062)	(0.054)
week 5	0.410	0.086**	0.590	0.220	0.064	0.12*
		(0.041)			(0.055)	(0.061)
week 6	0.390	0.12**	0.560	0.200	0.094	0.15*
		(0.051)			(0.056)	(0.087)
week 7	0.420	0.051	0.640	0.200	0.074	0.011
		(0.041)			(0.054)	(0.053)
week 8	0.390	0.069	0.560	0.210	0.12**	0.006
		(0.042)			(0.051)	(0.066)
week 9	0.370	0.099*	0.540	0.180	0.120	0.052
		(0.054)			(0.079)	(0.066)
week 10	0.420	0.072	0.630	0.210	0.10**	0.017
		(0.045)			(0.050)	(0.074)
week 11	0.380	0.090*	0.550	0.180	0.12*	0.037
		(0.048)			(0.073)	(0.059)
week 16	0.430	0.072*	0.540	0.290	0.110	0.037
		(0.043)			(0.068)	(0.041)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is a dummy variable equal to one if the individual reported having visited the job vacancy boards in the last 7 weeks. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.2: Trends in the Effects of treatment on visiting the vacancy boards

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	0.058**	0.015	-0.011	0.075**	0.019	-0.009	0.038	0.011	-0.014
	(0.025)	(0.025)	(0.024)	(0.029)	(0.034)	(0.033)	(0.041)	(0.036)	(0.036)
Treat X Time		0.0058**	0.018**		0.0080*	0.021*		0.003	0.014
		(0.0025)	(0.0086)		(0.0042)	(0.011)		(0.0022)	(0.014)
Treat X TimeSq			-0.001			-0.001			-0.001
			(0.00056)			(0.00077)			(0.00082)
CM	0.430			0.600			0.230		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.576	0.620	0.620	0.576	0.620	0.620	0.576	0.620	0.620

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms [2] Dependent Variable is a dummy variable equal to one if the individual reported having visited the job vacancy boards in the last 7 weeks.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.3: Effects of treatment on number of days searched in the last week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	2.060	0.010	2.060	2.050	-0.100	0.160
		(0.12)			(0.089)	(0.24)
week 1	2.150	0.200	2.510	1.770	0.170	0.160
		(0.13)			(0.20)	(0.16)
week 2	2.010	0.140	2.240	1.740	0.030	0.260
		(0.16)			(0.20)	(0.24)
week 3	1.570	0.32**	1.840	1.290	0.220	0.41**
		(0.15)			(0.21)	(0.20)
week 4	1.410	0.130	1.550	1.290	0.230	0.029
		(0.14)			(0.22)	(0.18)
week 5	1.510	0.091	1.800	1.210	0.026	0.150
		(0.14)			(0.18)	(0.22)
week 6	1.400	0.45*	1.620	1.170	0.57*	0.250
		(0.23)			(0.33)	(0.27)
week 7	1.880	-0.250	2.460	1.270	-0.390	-0.160
		(0.23)			(0.43)	(0.14)
week 8	1.730	0.170	2.080	1.370	0.240	0.047
		(0.25)			(0.46)	(0.15)
week 9	1.400	0.30*	1.640	1.130	0.250	0.34*
		(0.16)			(0.25)	(0.19)
week 10	1.610	0.50**	1.830	1.380	0.80**	0.088
		(0.22)			(0.34)	(0.19)
week 11	1.400	0.38**	1.610	1.170	0.63**	0.011
		(0.18)			(0.27)	(0.18)
week 16	1.810	0.060	1.910	1.680	0.220	-0.120
		(0.14)			(0.17)	(0.22)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is the number of days an individual reported searching for work in the last 7 weeks. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.4: Trends in the Effects of treatment on number of days searched in the last week

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	0.18**	0.120	0.054	0.24*	0.030	-0.063	0.110	0.24**	0.200
	(0.088)	(0.084)	(0.090)	(0.13)	(0.12)	(0.094)	(0.12)	(0.11)	(0.15)
Treat X Time		0.007	0.039		0.028**	0.071*		-0.019*	-0.003
		(0.0089)	(0.028)		(0.013)	(0.036)		(0.0098)	(0.042)
Treat X TimeSq			-0.002			-0.003			-0.001
			(0.0017)			(0.0022)			(0.0025)
CM	1.670			1.930			1.390		
Obs	4,949	5,690	5,690	4,949	5,690	5,690	4,949	5,690	5,690
R^2	0.481	0.502	0.503	0.481	0.503	0.503	0.481	0.503	0.503

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms [2] Dependent Variable is the number of days an individual reported searching for work in the last 7 weeks.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.5: Effects of treatment on Having a job in each week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.260	-0.013	0.200	0.330	-0.013	-0.012
		(0.037)			(0.042)	(0.060)
week 1	0.410	-0.048	0.300	0.520	0.063	-0.16**
		(0.047)			(0.058)	(0.072)
week 2	0.380	0.003	0.390	0.370	0.006	-0.003
		(0.041)			(0.059)	(0.054)
week 3	0.410	-0.007	0.410	0.420	-0.026	0.021
		(0.040)			(0.063)	(0.046)
week 4	0.400	-0.007	0.280	0.500	0.056	-0.051
		(0.047)			(0.063)	(0.074)
week 5	0.380	-0.036	0.340	0.420	-0.017	-0.052
		(0.047)			(0.069)	(0.064)
week 6	0.400	-0.063	0.420	0.380	-0.11*	-0.008
		(0.048)			(0.061)	(0.078)
week 7	0.450	-0.012	0.490	0.410	-0.081	0.068
		(0.047)			(0.064)	(0.062)
week 8	0.490	0.030	0.540	0.440	-0.027	0.092
		(0.047)			(0.059)	(0.073)
week 9	0.500	0.022	0.500	0.490	0.018	0.028
		(0.044)			(0.068)	(0.054)
week 10	0.460	0.053	0.480	0.440	0.042	0.062
		(0.045)			(0.067)	(0.060)
week 11	0.530	0.021	0.570	0.490	0.004	0.036
		(0.047)			(0.066)	(0.064)
week 16	0.530	0.060*	0.580	0.460	0.044	0.082*
		(0.035)			(0.051)	(0.047)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is a dummy variable equal to one if the individual reported having take some step to look for work in the last 7 weeks. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.6: Trends in the Effects of treatment on having a job in each week

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	0.004	-0.030	-0.020	-0.000	-0.012	0.016	0.009	-0.052	-0.065
	(0.029)	(0.030)	(0.032)	(0.038)	(0.040)	(0.042)	(0.043)	(0.043)	(0.047)
Treat X Time		0.0052*	0.001		0.002	-0.011		0.0097**	0.015
		(0.0029)	(0.011)		(0.0040)	(0.016)		(0.0041)	(0.013)
Treat X TimeSq			0.000			0.001			-0.000
			(0.00062)			(0.00096)			(0.00070)
CM	0.450			0.460			0.450		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.493	0.478	0.478	0.493	0.478	0.478	0.493	0.478	0.478

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms [2] Dependent Variable is a dummy variable equal to one if the individual reported having a any kind of paid work in the last 7 weeks.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.7: Effects of treatment on having a Permanent job in each week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.006	0.002	0.000	0.013	0.002	0.003
		(0.0083)			(0.0056)	(0.015)
week 1	0.035	-0.014	0.026	0.045	0.004	-0.026
		(0.018)			(0.022)	(0.024)
week 2	0.039	-0.013	0.035	0.043	-0.005	-0.020
		(0.019)			(0.024)	(0.027)
week 3	0.041	-0.025	0.056	0.028	-0.035	-0.010
		(0.018)			(0.025)	(0.022)
week 4	0.038	-0.007	0.050	0.027	-0.003	-0.010
		(0.018)			(0.027)	(0.022)
week 5	0.039	-0.018	0.057	0.020	-0.027	-0.001
		(0.019)			(0.027)	(0.023)
week 6	0.042	-0.008	0.064	0.019	-0.013	-0.000
		(0.020)			(0.030)	(0.022)
week 7	0.064	-0.002	0.110	0.019	-0.008	0.002
		(0.023)			(0.039)	(0.021)
week 8	0.069	-0.001	0.120	0.019	-0.007	0.000
		(0.024)			(0.042)	(0.022)
week 9	0.078	-0.004	0.130	0.021	-0.008	-0.006
		(0.025)			(0.043)	(0.022)
week 10	0.088	0.017	0.140	0.030	0.032	-0.013
		(0.029)			(0.051)	(0.022)
week 11	0.120	0.003	0.190	0.030	0.000	-0.009
		(0.032)			(0.055)	(0.022)
week 16	0.130	0.033	0.190	0.065	0.070*	-0.010
		(0.027)			(0.038)	(0.032)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is a dummy variable equal to one if the individual reported having a permanent job in the last week. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.8: Trends in the Effects of treatment on having a Permanent job in each week

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	-0.002	-0.015	-0.004	0.003	-0.022	0.001	-0.008	-0.008	-0.010
	(0.017)	(0.014)	(0.011)	(0.025)	(0.020)	(0.015)	(0.022)	(0.018)	(0.016)
Treat X Time		0.002	-0.003		0.004	-0.007		0.000	0.001
		(0.0016)	(0.0058)		(0.0026)	(0.010)		(0.0014)	(0.0028)
Treat X TimeSq			0.000			0.001			-0.000
			(0.00037)			(0.00064)			(0.00019)
CM	0.071			0.110			0.033		
Obs	5,010	5,751	5,751	5,010	5,751	5,751	5,010	5,751	5,751
R^2	0.164	0.158	0.159	0.164	0.159	0.160	0.164	0.159	0.160

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms [2] Dependent Variable is a dummy variable equal to one if the individual reported having a permanent job in the last 7 weeks.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.9: Effects of treatment on being Discouraged in each week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.130	-0.011	0.018	0.270	0.003	-0.029
		(0.022)			(0.018)	(0.041)
week 1	0.084	0.043	0.052	0.120	0.040	0.038
		(0.029)			(0.032)	(0.054)
week 2	0.130	-0.012	0.053	0.220	0.012	-0.039
		(0.029)			(0.033)	(0.052)
week 3	0.200	-0.069*	0.130	0.280	-0.007	-0.14***
		(0.035)			(0.044)	(0.049)
week 4	0.240	-0.026	0.250	0.240	-0.050	-0.013
		(0.040)			(0.060)	(0.051)
week 5	0.250	-0.020	0.190	0.320	-0.008	-0.036
		(0.044)			(0.059)	(0.067)
week 6	0.240	-0.030	0.170	0.310	-0.005	-0.058
		(0.046)			(0.054)	(0.079)
week 7	0.180	-0.008	0.120	0.240	-0.040	0.033
		(0.038)			(0.047)	(0.061)
week 8	0.220	-0.092***	0.150	0.300	-0.070*	-0.12**
		(0.034)			(0.040)	(0.058)
week 9	0.200	-0.065**	0.140	0.280	-0.051	-0.078**
		(0.032)			(0.047)	(0.039)
week 10	0.210	-0.074**	0.140	0.270	-0.10**	-0.036
		(0.031)			(0.040)	(0.046)
week 11	0.200	-0.083***	0.120	0.280	-0.079**	-0.082*
		(0.028)			(0.036)	(0.047)
week 16	0.170	-0.054*	0.110	0.260	-0.030	-0.086*
		(0.029)			(0.032)	(0.049)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is a dummy variable equal to one if the individual reported having no job, and having made no attempt to find a job in the last 7 days. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.10: Trends in the Effects of treatment on being having temporary work and *not* searching in each week

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	-0.022	-0.015	-0.008	-0.035*	-0.004	-0.007	-0.006	-0.028	-0.008
	(0.016)	(0.017)	(0.017)	(0.019)	(0.019)	(0.017)	(0.027)	(0.029)	(0.031)
trans trend		-0.001	-0.004		-0.0044**	-0.003		0.004	-0.005
		(0.0020)	(0.0070)		(0.0020)	(0.0076)		(0.0037)	(0.012)
trans trendsq			0.000			-0.000			0.001
			(0.00043)			(0.00049)			(0.00071)
CM	0.180			0.130			0.230		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.216	0.209	0.209	0.216	0.210	0.210	0.216	0.210	0.210

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms [2] Dependent Variable is a dummy variable equal to one if the individual reported having no job, and having made no attempt to find a job in the last 7 days.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.11: Effects of treatment on having temporary work and *not* searching in each week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.043	0.004	0.011	0.084	-0.003	0.011
		(0.016)			(0.010)	(0.035)
week 1	0.140	-0.060**	0.086	0.200	-0.048	-0.069
		(0.025)			(0.030)	(0.044)
week 2	0.150	-0.015	0.110	0.200	-0.002	-0.030
		(0.031)			(0.036)	(0.057)
week 3	0.200	0.013	0.150	0.250	-0.000	0.034
		(0.035)			(0.042)	(0.057)
week 4	0.180	-0.006	0.090	0.260	0.007	-0.008
		(0.030)			(0.037)	(0.053)
week 5	0.170	0.006	0.120	0.230	0.009	0.003
		(0.031)			(0.045)	(0.042)
week 6	0.210	-0.076**	0.170	0.240	-0.083*	-0.066
		(0.035)			(0.045)	(0.055)
week 7	0.170	-0.031	0.098	0.250	-0.027	-0.028
		(0.025)			(0.034)	(0.037)
week 8	0.190	-0.028	0.150	0.230	-0.058	0.011
		(0.035)			(0.037)	(0.064)
week 9	0.220	-0.059	0.160	0.290	-0.036	-0.081
		(0.044)			(0.060)	(0.062)
week 10	0.160	0.008	0.110	0.210	-0.022	0.053
		(0.036)			(0.031)	(0.069)
week 11	0.210	-0.028	0.160	0.260	-0.069	0.034
		(0.044)			(0.055)	(0.067)
week 16	0.170	-0.016	0.160	0.180	-0.076**	0.058
		(0.025)			(0.031)	(0.037)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is a dummy variable equal to one if the individual reported having no job, and having made no attempt to find a job in the last 7 days. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.12: Trends in the Effects of treatment on being Discouraged in each week

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	-0.040**	-0.008	0.007	-0.030	0.005	0.029*	-0.051	-0.024	-0.021
	(0.019)	(0.021)	(0.017)	(0.024)	(0.024)	(0.017)	(0.032)	(0.037)	(0.030)
Treat X Time		-0.0047**	-0.011*		-0.0051**	-0.016*		-0.004	-0.005
		(0.0018)	(0.0067)		(0.0022)	(0.0082)		(0.0031)	(0.011)
Treat X TimeSq			0.000			0.001			0.000
			(0.00045)			(0.00054)			(0.00074)
CM	0.190			0.130			0.260		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.237	0.241	0.241	0.237	0.241	0.241	0.237	0.241	0.241

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms [2] Dependent Variable is a dummy variable equal to one if the individual reported having no job, and having made no attempt to find a job in the last 7 days.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.13: Iterative 4 week average treatment effects (one regression per coefficient)

	(1) work	(2) work perm	(3) searchnow	(4) searchboards	(5) discouraged	(6) days search	N
weeks 0-3	-0.019	-0.012	0.034	0.0090	-0.011	0.19*	1227
	(0.034)	(0.016)	(0.026)	(0.033)	(0.023)	(0.10)	
weeks 1-4	-0.0044	-0.0091	0.042	0.025	-0.037	0.17	1191
	(0.035)	(0.016)	(0.029)	(0.034)	(0.025)	(0.11)	
weeks 2-5	-0.013	-0.011	0.041	0.040	-0.038	0.15	1186
	(0.039)	(0.016)	(0.032)	(0.034)	(0.030)	(0.12)	
weeks 3-6	-0.028	-0.0060	0.057	0.081**	-0.024	0.20	1175
	(0.039)	(0.018)	(0.036)	(0.039)	(0.035)	(0.14)	
weeks 4-7	-0.028	-0.0068	0.050	0.080**	-0.016	0.063	1194
	(0.040)	(0.019)	(0.036)	(0.038)	(0.033)	(0.11)	
weeks 5-8	-0.011	-0.0027	0.087***	0.080**	-0.044	0.11	1208
	(0.040)	(0.022)	(0.032)	(0.038)	(0.029)	(0.14)	
weeks 6-9	0.012	-0.0025	0.10***	0.076**	-0.058**	0.080	1194
	(0.040)	(0.024)	(0.031)	(0.038)	(0.029)	(0.17)	
weeks 7-10	0.028	-0.0017	0.12***	0.090**	-0.081***	0.33**	1161
	(0.039)	(0.025)	(0.032)	(0.037)	(0.027)	(0.13)	
weeks 8-11	0.023	-0.0062	0.11**	0.100**	-0.077***	0.41***	1141
	(0.040)	(0.028)	(0.042)	(0.038)	(0.026)	(0.14)	
weeks 9-12	0.026	-0.0081	0.092**	0.099**	-0.084***	0.45***	757
	(0.042)	(0.031)	(0.044)	(0.040)	(0.026)	(0.15)	

[1] Dependent Variables are listed at the top of each column. Results are from OLS regressions on phone survey outcomes, with different treatment effects estimated as the average of groups of 4 weeks.

[2] Each coefficient gives the estimate for the treatment effect of *transport* with the sample restricted to the weeks denoted in the first column. The total number of observation used all regressions in each *row* is given in the last column (N)

[3] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Employment Exposure: Employment and Wage Effects

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Abstract

This paper exploits an experiment that randomized probabilistic job offers and estimates the employment and wage effects of the short term jobs. I find the following key results. First, there is a 10.6 to 13.9 percentage point increase in average employment during the eight months following the job. Second, there is a sizeable increase in wages. Individuals earn approximately 60 to 67 percent more per day. There is suggestive evidence that individuals are switching into different occupations particularly clerical and related work away from agricultural based activities. Lastly, the estimated returns to the job are larger among those who perform worst on a high stakes numeracy and literacy test suggesting education is to some degree substitutable with the type of work experience offered here.

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1. Introduction

Understanding the key determinants of wage growth can inform policy interventions that reduce poverty. While the determinants of wages have been extensively studied in the United States and other developed country settings, much less evidence exists for developing countries. One exception to this is the extensive literature examining the returns to schooling. This literature shows that the returns to schooling are larger for females, and in countries with lower GDP per capita (Psacharopoulus, 1973, and 1994). An explanation for the higher returns is the scarcity of skilled labor (Mwabu and Schultz, 2000). Heterogeneous initial conditions in terms of the stock of skilled labor and other factors that affect the productivity of labor may imply that the determinants of wages differ across countries. Identifying the importance of factors such as experience, tenure and job mobility (or stability) is therefore important to understanding wage growth in different contexts. In this paper, I study the effect of experience on employment and wages in urban Malawi.

A key challenge in estimating the impact of work experience is that experience is endogenous and likely to be correlated with other factors that affect employment or wages. For example, individuals who acquire work experience may exhibit better non-cognitive skills not observable in the data. Several papers have shown that non-cognitive influence labor market outcomes (Bowles, Gintis, and Osborne 2001; Jacob 2002; Heckman, Stixrud and Urzua, 2006). Because so many characteristics are likely correlated both with past acquired work experience and future labor market outcomes, the assumptions for selection on observables are unlikely to be satisfied even when high quality survey or administrative data are available. Estimating the returns to work experience in developing countries is further constrained by the dearth of detailed labor force and panel data, particularly in Africa. Even though the prevalence of labor force panel studies is increasing they often lack detailed retrospective employment histories or

sufficient detail on jobs to accurately measure acquired work experience. To circumvent this data limitation, most existing studies use a measure of "potential experience" that is the difference between an individual's age and his years of schooling in estimating the employment and wage effects of experience. However, the prevalence of interrupted or delayed schooling and periods of unemployment renders potential experience a poor proxy for actual experience in developing countries.

In this paper I overcome the identification challenge by exploiting an unusual source of random variation in short term employment. I also collect data that contain more detailed information about employment history than typically available, and measure actual rather than potential work experience. The exogenous variation I exploit derives from an experimental study conducted in Malawi and discussed in detail in Godlonton (2013). Specifically, job-trainees were randomly allocated a probabilistic chance of short term employment in a real job. There were six treatment groups. Individuals were assigned to receive a 0-, 1-, 5-, 50-, 75- or 100-percent chance of employment in research assistance activities at the completion of the training and recruitment process (even if they were not hired by the recruiter). These probabilistic chances of jobs can be used as an instrument for short term work experience. I have rich baseline data, including a baseline survey and resume for each of the 268 job trainees. Outcome data come from a follow-up survey that collects data on retrospective work histories for the eight month period following the experiment.

By instrumenting for an individual's work experience using his randomly-assigned chance of gaining experience from the short term job, I am able to estimate the effect of short term work experience on employment and job search strategies. First, the estimated impact on employment after eight months is positive, though imprecisely estimated. Individuals offered an

alternative job were between 10.6 and 13.9 percentage points more likely to be employed on average during the post-intervention. The estimated impact of experience on the probability of job search and the likelihood of holding multiple concurrent jobs across the eight month period following the intervention is positive but not statistically significant.

Second, I do find a sizeable wage return to work experience. Individuals who were assigned to receive work experience earn on average approximately $3.80 to $4.19 more per day, as estimated in specifications that do not condition upon employment. This is a large return representing a 75 to 83 percent increase in daily wages. In specifications that exclude the unemployed and use logged wages, the estimated effect is only somewhat smaller, with experience increasing wages by between 60 and 67 percent. Some of the increase in the wage may be attributed to an increase in the number of hours worked as this increases by approximately four hours per week (although the effect is not statistically different from zero). Another mechanism for the increase in wages is changes in occupation. I find that the short term research assistance experience prompts a shift away from agriculture and related occupations and towards clerical and related occupations. I examine a number of potential mechanisms through which experience causes wage increases. The data do not support the hypotheses that expanded social networks, signaling of ability from letters of reference, or increased reservation wages are behind the increase in wages. Indirect evidence is most consistent with the idea that experience facilitates skill acquisition, and skill is rewarded in the external labor market.

Furthermore, there is interesting heterogeneity in the employment and wage effects. Specifically, individuals of lower ability (as assessed by a numeracy and literacy test) benefit the most from the work experience. For this subgroup, the effect of experience on the probability of employment is statistically significant. Although the small sample size limits statistical precision,

there is suggestive evidence that the employment effects are more are in fact growing over time for low ability types.

Overall, the results in this paper suggest substantial wage returns to even very limited work experience. The results are large when compared to non-experimental estimates that rely on variation in potential experience. However, making direct comparisons to the non-experimental estimates is difficult given the lack of variation in the amount of experience acquired for those induced to work by the experiment. The impacts are also large relative to experimental estimates of job training programs, which typically find modest effects at best (Heckman, Lalonde, Smith, 1999; Kluve, 2006). However, in this paper I study a very different context where the returns to experience may be significantly larger due to the scarcity of skills. Also, unlike most job training programs in developed countries, experience in this context is actually targeted to relatively skilled individuals, and individuals possibly gain general skills.

The paper is organized as follows. Section 2.2 provides background information both on related literature, relevant aspect of Malawian urban labor markets where this study is conducted and a description of the intervention. Section 2.3 describes the data used and Section 2.4 presents the empirical strategy. Section 2.5 presents and discusses the results. Section 2.6 concludes.

2 Context and data

2.1 Malawi: Education, experience and earnings in wage employment

Like much of Sub-Saharan Africa, the majority of Malawians depend primarily on subsistence agriculture. Internal migration to urban centers is high and rising (HDR, 2009), however. The trend towards urbanization means that understanding wage growth is particularly important in order to inform labor policies targeted to the growing urban labor force.

Previous studies of the return to education in Malawi estimate wage increases of between six and ten percent per additional year of schooling (Chirwa and Zgovu, 2001; and Chirwa and

Matita, 2009). These estimates of returns to each additional year of schooling are consistent with relatively high point estimates of the effects of completing primary, secondary and tertiary (Psacaharopoulos and Patrinos 2002; Castel, Phiri and Stampini, 2001). One study has estimated the Mincerian return to experience for Malawi, finding that every additional year of potential experience is associated with a wage increase of approximately five percent (Chirwa and Matita, 2009). A five percent return to each year of experience is high relative to the marginal value of education in other countries; King, Montenengro and Orazem (2012) review Mincerian estimates of the return to experience from 122 datasets across 86 developing countries and find estimates between -1 and 4.25 percent per additional year of experience.

However, using potential experience as a measure of accumulated experience has been widely criticized, particularly in labor markets where there is high job turnover and general employment instability. Light and Ureta (1995) use work history data from the United States to show that specifications using cumulative experience and potential experience produce misleading estimates of the returns to tenure and experience in the United States. Using potential experience to measure work experience is particularly flawed in low-income countries due to high rates of grade repetition in school; exit and re-enrollment in schooling; and long spells of unemployment (Lockheed, Verspoor, et al. 1991; Lam, Ardington and Leibbrandt 2011; and Pugatch, 2013).

In this paper, I exploit the experimental variation from a randomized controlled trial conducted in urban Malawi discussed in greater detail in Godlonton (2013). The exogenous variation in work experience generated by that experiment provides the opportunity to examine the causal impact of a short term work opportunity on later labor outcomes.

2.2 Experimental variation

This paper makes use of the exogenous variation in work experience generated by a randomized controlled trial that offered individuals undergoing a real recruitment process a probabilistic chance of an alternative employment opportunity. Individuals were assigned a 0-, 1-, 5-, 50-, 75- or 100-percent chance of alternative employment in the event that they failed to secure employment through the recruiter's competitive hiring process. Thus, the probabilistic job guarantee provides a lower bound on the probability that an individual had the opportunity for employment at the conclusion of the recruiting process. The randomization was stratified by ability and prior experience with the recruiter. The alternative employment opportunity offered the same duration and wage as the standard employment offer from the recruiter. Individuals were still able to earn a job through the recruitment process by performing well during the job training, and those who secured both jobs were required to take the recruiter's job or turn down both job offers. Given that the recruiter's job and the alternative jobs were of equal duration and paid the same wage, those who became employed through the project acquired the same amount of work experience at the same pay whether they ultimately worked for the recruiter or in the alternative job. Estimation of the effect of the probabilistic job guarantee must account for the fact that the probabilistic jobs increased the likelihood of both being selected for the recruiter's job and being eligible for the alternative job (see Godlonton, 2013 for details).

The work experience acquired is short term. The job provided individuals with five days of paid work experience. The recruiter's job was for employment as an interviewer. The alternative jobs were different research assistant tasks, including archival research, data entry, and translation and transcription of qualitative interviews. Many of these tasks may embody some real acquisition of new and transferable skills for the participants. Upon completion of the job, participants a generic letter of reference.

Once the recruitment process was completed, the probabilistic chances of employment were realized. For individuals assigned a 1-, 5-, 50- or 75 percent chance of an alternative job; random draws were conducted. For example, an individual assigned a 75-percent chance of an alternative job drew a token from a bag that contained 75 red tokens and 25 green tokens. If the individual drew a red token then he was offered the alternative job; if he drew a green token, he was not. Similar draws were conducted by each individual, with token distributions adjusted for his randomly-assigned probabilistic treatment groups. Individuals assigned a 0-percent chance knew with certainty they were not eligible for alternative jobs and those assigned a 100-percent chance knew they were guaranteed alternative jobs, so no draws were conducted in those cases.

I use the treatment assignment (i.e. the probability of an alternative job) to instrument for acquired short term work experience. This unusual random determination of employment allows a unique opportunity to measure the causal effect of short term work experience on future labor market outcomes.

3 Data

Figure 2.1 outlines the timeline of the data used in this paper. The sample of respondents is drawn from a recruitment process hiring male interviewers, during which trainees also participated in an experiment that offered randomly determined probabilistic jobs. Data come from a baseline survey collected prior to the start of the recruitment process, administrative records about treatment assignment and employment realizations for both probabilistic alternative jobs and hiring by the recruiter, and a follow-up survey that was conducted nine months after the completion of the work opportunities presented by the experiment.

Baseline data: Prior to the start of the recruitment process, respondents completed numeracy and literacy tests and submitted their resumes. Using the numeracy and literacy scores I construct an ability measure. In addition to this information a baseline survey was conducted.

The baseline survey collected information on basic demographics, general education and work experiences, as well as mental and physical health. The baseline survey was self-administered by respondents.

Probabilistic alternative job offers: I use both the assignment to treatment records, as well as the realization of the probabilistic draws (i.e. whether or not each participant was actually offered a job, conditional on the distribution he was randomly assigned to). Assignment to an employment probability was stratified by baseline ability quintile and prior experience with the recruiter. In Godlonton (2013) it is shown that the treatment assignment is balanced; in other words, there are no systematic differences in covariates between the different treatment groups.

Follow-up survey data: A follow-up survey was conducted nine months after the implementation of the experiment. While the reference period for the survey questions is the nine months following the completion of the work experience opportunities, some participants erroneously report work tied to the experiment. To deal with this survey recall error, I exclude the first month of recall data and rely only on the eight month period beginning one month after the completion of the work experience opportunities. The follow-up survey was conducted by phone and included an extensive module on job search, labor market perceptions (current and future likelihood of finding employment), current employment and employment experiences over the last eight months, current and past wages as well as a mental health module.

Table 2.1 shows that attrition was not statistically significantly associated with the treatment status. A total of 84.7 percent of the sample was successfully interviewed at follow-up. The attrition rate was lowest among participants who had received the 75-percent job guarantee (7.1 percent). Individuals assigned a 0-percent chance of an alternative job have the highest rate of attrition (18.9 percent). The difference in attrition between these two groups, although large, is

not statistically significant (p=0.168). Moreover, the probability of receiving an alternative job does not predict the probability of being interviewed at follow-up (coeff. = 0.049, p-value = 0.433).

Table 2.2 shows that there is not differential attrition for other baseline characteristics including age, education, ability and previous work experience (Column 5). Respondents of the Ngoni tribe and those that had worked in the six months prior to baseline are slightly less likely to attrit (significant at the 5 percent level and 10 percent level respectively). However, these differences are not large in magnitude. Moreover, there is no systematic differential attrition by treatment status (i.e. the probability of the alternative job) that is correlated with baseline characteristics. To test this, I regress an indicator for being in the follow-up sample on the probability of being assigned an alternative job, the baseline characteristic of interest, and that probability interacted with the baseline characteristic (Appendix Table C.1).

The final analytical sample includes the 227 respondents found at follow-up. The average respondent in this sample is approximately 26 years old and 17.2 percent are married. Approximately 16.7 percent of the sample have at least one child, and of those that do have at least one child they have an average of 1.8 children. Respondents are relatively well educated for Malawi with an average of 13 years of education, but this is driven by the eligibility criteria of the recruiter which required individuals to have at minimum completed their secondary school education. Despite being relatively well-educated for Malawi all these men were actively seeking work at the time of the baseline sample and they reported earnings of only approximately $210 per month over three months prior to the experiment (Table 2.2, Column 2).

4 Empirical strategy

If experience was randomly assigned across individuals, then we could estimate the average treatment effect of experience on employment and wages using ordinary least squares (OLS). In that case, one would estimate the following regression equation:

$$y_i = \alpha + \beta_1 JO_i + X_i'\delta + \varepsilon_i \quad (1)$$

where y_i = employment (or wages) for individual i, T_i is a dummy indicator for whether or not the individual received a job, and X_i is a set of individual characteristics. However, in this setting work experience was not itself randomly assigned. Instead, individuals were randomly assigned different probabilities of obtaining work experience. These probabilistic job guarantees affected their likelihood of obtaining experience from one of two different types of jobs – the recruiter's job and the alternative job. I therefore implement an instrumental variables approach. The system of equations then estimated is:

$$Y_i = \alpha_0 + \beta_1 AnyJO_i + X_i'\delta + \varepsilon_i \qquad (3)$$

$$AnyJO_i = \pi_0 + \pi_1 P1_i + \pi_1 P5_i + \pi_1 P50_i + \pi_1 P75_i + \pi_1 P100_i + X_i'\varphi + \varepsilon_i \qquad (4)$$

where JO_i measures whether individual i was offered a short term job; $P1_i$, $P5_i$, $P50_i$, $P75_i$, $P100_i$ indicates the binary indicators for the treatment arms; and X_i represents a set of covariates. The set of covariates used is the same as those used in equation (2) and listed above. I also include stratification cell fixed effects to account for the fact that treatment assignment was stratified by ability and prior work experience with the recruiter. The key coefficient of interest is β_1. Y_i measures labor market outcomes of interest to examine both the intensive and extensive margins. To examine changes at the extensive margin I measure the impact the probability of being employed nine months after the experiment, and the fraction of months in which individuals are employed in the eight months following the intervention. To measure impacts at the intensive margin, I examine the average daily wage earned by individual i across that the eight month

period. I allow for possible heteroskedasticity in the error terms by using heteroskedastic-robust standard errors.

For the probability of assignment to the alternative job to serve as a valid instrument for work experience, it needs to satisfy two conditions: i) the instrument must be correlated with the endogenous variable; ii) the probabilistic job offers must not affect later labor market outcomes except through the acquired work experience.

The first condition implies that assigned probability of alternative employment should predict whether or not the job-seeker acquired any job (recruiter or alternative job) through this intervention. Estimating the first stage relationship shows that the instrument is, indeed, relevant:

$$AnyJO_i = \pi_0 + \pi_1 P1_i + \pi_1 P5_i + \pi_1 P50_i + \pi_1 P75_i + \pi_1 P100_i + X_i'\varphi + \varepsilon_i \qquad (2)$$

In the equation above, $AnyJO_i$ is defined as a binary indicator equal to one if the respondent either received a randomly determined job or a recruiter's job. I use indicator variables for each of the treatment arms. $P1_i$ equals one if the individual received a one-percent probabilistic chance of a job, and $P5_i, P50_i, P75_i,\ and\ P100_i$ are similar indicator variables for the 5-, 50-, 75- and 100-percent treatment arms. The omitted category is the group who received no chance of an outside job. X_i represents a set of covariates and includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

Table 2.3 presents the first stage estimates. The first stage results show that the probabilistic jobs strongly predict the probability participants received any job (recruiter or

alternative). This expected result derives mechanically from the assignment of alternative jobs, as well as through a behavioral response by participants to the job guarantees. As shown in Godlonton (2013) the probability of being hired by the recruiter was higher among those who received the 75- or 100- percent chance of an alternative job, likely because the improved outside option lowered stress and increased performance during the recruiting process. Both mechanisms work in favor of a higher probabilistic job guarantee causing a higher chance of subsequent employment. Table 2.3 Column 1 confirms this hypothesis. A total of 16.3 percent of individuals assigned a zero chance of an alternative job got a job. Individuals assigned a 1- or 5-percent chance of an alternative job are not more likely than those who were assigned a 0-percent chance to get any job. The coefficients are positive as predicted, though the standard errors are large. Individuals assigned a 50-, 75- and 100- percent chance of an alternative job are respectively 40.2, 56.8 and 83.7 percentage points more likely to get any job than those with no chance of the alternative job. The first stage F-statistic is 101.11, far above the rule of thumb threshold for weak instrument concerns. These results are robust to the inclusion of stratification cell fixed effects (column 2) and additional covariates (column 3).

The exogeneity condition for the IV strategy requires that, conditional on baseline characteristics, the probabilistic job offers do not affect later employment outcomes independently of acquiring a job through the experiment (recruiter or alternative). Monotonicity would have been violated if higher probabilistic job offers had reduced the likelihood of acquiring the recruiter's job. However, as shown in Godlonton (2013) this is not the case. In fact, individuals assigned a 75- or 100 –percent chance of an alternative job were about twice as likely to be hired by the recruiter as those who were not eligible at all for alternative jobs. A second concern is that the probabilistic job offers may have affected individuals' perceptions about their

own ability to find employment. Results in Godlonton (2013) show that there is no effect of the probabilistic job offers on perception of ones' own likelihood of employment.

A third concern is that the probabilistic job offers affected skill acquisition during training, and that skill was subsequently rewarded by the labor market. The finding in Godlonton (2013) that individuals perform differentially on recruiter administered training tests during the recruitment process may initially heighten that concern. However, it is unlikely that there were general benefits to this training. The training conducted by the recruiter and evaluated in the performance tests was tailored to the specific needs of that particular recruiter's temporary job, interviewer positions for a health survey. Participants worked systematically through the questionnaire the recruiter planned to administer, in order to understand the terminology of and instructions for filling in each item. Participants were given systematic explanations about how to interpret questions, but the training was very specific to the survey in question. Skills related to this particular questionnaire are highly firm-specific and are unlikely to be marketable to the labor market. Moreover, for the training to have an impact in the labor market the differential performance of the participants needs to be observable to future employers. Individuals did not receive their grades on these assessment tests and letters of reference only described the nature of the job but not the employee's specific performance. As such, the only way for the differential performance during training to affect subsequent employment and earnings in the outside labor market after the intervention is for outside employers to value the specific content of the training conducted by the recruiter during the experiment. Given the nature of the recruiter's training, this is unlikely.[1] Generally, in this

[1] I restrict the analysis by excluding those assigned the 100-percent treatment group; and those assigned the 0-percent treatment group. These sub-groups show that the results are slightly smaller and in some cases lose statistical significance which is not surprising as the sample sizes are small. These estimates also show that the results are not eliminated by dropping either of these groups which suggests that the results are not driven by differential learning (results not shown).

context when individuals apply for a new interviewer position even within the same firm they still are required to undergo the same training for each new survey as the content of each training and skills taught are specific to that survey. In other words, experienced and novice interviewers undergo the same training for each survey they work on.

Conditional on instrument validity, β_1 captures the local average treatment effect (LATE) of the short term job on labor market outcomes – employment and wages.

5 Results

Work experience may affect employment at the extensive margin, by changing the probability of employment, and the intensive margin, changing wages conditional on employment. In this section, I use the variation generated by the experiment to study the return to experience at each of these margins.

5.1 Returns to experience

Table 2.4 presents the impact of the short term work experience on job search, employment, and the concurrent number of jobs held. This table uses data aggregated by individual across the eight month post-intervention time period. The employment variable used is the probability of employment during this timeframe. This is constructed by calculating the fraction of months that the individual is employed over the eight months following the intervention. Similarly, the job search variable is defined as the average probability an individual actively sought work (whether or not they were employed). Again, like the employment variable this is constructed as the fraction of months an individual actively sought work in the post-intervention period. The measure of concurrent number of jobs held is constructed as the average number of concurrent jobs held during the last eight months.

Work experience increases the probability of employment by all three measures. The short term work experience provided by the experiment increased the probability of subsequent by 10.6 to 13.9 percentage points. The estimated coefficients increase in magnitude and precision when we include stratification cell fixed effects (column 2) and covariates (column 3). The estimated effect is large, representing a 25 to 33 percent increase in the probability of being employed. To explore the time dynamics behind the average effect estimated in Table 2.4, Figure 2.2 plots the estimated employment impacts of the job separately for each of the eight months following the intervention. Although the one-month estimates are imprecise, the effects are positive in each of the eight months and statistically different from one another.

Work experience also increases the probability of searching for a job (column 4) and the number of concurrent jobs held (column 7). These estimates are robust to including controls for stratification cell fixed effects and covariates.

Another margin along which employment may adjust is the number of days worked. Underemployment in Malawi is high, and there is plenty of scope to increase labor supply along the intensive margin. Data from a nationally representative household survey shows that urban men who have completed secondary school, the relevant comparison group for the experimental sample, work only 23.4 hours per week conditional on being employed. The follow up survey uses the standard labor supply survey instrument (2010/2011 IHS), so it measures hours of work rather than days of work in the past week. While I cannot measure the change in days of work, I can examine the change in the number of hours worked, and compute the implied average wage per hour. These results are also presented in Table 2.5. I find that among the employed, individuals are working approximately 40 percent more hours per week. In the local context, however, individuals are more likely to be able to adjust their labor supply at the daily than

hourly margin, and they are paid per day rather than per hour. It is probably more accurate to interpret differences in hours as indicative of differences in the responsibilities of the job. Therefore, the results for hourly wage should be interpreted with caution. These estimates and show no statistically significant impact on the hourly wage (Table 2.5 columns 4 through 6). The magnitude of the coefficient indicates an increase of $0.72 per hour which is large in magnitude but it is not statistically significant.

Before turning to the mechanisms behind the increase in employment, Table 2.5 explores the impact of work experience on wages. The outcome measure is the individual's average daily wage over the eight-month follow up period. This measure is not conditional on employment, so periods when the individual is unemployed are included (as zeros) in the average. Daily wages – rather than the hourly wages used in much of the related literature – are the relevant unit in this context. Institutionally, all Malawian labor policies pertain to daily employment; for example, the minimum wage law is with respect to daily wages, not hourly wages. Daily or even more highly aggregated wages are also salient to respondents. The follow-up survey allowed individuals to choose the time unit for reporting their wages, with, 75.8 percent of respondents reporting monthly wages and 18.5 percent reporting daily wages. Therefore, while the literature about employment in developed countries uses hourly wages as the primary outcome of interest, daily wages are a more appropriate measure in this context.

Table 2.5 shows that individuals who gained work experience as a result of the experiment earn $3.80 to $4.19 more per day (Columns 7 through 9). This estimated effect is large relative to the average daily wage of approximately $5.08 among the control group. The estimated impact represents a 75 to 83 percent increase in daily wages. As we did with the extensive margin effects, we can also consider the effect on wages separately for each of the eight months in the

follow up period. Month-by-month estimates are plotted in Figure 2.3. In all months, the effect on daily wages is positive; it ranges between approximately one and six dollars.

The estimated wage impacts are surprisingly large and deserve further discussion. First, these results are not conditional on being employed; the outcome measure incorporates periods of unemployment as wages of zero. Therefore, part of the increase in wages is attributable to the gains in employment as shown in Table 2.4. Logged wages drops the unemployed, these results are present in Table 2.5 columns 10 through 12. The positive wage results persist, but are as expected the estimated coefficients are smaller in magnitude. However it is still large - the impact on the daily wage is 60 to 67 percent. These large point estimates are not driven by outliers. Figure 2.4 documents the wage distributions for those who did and did not receive a job and shows that the wage distribution among those who received a job is shifted to the right.

5.2 Mechanisms

Understanding the mechanisms may be helpful in reconciling the effects in this experiment with the much smaller effects estimated from non-experimental Mincerian estimates in Malawi and other settings. I find that only five days of work experience results in a 57 to 63 percent increase in subsequent earnings. This is equivalent to approximately ten years of experience in the Malawi non-experimental estimates (Chirwa and Matita, 2009). There are many reasons why the non-experimental estimates may be substantially smaller. First, the non-experimental study also uses an inferior measure of work experience. Potential experience overstates the amount of accumulated experience (considerably) in this context. Second, the type of experience studied by the experiment may be of higher quality than experience otherwise available to even educated Malawian men. While the experience provided through the experiment was short term, it was

with a private, international employer. It is unlikely that five daysworth of work in the civil service will yield impacts similar to that observed here. Finally, the non-experimental estimates represent average returns to experience for a population that is less educated than the highly-skilled men included in the experiment. While the experimental subjects still experience frequent periods of unemployment, they may experience substantively different returns than a less educated counterpart.

There are many theoretical reasons to expect that experience (even short term informal work experience) leads to increased employment and wages. In this section, I discuss a number of these possibilities and discuss which might be most relevant in the current context. The particular mechanisms that I consider include changes in job search strategies or occupational choice; changes in contract type, altered social networks; skills acquisition; altered wage expectations; and human capital accumulation. The experimental setting was not designed to test these mechanisms directly. However, I present suggestive evidence against the backdrop of these outlined mechanisms, before turning an exploration of heterogeneity in the return to experience.

Shifts in occupation

One possibility is that individuals change their occupation if they are induced to receive a job. Using the retrospective calendar job histories, I classify each job according to the standard two-digit ILO occupation classification codes (using the ISCO-08 classification system). I then analyze employment in each industry separately, using three measures of occupation-specific employment. The first is a binary indicator for whether each individual ever worked in a given occupation. The second is the total number of months the respondent worked in each occupation. The third indicator is a binary for the respondent's modal occupation over the eight month follow up period.

In Table 2.6, each row reports the effect of work experience on employment in a separate occupation from an IV regression. The left hand panel corresponds to the binary ever-worked outcome; the middle panel is the number of months in the occupation; and the right hand panel is an indicator for modal occupation, as described above. Increased work experience as a result of the experimental variation caused increases in employment in the following occupations: administrative and managerial; and clerical and related worked. The same pattern is observed for the modal occupation held. Individuals were also more likely to have recent experience as professional, technical or related occupations but this pattern does not hold for the modal occupation. For clerical and related occupations the effect is large large, with the 13.1 percentage point increase in the probability of working in clerical or related occupations representing a 62 percent increase in the probability of employment in that field. Individuals appear to be switching from agriculture related, service and production and related occupations, but stronger claims are limited by the lack of statistical precision.

Employment contract type

Another mechanism through which experience may have affected wages is by altering the type of wage contract individuals secured after the intervention. Jobs vary in their duration, and short term positions are common in Malawi. I do not directly observe the duration of contracts in the follow up survey, but I can use information from the unit in which individuals reported their current job as a proxy for contract duration. Individuals self-reported the unit of payment for their current (primary) job at the daily, weekly, fortnightly or monthly level. I infer that lower-frequency reporting levels correspond to longer duration contracts, and construct a frequency of payment variable equal to one if the individual reports daily remuneration, two if weekly, three if fortnightly and four if monthly remuneration is reported. Table 2.7 reports effects of work experience on this proxy for job permanence. The estimated impact of work

experience on payment frequency is -0.7. Individuals induced to receive work experience through the experiment appear to be working in less permanent positions. In this context, the change is consistent with higher wages, because wages for short term positions as research assistants or consultants on projects for international NGOs or donor agencies are often much higher than wages paid for the longer-term work offered by local employers or government agencies.

Social networks

Social networks have been touted as an important mechanism through which individuals acquire employment opportunities.[2] There are several theoretical reasons for why social connections are important in accessing employment. For the job-seeker, social connections can reduce search costs and lead to better quality matches (Calvo-Armengol, 2004; Mortensen and Vishwanath, 1994; Galeotti and Merlino, 2009).

Simply participating in jobs provided by this experiment may have facilitated new social connections between participants. These social connections may increase employment opportunities independently of the experience accrued. Unlike the experiments undertaken by Beaman and Magruder (2012) and Beaman et al. (2013) that are specifically set up to test various aspects regarding the role of social connections in job referrals, this experiment was not designed to induce variation in social connections or to test specific manner in which social connections might matter. However, I do measure the prevalence of social interactions that may have facilitated employment, such as whether individuals heard about job opportunities through individuals they met during the job opportunity, and whether the jobs they held during the eight month period following this job opportunity were a direct result of a referral.

[2] See for example Beaman (2010) and Granovetter (1973).

Table 2.8 panel A shows that individuals who received work experience as a result of the experiment are 23.4 percentage points more likely to have heard about a work opportunity through someone they met during this intervention. However, while individuals claim to hear more about job opportunities, they are not more likely to secure employment through one of the new connections. Individuals are 12.6 percent less likely to report securing a job through someone they met during this intervention, but the estimate is not statistically significant at conventional levels.

In sum, while the broadened network does suggest a modest impact on information about job opportunities, this information does not translate into employment and therefore does not explain the effect of experience in this experiment.

Signaling

Another mechanism is signaling of worker quality to employers (Spence, 1973). In this case it is possible that employers do not infer any inherent value of the work experience on worker productivity, but merely interpret it as a signal of ability. Upon completion of the work experience all participants received a standard letter of reference, which described the job in general terms but did not provide information about individual-specific performance. Given that these letters came from an international employer, however, employers may value the letter as a signal of underlying ability, rather than certification of skills acquired through experience.

Table 2.8 panel B shows that those who received work experience as a result of the experimental treatment were actually 7.4 percentage points *less* likely to use the reference letter than to those who did not receive a job.[3] Therefore, employers would not have received any signal about worker ability from the reference letters, and these letters are unlikely to have

[3] Individuals who received work in the alternative job and those who worked for the recruiter received reference letters as such it is possible that individuals who did not receive the randomly determined job used a reference letter. However, the large difference is not too surprising as a low fraction of those who received no alternative job offer worked for the recruiter, and therefore did not receive any reference letter that could be used for this purpose.

contributed later labor market outcomes. However, it may still be possible that individuals put the work experience on their resume and this acts as a signal of ability.

Wage expectations

The job may have altered individuals' wage expectations and reservation wages, with implications for job search strategies, duration of unemployment, and match quality. The wages paid during this experiment may have been higher than reservation wages at baseline. If individuals updated their expectations by increasing their reservation wage, then the estimated impact on the employment effect might be muted, as individuals may be searching longer and differently for better paying jobs.

I examine this mechanism by looking at self-reported reservation wages. Table 2.8 Panel C presents the results from this exercise. The impact of receiving a job on the monthly reservation wage is $121.25, but it not statistically significant at conventional levels. More generally, the reported reservation wages are high, approximately 1.5 times higher than the average monthly income earned at baseline. Self-reported reservation wages also high relative to wages reported in the follow up survey. Transforming reported wages into full-time equivalent salaries with the assumption that individuals worked 20 days per month, then the average monthly wage earned at follow up was approximately $240, higher than at baseline but considerably lower than the reported reservation wage. While measurement error in the reservation wage complicates the interpretation of these results, there is no evidence that an increase in reservation wages is an important mechanism.

Human capital accumulation

A final potential mechanism is that individuals acquired skills attributable to the work experience induced by the experiment. Individuals who secured a job either worked as an

interviewer or were assigned to data entry; data transcription or translation; or archival research jobs.

The results discussed in section 2.5.2 and presented in Table 2.6 show a change in occupational type. Individuals who received work experience are less likely to be employed in agriculture and more likely to be employed in clerical activities. Furthermore, individuals are 18.1 percentage points more likely to report having worked as a research assistant, the specific occupation in which they acquired experience. This is suggestive evidence that the work experience provided through the experiment generated occupation-specific skills that were rewarded by future employers.

While the data do not permit a direct test of the mechanism through experience increases which wages and employment, the indirect evidence suggests individuals may have acquired skills that are rewarded by the external labor market.

Heterogeneity

Understanding heterogeneous returns to work experience can help us interpret the large average effects and design policies to use work experience to improve employment outcomes. I explore heterogeneous returns by ability, work experience and education. To do so, I interact an indicator variable for having received an alternative job (JO_i) with the baseline characteristic of interest ($Base_i*JO_i$), using the set of treatment dummies as instruments for work experience. In this specification I instrument the endogenous regressors with the probability of an alternative job and this probability interacted with the baseline characteristic. Therefore, to examine the heterogeneity of the impacts I estimate the following set of equations:

$$Y_{it} = \alpha_0 + \beta_1 JO_i + \beta_2 Base_i + \beta_3 (JO_i * Base_i) + X_i'\delta + \varepsilon_{it} \qquad (5)$$

$$JO_i = \pi_0 + \pi_1 P_i + X_i'\varphi + \varepsilon_i \qquad (6)$$

$$(JO_i * Base_i) = \pi_2 + \pi_3 (P_i * Base_i) + X_i'\gamma + \varepsilon_i \qquad (7)$$

where: $Base_i$ is, in turn, the baseline ability score as determined by numeracy and literacy tests; a binary indicator for having completed college; and measures of current and cumulative labor market work experience.

Table 2.9 Panel A examines the heterogeneity of impacts by ability. To measure ability, I use test scores from a numeracy and literacy test administered to the respondents at baseline. I use a composite measure of ability that combines the numeracy and literacy test scores.[4] The estimated impacts are larger for individuals at the lower end of the ability distribution. To see this, consider an individual at the 25th percentile and the 75th percentile of the ability distribution. Individuals at the 25th percentile were 25 percentage points more likely to be employed if they were induced to receive job experience through the experiment, and they earn approximately $11.01 more per day. On the other hand, individuals at the 75th percentile were 1.5 percentage points less likely to be employed, though they earn approximately $2.20 more per day.

Figure 2.5 plots the average post-treatment employment rate by month for low and high ability types. Individuals are classified as low ability if they scored below the mean on the composite literacy and numeracy test; and as high ability otherwise. The small sample limits the precision of the estimates by ability level, but the pattern is informative. The estimated impact on employment for low ability types is increasing over time, while there is no consistent pattern for the high ability types. The pattern for wages is relatively constant across the time period (not shown). This pattern of results suggests that the low ability types not only gain the most from the job but also that the employment returns are increasing over time.

Education and experience can serve as substitutes or complements in a Mincerian model. To examine the relationship in this context I consider heterogeneity by whether or not the

[4] The results are similar when using the numeracy and literacy scores separately.

respondent has a degree (Table 2.9 panel B). Due to sample restrictions imposed by the recruiter, the sample is composed entirely of individuals who have completed secondary schooling. Therefore, there is limited variation in educational attainment. The results show that the estimated impacts are largest for those without a university degree and are actually negative for those who have completed university.

Lastly, one possible reason that the estimated impacts are so large is that the experience provided in the experiment is the first job held by respondents. Table 2.9 Panels C and D explore the heterogeneity of the impacts with respect to work experience. Panel C uses recent job market attachment defined as whether the respondent was working a month prior to baseline; and Panel D uses an indicator for whether the individual has ever worked. Roughly 15 percent of the sample had no previous work experience. Perhaps surprisingly, the effects of work experience on subsequent employment do not differ by pre-experimental work experience.

6 Conclusion

This paper uses a novel experiment that generated exogenous variation in short term work experience in order to estimate the effect of such experience on employment in wages. The return to experience is large, with a 10.6 to 13.9 percentage point increase in post-intervention employment for those who received experience through the experiment relative to those who did not. Not only does experience increase the probability of being employed, but also, it has a sizeable effect on wages. Individuals who received work experience earn approximately 60 to 67 percent more per day than those who did not, with results concentrated among lower-ability individuals. This return to work experience is present in each of the eight months of the follow up period, and the average effects are larger than in previous estimates of the returns to

experience in Malawi and other settings. Individuals shifted away from agricultural based occupations and into clerical and related work; they worked more hours per day and on contracts with shorter durations.

These results add to the policy debate about active labor market programs, which are designed to improve employment outcomes by providing participants with work experience. Proponents of work based programs believe that any job is a good job, and that getting a job will lead to job advancement and wage growth (Holcomb et al., 1998). However, the empirical evidence provides mixed results. In systematic reviews of the literature, the key take away is that the impact of job-training programs are modest at best (Heckman, Lalonde, Smith, 1999; Kluve, 2006). However, just like the returns to education, the impacts of such programs might be larger in low income countries. Betcherman, Olivas and Dar (2004) review the literature about impact evaluations of job training programs and find only 19 studies (none of which are in Africa) conducted in developing countries. In both this review and in another, by Nopo and Saavedra (2003) of the non-experimental literature in Latin America, the estimated impacts of job training programs appear to be larger in developing than developed countries.

The results may not be generalizable to a less skilled population within Malawi, or to a country whose underlying skill distribution and labor market conditions are different from Malawi. Even within Malawi, the treatment provided in the experiment is not available through any current public or private sector job training initiatives. Because the job opportunity provided within the experiment was of uniform duration, we cannot extrapolate from these results to the return to a longer period of experience. Lastly, the general equilibrium effects of such a program are not estimated. Given the small size of this intervention, it is not possible to determine if and the extent such a program if rolled-out would have on those individuals not participating. It is not

clear if non-participants would be crowded out of the labor market or whether the returns are driven by increases in wages earned through entrepreneurship activities which would result in a net increase in employment.

While these caveats cannot be dismissed, the results presented here do provide the first experimental evidence about the effect of work experience on subsequent employment outcomes in a developing country. The effects are substantial, suggesting that short term training or employment programs that include work experience have transformative potential, and providing justification for further research on the topic.

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Figure 1: Timeline of experiment, and data collection activities

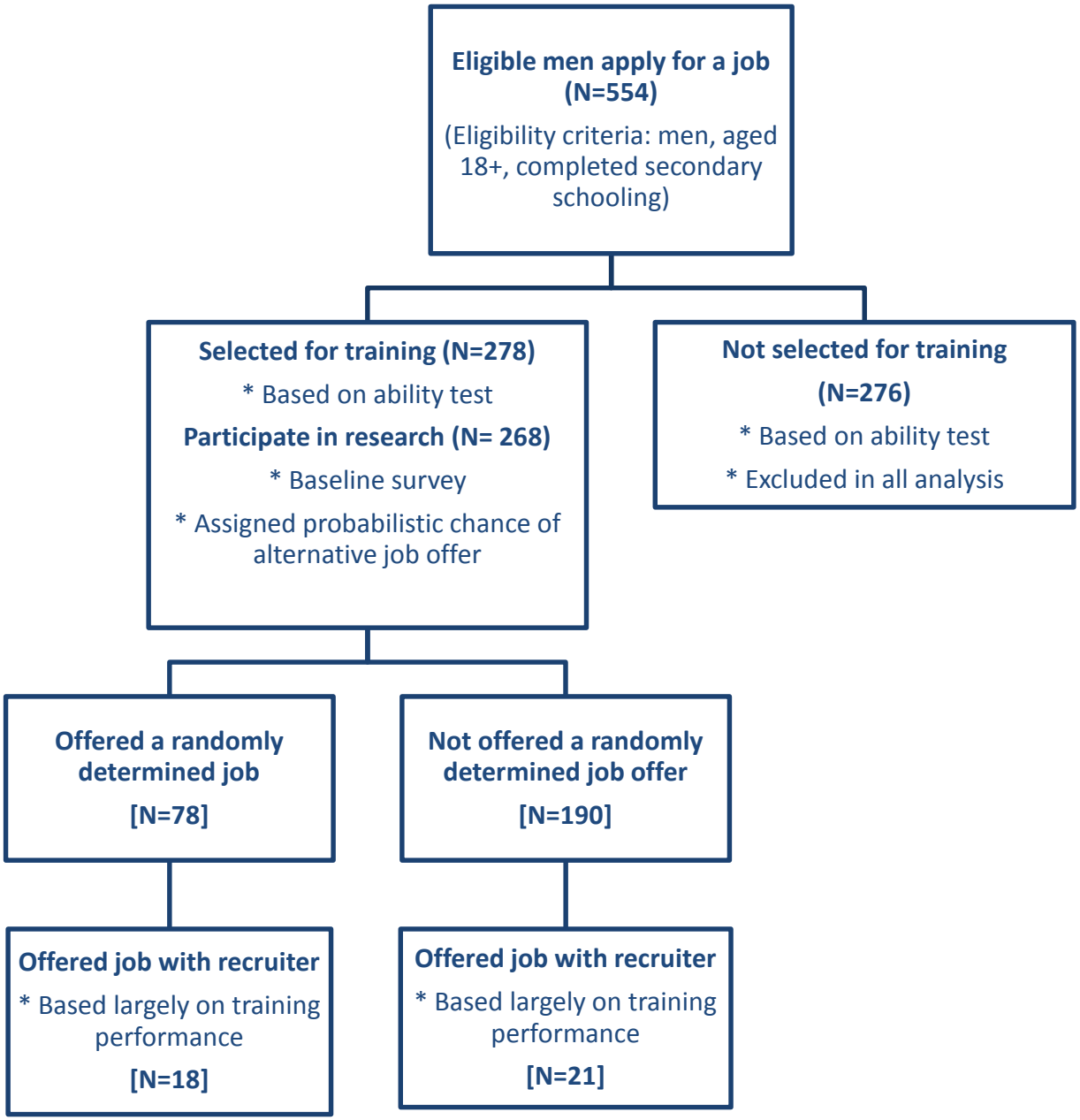

Figure 2: Estimated employment impact of job offer by month (IV estimates)

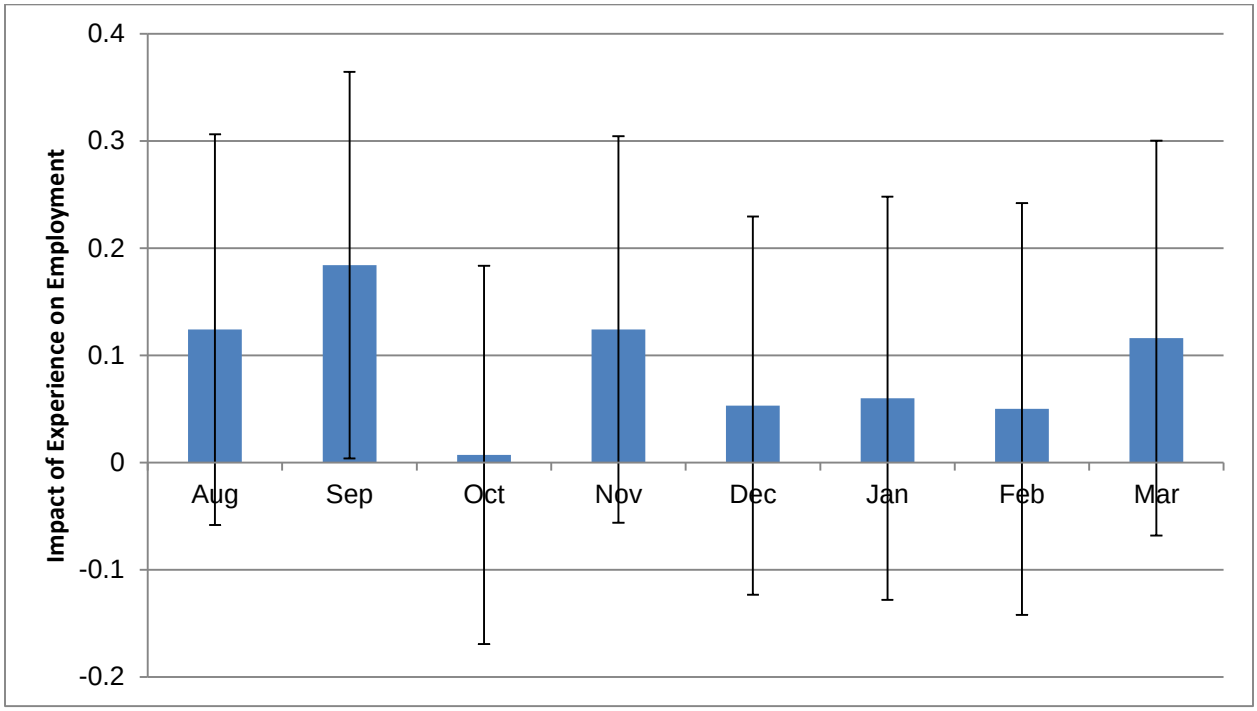

Figure 3: Estimated wage impact of job offer by month (IV estimates)

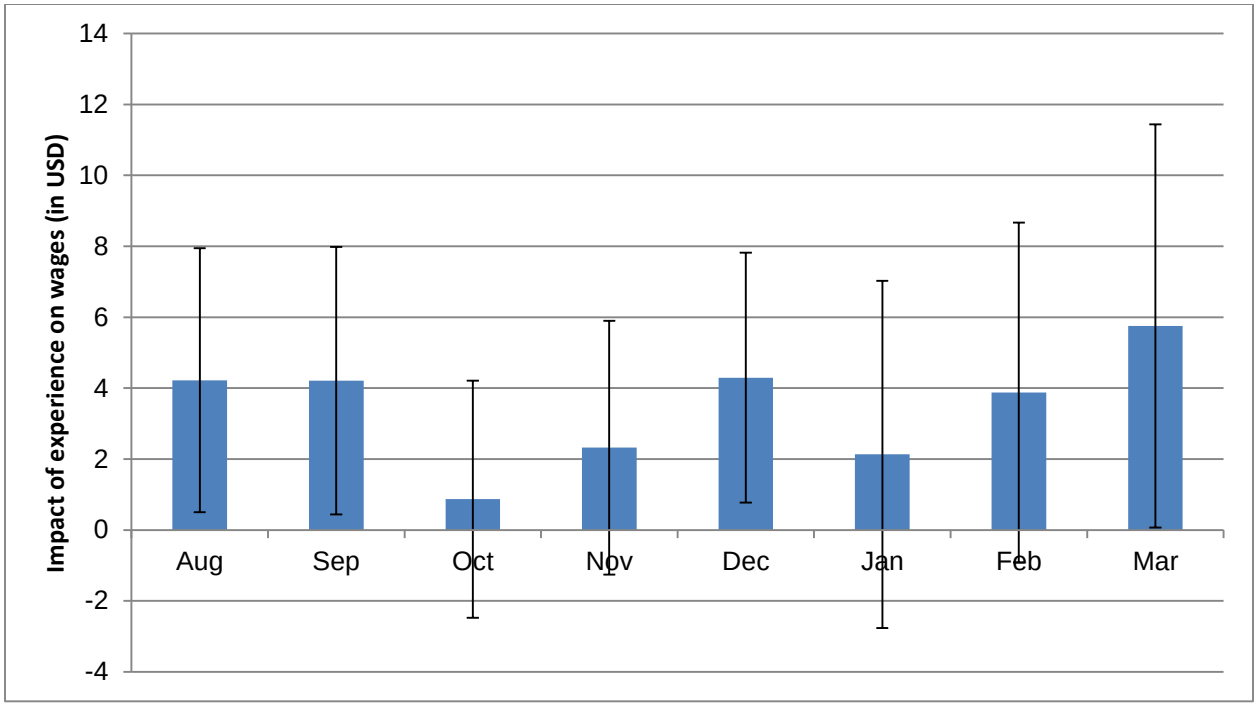

Figure 4: Distribution of wages

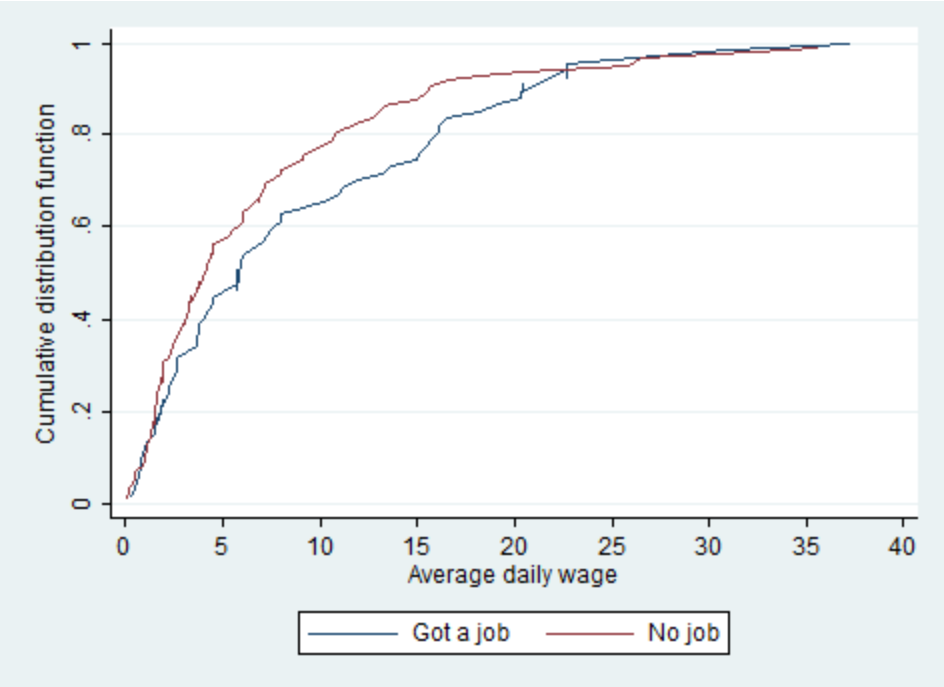

Figure 5: Estimated employment impact by ability of job offer by month (IV estimates)

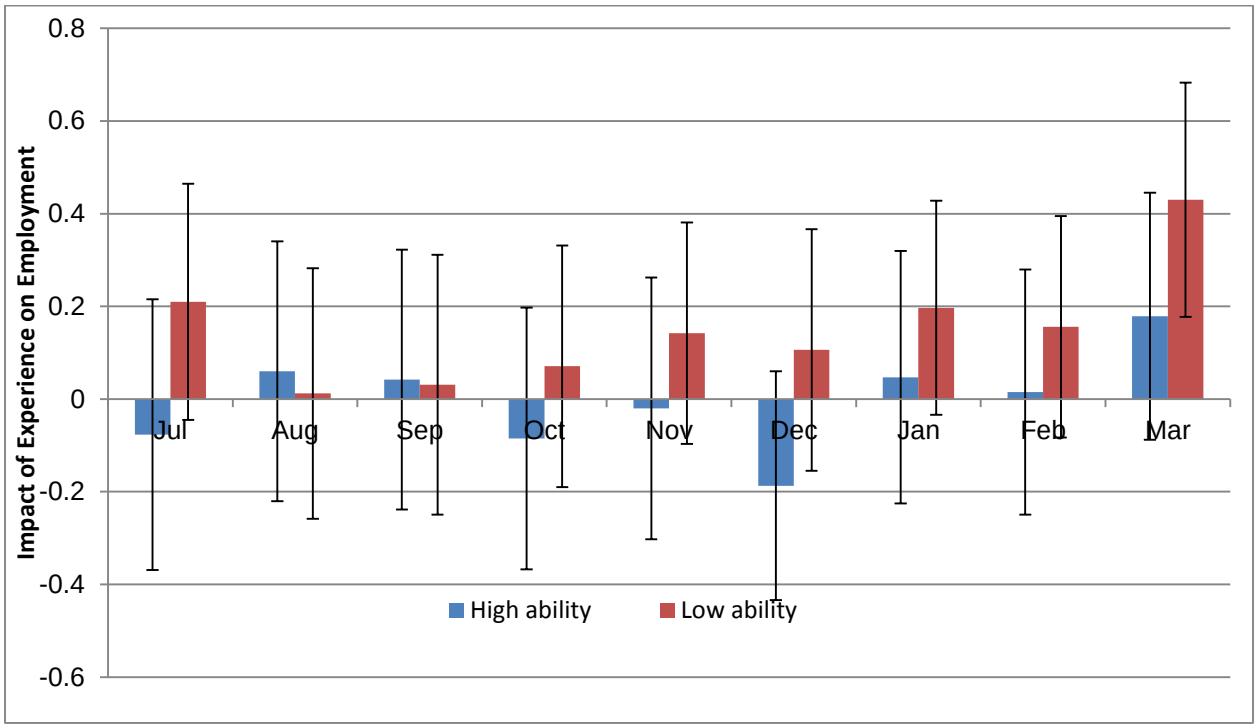

Table 1: Sample size and attrition

Treatment conditions:	N (1)	Mean (2)	SD (3)
0% Probability	53	0.811	0.395
1% Probability	56	0.857	0.353
5% Probability	52	0.827	0.382
50% Probability	54	0.852	0.359
75% Probability	28	0.929	0.262
100% Probability	25	0.840	0.374
Full sample:	**268**	**0.847**	**0.361**

p-value of F-test of joint significance:

0% = 1% = 5% = 50% = 75% = 100% 0.827

p-values of t-tests of pair-wise differences:

	1%	**5%**	**50%**	**75%**	**100%**
0%	0.510	0.826	0.564	0.168	0.745
1%		0.666	0.939	0.396	0.844
5%			0.724	0.233	0.882
50%				0.364	0.893
75%					0.376

Notes:

Individuals were assigned to one of the six treatment groups. If they received a 0-percent chance of an alternative (i.e. in 0% probability treatment group) then they had no chance of receiving the alternative job. If they were assigned to the 1% probability group then they had 1 percent chance of receiving an alternative job. Similarly for the 5-, 50-, 75- and 100 percent probability groups. There were twice as many assigned to the high probability groups as compared to the lower groups due to budgetary considerations. The p-values denote the p-value associated with the F-test of whether the mean finding rate is the same in all treatment groups or in the case of the table the pair-wise t-test of differential attirion rates.

Table 2: Sample and Attrition

	Baseline N=268		Follow-Up N=227		Difference (3) - (1)	
	Mean	SD	Mean	SD		
	(1)	(2)	(3)	(4)	(5)	
Demographics:						
Age	25.604	4.638	25.718	4.662	-0.114	
Married	0.172	0.378	0.172	0.378	0.000	
Any child?	0.164	0.371	0.167	0.374	-0.003	
Number of children	0.299	0.784	0.313	0.811	-0.014	
Number of fin dependents	7.959	9.355	8.264	9.406	-0.305	
Years of education	13.183	0.940	13.220	0.938	-0.037	
Income (USD, 3 months)	206.123	228.803	210.617	237.777	-4.494	
Ability score	-0.001	1.003	0.030	1.017	-0.031	
Tribe:						
Chewa	0.310	0.463	0.300	0.459	0.010	
Lomwe	0.108	0.311	0.110	0.314	-0.002	
Ngoni	0.164	0.371	0.181	0.386	-0.016	**
Tumbuka	0.190	0.393	0.189	0.393	0.001	
Other	0.201	0.402	0.198	0.400	0.003	
Education and Work:						
Ever worked?	0.869	0.338	0.863	0.344	0.006	
Ever worked with recruiter?	0.104	0.306	0.097	0.296	0.008	
Any work in last month	0.646	0.479	0.665	0.473	-0.020	
Any work in last 6 months	0.869	0.338	0.890	0.314	-0.020	*
Frac of 6 mths worked	2.657	2.176	2.727	2.175	-0.070	
Any job search last month	0.116	0.320	0.110	0.314	0.006	

Notes:

The baseline sample consists of 268 individuals who participated in the recruitment process and experiment discussed in Section 2. The follow-up sample (227 respondents) is the main sample used in this paper. The ability score is determined prior to the experiment. It consists of a numeracy and literacy component, and has been standardized.

Table 3: First Stage: Using dummy indicators for each treatment group to predict any job offer (recruiter or random job)

Dependent Variable:	**Job offer or recruiter's job offer**		
	(1)	**(2)**	**(3)**
1% Job Guarantee	0.025	0.030	-0.004
	[0.081]	[0.078]	[0.083]
5% Job Guarantee	0.047	0.045	0.038
	[0.085]	[0.079]	[0.085]
50% Job Guarantee	0.402***	0.423***	0.439***
	[0.094]	[0.090]	[0.093]
75% Job Guarantee	0.568***	0.543***	0.565***
	[0.105]	[0.104]	[0.108]
100% Job Guarantee	0.837***	0.860***	0.866***
	[0.057]	[0.055]	[0.067]
Constant	0.163***	0.804***	0.544
	[0.057]	[0.153]	[0.370]
Observations	227	227	227
R-squared	0.327	0.382	0.431
Stratification cell FE's	No	Yes	Yes
F-stat (of instruments)	101.11	87.47	76.79
Average of dep variable		0.361	

Notes:

The sample used here is the sample of 227 men found at follow-up. The zero percent chance of alternative employment treatment group is the omitted category in these regressions. The dependent variable "Got a job" is whether or not the individual received an alternative job offer. Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously. The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 4: Returns to Work Experience: Extensive Margin

Dependent Variable:	**Frac. months employed**			**Frac. months looked for work**			**Ave # concurrent jobs**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Got a job or recruiters job	0.106	0.128	0.139*	0.084	0.105	0.113	0.079	0.098	0.071
offer (IV)	[0.086]	[0.086]	[0.076]	[0.091]	[0.090]	[0.079]	[0.082]	[0.081]	[0.072]
Constant	0.376***	0.538***	-0.015	0.395***	0.520***	0.043	0.597***	0.754***	0.024
	[0.041]	[0.128]	[0.349]	[0.043]	[0.140]	[0.355]	[0.038]	[0.114]	[0.275]
Stratification cell FE's	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other covariates?	No	No	Yes	No	No	Yes	No	No	Yes
Observations	227	227	227	227	227	227	227	227	227
R-squared		0.087	0.282		0.085	0.279	0.019	0.047	0.249
Ave of dep variable (no job)		0.421			0.586			0.532	

Notes:

The regressions are IV estimates, where dummy indicators for the treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

The fraction months employed variable is calculated as the number of months the individual was employed over the last 8 months, divided by 8. Similarly, the fraction months looked for work variable is computed using a retrospective calendar history, and is calculated as the number of months the individual actively sought work over the last 8 months, divided by 8.Lastly, the average number of concurrent jobs is the average of the total number of jobs held each month across the 8 month period.

Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 5: Returns to Work Experience: Intensive Margin

Dependent Variable:	Ave hrs worked per week			Hourly wage			Ave daily wage (incl. Unemployed)			Log (Ave daily wage)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Got a job or recruiters job offer	-0.576	-0.567	-0.691*	5.463	5.836	7.854*	3.801*	4.191*	3.928**	0.668*	0.687*	0.605*
	[0.381]	[0.376]	[0.387]	[4.361]	[4.404]	[4.173]	[2.149]	[2.218]	[1.885]	[0.373]	[0.387]	[0.354]
Constant	3.223***	4.033***	4.720***	21.559***	14.082***	18.413	4.133***	10.784	-1.611	1.206***	0.621	0.216
	[0.185]	[0.433]	[1.139]	[2.112]	[4.321]	[15.205]	[0.864]	[7.161]	[5.779]	[0.173]	[0.559]	[1.135]
Stratification cell fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other covariates?	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	166	166	166	167	167	167	227	227	227	164	164	164
R-squared	0.029	0.069	0.154		0.035	0.199		0.045	0.251		0.036	0.262
Ave of dep variable (no job)		23.265			0.489			5.079			1.361	

Notes:

The regressions are IV estimates, where dummy indicators for the treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

The average daily wage is calculated using the restrospective job work history. The average daily wage is calculated as the average wage on the individual's main job in the last month. Columns 1 through 3, those who are unemployed are coded as 0's. Columns 4 through 6 uses the logged wage, therefore for individuals who earned $0 across all eight months are omitted.

Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 6: Shifts in occupations

Occupation:	Any job held in past 8 months: Avg dep var (no job) (1)	Coeff (2)	SE (3)	Num months in each occupation in past 8 months: Avg dep var (no job) (4)	Coeff (5)	SE (6)	Modal occupation in past 8 months Avg dep var (no job) (7)	Coeff (8)	SE (9)
Professional, technical, and related workers	0.368	0.158	[0.108]	1.515	0.661	[0.599]	0.475	-0.075	[0.142]
Administrative and managerial workers	0.007	0.052	[0.040]	0.007	0.279	[0.251]	0.000	0.083	[0.053]
Clerical and related workers	0.213	0.150	[0.106]	0.691	0.695	[0.485]	0.212	0.057	[0.125]
Sales workers	0.044	-0.015	[0.043]	0.096	0.117	[0.190]	0.030	0.009	[0.055]
Service workers	0.066	-0.053	[0.040]	0.419	-0.352	[0.259]	0.091	-0.056	[0.057]
Agriculture, animal husbandry, and forestry workers, fishermen, and hunters	0.066	-0.032	[0.038]	0.346	-0.130	[0.227]	0.081	-0.019	[0.052]
Production and related workers, transport equipment operators, and labourers	0.110	-0.029	[0.061]	0.471	-0.074	[0.284]	0.111	0.001	[0.079]

Notes:

The regressions are IV estimates, where dummy indicators for the treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

	Table 7: Contract type		
Dependent Variable:	**Unit of pay (1 = daily, 2 = weekly, 3 = fortnightly; 4 = monthly)**		
	(1)	**(2)**	**(3)**
Got a job or recruiters job offer	-0.576	-0.567	-0.691*
(IV)	[0.381]	[0.376]	[0.387]
Constant	3.223***	4.033***	4.720***
	[0.185]	[0.433]	[1.139]
Stratification cell FE's	No	Yes	Yes
Other covariates?	No	No	Yes
Observations	166	166	166
R-squared	0.029	0.069	0.154
Ave of dep variable (no job)		3.169	

Notes:

The regressions are IV estimates, where dummy indicators for the treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for The average daily wage is calculated using the restrospective job work history. The average daily wage is calculated as the average wage on the individual's main job in the last month. Columns 1 through 3, those who are unemployed are coded as 0's. Columns 4 through 6 uses the logged wage, therefore for individuals who earned $0 across all eight months are omitted. Ave hours worked per Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past

Table 8: Channels

	Avg dep var	Coeff	SE
Panel A: Social Networks:	**(1)**	**(2)**	**(3)**
Heard about a job opportunity	0.438	0.234**	[0.115]
# job opportunities	0.795	0.126	[0.268]
Secured a job opportunity	0.091	0.085	[0.056]
# job opportunities secured	0.080	0.077	[0.056]
Panel B: Signalling:			
Used any reference letter for a job in last 8 months	0.648	-0.074	[0.110]
Panel C: Wage Expectations:			
Self-reported monthly reservation wage	361.873	121.253	[84.563]

Notes:

The regressions are IV estimates, where dummy indicators for the treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.
Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported

Table 9: Heterogeneity of wage and employment impacts

Panel A: Ability Inteactions	**Frac. months worked**	**Avg daily wage (incl. unemployed)**	**Logged (Avg daily wage)**
	(1)	**(2)**	**(3)**
Got a job	0.119	4.443**	6.711**
	[0.074]	[1.820]	[3.137]
Ability score X Got job	-0.169**	-3.046*	-5.654*
	[0.079]	[1.773]	[3.038]
Ability score	0.099	-1.396	2.636
	[0.099]	[1.741]	[4.166]
Panel B: Degree interactions			
	(1)	**(2)**	**(3)**
Got a job	0.057	7.431*	1.545**
	[0.209]	[4.407]	[0.759]
Degree X Got a job	0.359	-21.501	-4.144
	[1.254]	[24.915]	[3.204]
Degree	-0.033	0.000	3.513**
	[0.000]	[12.706]	[1.645]
Panel C: Current labor attachment interactions:			
	(1)	**(2)**	**(3)**
Got a job	0.761	6.851	5.126
	[1.108]	[23.227]	[4.178]
Any work in last month X Got a job	-0.918	-4.075	-6.233
	[1.532]	[33.295]	[5.784]
Any work in last month	0.334	2.087	2.295
	[0.483]	[10.858]	[1.778]
Panel D: Any previous experience interactions:			
	(1)	**(2)**	**(3)**
Got a job	-0.233	9.528	8.844
	[0.478]	[9.609]	[9.874]
Ever worked X Got a job	0.530	-8.900	-11.214
	[0.742]	[15.287]	[13.496]
Ever worked	-0.189	4.898	4.136
	[0.277]	[5.617]	[4.853]

Notes:

The probability of alternative employment (P_i) and the interaction of the baseline characteristic and the probability of alternative employment assigned ($Base_i * P_i$) are used to instrument for the binary indicator JO_i and the interaction of the baseline characteristic and the job offer ($Base_i * JO_i$). The fraction months employed variable is calculated as the number of months the individual was employed over the last 8 months, divided by 8. The average daily wage is calculated using the restrospective job work history. Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 10 percent level. Robust standard errors are reported.

A Compensating Differentials Theory of Informal Labor Markets: Quantitative Model and Implications for a Developing Country*

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and

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Abstract

This paper develops a model of informal labor markets with search frictions, worker and firm heterogeneity, a comprehensive set of labor regulations, and explicit compliance decisions by workers and firms. Our implementation of intra-firm bargaining extends the literature by incorporating two sectors, firm heterogeneity, and minimum wages. In equilibrium, firms and workers self-select into the formal and informal sectors following a compensating differentials logic. The model does not resort to intrinsic differences across sectors and generates informality directly as a result of regulatory distortions, but it is still able to reproduce the main stylized facts associated with informal labor markets. The quantitative model is used to shed light on the reduction of informality observed in Brazil between 2003 and 2012 and to assess the effectiveness of alternative policies aimed at reducing informality. In the model, a progressive payroll tax reduces informality and unemployment while increasing government revenues.

Keywords: informality, labor market, search, compensating differentials, Brazil
JEL Codes: J24, J31, J46, J64, O17

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1 Introduction

Labor market informality has been a major policy concern in developing countries for several decades. In the end of the last century, the shadow sector was on the rise throughout the world, reaching over a quarter of GDP in most non-OECD economies (Schneider and Enste, 2000). Surprisingly, this trend was sharply reversed in some Latin American countries in the early 2000s, with informality rates among salaried workers in Brazil, Ecuador, Peru and Uruguay, among others, declining by one fifth or more by 2010 (Tornarolli et al., 2012). These shifts remain largely unexplained and cannot be easily accounted for by current models of informality.

The decline in Brazilian informality, amounting to 10.7 percentage points between 2003 and 2012, is particularly difficult to rationalize based on factors commonly stressed in the literature. While changes in labor legislation and payroll taxes were negligible, the minimum wage increased by 61% in real terms, more than twice the growth rate of GDP per capita. But some factors that do not feature prominently in either the policy or academic debates on informality – such as increased productivity, stricter enforcement of labor regulations, and changes in the composition of the workforce – may have played a role in this process. Still, there is no theoretical framework in the literature capable of simultaneously assessing the importance of these factors in determining the informality rate observed in a given labor market.

This paper develops a novel search and matching model of informality that allows for worker and firm heterogeneity, minimum wages, payroll taxes, mandated benefits, and explicit compliance decisions by both workers and firms. The model is used to analyze the evolution of informality in the Brazilian labor market from 2003 to 2012 and to assess the effectiveness of alternative policies aimed at reducing informality. In the model, workers can be either skilled or unskilled, and, when unemployed, search simultaneously for formal and informal jobs. Firms are heterogeneous in a skill-biased productivity parameter, in the sense that more productive firms are also more intensive in skill. Firms decide on whether to comply with labor regulations and then, at each instant, on how many skilled and unskilled vacancies to post. By not complying with regulations, firms avoid paying payroll taxes and are not subject to the minimum wage, but face an informality penalty that is increasing in firm size (representing the probability of being audited and the associated fine). The labor regulation also includes mandated benefits, which, from the perspective of employees, make formal jobs more valuable than informal jobs for a given wage. Finally, wages are set by intra-firm bargaining under non-binding contracts, such that changes in firm size may lead to wage renegotiation with all workers.

In constructing and solving this model, we make some methodological contributions to the literature. In order to accommodate labor heterogeneity and decreasing returns to scale within a search framework, we start from the intra-firm bargaining model proposed by Cahuc, Marque and Wasmer (2008) – who build on Stole and Zwiebel (1996*a*) – and extend it in three directions. First, we characterize an equilibrium where labor can move across different sectors: formal and informal. Second, we consider heterogeneous firms, with different productivity levels, as opposed to a single representative firm. And third, we incorporate a more realistic set of labor regulations, in particular, minimum wages. Incorporating minimum wages to the problem adds a non-trivial degree of complexity to the characterization of the solution. The numerical procedure we use to solve and calibrate the model can be used as a starting point for quantitative studies in similar contexts, where heterogeneity and labor regulations are

important.

The model leads to an equilibrium where firms and workers self-select into the formal and informal sectors, following a compensating differentials logic. Firms do not want to comply with labor regulations, but non-compliance becomes increasingly costly as firms grow. Workers want to receive employment benefits, but may be willing to accept an informal job and leave unemployment for a sufficiently high wage. The only labor market distortions are those introduced by labor regulations and the search and matching frictions. The marginal informal firm is technologically indistinguishable from the marginal formal firm, and skilled and unskilled workers employed in the two sectors are identical. So there is no sense in which firms and workers allocated to different sectors are intrinsically different, as the classic labor market segmentation hypothesis would suggest (see Cain, 1976). We try to understand several stylized factors associated with – and the evolution of – informality resorting only to regulatory distortions and to search and matching frictions commonly associated with the functioning of the labor market.

In a steady-state equilibrium, firms with lower productivity employ fewer workers and choose to operate informally. These firms also employ a lower fraction of skilled workers. In general, informal workers are compensated for the lack of mandated benefits by receiving higher wages, but this equalizing differentials condition can be broken by minimum wages. If the minimum wage binds for unskilled workers, then these workers strictly prefer to hold a formal job, but are willing to accept informal offers in equilibrium to avoid unemployment. In this equilibrium, the formal wage premium is decreasing in the skill level, becoming negative for skilled individuals. Average wages are higher in the formal sector due to workforce composition. Still, for skill levels for which the minimum wage does not bind, workers are indifferent between formal and informal employment.

We calibrate the model to depict the Brazilian labor market in 2003 and then examine whether it can explain the evolution of labor market outcomes from 2003 to 2012. The model is able to reproduce several stylized facts from the cross sectional distribution of workers across firms, and formal and informal sectors: size distribution of firms, wage patterns across and within sectors, and unemployment. We analyze the role of changes in tax rates, mandated benefits, enforcement of labor regulation, minimum wages, workforce composition, and aggregate productivity in explaining the trends observed in the past decade. By assessing the contribution of each of these factors one at a time, we verify that our comparative statics are in line with those of many other labor market models. For instance, increases in minimum wages or payroll taxes lead to more informality and unemployment, while increases in enforcement lead to less informality but more unemployment. A larger proportion of skilled workers is associated with a decline in the wage gap between the skilled and unskilled in both sectors, and also causes substantial reductions in unemployment and informality. Once all factors are accounted for, the model reproduces qualitatively all the patterns observed in the data and quantitatively explains 60% of the decline in informality observed in the period. The predicted evolution of unemployment and wages also matches the data with reasonable precision. We find that shifts in workforce composition are the most important factor behind the observed reduction in informality in Brazil: without increases in skill levels, the informality rate would have gone up by 4.8 percentage points instead of declining. Understanding the pattern of changes experienced by Brazil, besides being important on its own, has clear policy implications for many developing countries where labor informality is still prevalent and rising.

Following, we use the model to examine two policies that subsidize formal low wage employment as a means to reduce informality. In the first policy, the subsidy is implemented in the form of lower tax rates for low wage

positions, as in a progressive payroll tax. In the second, the subsidy is instead a direct government transfer to low wage formal workers, similar to a current policy adopted in Brazil (*Abono Salarial*). Our results show that the first alternative can improve labor market outcomes and increase government revenues, while the second is much less cost-effective. The reason behind the sharp contrast in outcomes of these apparently similar policies lies in the binding minimum wage. While a reduction in payroll taxes induces employers to create formal jobs, there are no incentives for employers under the second policy, since they do not benefit from the government transfer to workers if wages cannot adjust downward.

In addition to the quantitative exercises described above, the paper makes two conceptual contributions to the informality literature. The first is to show that both the cross-sectional and time-series variations in informality are consistent with a model in which informality is entirely due to the existence of labor market regulations, with no trace of duality or segmentation of any other nature. The second is to show that this same model rationalizes the widely documented reduction in the formal wage premium as we move along the wage distribution, becoming null or negative at the top. Many authors suggest that the heterogeneity in the formality wage premium indicates that the informal sector is composed of two distinct tiers. For the more productive workers at the top tier, informality is a matter of opportunity, which is reflected on their wages being equal to or higher than they would be in the formal sector. For the bottom tier, informality is strictly worse than formal employment, since informal workers earn lower wages and lack valuable mandated benefits.[1] In our model, the two tiers are clearly identified by the two skill levels, and the pattern of decreasing wage gaps results from the binding minimum wage for unskilled workers. To our knowledge, Araújo and Ponczek (2011) present the only alternative model that explains this pattern among salaried workers, using a one-to-one random matching model where there is asymmetric information and workers can take employers to court. Bargain et al. (2012) also develop a model to account for heterogeneity in income gaps between formal and informal sectors, but focus instead on self-employed workers.

To the best of our knowledge, this is the first search and matching model of informality that combines worker heterogeneity, firm heterogeneity, minimum wages, mandated benefits, payroll taxes, and explicit compliance decisions by both workers and firms. We build upon many models from the informality literature, but differ from them in important aspects. Boeri and Garibaldi (2007) propose a simple model where the most productive workers sort themselves into the formal sector. However, institutional details such as mandated benefits and minimum wages are missing, and the model does not allow workers to search simultaneously for both formal and informal jobs.[2] Albrecht, Navarro and Vroman (2009) avoids both problems, but assume strong structural differences between sec-

[1] Bargain and Kwenda (2011) find this pattern in fixed-effects models using data from Brazil, Mexico and South Africa. Botelho and Ponczek (2011) reach similar conclusion with Brazilian data under different specifications (also using panel data), and observe that the formal wage premium decreases as workers become older and more educated. Lehman and Pignatti (2007) find similar results for the Ukrainian labor market. The idea of a two-tiered informal sector goes back at least to Fields (1990). Günther and Launov (2012) develop an econometric model of selection to test the hypothesis of heterogeneity inside the informal sector. They find that there are two distinct groups in the informal sector in Côte d'Ivoire. Some of these authors, as well as others, have used the term "segmentation" to describe the bottom tier of the informal sector. By that, they mean that wages are not fully determined by individual productivity and compensating differentials. This interpretation, which is replicated theoretically in models such as Fields (1975), Rauch (1991) and our own, is different from the original concept of segmented labor markets, as described in Dickens and Lang (1985) or Cain (1976). In the case we discuss, increases in education (or, more generally, productivity) can lead every worker to better jobs, a view that contrasts with labor market duality. In addition, the significant flow of workers in and out of the informal sector, particularly among those with lower skills, undermines the hypothesis of strong non-economic barriers of entry to the so-called primary sector.

[2] In Boeri, Garibaldi and Ribeiro (2011), the authors add a minimum wage to a simplified version of the model in Boeri and Garibaldi (2007). This results in a change in the sorting behavior: the least skilled workers now find it more profitable to search for formal jobs instead of informal ones, increasing the average productivity in the informal sector. Still, the other limitations remain.

tors, with no compliance decision on the firm side. The informal sector is simply an exogenous subsistence sector where there is no wage dispersion, regardless of worker productivity.

The models in Ulyssea (2010), Bosch and Esteban-Pretel (2012) and Meghir, Narita and Robin (2012) have more sophisticated compliance decisions and are better equipped in institutional details, but forgo worker heterogeneity. Ulyssea (2010) still assumes structural differences between sectors, with imperfect substitutability between goods produced by the formal and informal sectors, less productive informal firms, and higher entry costs into the formal sector. In Bosch and Esteban-Pretel (2012) and Meghir, Narita and Robin (2012), formal and informal firms differ only in that the former must abide to labor regulations, while the latter face higher job turnover or informality costs related to firm size. From this perspective, lower productivity in informal firms is the result of self-selection, as opposed to structural differences between sectors.[3] The problem of the firm in our model also follows this interpretation. On the institutional side, Ulyssea (2010) and Meghir, Narita and Robin (2012) include some mandated benefits in the problem of the worker, focusing on unemployment insurance and severance payments. Finally, Meghir, Narita and Robin (2012) is the only model that explicitly accounts for minimum wages, but with very different implications since workers are homogeneous.

The remainder of this paper is organized as follows. Section 2 sets the background by describing some stylized facts from the Brazilian labor market and explaining why the recent increase in formalization is a puzzle under existing theories of informality. Section 3 presents the model and discusses some of its properties. Section 4 describes the calibration of the model. Section 5 uses the calibrated model to analyze the evolution of labor market outcomes between 2003 and 2012, and presents our policy experiments. Section 6 concludes the paper.

2 Empirical Context

The term "informality" is used to describe many different aspects of non-compliance with regulations. In this paper, we focus on the decision by firms and workers not to comply with labor law when contracting with each other, thus excluding self-employed and domestic workers from our analysis. We also follow the bulk of the literature and restrict our attention to urban informality.

In the Brazilian labor market, a salaried job position is considered formal if the worker's "labor card" (*carteira de trabalho*) is signed by the employer. This is the definition we use henceforth. An employee with a signed labor card is entitled to social security benefits, such as severance payments, pensions, and unemployment insurance, while her employer is obliged to pay social security contributions and payroll taxes. Appendix A contains a thorough description of the benefits available to formal workers and costs associated with formal employment in Brazil.

With a clear definition of informality, we turn to the data. First, we discuss some aspects of the Brazilian labor market as of 2003, the baseline year for our quantitative exercises. In particular, we highlight specific patterns that underlie our modeling choices. Following, we analyze the trend in labor informality up to 2012 and relate it to other changes in the labor market during the same period. Most of the data we use come from the Monthly Employment Survey (*Pesquisa Mensal de Emprego*, from now on PME), a household survey conducted

[3] This perspective is supported by the experiment in De Mel, McKenzie and Woodruff (2013) and also by other empirical evidence showing that firms change their compliance decision in response to changes in tax rates (Monteiro and Assunção, 2012 and Fajnzylber, Maloney and Montes-Rojas, 2011) or in the intensity of enforcement of labor regulation (Almeida and Carneiro, 2012).

Table 1 – Labor Market Outcomes, Brazil, 2003-2012

	Informality		Wage gap		Wage growth		Unemployment	
Sample	2003	2012	2003	2012	Formal	Informal	2003	2012
Whole workforce	28.4%	17.7%	-31.9%	-13.4%	13.1%	43.9%	12.6%	5.4%
By schooling:								
Less than 8 years	35.8%	25.9%	-20.2%	-11.8%	26.0%	39.3%	12.2%	4.5%
8 to 10 years	32.1%	23.6%	-21.1%	-10.5%	18.2%	33.9%	16.9%	7.4%
High school, college dropouts	24.0%	14.5%	-14.2%	-3.2%	1.6%	14.7%	13.4%	6.2%
College or more	17.3%	12.6%	-16.1%	10.8%	-12.3%	15.7%	4.3%	2.7%

Source: IBGE/PME, author's calculations.

Notes: Data is presented for October 2003 and October 2012. Informality is fraction of salaried workers in the private sector with a signed work card. Wage gap is the difference between informal and formal average wages as a fraction of formal wages. Wage gain is the relative increase in average wage from 2003 to 2012.

by the Brazilian Census Bureau (*Instituto Brasileiro de Geografia e Estatística*, IBGE) that collects information on workers and their employment status in the six largest metropolitan areas in Brazil. We concentrate on the period between 2003 and 2012 due to availability of data from the PME under a consistent methodology.

The average informal worker in Brazil earns a lower wage, is less educated, and works in a smaller firm than her formal counterpart. The first claim is evident from the top row in Table 1. While the average formal hourly wage was 4.83 Brazilian Reais in 2003 (around 1.60 US dollars), the average informal wage was 32% lower, at 2.67 Brazilian Reais. Table 3 also presents the distribution of workers across sectors, firm sizes, and educational categories. By comparing the totals along rows for each sector, the differences in average schooling become apparent: 40% of informal employees had less than 8 years of schooling, while the analogous number was less than 28% in the formal sector. The differences in firm size can be seen in the column totals. While only a minority (roughly 1/16) of formal employees worked in firms with 5 people or less, this fraction was over one third for informal employees. These stylized facts are consistent with many papers that discuss the empirical regularities of informality in the developing world, such as La Porta and Shleifer (2008) and Maloney (2004).

These aggregate patterns have been interpreted as evidence that informality is circumscribed to low-earning, unskilled workers, but a closer look at the data reveals that this assertion is not accurate. Table 1 shows that the informality rate among workers with college education is 17.3%, not dramatically lower than the overall rate of 28.4%. Moreover, informal workers with college education earn almost three times as much as the average formal employee. Note that these individuals are not self-employed professionals defaulting on taxes or social security contributions, since we have restricted our sample to wage earners. The table also suggest that there is no labor market segmentation in the traditional sense: as workers become more educated, they are more likely to be employed formally, and also more likely to receive higher wages if they stay in the informal sector. Finally, the fact that some informal firms are willing to pay high wages for skilled workers shows that the technology used by these firms displays significant returns to human capital, contradicting many depictions of labor market duality where informal firms are structurally different from formal firms.

But the idea that education encompasses all dimensions of skill relevant in determining an individual's success in the labor market is also misleading. To illustrate this point, Table 2 shows the distribution of wages in the formal sector by educational level. There is a wide dispersion in wages across all levels of schooling, with the exception of

Table 2 – Formal Wage Distribution by Schooling Levels and Workforce Composition, Brazil, 2003 or 2012 (when indicated)

	Formal wage as multiple of minimum wage					Fraction of workforce	
Worker education	$(0, 1.2]$	$(1.2, 1.5]$	$(1.5, 2]$	$(2, 5]$	$(5, \infty)$	2003	2012
Less than 8 years	18.7%	16.7%	26.9%	35.0%	2.7%	33.8%	20.9%
8 to 10 years	15.3%	14.6%	25.6%	40.2%	4.4%	20.1%	17.1%
High school, college dropouts	8.4%	9.4%	19.4%	47.3%	15.5%	33.6%	43.1%
College or more	0.5%	0.7%	2.2%	22.2%	74.4%	12.5%	18.9%

Source: IBGE/PME, author's calculations. Salaried workers only. Data from October 2003 and October 2012.

Table 3 – Educational Distribution of Workers by Sector and Firm Size, Brazil, 2003

	Formal workers, by size of employer				Informal workers, by size of employer			
Worker education	2 - 5	6 - 10	11+	Total	2 - 5	6 - 10	11+	Total
Less than 8 years	36%	30%	27%	28%	49%	37%	33%	39%
8 to 10 years	24%	23%	20%	20%	25%	23%	22%	23%
High school, college dropouts	37%	41%	42%	41%	24%	35%	36%	32%
College or more	4%	6%	12%	11%	2%	5%	9%	6%
Total	1,133	1,226	13,937	16,296	2,363	731	3,196	6,290

Source: IBGE/PME, author's calculations. Salaried workers only. Employer size is reported by the worker in the household survey. The percentage values sum to one along columns. Data from October 2003.

college or mode. For example, 15.3% of individuals between 8 and 10 years of schooling earn around the minimum wage, while 4.4% of this groups earns more than 5 times the minimum wage. For those with complete high school and college drop outs, there is still a fraction of 8.4% earning roughly one minimum wage, while 15.5% earn more than 5 times the minimum wage. Excluding the group with college education, wage dispersion seems to be almost as large within as across educational categories, despite the fact that average wages – and, therefore, skills – do increase with educational levels. So, even in the formal sector, schooling seems to be a relatively poor proxy for the overall level of skill, maybe because of heterogeneity in the quality of schooling or of other skills not directly associated with formal education.

We can use the data on firm size in Table 3 to infer the hiring behavior of firms in both sectors. Comparisons between different columns in the same sector show that, as firm size increases, the proportion of educated workers also increases. In other words, larger firms are more likely to have a higher fraction of educated workers. An important takeaway is that this pattern is observed for workers in both sectors, suggesting again that the technologies used by formal and informal firms, at the margin, are not substantially different.

Now we turn to the evolution of informality in Brazil since the 1990s. Figure 1 shows that the rate of informality was rising up to 2002, but then started declining sharply.[4] In Appendix B, we show that the decline was widespread in the economy and not driven by workforce reallocation (i.e., a movement of employment to sectors of economic activity that are intrinsically more formal). What makes this pattern intriguing is the observation that, while the upward trend has been credited to increasing costs of formal employment during the 1990s, these costs continued to rise even after the reversal.[5] In particular, the minimum wage increased dramatically throughout the period, accumulating real gains of 60% from early 1995 to the end of 2003, and another 61% between 2003 and the end of

[4] In Figure 1, we use data from the National Household Survey (PNAD) instead of the PME because of methodological changes in the latter survey in 2002.

[5] Barros and Corseuil (2001)explain how the 1988 Constitution significantly raised employment costs (payroll and firing costs and mandated benefits). Bosch, Goni and Maloney (2007) claim that these changes were the most important factor behind the increase in informality during the 1990s. We present a brief discussion of changes in labor legislation and tax rates after 2003 in Appendix A.

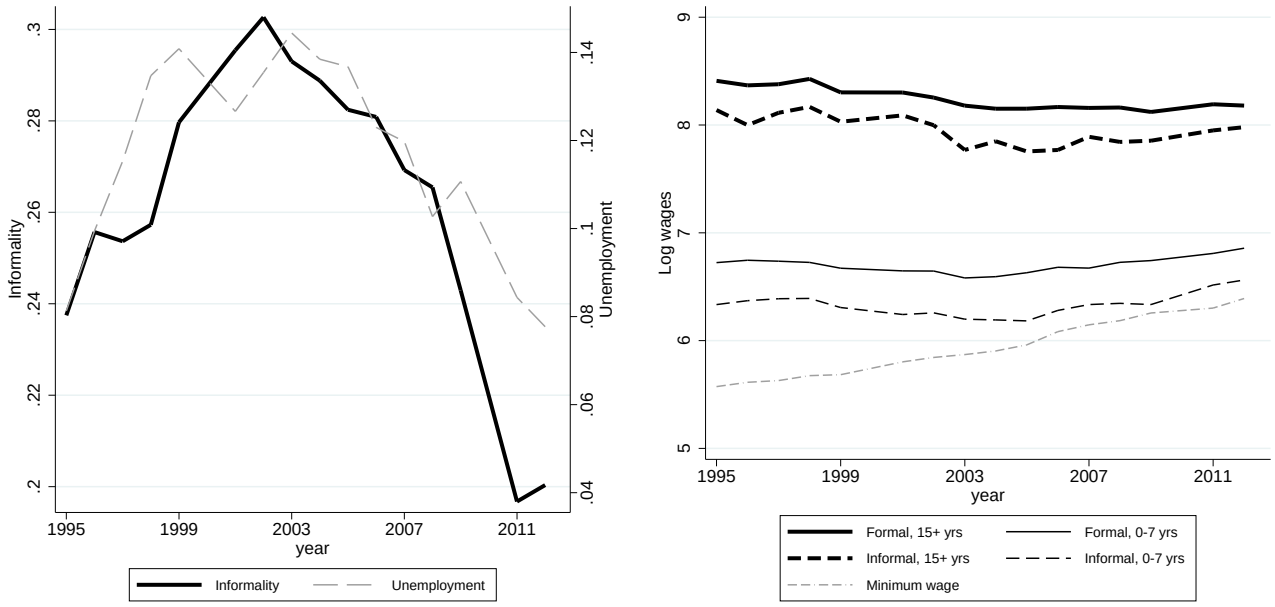

Figure 1 – Evolution of Informality, Unemployment and Real Wages for Salaried Workers, Brazil, 1995-2012
Source: IBGE/PNAD, author's calculations. The sample is restricted to the six metropolitan regions surveyed in the IBGE/PME.

2012.

Changes in overall productivity and in enforcement of regulation could explain part of the decline in informality, but they cannot account for other important shifts in labor market outcomes. The close relationship between the two series in Figure 1 suggests that informal employment has a counter-cyclical component, as proposed by Boeri and Garibaldi (2007) and Bosch and Esteban-Pretel (2012). In addition, there is some evidence that the enforcement of labor regulation in Brazil has become more efficient, a factor that could also bring down both the unemployment and informality rates.[6] However, it is difficult to reconcile the evolution of wage patterns in Table 1 with any of these hypotheses. While average wages increased in both sectors from 2003 to 2012, the gains accrued primarily to less educated workers. In addition, wage gains were larger in the informal sector, suggesting that the minimum wage was not the cause behind this heterogeneous pattern. It is hard to rationalize why an increase in overall productivity would not result in higher wages for the more educated workers, even more so if one considers the technological nature of recent productivity gains. It is even harder to conciliate these wage patterns with increases in enforcement: simulations in Bosch and Esteban-Pretel (2012) and Meghir, Narita and Robin (2012) predict that the gap between formal and informal wages should increase as a consequence of more enforcement, which is the opposite of what happens in the data.

The changing composition of the workforce, evident in the last columns in Table 1, may have contributed to the patterns described above, despite rarely appearing in the literature as an important determinant of informality. Two intuitive arguments can hint at this potentially important role. First, since informality is much lower among skilled workers, increases in the share of workers in this group would lead to a decline in informality simply due

[6] The effect of enforcement on unemployment is ambiguous in most models, and quantitative analyses show diverging results. While Boeri and Garibaldi (2007) and Ulyssea (2010) find that increased enforcement leads to higher unemployment, Bosch and Esteban-Pretel (2012) and Meghir, Narita and Robin (2012) reach the opposite conclusion.

to a compositional effect, abstracting from general equilibrium considerations. In fact, Mello and Santos (2009) and Barbosa Filho and Moura (2012) find that changes in workforce composition, particularly in skill level, can statistically account for a major part of the reduction in informality rates from 2002 to 2007. Second, the reduction in the wage gap across schooling levels is consistent with increases in the relative supply of skilled workers.

3 The Model

In this section, we develop a continuous time model of labor markets with search frictions, firm and worker heterogeneity, informality, a minimum wage, and mandated benefits. There is a continuum of measure 1 of infinitely-lived, income-maximizing workers with identical preferences. Workers can be either skilled or unskilled, and the fraction η of skilled workers in the population is exogenous. There is a measure m of firms and all firms are risk-neutral profit maximizers. They use both types of labor in producing the single consumption good in the economy.

In our model, the compliance decision refers to labor informality, not firm informality. Although these concepts are highly correlated in the data, there are some important differences which are reflected in our modeling choices. For instance, we focus on payroll taxes, leaving taxes over sales and profits outside of the model. Moreover, we do not consider the possibility of an intensive margin choice of labor informality within firms, as proposed in Ulyssea (2011). Instead, firms make one single formality decision encompassing all of their job relations. From now on, we use the term "informal firm" or "formal firm" to refer to establishments that offer informal or formal jobs, respectively.

Before describing the model in detail, we first provide a sketch of its basic logic. There are four aggregate variables that are taken as given by firms and workers and pinned down by equilibrium conditions. The first two are labor market tightnesses for skilled and unskilled workers, θ_s and θ_u. These variables are important for firms and workers because they determine the probability that vacancies posted by firms are filled, and, accordingly, the probability that unemployed workers find a job. The other two variables are the values of unemployment for skilled and unskilled workers, U_s and U_u. These are the outside option of workers when bargaining, and so are important determinants of wages. The bargained wage is, for each firm, a function of the number of workers currently employed, as firm size affects the marginal productivities of the different types of worker. The problem of the firm is then to choose a vacancy posting strategy – or, equivalently, firm size – conditional on its specific wage function and on its compliance decision, made at the beginning of time. Workers accept or reject the offers they receive from firms and bargain over wages. An equilibrium is found by determining the values of θ_s, θ_u, U_s and U_u that are consistent with the aggregate behavior of firms and workers.

3.1 Labor Markets

We model search frictions following Pissarides (2000). There are two separate labor markets, one for each skill level. Firms need to post vacancies in order to find workers, paying an instantaneous cost ξ for each vacancy. The number of matches taking place at each moment is given by a matching function $M(V_i, u_i)$, where V_i and u_i are the measures of open vacancies and unemployed workers in job market $i \in \{s, u\}$, for skilled and unskilled workers, respectively. We make the standard assumptions that $M(\cdot)$ is increasing in its arguments, concave and has constant returns to

scale. This enables us to use the more convenient form $q(\theta_i)$ for the instantaneous probability of a vacancy being filled. This means that, over a short time interval dt, the probability that a vacancy gets matched to an unemployed worker is $q(\theta_i)dt$. θ_i is the labor market tightness in market i, that is, the ratio of vacancies to unemployed workers: $\theta_i = \frac{V_i}{U_i}$, $i \in \{s, u\}$. The probability that an unemployed worker finds a job in a small time interval dt is given by $\theta_i q(\theta_i)dt$.

We make no distinction between formal and informal firms in the search process. The aggregate $V_i = V_i^{for} + V_i^{inf}$ is the sum of all vacancies posted by formal and informal firms, and unemployed workers search simultaneously in both sectors. After a worker is matched to a vacancy, the probability that this vacancy is offered by a formal firm is given by $\phi_i = \frac{V_i^{for}}{V_i}$, which is simply the fraction of vacancies posted by formal firms in the market i. With this assumption, as with many others, we try to minimize the structural differences between formal and informal sectors, and focus instead on the regulatory asymmetries. Our modeling of the search process is most similar to that in Bosch and Esteban-Pretel (2012). Other models with undirected search, such as Ulyssea (2010) and Meghir, Narita and Robin (2012), assume exogenous differences in the matching technology across sectors.

3.2 Problem of the Firm

All firms share the same production function $F(k, n_s, n_u) = F^k(n_s, n_u)$, assumed to be continuous and twice differentiable, where n_s and n_u denote units of skilled and unskilled labor employed as inputs. The term k is an exogenous productivity parameter distributed across firms according to a distribution function $G(k)$. We assume that $F^k(\cdot)$ is strictly concave in (n_s, n_u) for any k in the support of $G(k)$, and increasing in k. Moreover, we assume that $\sigma_{k,n_s} < \sigma_{k,n_u}$, where $\sigma_{i,j}$ denotes the partial elasticity of substitution between inputs i and j (see Hamermesh, 1993). So, as k increases, so does the marginal productivity of skilled workers. The parameter k is most easily interpreted as entrepreneurial talent, as in Lucas (1978), with the idea that entrepreneurs cannot efficiently manage a large number of skilled workers if they are not highly talented themselves. A second interpretation is that k refers to the firm's capital endowment, assumed to be fixed due to financial frictions or other adjustment costs. In this case, the assumption about the partial elasticities of substitution corresponds to capital-skill complementarity. This second interpretation seems more reasonable over short periods of time, but looses appeal when long intervals are considered.

Due to search frictions, firms cannot directly choose the amount of labor inputs employed in production. Instead, the control variable is the number of vacancies posted at each instant, $v_s(t)$ and $v_u(t)$. Firms also decide on whether to comply with labor regulations or not. For simplicity, we assume that this decision is taken at the beginning of time and cannot be changed thereafter. If a firm complies, it must pay taxes τ over its total payroll. If a firm chooses instead to hire workers informally, it avoids payroll taxes but incurs in an informality penalty $\rho(n)$, where n is the total number of workers hired by the firm. We assume that $\rho(n)$ is strictly increasing and convex. As in Meghir, Narita and Robin (2012), we do not specify how the informality penalty emerges. In general, it can be seen as the product of the probability of being caught by labor inspectors and the monetary value of the corresponding sanction. It can also encompass the lack of access to some public goods available to formal firms, such as courts.

Skill-biased productivity and the informality penalty are the ingredients behind the aggregate differences between sectors in equilibrium. First, the penalty induces larger firms to formalize. Since larger firms are the most productive

ones, it follows that the formal sector has higher average productivity due to selection. Finally, due to skill bias in productivity, there is a higher proportion of skilled workers in formal firms. Still, there are skilled workers employed in the informal sector.

Normalizing the price of the final good to 1, the instantaneous profit function of the firm with productivity k, according to its compliance decision j, is

$$\pi^{k,j}\left(n_s,n_u,v_s,v_u\right)=\begin{cases}F^k\left(n_s,n_u\right)-(1+\tau)\sum\limits_{i=s,u}n_iw_i^{k,for}\left(n_s,n_u\right)-(v_s+v_u)\xi, & \text{if } j=for, \text{ and}\\ F^k\left(n_s,n_u\right)-\sum\limits_{i=s,u}n_iw_i^{k,inf}\left(n_s,n_u\right)-\rho\left(n_s+n_u\right)-(v_s+v_u)\xi, & \text{if } j=inf,\end{cases}$$

where $w_i^{k,j}(n_s,n_u)$ is the wage that the firm pays to workers of type i, according to its compliance status j, and the current number of employees, n_s and n_u, and ξ is the cost of posting a vacancy, assumed to be the same across types of workers and sectors (again, in order to minimize structural differences between sectors). We describe how the wage function $w_i^{k,j}(n_s,n_u)$ is determined in the next subsection. From left to right, instantaneous profits are given by total production minus total payroll, payroll taxes (in the case of formal firms) or the informality penalty (for informal firms), and the costs of vacancy posting.

Job relations are destroyed at exogenous separation rates s^{for} and s^{inf}, which depend on the compliance decision. This captures the empirical fact that informal firms have a much higher labor turnover than their formal counterparts.[7] The dynamics of labor quantities inside each firm are given by

$$\dot{n}_i=v_iq\left(\theta_i\right)-s^jn_i,\ \text{with } i\in\{s,u\} \text{ and } j\in\{for,inf\}.$$

The instantaneous variation in the number of workers of type i is equal to the number of vacancies multiplied by the probability of each vacancy being filled, minus the rate of job destruction in that firm. In this equation, we implicitly assume that every match turns into a job relation. Later in the paper we show that, in equilibrium, all job offers are accepted.

We are ready to state the problem of the firm, which we choose to present in its recursive Bellman formulation:

$$\begin{aligned}\Pi^k &= \max_{j\in\{for,inf\}}\Pi^{k,j}\left(n_s,n_u\right),\ \text{with}\\ \Pi^{k,j}(n_s,n_u) &= \max_{\{v_s,v_u\}}\left(\frac{1}{1+rdt}\right)\left\{\pi^{k,j}\left(n_s,n_u,v_s,v_u\right)dt+\Pi^{k,j}(n_s^+,n_u^+)\right\}\\ &\text{s.t.}\quad n_i^+=n_i+\dot{n}_idt=\left(1-s^jdt\right)n_i(t)+v_iq\left(\theta_i\right)dt,\quad i=s,u.\end{aligned}\tag{1}$$

For a firm with productivity k and a given compliance decision j, the total present value of profits is the sum of instantaneous profits earned at the end of the small time interval dt plus the present value of profits after dt. The

[7] See the turnover analysis in Gonzaga (2003) and Bosch and Maloney (2010), and also the calibration results in Bosch and Esteban-Pretel (2012) and Meghir, Narita and Robin (2012). The existence of high dismissal costs in the formal sector provides strong incentives for keeping an employee. Albrecht, Navarro and Vroman (2009) formally develop this argument, using a search and matching model with endogenous job destruction and an informal sector. Moreover, as mentioned in the introduction, our target equilibrium is the one in which the minimum wage is binding for unskilled workers, who strictly prefer formal employment. Thus, the formal employees also should have stronger incentives to maintain the job relation. It would be interesting to use a model with endogenous separation rates, but, in our setting, we do not believe that the gains would offset the additional analytical complexity.

discount rate r is the same for all firms. Given its initial conditions and productivity, the firm makes the compliance choice that maximizes total profits.

Denote by $J_i^{k,j}(n_s, n_u)$ the marginal value of an additional worker of type i in a firm of type k, with compliance status j: $J_i^{k,j}(n_s, n_u) = \frac{\partial \Pi^{k,j}(n_s,n_u)}{\partial n_i}$. We find the first order conditions for the firm's problem in Appendix C. From now on, we restrict attention to steady-state solutions where the numbers of workers of different types are constant in each firm. By imposing $\dot{n}_i = 0$ in the F.O.C.'s, the expressions simplify to:

$$(r+s^j)J_i^{k,j}(n_s,n_u) = \begin{cases} F_i^k(n_s,n_u) - (1+\tau)\left[w_i^{k,for}(n_s,n_u) + \sum_{l=s,u} n_l \frac{\partial w_l^{k,for}(\cdot)}{\partial n_i}\right] & \text{, for } j = for \\ F_i^k(n_s,n_u) - \rho'(n_s+n_u) - \left[w_i^{k,inf}(n_s,n_u) + \sum_{l=s,u} n_l \frac{\partial w_l^{k,inf}(\cdot)}{\partial n_i}\right] & \text{, for } j = inf, \text{ and} \end{cases} \tag{2}$$

$$J_i^{k,j}(n_s,n_u) = \frac{\xi}{q(\theta_i)}, \tag{3}$$

with $F_i^k(n_s,n_u) = \frac{\partial F^k(n_s,n_u)}{\partial n_i}$.

Equation 2 is an intuitive description of the marginal value of a worker as the discounted sum of expected rents, taking into account the discount rate r and the separation hazard s_i. The instantaneous rent is given not only by the difference between marginal product and wage, but also by the effect of this additional employee on the wages of all other workers currently employed by the firm, due to changes in marginal productivities. At the time of the hiring decision or bargaining, vacancy costs are sunk and thus do not appear in this expression.

Equation 3 is the optimality condition in a steady state. Its interpretation is straightforward: the value of the marginal worker must be equal to the expected cost of hiring another worker, which is the cost ξ per vacancy multiplied by the expected number of vacancies needed to hire a worker. By combining both expressions, we can find an equation similar to the standard first order condition of the firm, in which marginal product equals marginal costs:

$$\underbrace{F_i^k(n_s,n_u)}_{\substack{\text{Marginal}\\ \text{productivity}}} = (1+\tau)\underbrace{w_i^{k,for}(n_s,n_u)}_{\text{Own wage}} + (1+\tau)\underbrace{\sum_{l=s,u} n_l \frac{\partial w_l^{k,for}(\cdot)}{\partial n_i}}_{\substack{\text{Effect on other}\\ \text{workers' wages}}} + \underbrace{(r+s_i)\frac{\xi}{q(\theta_i)}}_{\text{Hiring costs}}.$$

The case for informal firms is analogous, just omitting the payroll tax τ and adding the marginal effect of n_i on the informality penalty $\rho(n_s + n_u)$.

3.3 Wage Determination

Wage is determined through Nash bargaining, with workers and firms sharing the rents created by the match. The share of the surplus appropriated by a worker is given by the exogenous parameter σ, which corresponds to the bargaining power of workers. Differently from the standard model in Pissarides (2000), we do not assume homogeneous labor nor constant returns to scale in the production function, and allow workers and firms to engage in renegotiation after the initial match. As discussed in Stole and Zwiebel (1996*a*), these assumptions imply

that changes in firm size lead to wage renegotiation due to changes in marginal productivities, and this must be anticipated by firms in their hiring decisions. We follow the solution developed by Cahuc, Marque and Wasmer (2008), who analyze this type of problem in a context with search frictions.

Also differently from many models of informality, such as Ulyssea (2010) and Bosch and Esteban-Pretel (2012), we do not allow formal and informal workers to have different bargaining power. Once more, this reflects our strategy of minimizing structural differences across sectors. Adding this degree of freedom can be a straightforward way to create a formality wage premium. In our model, worker heterogeneity and minimum wages play this role, while also allowing for a richer pattern of wage dispersion.

We first describe how wages are determined in the absence of a binding minimum wage. Following, we explain how the introduction of a binding minimum wage changes the results. Define $E_i^j(w)$ as the value that workers of type $i \in \{s, u\}$ place on holding a job position of type $j \in \{for, inf\}$ that pays wage w. Also, call U_i the opportunity cost of the worker – that is, the expected present value of being unemployed, which is taken as given by firms and workers. Note that, in a context of mandated benefits which possibly include unemployment benefits, we might be worried that U_i should be a function of factors related to eligibility, such as having worked in a formal firm before or not having reached the maximum number of payments. We avoid this additional complication by including the expected value of unemployment benefits in the expressions for $E_i^{for}(w)$, instead of in U_i, as done by Ulyssea (2010). Since workers are assumed to be risk neutral, this framing of the problem greatly simplifies the solution and does not lead to further loss of generality.

We can write the flow equations that define the value of employment as

$$rE_i^{for}(w) = a_i w + b_i + s^{for}\left[U_i - E_i^{for}(w)\right], \text{ and} \tag{4}$$

$$rE_i^{inf}(w) = w + s^{inf}\left[U_i - E_i^{inf}(w)\right], \tag{5}$$

where a_i and b_i represent mandated benefits that may increase (or decrease) the value of holding a formal job.

The value $E_i^j(w) - U_i$ is the rent earned by workers of type i when they accept a job offer in sector j. For firms, the marginal value of a worker of type i is given by $J_i^{k,j}(n_s, n_u)$, which was discussed in the previous subsection. So the Nash sharing rule imposes that the wage function $w_i^{k,j}(n_s, n_u)$ must satisfy

$$(1-\sigma)\left[E_i^j\left(w_i^{k,j}(n_s,n_u)\right) - U_i\right] = \sigma J_i^{k,j}(n_s, n_u), \text{ where } i \in \{s,u\}, \text{ and } j \in \{for, inf\}, \ \forall\ k,\ n_s, \text{ and } n_u. \tag{6}$$

Due to the derivative terms in expression 2 (for $J_i^{k,j}$), the set of Nash bargaining equations results in a system of nonlinear differential equations. In Appendix D, we adapt the solution in Cahuc, Marque and Wasmer (2008) to account for two sectors, heterogeneous firms, mandated benefits, and payroll taxes. The resulting wage functions are

$$w_i^{k,for}(n_s,n_u) = \frac{1-\sigma}{c_i}(rU_i - b_i) + \frac{1}{1+\tau_i}\int_0^1 z^{\frac{1-\sigma}{\sigma}\frac{a_i}{1+\tau_i}} \frac{\partial F^k\left(z^{\frac{1+\tau_s}{a_s}\frac{a_i}{1+\tau_i}} n_s, z^{\frac{1+\tau_u}{a_u}\frac{a_i}{1+\tau_i}} n_u\right)}{\partial n_i} dz, \text{ and}$$

$$w_i^{k,inf}(n_s,n_u) = (1-\sigma)rU_i + \int_0^1 z^{\frac{1-\sigma}{\sigma}} \frac{\partial H^k(zn_s, zn_u)}{\partial n_i} dz,$$

with $c_i = [(1-\sigma)a_i + \sigma(1+\tau_i)]$ and $H^k(n_s, n_u) = F^k(n_s, n_u) - \rho(n_s + n_u)$. Notice that we allow for skill-specific

payroll taxes (τ_s and τ_u) in this solution, since we will use these results later on in our policy exercises.

As in the solution of the standard bargaining problem with search frictions, wages are a weighted sum of the reservation wage, rU_i, and a term related to the productivity of the marginal worker. In the standard search and matching model, where marginal productivities are not related to firm size, the wage equation reduces to $w_i^{k,for}(n_s, n_u) = \frac{1-\sigma}{c_i}(rU_i - b_i) + \frac{\sigma}{c_i}\frac{\partial F^k}{\partial n_i}$ (with $b_i = 0$ and $c_i = 1$ for informal firms). However, with decreasing returns to scale, heterogeneous labor, and intra-firm bargaining, the second term is not simply the marginal productivity of the input considered, but instead a weighted average of infra-marginal productivities, with weights $z^{\frac{1-\sigma}{\sigma}\frac{a_i}{1+\tau_i}}$ higher for points closer to the margin. We refer the reader to Stole and Zwiebel (1996*b*), Stole and Zwiebel (1996*a*), and Cahuc, Marque and Wasmer (2008) for a detailed discussion of the characterization of this type of solution. In Appendix D, we derive our results and compare them to those from Cahuc, Marque and Wasmer (2008).

Now we explain how the introduction of a minimum wage changes these results. If the bargained wage in a formal firm for one type of worker – typically, the unskilled – is lower than the minimum wage, then the minimum wage restriction is binding. The Nash bargaining equation is not satisfied anymore for unskilled workers; indeed, in this situation, these workers receive a share of rents larger than σ. This also implies that the previous wage function for skilled workers is not valid anymore, since the term $\frac{\partial w_u^{k,for}}{\partial n_s}$ in equation 2 is equal to zero (marginal changes in the number of skilled workers do not affect wages of unskilled workers, which are binding at the minimum wage). In Appendix D, we show that the wage equation for skilled workers in the formal sector, when the minimum wage binds for unskilled workers, is

$$w_s^{k,for}(n_s, n_u) = \frac{1-\sigma}{c_s}(rU_s - b_s) + \frac{1}{1+\tau_s}\int_0^1 z^{\frac{1-\sigma}{\sigma}\frac{a_s}{1+\tau_s}} \frac{\partial F^k(zn_s, n_u)}{\partial n_i} dz.$$

From the perspective of a firm, whether the minimum wage binds is not only a function of parameters, but also of firm size. This introduces a discontinuity in the first order condition of the problem of the firm. Consider a case where there are complementarities between labor types, as the one in our calibration exercise. Without a minimum wage, hiring an additional skilled worker decreases skilled wages and increases unskilled wages, and the reverse is true for hiring an unskilled workers. This effect is taken into account in the value of the marginal worker of type i, $J_i^{k,j}$. At the margin, when the minimum wage is binding for unskilled workers, the effect of firm size on unskilled wages disappears, leading to a discontinuous increase in $J_s^{k,j}$ and a discontinuous decrease in $J_u^{k,j}$. The increase in $J_s^{k,j}$, in turn, causes a discrete increase in skilled wages, which might give an incentive for firms to strategically reduce the number of skilled workers so that the unskilled wages are just above the minimum wage.

In Appendix D, we show that, because of this discontinuity, there might not be a solution to the first order conditions when the unconstrained (freely bargained) unskilled wage is slightly lower than the minimum wage, although it is always unique when it exists. In those cases, firms engage in the strategic manipulation of firm size described above.[8] In the remainder of this section, we assume that first order conditions hold. Still, we deal explicitly with the possibility that they may not hold in our quantitative exercises and discuss this issue in further

[8] It is not trivial to infer the partial equilibrium consequences of the binding minimum wage in terms of the firm choice of skilled labor. On the one hand, the minimum wage increases the cost of unskilled labor, which reduces the return to skilled labor due to complementarity between the two inputs. On the other hand, the discontinuity mentioned above increases the return to unskilled labor, going in the opposite direction. In simulation exercises we performed, the effect on the demand for skilled labor was always negative, though in general it should depend on the degree of complementarity between the two factors.

detail in Appendix D.

Now we turn to the analysis of wage determination in equilibrium. If we replace 3 in 6, and take into account that the bargaining equation is not satisfied if the minimum wage is binding, we have

$$\begin{aligned}(1-\sigma)\left[E_i^{for}\left(w_i^{k,for}\right)-U_i\right] &\geq \sigma\frac{\xi}{q(\theta_i)}, \quad i\in\{s,u\}, \quad \text{with} > \text{only if } w_i^{k,for}=\bar{w}, \text{ and}\\ (1-\sigma)\left[E_i^{inf}\left(w_i^{k,inf}\right)-U_i\right] &= \sigma\frac{\xi}{q(\theta_i)}, \quad i\in\{s,u\}.\end{aligned}$$

Recalling expressions 4 and 5, notice that E_i^j does not depend directly on firm size or productivity. So neither n_i nor k appear in the expressions above. In a steady-state equilibrium, wages paid for a worker of a given type, working in a firm in a given sector, are the same for all firms in that sector, irrespective of firm size. In other words, in equilibrium, there are only four wages in this economy: w_s^{for}, w_u^{for}, w_s^{inf} and w_u^{inf}.

This result comes immediately from the fact that the matching technology and the cost of posting a vacancy are the same across firms of different sizes. The intuition behind it is that, regardless of productivity, all firms adjust the number of workers so as to equate the marginal value of workers to the expected search cost, which does not depend on productivity or firm size. Thus, the value added by the marginal worker in equilibrium is the same across the productivity distribution. Finally, since we assume that the worker's bargaining power is not related to firm size or productivity, the solution to the Nash bargaining cannot vary with k.[9]

3.4 Equilibrium

So far, we have described the behavior of firms taking θ_i and U_i as given. In equilibrium, these values have to be consistent with the aggregate behavior of firms and workers. The labor market tightness, as explained in subsection 3.1, is given by the ratio of vacancies to unemployed workers. Define the measure of workers of type i employed in sector j as

$$N_i^j = m\int_{-\infty}^{\infty} n_i^k\,\mathbf{1}\left(\text{Firm } k \text{ chooses compliance } j\right) dG(k),$$

where n_i^k denotes the optimal employment of type i workers for a firm with productivity k. Since, in equilibrium, $\dot{n}_i = 0$ for all firms, $v_i^k = s^j n_i^k/q(\theta_i) \Longrightarrow V_i^j = s^j N_i^j/q(\theta_i)$. We can therefore find the expressions that pin down θ_i

$$\theta_s = \frac{s^{for}N_s^{for}+s^{inf}N_s^{inf}}{q(\theta_s)\left(\eta-N_s^{for}+N_s^{inf}\right)} \quad \text{and} \quad \theta_u = \frac{s^{for}N_u^{for}+s^{inf}N_u^{inf}}{q(\theta_u)\left(1-\eta-N_u^{for}+N_u^{inf}\right)}. \tag{7}$$

To find the equilibrium value of U_i, we write the standard flow value equation for the reservation wage

[9] This result greatly simplifies the solution and interpretation of the model. However, it eliminates the possibility of accommodating the widely documented firm size wage premium within the model. A simple way to account for this pattern would be to assume that the bargaining power of workers increases with k, as a result of greater worker unionization, for example. Pratap and Quintin (2006) and Badaoui, Strobl and Walsh (2010) provide a discussion of the relationship between the formality wage premium and the firm size wage premium.

$$
\begin{aligned}
rU_i &= \theta_i q(\theta_i)\left[\phi_i E_i^{for}(w_i^{for}) + (1-\phi_i)E_i^{inf}(w_i^{inf}) - U_i\right] \\
&= \begin{cases} \frac{\sigma}{1-\sigma}\xi\theta_i & \text{, if } w_i^{for} > \bar{w} \text{ and} \\ \frac{\theta_i}{1+\frac{\phi_i\theta_i q(\theta_i)}{r+s^{for}}}\left[\phi_i q(\theta_i)\frac{a_i\bar{w}+b_i}{r+s^{for}} + (1-\phi_i)\frac{\sigma}{1-\sigma}\xi\right] & \text{, otherwise.} \end{cases}
\end{aligned} \tag{8}
$$

For simplicity, since we incorporate unemployment benefits in the parameters a_i and b_i, we assume that individuals derive no utility flow from unemployment. The instantaneous return of being unemployed is the expected value of finding a job and leaving unemployment. In case a worker finds a job, which happens with probability $\theta_i q(\theta_i)$, there is a probability $\phi_i = \frac{V_i^{for}}{V_i^{for}+V_i^{inf}} = \frac{s^{for}N_i^{for}}{s^{for}N_i^{for}+s^{inf}N_i^{inf}}$ that the match is with a formal firm. The second expression is the result of inserting the first order condition of the firm, equation 3, in 8.

An equilibrium in our model is defined as a set of wage functions $w_i^{k,j}(n_s, n_u)$, schedules of firm decisions $j(k)$ and n_i^k, labor market tightnesses θ_i, and unemployment values U_i, such that:

1. The wage functions solve the system of differential equations given by 2 and 6;
2. The labor schedules n_s^k and n_u^k solve equation 3 given the compliance decision $j(k)$ and the wage functions;
3. The compliance decisions $j(k)$ maximize the present value of discounted profits in problem 1;
4. The labor market tightnesses are consistent with 7; and
5. The unemployment values are consistent with 8.

3.5 Discussion: Compensating Differentials

From the final Nash bargaining equations, we can show that:

$$
E_i^{For}\left(w_i^{for}\right) \geq E_i^{inf}\left(w_i^{inf}\right), \quad i \in \{s, u\}.
$$

This expression holds as an equality if the minimum wage is not binding for skill level i. In this case, we can use the definition of $E_i^j(w_i^j)$ to show that

$$
w_i^{inf} = \frac{r+s^{inf}}{r+s^{for}}\left(a_i w_i^{for} + b_i\right) - \frac{rU_i\left(s^{inf}-s^{for}\right)}{r+s^{for}}.
$$

In words, wages in both sectors adjust to exactly compensate workers for the differences in benefits and job duration across sectors. If the minimum wage is not binding and jobs in both sectors have the same expected duration ($s^{for} = s^{inf}$), then the difference between formal and informal wages is equal to the value that workers attribute to mandated benefits. If the expected duration in the formal sector is longer, as we see in the data, then the wage differentials should be even higher to compensate for that. If the minimum wage is binding, on the other hand, then this equation is no longer valid: informal wages are lower than the value needed to make

workers indifferent between sectors, and formal jobs are strictly preferred. However, workers still accept informal job offers, since it is too costly to remain unemployed and wait for a good job. In this case, formal jobs are rationed in equilibrium and compensating differentials do not hold exactly. Still, informal wages have to be high enough to compensate for the expected benefits of informal jobs, once one also considers the lower probability of obtaining such positions.

On the side of the firms, with a continuous distribution of k, the marginal formal firm is virtually identical to the marginal informal firm. The marginal firm is indifferent between operating in the formal and informal sectors and is willing to change its compliance status given marginal changes in the parameters.

4 Calibration

In this section, we calibrate the model to replicate some key features of the Brazilian labor market in 2003. We choose this date because it is close to the reversal of the informality trend (Figure 1) and it is when the second wave of the Informal Urban Economy survey (*Economia Informal Urbana*, ECINF) was conducted by the Brazilian Census Bureau (IBGE). The ECINF targeted small urban firms, most of which were unregistered, thus providing an estimate of the number of informal firms in the economy. We use the survey's micro data in the next section, but, since the ECINF is relatively small and was not repeated after 2003, it is not our main source.

Most of the data we use come from the Monthly Employment Survey (*Pesquisa Mensal do Emprego*, PME), also conducted by IBGE. The PME is a household survey that provides information on employment, wages, occupational choice, formality status, and other characteristics of the workforce, including educational attainment. We use two other data sources from IBGE: the Central Registry of Firms (*Cadastro Central de Empresas*, CEMPRE), a register of formal firms, and the annual projections for the workforce size.

4.1 Functional Forms

We assume that the production function takes the following functional form:

$$F(k, n_s, n_u) = A\left[Bkn_s{}^{\alpha\gamma} + (1-B)n_u{}^{\beta\gamma}\right]^{\frac{1}{\gamma}},$$

where the parameter A is a standard total factor productivity term, and α, β, γ, and B are parameters. We restrict the exponents α and β to be smaller than one, so that the function has decreasing returns to scale for any given k. This production function implies that an entrepreneur with zero productivity can still generate output, but only uses unskilled labor. We restrict the parameter γ to the interval $(0, 1]$ to ensure that the parameter k denotes skill-biased productivity. In the limiting case where $\gamma = 1$, increases in k only raise the productivity of skilled labor. If $\gamma \in (0, 1)$, unskilled workers are more productive in a firm with a higher productivity parameter and with more skilled workers.[10]

The parameter k is assumed to follow a Generalized Pareto distribution, to account for the fact that the majority of firms are small but a large part of the workforce is employed by large firms (see IBGE (2005)). We set the location

[10] If $\gamma = 0$, the production function collapses to a Cobb-Douglas and the elasticity of substitution between any two pairs of inputs, including k, will be the same. If $\gamma < 0$, unskilled labor is a better complement to k than skilled labor.

parameter to zero, so that the smallest firms have $k = 0$. Also, we normalize the scale parameter to $1 - T$, where T is the shape (tail) parameter, so that the average productivity is normalized to one.[11] Increases in T are thus mean-preserving spreads that add probability mass to extreme values of productivity. The cumulative distribution of productivity is given by[12]

$$G(k) = 1 - \left(1 + \frac{Tk}{1-T}\right)^{-\frac{1}{T}}.$$

Since the informality penalty must be increasing and convex, we use a quadratic function, $\rho(n) = Cn^2$. This choice results in significant computational gains because of the linearity of the first derivative. In the specification of the matching technology, we follow the literature and use a Cobb-Douglas function. We thus have $q(\theta) = D\theta^{-E}$, where D is the matching scale and E is the matching elasticity.

Finally, the valuation of fixed benefits by workers takes the form

$$b_i = \left(b_i^F + s^{for} b_i^D\right) \bar{w}.$$

The term b_i^D is the present value of the expected unemployment insurance flow, measured in multiples of the minimum wage $\bar{w}$, and b_i^F represents transfers received by the worker (also measured in multiples of the minimum wage). The details on the computation of these benefits, along with that of a_i and τ, are provided in Appendix A.

4.2 Parameters

Table 4 presents a first subset of the parameter values we use.

A non-trivial problem in our calibration exercise is how to map observed traits in the data to skill levels in the model. In the model, skills map directly into wages. In the relevant case from the perspective of the quantitative analysis, formal sector minimum wages bind only for unskilled workers. This gives an empirical interpretation of skills for formal workers that does not match schooling. As mentioned in section 2, there is a wide dispersion of wages for each level of schooling in the data, indicating that skills do not map closely into formal schooling, despite the fact that they are positively correlated. Unskilled workers in the model represent workers in the data who receive close to the minimum wage when employed in the formal sector. If they receive significantly more than the minimum wage in a formal job, then they must correspond to skilled workers in the model. The dispersion of wages by educational level in the data reinforces the idea that it is difficult for us to interpret schooling in the data as referring directly to skills in the model.

The downside of this interpretation is that we cannot observe skills in the data for all workers, thus requiring a somewhat arbitrary choice of which number to use as the exogenous fraction of skilled workers. In this perspective, in the data, we only observe skills for formal workers (based on those who earn the minimum wage and those who earn higher wages). We would like to infer the skills of unemployed and informal workers using observable characteristics such as age or education, but it is impossible to make such straightforward connection given the

[11] Allowing for other values for the scale parameter would not add information to the model, since the changes in the scale of k can be offset by changes in the parameters A, B, and γ in the production function.

[12] For computational purposes, we divide this distribution in 200 atoms with linearly decreasing productivity mass, in a way that the first atom has 100 times the probability mass of the last atom. This is to add precision while requiring less computational cost, as firms at the bottom of the distribution are much more similar than at the top due to the nature of the Generalized Pareto distribution.

Table 4 – Parameters Imputed from the Data or from the Literature

Parameter	Value	Source
η (measure of skilled workers)	0.662	Share 8+ years of schooling
m (measure of firms)	0.0905	Ratio of firms to workforce
s^{for} (formal hazard rate)	0.030	Gonzaga (2003)
s^{inf} (informal hazard rate)	0.082	Gonzaga (2003)
τ (payroll tax rate)	0.7206	Appendix A
a_s, a_u (variable benefits)	0.235, 0.306	Appendix A
b_s^F, b_u^F (fixed benefits)	0.02, 0.05	Appendix A
b_s^D, b_u^D (unemp. insurance)	7.48, 4.00	Appendix A
r (discount rate)	0.08	Real interest rate
D (matching scale)	0.30	Ulyssea (2010)
E (matching elasticity)	0.50	Ulyssea (2010)
σ (worker bargaining power)	0.45	Ulyssea (2010)

patterns observed in the data. Although older and more educated workers are less likely to earn the minimum wage, this relationship is far from precise: there are many uneducated workers receiving more than the minimum wage in the formal sector, and also some highly educated workers in the opposite situation. More generally, a probabilistic regression of our definition of skill on schooling, age, and race would have a very poor predictive power. Thus, we simply use the fraction of the workforce with complete elementary education – that is, 8 or more years of schooling – as a proxy for the fraction of skilled workers in the model, even though we are aware that this is not a precise measure at the individual level. We combine this aggregate definition of the fraction of skilled workers with the individual level implications of the model in terms of the relationship between wages and skills. In other words, despite assuming that the measure η of skilled workers corresponds to the fraction with 8 years of schooling or more, we do not assume that a given individual with 8 years of schooling or more is necessarily skilled. We use the aggregate number as a proxy to represent the compositional change in the Brazilian labor force during this period.

We estimate the measure of firms m using the total number of salaried workers and the number of firms, both formal and informal. The PME asks unemployed workers what was the nature of their last employment. We use this information to calculate the fraction of unemployed workers who are looking for salaried jobs. We estimate that salaried workers, either employed or unemployed, account for 73% of the workforce. Since the PME covers only the 6 main metropolitan regions in Brazil, we multiply this fraction by the total size of the workforce in 2003, calculated by IBGE, to get the total number of salaried workers. We obtain the number of formal firms from CEMPRE and the number of informal firms from ECINF, excluding self-employed workers. The measure m is the ratio of firms to salaried workers.

The job destruction rates s^j are taken from estimates of the duration of employment spells in Gonzaga (2003). The values for the payroll tax rate and benefits are calculated in Appendix A, according to the methodology suggested by Souza et al. (2012). The discount rate for workers and firms is assumed to be the real interest rate. Finally, we use the same values for the parameters of the matching function and the bargaining power of workers as Ulyssea (2010).

We use a minimum distance procedure to set the remaining eight parameters displayed in Table 5. The algorithm minimizes the norm of a vector where each element is the relative distance between the model's outcome and the

Table 5 – Parameters Calibrated - Minimum Distance Results

Parameter	Value
A (productivity)	10.7117
B (technology bias)	0.7870
α (skilled exp.)	0.5085
β (unskilled exp.)	0.8391
γ (CES param.)	0.7407
C (informality cost)	0.0862
ξ (search cost)	1.2269
T (firm dist. shape)	0.2737

Table 6 – Calibration Results

Outcomes	Target Value	Model Value
Unemployment	12.6%	12.8%
Share informal workers	28.4%	29.1%
Formal skilled wage	4.30	4.37
Unskilled formal workers	11.4%	11.8%
Informal unskilled wage	0.92	0.91
Avg. informal wage	2.67	2.59
Labor share of income	52.8%	52.5%
Firms w/ 10 or fewer emp.	93.4%	92.4%

Note: Wages in multiples of the minimum wage in 2003, the numeraire in the model.

eight targets listed in Table 6. The targets were selected from observable characteristics that are either important for our analysis or informative about parameters that we cannot directly observe.

The first two targets, unemployment and informality rates, are directly observable in the PME data set. The next four targets refer to wage differentials across types of workers and sectors. For all workers in the data, we compute hourly earnings in their main job and divide by the hourly equivalent of the minimum wage. For workers in the formal sector, we consider those who earn up to 120% of the minimum wage as unskilled, and others as skilled. With this definition, we compute the average wage for skilled formal workers, as well as the fraction of unskilled workers in the formal sector. In the informal sector, we cannot distinguish between skilled and unskilled workers, and so we compute only the average wage among all informal employees. However, we can set a reasonable target for the informal wage penalty among unskilled workers from Bargain and Kwenda (2011). Using the same PME data set, they find that, for workers at the quantile 0.2 of the wage distribution, the wage penalty associated with informality is around 8%.

The labor share of income is defined in the model as the fraction of total production (net of search costs and informality penalties) that is not firm profits nor government surplus. Although not directly related to our analysis, this is a sensible way to add information to pin down the concavity of the production function, as it is directly related to profits. We calculate the empirical counterpart of this measure using the National Accounts System, applying the corrections proposed in Gollin (2002). The last target, the fraction of firms with 10 or fewer employees, is set as a means to determine the shape parameter of the productivity distribution. We use 10 workers as the threshold to correspond to one of the categories in the distribution of firm sizes in the CEMPRE report.

Table 6 shows that the model can replicate all of the targeted variables with reasonable accuracy. Before we proceed to the next subsection, it is interesting to use our baseline specification to characterize some properties of the equilibrium, particularly as it relates to the cross-sectional distribution of firms. Each row in Table 7 characterizes firms in a specific position in the distribution of productivity. The top row refers to the smallest firms in the model, while the bottom row refers to the largest ones. The columns show the productivity parameter, the number of workers, the fraction of skilled workers, the compliance status, and the average wage for all workers in the firm. As expected, firm size and the fraction of skilled workers increase with productivity.

Table 7 – Firms in the Model

Percentile	k	Size	Fraction Skilled	Formal?	Average Wage
Bottom 1%	0.00	0.9	0.0%	No	0.91
50.0%	0.56	2.0	8.9%	No	1.37
74.6%	1.22	4	25.7%	No	2.23
89.8%	2.32	8	55.5%	No	3.75
94.9%	3.38	14	76.5%	No	4.83
97.4%	4.62	42	76.9%	Yes	3.59
99.0%	6.76	110	82.4%	Yes	3.77
Top 0.01%	42.93	14,572	96.0%	Yes	4.23

Note: Wages in model units (one model unit is equivalent to the minimum wage in 2003).

An important feature of the model is the non-monotonicity of firm average wages along the productivity distribution. Within each sector, wages are monotonically increasing with productivity because of the compositional effect of hiring a larger fraction of skilled workers. However, at the margin between informality and formality, average wages decrease. This happens because skilled workers receive more when working in informal firms, to compensate for the lack of mandated benefits. At the margin, formal and informal firms are almost identical in terms of productivity. So, in order to pay benefits for all of its workers and a minimum wage to its unskilled workers, the marginal formal firm must pay lower wages to skilled workers.

5 Quantitative Results

5.1 The Recent Reduction in Informality in Brazil

In this subsection, we use the model to shed light on the decline in informality observed in Brazil from 2003 to 2012. First, we look at the main institutional changes observed during the period and analyze how each of them separately affected the labor market. Then we evaluate whether the model is able to account for the aggregate movements in informality, unemployment, and wages by considering all the institutional changes simultaneously.

Throughout the analysis, we often refer to Table 8, where each row describes a particular labor market outcome. The first column describes how the Brazilian labor market changed from 2003 to 2012 using the same data sets and definitions used in the calibration. Each other column considers how changes in one or more parameters affects labor market outcomes in the model, by comparing the baseline calibration with a new steady-state equilibrium where only the parameter in question is different (set to their 2012 levels).

In the period we study, the unemployment rate fell by 7.1 percentage points (from 12.6% to 5.5%), while the informality rate dropped by 10.4 points (from 28.1% to 17.7%). Average wages increased by 20.6%, but, as pointed in section 2, the gains were larger for low-skill formal workers and for informal workers. Since, following the model's logic, we cannot directly observe the skill level of workers in the informal sector, it is not possible to disentangle the increases in wages separately for these workers. However, informal wages as a whole increased by 42%, significantly more than for formal skilled workers.

Table 8 – Quantitative Experiments, Changes in the Brazilian Labor Market between 2003 and 2012

Changes in:	(1) Data	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Model							
		Minimum wage	Payroll tax	Benefits	Informality penalty	Fraction skilled	All but productivity	Productivity	All simultaneously
Outcomes		$\Delta\bar{w} = 61.2\%$	$\Delta\tau = -0.63$p.p.	Δa_i and $\Delta b_i \simeq 0$	$\Delta C = 33.9\%$	$\Delta\eta = 12.9$p.p.		$\Delta A = 18.9\%$	
Unemployment (p.p.)	-7.2	1.0	-0.1	0.0	0.4	-6.6	-2.8	-3.6	-3.8
Informality (p.p.)	-10.7	6.7	-0.1	0.5	-2.9	-16.7	-6.9	-7.4	-6.4
Wages (%):									
Average	21.5	-0.3	0.2	0.0	-0.6	-0.7	1.6	16.5	21.5
Formal, skilled	18.5	-1.4	0.3	0.0	0.0	-8.6	-9.5	19.8	8.3
Formal, unskilled	61.2	61.2	0.0	0.0	0.0	0.0	61.2	0.0	61.2
Informal, average	43.9	-7.8	0.1	0.6	-6.3	57.1	6.5	40.9	37.5
Informal, skilled	-	1.2	0.3	-0.3	0.0	-8.0	-6.6	18.4	9.8
Informal, unskilled	-	-15.6	0.1	-0.6	-5.2	76.3	9.5	49.5	43.3
Product[a] (%)	27.0	-1.8	0.0	-0.1	0.2	9.2	6.7	21.5	27.8
Govt. net revenues (%)[b]	-	-22.6	-1.4	0.8	3.2	13.9	-15.7	37.4	15.2

Notes: [a]For column 1, product is GDP per capita. For the remaining columns, product is total production in the model net of search costs and the informality penalty. [b]In the baseline calibration, the government appropriates 8.4% of total production. Numbers in this line represent relative changes over the baseline amount, not changes in fraction of total production.

5.1.1 Minimum Wage

The minimum wage increased by 61.2% from 2003 to 2012. The effects of a change of this magnitude over the baseline calibration are shown in column 2 of Table 4. Wages for skilled workers in both sectors are only marginally affected. However, for informal unskilled workers, wages fall by almost 9%. The reason for this decline is the reduced demand for unskilled labor by formal firms, which increases unemployment and lowers the outside option of workers being hired by informal firms.

Informality and unemployment increase, as expected, but the latter effect is relatively small when compared to the magnitude of the increase in the minimum wage. From the changes in the minimum wage alone, informality increases by 6.7 percentage points and unemployment by 1 percentage point. The reason is that part of the negative effect on unemployment is attenuated by marginal firms entering informality (and thus not being subject to the minimum wage anymore), and also by the fact that informal unskilled wages decrease, leading to increased labor demand by informal firms. This logic resembles the traditional view of the informal sector, where, for some workers, informality is an alternative to unemployment, as discussed in Fields (1975), Rauch (1991), and Boeri and Garibaldi (2007). In our model, this applies to unskilled workers when the minimum wage binds, in the sense that formal jobs are strictly better than informal ones, but are also more difficult to find, so unskilled workers accept informal job offers to avoid unemployment.

There is a reduction of 2.2% in aggregate production, which again does not seem particularly large relative to the magnitude of the increase in the minimum wage. Government revenues from labor taxes and benefits experience a more sizable decline of 24%. This is mainly because some benefits that accrue to all formal workers, such as unemployment insurance, are indexed by minimum wages. On the other hand, revenues from labor taxes increase only for unskilled workers, and increased informality and unemployment reduce the tax base.

5.1.2 Payroll Taxes

The only change in the costs of formal employment from 2003 to 2012 was the phasing out of a temporary additional contribution to the worker's severance payment fund (*Fundo de Garantia por Tempo de Serviço*, FGTS). As described in Appendix A, we estimate that this change decreased the final payroll tax rate only slightly, from 72.06% of the nominal wage to 71.43%. Column 3 shows that, as standard models would predict, informality falls. Wages rise for all workers, except for those who receive exactly the minimum wage. This is a consequence of the axiomatic bargaining approach, through which workers receive part of the increased profits by firms. Product rises and government revenues decline. All effects are quantitatively small.

5.1.3 Mandated Benefits

There were minor changes in mandated benefits, specifically in the formulas for calculating the income tax and social security contributions, which are both deducted from the wage of formal employees and thus are included in our parameter a_i. However, on average, they did not result in sizable changes in the size of deductions. When we recalculate the parameters a_i and b_i using 2012 data (Appendix A), we find that the differences are negligible. Hence, they do not have any relevant impact on labor market outcomes, as is evident from column 4 in Table 8.

5.1.4 Enforcement of Regulation

We use data from the Ministry of Labor to estimate changes in the enforcement of labor regulations from 2003 to 2012. Reports of labor inspections, available in MTE (2013), show that the number of workers targeted by inspections rose during the last decade both in absolute terms and as a fraction of the workforce.[13] We use the relative increase as a proxy for increases in the enforcement of regulations in the model. We find that the fraction of the workforce that was inspected rose by about 39% from 2003 to 2012. We raise the parameter C by this same proportion.

The fifth column of Table 8 shows how this change would impact our baseline calibration. First, informality decreases, as expected. We argued in section 2 that the effects of increased enforcement over unemployment are ambiguous in many models, and this is also true in ours. There is an extensive margin effect because firms who change their compliance decision might hire more workers, and also an intensive margin effect because the remaining informal firms hire fewer workers. For our calibration, unemployment rises marginally with the observed change in enforcement. The only noticeable change in wages is a decline in earnings for informal, unskilled workers. Thus, in this respect, our model replicates the results found in Bosch and Esteban-Pretel (2012) and Meghir, Narita and Robin (2012). Government revenues increase, but one should be cautious about drawing additional implications from this result since we do not take into account operational costs associated with increased enforcement.

5.1.5 Workforce Composition

Over the last two decades, there has been a consistent increase in school attendance among Brazilian school-aged children, which has led to a corresponding increase in workforce education. This is not only because young adults are now more educated than previous generations, but also because more individuals enter the labor market at later ages. At the same time, demographic changes associated with historical reductions in fertility and population aging are leading to an older and, therefore, more experienced workforce. From 2003 to 2012, the fraction of the workforce with complete elementary education – in Brazil, 8 or more years of schooling – increased from 66.0% to 79.1%. In column 6 of Table 8, we assume that this change of 13.1 percentage points corresponds to the increase in the fraction of skilled workers in the model.

We find that the predicted changes agree with our discussion from section 2. Both unemployment and informality decrease sharply as a consequence of a more skilled workforce, falling, respectively, by 6.5 and 16.8 percentage points. Wages for informal unskilled workers increase by 87%, while they decrease for skilled workers in both sectors by around 8%. This is a direct consequence of the relative increase in the supply of skilled workers. The labor market for skilled workers becomes less tight (and the reverse happens for unskilled workers). Because firms hire more skilled labor in the new equilibrium, the productivity of unskilled work increases due to complementarities in the production function. The combination of a tighter labor market and greater productivity is behind the steep increase in the informal unskilled wage. Wages for unskilled formal workers also rise, by 1.8%, meaning that the minimum wage is not binding anymore under the new equilibrium. The model therefore predicts that, absent the observed increases in minimum wages between 2003 and 2012, the minimum wage would have become non-binding under the

[13] Other indicators, such as total revenues from fines, also increased during the period. For a thorough discussion of enforcement of labor regulation in Brazil, see Cardoso and Lage (2005).

2012 composition of the Brazilian labor force.

5.1.6 Estimating Changes in Productivity

Now we consider the performance of the model when the five dimensions discussed above are brought together. The results are shown in column 7. These changes explain 64% of the observed decline in informality, but only 39% of the decline in unemployment. Also, average wages and product increase by far less than observed in the data, explaining in both cases less than a quarter of the actual change. The increase in GDP per capita in the data – listed as product in the table – reflects in part an overall increase in TFP in the Brazilian economy during this period. Ferreira and Veloso (2013), for example, estimate an yearly growth of TFP in Brazil between 1.5% and 2.5% per year from 2003 to 2009. These observations suggest that there was an increase in overall productivity in the economy that, not surprisingly, cannot be captured by the model, which does not display capital accumulation nor technological change. To calibrate TFP gains in the model, we raise the parameter A until the increase in average wages – taking into account all changes during the period – match that observed in the data. We find that TFP in the model must increase by 18.9% between 2003 and 2012 in order for the model to reproduce the increase in average wages in the data. This number falls precisely in between the cumulative growth in TFP that would be obtained from the Ferreira and Veloso (2013) estimates.

Before we assess the performance of the model including the increase in productivity, we study the effects of productivity gains in isolation. Column 8 shows that unemployment declines by 3.6 percentage points and wages rise by 16.5% when productivity increases. There is also a decrease in informality of 7.4 percentage points, consistent with many other models where informal employment is counter-cyclical. Wages rise for most workers, but particularly for the unskilled in the informal sector. This is because most unemployed workers in the baseline calibration are unskilled, and thus the decline in unemployment has larger effects on the tightness of the unskilled labor market. Wages do not rise for formal unskilled workers because the minimum wage is still binding after the productivity gain.

5.1.7 Explaining the Evolution of Labor Market Outcomes

In column 9, we consider changes in minimum wages, taxes, benefits, enforcement, skills, and productivity simultaneously. The qualitative implications of the model, in terms of direction of predicted changes, matches precisely the pattern of movements observed in the Brazilian labor market between 2003 and 2012: reductions in unemployment, reductions in informality, and increases in average wages, with proportionally higher increases for informal and unskilled workers. Quantitatively, the model does a good job in explaining the reduction in informality, generating a decline of 6.4 percentage points while the observed decline was 10.7 points. It also predicts a decline in unemployment of 3.8 percentage points, which is a little more than half of the observed decline of 7.2 points. Predictions regarding wages are close to the empirical patterns, though the model underestimates the gains for formal skilled workers. Overall, the model is able to explain quantitatively the main outcomes of the Brazilian labor market with a reasonable degree of precision.

Going back to the discussion in section 2, we can use the model to determine which factor was the main driver behind the reductions in informality and unemployment. Table 8 already addressed this issue, by looking at the

Table 9 – Individual Contribution of Each Factor, Changes in the Brazilian Labor Market from 2003 to 2012

	All	All changes, except:					
	changes	Minimum	Payroll	Benefits	Informality	Fraction	Productivity
Outcomes:		wage	tax		penalty	skilled	
Unemployment (p.p.)	-3.8	-7.1	-3.7	-3.8	-3.9	0.3	-2.8
Informality (p.p.)	-6.4	-16.5	-6.3	-6.8	-3.7	4.8	-6.9
Wages (%):							
Average	21.5	18.8	21.2	21.5	22.5	18.8	1.6
Formal, skilled	8.3	9.2	8.0	8.3	8.2	18.3	-9.5
Formal, unskilled	61.2	22.7	61.2	61.2	61.2	61.2	61.2
Informal, average	37.5	91.3	37.1	36.7	45.8	8.8	6.5
Informal, skilled	9.8	8.3	9.5	10.2	9.8	19.1	-6.6
Informal, unskilled	43.3	118.7	42.9	43.7	49.9	1.6	9.5
Product[b] (%)	27.8	30.2	27.8	28.0	27.5	17.8	6.7
Govt. net revenues (%)[c]	15.2	41.8	17.1	13.1	11.5	8.1	-15.7

Notes: [a]Change from 2003 to 2007 (IBGE/SCN is only data available up to 2007). [b]Product is total production in the model net of search costs and the informality penalty.

effect of each factor one at a time. In Table 9, we conduct the opposite comparative static exercise: we analyze what happens in the model when all but one of the factors discussed before is taken into account. We find that the declines in both unemployment and informality would have been considerably larger – respectively, 7.1 and 16.5 percentage points – if the minimum wage had not increased. We also reinforce the idea that changes in labor force composition were the main driver behind the observed reductions in informality: without a larger fraction of skilled workers, informality would have increased by 4.8 percentage points, instead of declining by 6.4. The model is unable to reproduce the reduction in informality when changes in labor force composition are ignored. The relevance of enforcement (informality penalty) is secondary: without changes in this parameter, the decline in informality would have been two percentage points smaller. As before, the effects of changes in payroll taxes and benefits were negligible.

One might be suspicious of the main implication of Tables 9 and 8 due to the fact that our definition of skill in the model does not match exactly education, and, therefore, our calibration of the change in the composition of the workforce may seem arbitrary. To strengthen our argument and to show that changes in workforce composition are strictly necessary for replicating the patterns observed in the data, we conduct an additional exercise. Suppose that we want to explain the evolution of labor market outcomes in the model without resorting to changes in the fraction of skilled workers. Since we observe directly minimum wages, payroll taxes, and benefits, we have two degrees of freedom in this exercise: aggregate productivity and enforcement (informality penalty). We therefore choose the total factor productivity parameter and the informality penalty such that the model – with a fixed composition of the labor force – reproduces the same declines in informality and unemployment from column 9 in Table 9. In order to match these numbers, productivity must increase by more than 80% and the costs of informality must increase by around 150%. No estimates of productivity and enforcement currently available suggest increases remotely similar to these orders of magnitude. In addition, under this scenario, production per capita and average wages would have gone up by around 90% , and wage increases would have been roughly similar across sectors and skill levels. These results are clearly at odds with the data, suggesting that changes in workforce composition are really crucial in any attempt to rationalize the changes in labor market outcomes in Brazil between 2003 and 2012.

Finally, this exercise also shows that the impact of productivity on informality may depend on the initial level of unemployment. While an increase in A starting from the baseline model decreased informality by a substantial amount (column 8 Table 8), the same change led to a slight increase when using parameters of 2012 (the difference between columns 9 and 7 in the same Table). In our model, increases in productivity can lead to more formalization because firms hire more workers, and the informality penalty is increasing in size. On the other hand, more productivity leads to higher wages, and thus increased taxes. If the economy has high unemployment, firms can hire more workers without putting much pressure on wages, since marginal productivities decrease with firm size. In this case, the firm size effect dominates and informality is reduced. If instead unemployment is low, firms cannot grow much with gains in productivity, and wages increase more to sustain the labor market equilibrium. Then, payroll taxes increase relative to the informality penalty and marginal firms may decide to switch to the informal sector.

5.2 Policy Experiments

One major policy concern in developing countries has been how to bring down informality without increasing unemployment. In this subsection, we use the model to assess the effectiveness of alternative labor market policies in achieving this goal, while also keeping track of the fiscal burden imposed on the government.

The first labor market policy we consider is a reduction in payroll taxes for low wage workers. In column 3 from Table 8, we had the result that a lower payroll tax rate can lead to a decline in informality, with no adverse effect on unemployment. On the other hand, it also leads to a reduction in government revenues that is substantial when compared to the decline in informality. However, informal firms are more intensive in unskilled labor than formal firms. In addition, only a fraction of government revenues come from payroll taxes on low skill workers, since their wages are lower and they account for a small fraction of formal employment. Thus, an intermediate alternative might be for the government to subsidize the employment of low wage formal workers through a progressive payroll tax, with the tax rate increasing with the wage. Proposals like this have been considered as ways to fund health care expenditures in developed countries, but rarely feature in the informality discussion in the developing world.

In Table 10, we examine the progressive payroll tax policy using as starting point the model as of 2012 (column 9 in Table 8). In the first column, we show, as a reference point, the result of simply reducing the overall tax rate by 1 percentage point (to 0.7043). As argued above, although this reduction leads to positive effects on informality, there are significant costs in terms of government revenue. In columns 2 to 5, we assess similar policies where the reduction in payroll taxes is restricted to low wage workers (in the model, equivalent to low skill). The policy achieves similar results for employment and formalization, but, for some values of τ_u, government revenues actually increase. The formalization induced by lower taxes among low skill workers is sufficient to induce marginal firms to comply, and thus enlarges the tax base. The taxes raised from skilled jobs in firms that formalize more than offset the revenue forgone from low skill workers in infra-marginal firms. In addition, wages increase substantially for unskilled workers in the informal sector because of a tighter labor market. Thus, this policy is also likely to have positive effects on poverty alleviation.

Next, we consider an apparently similar policy in which the government increases the attractiveness of formal jobs to unskilled workers, by increasing benefits for low wage earners in the formal sector. This policy is similar

Table 10 – Hypothetical Policy Experiments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	1 p.p. reduction in payroll tax	Progressive payroll tax $\tau_s = 0.7143$				Transfer to low wage	$\tau = 0.7416$,
Outcomes	$\tau = 0.7043$	$\tau_u = 0.7043$	$\tau_u = 0.50$	$\tau_u = 0.30$	$\tau_u = 0.10$	$b_u^F = 0.10$	$b_u^F = 0.10$
Unemp. (p.p.)	0.0	0.0	-0.1	-0.6	-4.6	0.1	0.1
Inform. (p.p.)	-0.5	-0.1	-2.7	-6.9	-18.3	-0.1	1.0
Wages (%):							
Average	0.4	0.0	0.5	0.7	-2.3	0.1	-1.0
Formal, skilled	0.5	0.0	0.4	0.9	1.2	0.0	-1.3
Formal, unsk.	0.0	0.0	0.0	0.0	11.3	0.0	0.0
Informal, avg.	-0.3	0.2	5.0	14.2	68.5	0.4	0.7
Informal, skilled	0.5	0.0	0.4	0.8	1.1	0.0	-1.2
Informal, unsk.	0.6	0.3	10.2	27.9	121.1	0.7	-0.7
Product[a] (%)	0.1	0.0	0.6	1.4	3.0	0.0	-0.3
Govt. revenues (%)	-2.0	0.0	2.0	4.2	5.0	-5.7	0.0

Notes: In all columns, the reference is the model as of 2012, with $\tau = 0.7143$.[a]Product is total production net of search costs and the informality penalty.

to a current program in Brazil in which the government transfers resources directly to low wage employees in the formal sector (*Abono Salarial*). In column 6, we assess the consequences of increasing the fixed payments by the government to low-skilled workers from 5% of the minimum wage to 10%. We find that there is a reduction in informality, although small when compared to the costs incurred by the government. If payroll taxes are raised by about 3 percentage points so that the program breaks even in terms of government revenues, the positive result vanishes and we see an increase in informality (column 7).

The second policy is ineffective because of the binding minimum wage. In an unrestricted scenario, the formal unskilled wage would drop after the increase in benefits, because of rent sharing between worker and firm. This would generate incentives for the posting of more formal unskilled vacancies, and the results would come closer to those of the progressive payroll tax. In the case we study, wages cannot adjust downward, so the supply of formal vacancies remains unchanged. The only channel left for lowering informality is the increase in informal wages, which results from an increase in the outside option of unemployed unskilled workers when bargaining (because formal jobs become more attractive).

Three important caveats should be made regarding our progressive payroll tax results. First, our model assumes that every firm hires both skilled and unskilled workers. This enables the government to increase its revenues by inducing firms to formalize through lower taxes for unskilled workers. If firms instead hire a single type of worker – either skilled or unskilled –, there would be far less potential to increase revenues with this policy. The second limitation is the assumption that there is a single compliance decision for all workers. If firms are free to make individual compliance decisions for each worker, then the policy would merely result in the formalization of low wage workers, while high wage employees would remain informal. Third, there is the possibility of under-reporting of wages in the formal sector, which is not taken into account in the model.

We believe that these concerns are not enough to compromise the qualitative implications of the analysis, though the quantitative results from Table 10 should not be taken at face value. To assess the relevance of the first two issues, we examine data from the ECINF, which surveyed small firms in the formal and informal sectors in Brazil.

For each of the small firms covered by ECINF, we have information on the number of employees, their formal status, wages, and schooling levels. Regarding the first point, we examine the degree of wage dispersion within firms in the informal sector. In 64% of the informal firms with five employees – the largest firms surveyed by ECINF and those more likely to be marginal firms in the informal sector – the highest wage was at least 50% above the lowest wage. In 20% of them, the highest wage was more than three times the lowest wage. The data also shows that, in most of these firms, workers belong to very different educational categories. This evidence suggests that there is a substantial degree of skill heterogeneity within marginal informal firms, as implied by the model.

On the second point, the formalization of low wage workers should increase the probability of formalization of high wage workers for two reasons. If firms formalize a fraction of their workforce, they become more visible to labor inspectors and thus the cost of employing informal workers increases. Also, the existence of formal ties to some workers may make it easier for others to take the employer to court. The data supports the view that most firms will hire all of their workers either formally or informally. Among firms in the ECINF data set with five employees, 32% hire all workers informally, while 46% hire all of them formally. Only 22% of the firms have both formal and informal employees. This number is even lower for smaller firms.

Finally, although this policy would certainly increase incentives to under-report wages, there are already large incentives for firms to do that under current labor law, since several contributions paid to the government by firms are proportional to the wage (see Appendix A). In addition, the value of many mandated benefits is also indexed by the contractual wage, so workers have an incentive to enforce truthful reporting by firms. Thus, it does not seem to be the case that the progressive payroll tax would dramatically change the incentives to under-report wages in the formal sector.

6 Concluding Remarks

This paper studies how the interplay between workforce composition and labor market institutions, particularly minimum wages, affects informality, unemployment and wages. The framework we develop allows for worker and firm heterogeneity, search frictions, and more institutional details than most other models in the literature. In addition, we model the compliance decision by firms and workers, so that agents self-select into formal and informal sectors, given their individual characteristics and the institutional setting. In the model, there are no intrinsic differences between individuals and firms in the formal and informal sectors, and all market imperfections are generated by labor regulations and the search and matching frictions.

The model is used to reproduce the cross-sectional characteristics of the Brazilian labor market and to study the decline in informality rates observed between 2003 and 2012. We show that the model is able to replicate important features of informal labor markets, particularly wage patterns, rates of unemployment, and informality. Following, we show that, using changes in tax rates, benefits, minimum wage, enforcement of regulation, workforce composition, and productivity, the model can explain with reasonable quantitative precision the evolution of labor market outcomes in Brazil. The increase in skill levels is the most important factor behind the sharp decline in informality among salaried workers observed during the period.

We also perform additional exercises to analyze the impacts of two policies aimed at reducing informality. First,

we show that decreasing the payroll tax rate for low wage workers can have positive effects on both employment and formalization, while at the same time increasing government revenues. On the other hand, a subsidy to formal unskilled workers is not cost-effective. The discrepancy between these two policies comes from by the binding minimum wage, which, in the second alternative, prevents downward adjustments of formal wages and the creation of more formal jobs. The model indicates that the change from flat to progressive payroll taxes could be an effective way to fight informality in the developing world, without increasing unemployment or compromising government revenue.

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Appendix A: Costs of Formal Labor and Valuation of Benefits by the Formal Employee

In this Appendix, we calculate the cost of formal employment and the valuation of mandated benefits by formal workers based on the methodology of Souza et al. (2012). In each subsection, we first show the results for the baseline calibration in October 2003. Then, we discuss the changes in regulations from 2003 to 2012 and calculate the parameters for October 2012.

In order to correctly reflect labor regulations and the differences between formal and informal jobs, it is important to have a clear grasp of what we call wage in the model and how it relates to the data. In the data set we use (PME), workers are asked to report their nominal monthly wages. If they are formal, they are asked not to include annual contributions such as the thirteenth salary. On the other hand, they report gross wages before formal deductions (such as income tax or social security contributions). However, if workers are informal, such concerns are irrelevant and the reported wage is actually what is being paid by the employer and received by the worker. On the employer side, a similar distinction must be made: while the cost of informal employment is essentially the reported wage, for formal workers the cost might be much higher once all contributions and mandated benefits are taken into account.

In the model, wages should reflect the reported wage in the PME data set, and the payroll tax (τ) and the $benefits$ term are used to adjust the costs of formal employment and the valuation of formal jobs by employees, respectively. Thus, for the purposes of the model, the payroll tax rate must encompass everything that a formal employer must pay but a informal employer must not, as a multiple of the reported wage. Likewise, the term $benefits$ is the difference between the valuation of formal jobs and reported wage. In principle, this term can be either positive or negative, depending on whether the advantages of formal employment (e.g., thirteenth salary, vacations) are quantitatively more important than the social security and income tax deductions. In the calculations below, we show that all parameters of the $benefits$ term are positive, meaning that formal jobs are preferred to informal jobs for a given reported wage.

Cost of Formal Employment

Under Brazilian labor laws, contributions paid by employees are fixed fractions of the base salary. Thus, the payroll tax rate is the same regardless of the type of worker in the model. Later, we discuss that this is not true regarding the valuation of formal jobs by employees; for instance, highly paid workers are subject to income tax, but low wage workers are not.

Table A.1 shows our calculations of the cost of formal employment in October 2003. For simplicity, we normalize the base salary to 100. Formal workers are entitled to a thirteenth salary annually and an additional stipend of 1/3 of the monthly wage when they leave for vacation. In addition, if they are dismissed, the employer must notify them at least 30 days earlier. During that period, the employee is entitled to use up to 25% of its work time in job search. As discussed in Gonzaga (2003), the advance notification is in practice an additional severance payment, since workers are not expected to devote much effort to their tasks during that month and the employer cannot rely on them.

Now we turn to the contributions that the employer is obliged to pay. These are levied over not only the nominal monthly wage, but also the additional payments described above (thirteenth salary, vacation stipend and advance notice). The first item is the monthly contribution of 8% of the wage to the worker's severance payment fund (FGTS). In the following row, we state the expected balance of this fund after 33.24 months, which is the expected duration of formal employment in the model. This information is used to calculate the severance payment, which is 50% of the total FGTS balance at the time of dismissal. Note that, of the 50% payment, 40% go to the dismissed employee and the remaining 10% are appropriated by the government. In addition, there was an additional temporary contribution to the FGTS fund of 0.5%, which expired in December 2006.

The largest cost that formal employers face is the social security contribution (INSS), which accounts for 20% of the nominal wage. Finally, there are some other smaller contributions, including mandatory insurance and contributions that are specific to the activity developed by the firm. We use Souza et al. (2012) as a reference in listing those contributions.

After all contributions are taken into account, we find that formal employers pay 57.7% more than the nominal monthly wage to each worker. However, this calculation does not take into account that formal employees are entitled to paid vacations of one month per year. Thus, although the employer pays for the 12 months in the year, each employee is only productive in 11 of them. In other words, for each 11 workers that the firm wants to use in production, 12 must be hired, because 1 in every 12 is expected to be in vacation at each time. After making the corresponding adjustments, we find that the total cost for each worker that the firm wants to use in production is 72.06% of the nominal wage in October 2003.

We then proceed to the calculation of the cost of formal employment in October 2012. The only change in regulations that affected the cost paid by the employer was the phasing out of the temporary FGTS contribution. When we exclude that contribution, we find that the equivalent payroll tax rate in October 2012 was 71.43% of the nominal wage.

Valuation of Mandated Benefits

In this subsection we account for all characteristics of formal employment that can make it more or less attractive to workers when compared with informal employment. Differently from the previous section, some of the items we consider affect low wage and high wage workers differently, such as the income tax. Thus, we have separate valuations for low wage workers and high wage workers. Low wage workers are those who earn exactly the minimum wage. The high wage worker is a representative agent for all other formal employees.

Table A.2 shows our calculations of the value attributed to benefits and contributions that calculated as fractions

Table A.1 – Cost of Formal Employment in October 2003

Item	Rationale	Value
Nominal wage (A)		**100.00**
13th salary (A.1)	1/12 of A	8.33
Vacation stipend (A.2)	0.33/12 of A	2.78
Advance notice	(A+A.1+A.2) x prob. dismissal	3.34
Raw total wage (B)		**114.45**
FGTS contribution (B.1)	8% of B	9.16
FGTS balance on dismissal (B.2)	*B.1 x average duration*	*304.36*
Severance payment	50% of B.2 x prob. dismissal	4.58
FGTS temporary extra	0.5% of B	0.57
Employer INSS contribution	20% of B	22.89
SAT, INCRA, S system	5.3% of B	6.07
Total with contributions (C)		**157.72**
Vacation adjustment	1/11 of C	14.34
Total cost		172.06
Payroll tax rate (τ)		**0.7206**

of the base salary. When taken together, these regulations compose the variable benefits parameters in the $benefits$ expression, a_s and a_u. The first five rows are similar to those in Table A.1: formal workers receive not only the nominal monthly wage, but also the thirteenth salary, the vacation stipend and the advance notification in case of dismissal. Two items are then deducted from the raw total wage: the social security (INSS) deduction and the income tax (IRPF). For the low wage workers, we use the lowest brackets: zero income tax in both years and social security deductions of 7.65% (in 2003) or 8.00% (in 2012). For the high wage workers, we calculate the deductions for each individual worker in the PME data set that receives more than the minimum wage, using the corresponding tax rates and brackets in each year. Then, we calculate the average deduction per worker.

The next four items are benefits that are valuable to formal workers. The first is the FGTS fund. Workers can withdraw money from their accounts in the FGTS fund, but only in a few special occasions: dismissal, retirement and when buying a house. In addition to being illiquid, resources in the fund are also less valuable than a direct payment because their returns are lower than the market interest rate. Souza et al. (2012) consider two extreme scenarios in their exercise: one in which the valuation of FGTS funds is 100% of the nominal balance, and other where workers do not value resources in the fund at all. They then report the valuation of benefits as a range. We take an intermediate route and assume that the value of deposits in the worker's FGTS account is 50% of the employer's actual disbursement.

The remaining benefits are the severance payment, the compulsory work accident insurance (SAT) and vacations. The first two items are calculated in a similar manner as in the previous subsection, when assessing the costs of formal employment. To input the valuation of vacations by workers, we use exactly the same value calculated as the cost of vacancy for employers. In this sense, vacations can be regarded as a transfer from firm to worker. Thus, if we calculate the difference between aggregate total payroll taxes and aggregate benefits, vacations and other transfers, such as the thirteenth salary, are canceled out, and we can use the result as government surplus in the model. We find that the net valuation of variable benefits is around 30% of the base salary for low wage workers, and around 23% for high wage workers.

The fixed benefits parameters (b_s^F, b_u^F) reflect a program called "abono salarial", which is an annual stipend

Table A.2 – Valuation of Variable Benefits

		October 2003		October 2012	
Item	Rationale	Low wage	High wage	Low wage	High wage
Nominal wage (A)		**240.00**	**848.00**	**622.00**	**1680.47**
13th salary (A.1)	1/12 of A	20.00	70.67	51.83	140.04
Vacation stipend (A.2)	0.33/12 of A	6.67	23.56	17.28	46.68
Advance notice	(A+A.1+A.2) x prob. dismissal	8.02	28.35	20.79	56.17
Raw total wage (B)		**274.69**	**970.57**	**711.90**	**1923.36**
INSS deduction	7.65%/7.93% (03) or 8.00%/8.27% (12) of B	-21.01	-76.97	-56.95	-159.06
Income tax (IRPF) deduction	0%/5.90% (03) or 0%/5.60% (12) of B	0.00	-57.26	0.00	-107.96
Valuation of FGTS fund	50% of employer contribution	10.99	38.82	28.48	76.93
Severance payment	40% of FGTS balance x prob. dismissal	8.79	31.06	22.78	61.55
Work accident insurance (SAT)	2% of B	5.49	19.41	14.24	38.47
Total with contributions (C)		**278.95**	**925.63**	**720.45**	**1833.29**
Vacation adjustment	Equal to the cost of vacation paid by employer	34.41	121.59	88.86	240.07
Total valuation		313.36	1047.22	809.30	2073.36
Variable benefits parameter		**0.306**	**0.235**	**0.301**	**0.234**

equal to the minimum wage paid to low wage workers (those who receive up to two times the minimum wage per month). To be eligible for this benefit, the employee must have been employed formally for at least five years (not necessarily in the same firm). We use the PME data set and estimate that 60% of formal employees who earn less than two minimum wages are entitled to the abono salarial. We thus find $b_u^F = 0.05$ $(0.6 \cdot 1/12)$. Only 40% of workers defined as high wage employees earn less than twice the minimum wage in the data. Thus, we set $b_s^F = 0.02$.

Finally, we calculate the unemployment insurance parameters (b_s^D, b_u^D). Unemployed workers who were previously employed formally for at least six months are entitled to unemployment benefits. Although the size of the monthly payments vary according to the wage in the last employment, there are caps on the minimum and maximum values paid. Low wage workers will always receive exactly one minimum wage, while most others will receive the maximum value of 1.87 times the minimum wage. The number of payments may vary from 3 to 5, according to the duration of all formal jobs in the last 36 months. For simplicity, we assume that the expected present value of these payments is equivalent to four times the value of each payment. Thus, $b_s^D = 4 \cdot 1.87 = 7.48$ and $b_u^D = 4$.

Appendix B: Informality Trends by Economic Activity

In this Appendix, we show that the decline in the informality rate in Brazil was widespread in the economy, and also that it was not caused by reallocation of workers across sectors. In the PME survey, workers report the economic activity to which their main job belongs, choosing one of 60 categories. In Table B.1, we list 15 economic activities with the largest number of workers. Together, they account for 76% of the workforce in 2003, and 78% in 2012. For each activity, we compute the formality rates in 2003 and 2012, and also the share of the workforce employed therein. Note that, since the PME targets workers in large metropolitan areas, few of them are employed in agricultural or extractive activities.

The first important observation is that formality increased in all economic activities listed. The share of formal workers increased more in activities that were initially more informal, but even the automotive and chemical industries experienced important gains in formalization. However, it is still possible that part of the decline was

Table B.1 – Informality Trends per Economic Activity

Economic activity	Formality rate			Share of workforce			Decomposition		
	2003	2012	Change	2003	2012	Change	Within	Between	Total
Construction	55.0	73.6	18.6	7.0	8.1	1.1	1.3	0.8	2.1
Leisure, culture, sports	55.3	65.7	10.4	2.5	2.1	-0.4	0.3	-0.2	0.0
Vehicle trading and repairs; fuel retail	60.2	73.5	13.3	4.3	3.9	-0.4	0.6	-0.3	0.3
Hospitality industry, restaurants	64.3	73.8	9.5	5.3	5.2	-0.2	0.5	-0.1	0.4
Trade and repair of personal/household objects	70.3	83.2	12.8	17.7	17.3	-0.4	2.3	-0.3	1.9
Education	72.6	81.6	9.0	4.4	4.2	-0.2	0.4	-0.1	0.3
Leather industry (including shoe crafting)	73.6	84.0	10.3	2.2	1.5	-0.8	0.2	-0.7	-0.4
Other activities	74.2	82.2	8.1	23.4	21.9	-1.5	1.9	-1.2	0.7
Terrestrial transportation	76.2	85.0	8.8	5.6	5.5	-0.1	0.5	-0.1	0.4
Food industry	77.2	86.1	8.9	2.7	2.6	-0.1	0.2	-0.1	0.1
Services for businesses	77.7	87.2	9.5	9.9	13.9	4.0	0.9	3.5	4.4
Metal crafting, including machines and equipment	78.7	83.9	5.2	2.4	1.9	-0.5	0.1	-0.4	-0.3
Health and social services	79.1	86.6	7.5	5.2	5.4	0.2	0.4	0.1	0.5
Real estate	80.8	84.2	3.4	3.5	2.6	-0.9	0.1	-0.7	-0.6
Chemical industry	88.5	92.9	4.4	2.3	1.8	-0.5	0.1	-0.5	-0.4
Automotive industry	93.1	95.9	2.8	1.5	2.1	0.7	0.0	0.6	0.7
Whole workforce	72.2	82.3	10.1	100.0	100.0	0.0	9.9	0.2	10.1

Notes: Informality is defined as proportion of workers without a signed labor card. Data does not include domestic workers, public servants or self-employed workers.

caused from workers migrating from less formal activities to others that are intrinsically more formal. To test this hypothesis, we decompose the contribution of each sector for the increase in formalization in the following way:

$$\begin{aligned}
\text{Total contribution}_i &= F_{i,2012}P_{i,2012} - F_{i,2003}P_{i,2003} \\
\text{Within contribution}_i &= P_{i,2003} \cdot (F_{i,2012} - F_{i,2003}) \\
\text{Between contribution}_i &= F_{i,2012} \cdot (P_{i,2012} - P_{i,2003})
\end{aligned}$$

where $P_{i,t}$ and $F_{i,t}$ denote the share of the workforce in and the formality rate of activity i in year t, respectively. The sum of the within contributions describe what would happen if the share of workers in each activity remained constant from 2003 to 2012, but the formality rates within each activity changed. The sum of between contributions accounts for the part of the decline in informality that can be attributed to changes in the size of each activity, given the formality rates in 2012. As can be seen in the bottom row of Table B.1, the decline in informality can be accounted for almost exclusively with changes within each activity.

The facts we show in this Appendix suggest that idiosyncratic shocks are unlikely to be the cause behind the formalization of the Brazilian labor market. This is the reason why we focus on factors that influenced the whole workforce, such as educational trends, enforcement policy and labor regulation.

Appendix C: Solution to the Problem of the Firm

Consider problem 1 and denote $\frac{\partial \Pi^{k,j}(n_s,n_u)}{\partial n_i} = J_i^{k,j}(n_s, n_u)$. The optimality of controls v_s, v_u yields:

$$-\xi + q(\theta_i)J_i^{k,j}(n_s^+, n_u^+) = 0$$

Also, differentiating the value function in n_i yields:

$$(1+rdt)J_i^{k,j}(n_s,n_u) = \frac{\partial \pi^{k,j}(\cdot)}{\partial n_i}dt + (1-s^j dt)J_i^{k,j}(n_s^+,n_u^+)$$

If we differentiate $\pi^{k,j}(\cdot)$ in n_i and restrict attention to steady-state equilibria, where $n_i^+ = n_i$, the two equations above result in 3 and 2 respectively.

Appendix D: Solution to the Wage Bargaining Equation

Throughout this exposition, we restrict attention to the problem of the formal firm. The solution is analogous for an informal firm, once we substitute $H(k,n_s,n_u) = F(k,n_s,n_u) - \rho(n_s+n_u)$ for the production function and set $\tau_i = b_i = 0$, $a_i = 1$. Also, for simplicity, we omit the productivity index in all functions.

The Nash bargaining equation is:

$$\sigma J_i(n_s,n_u) = (1-\sigma)\left[E_i\left(w_i(n_s,n_u)\right) - U_i\right]$$

Replacing equations 2 and 4 in the expression above, we find the following system of nonlinear differential equations:

$$c_i w_i(n_s,n_u) = (1-\sigma)(rU_i - b_i) + \sigma\left[F_i(n_s,n_u) - (1+\tau_s)n_s\frac{\partial w_s(\cdot)}{\partial n_i} - (1+\tau_u)n_u\frac{\partial w_u(\cdot)}{\partial n_i}\right] \tag{9}$$

where $c_i = [(1-\sigma)a_i + \sigma(1+\tau_i)]$.

The first step to solve this system is to write it in a more convenient way. Taking the partial derivative of 9 for $i = s$ with respect to n_u yields:

$$c_s\frac{\partial w_s(\cdot)}{\partial n_u} = \sigma\left[F_{su}(n_s,n_u) - (1+\tau_s)n_s\frac{\partial^2 w_s(\cdot)}{\partial n_s \partial n_u} - (1+\tau_u)n_u\frac{\partial^2 w_u(\cdot)}{\partial n_s \partial n_u} - (1+\tau_u)\frac{\partial w_u(\cdot)}{\partial n_s}\right]$$

where $F_{su}(n_s,n_u) = \frac{\partial^2 F(n_s,n_u)}{\partial n_s \partial n_u}$. Conversely, taking the derivative for $i = u$ with respect to n_s yields

$$c_u\frac{\partial w_u(\cdot)}{\partial n_s} = \sigma\left[F_{su}(n_s,n_u) - (1+\tau_s)n_s\frac{\partial^2 w_s(\cdot)}{\partial n_s \partial n_u} - (1+\tau_s)\frac{\partial w_s(\cdot)}{\partial n_u} - (1+\tau_u)n_u\frac{\partial^2 w_u(\cdot)}{\partial n_s \partial n_u}\right]$$

The difference between these two equations gives us the following expression:

$$\frac{\partial w_s(\cdot)}{\partial n_u}\left[c_s - \sigma(1+\tau_s)\right] = \frac{\partial w_u(\cdot)}{\partial n_s}\left[c_u - \sigma(1+\tau_u)\right]$$

Using the definition of c_i, we obtain:

$$\frac{\partial w_s(\cdot)}{\partial n_u} = \frac{a_u}{a_s}\frac{\partial w_u(\cdot)}{\partial n_s}$$

Which we can use to write the system of equations defined in 9 as:

$$c_i w_i(n_s,n_u) = (1-\sigma)(rU_i - b_i) + \sigma\left[F_i(n_s,n_u) - (1+\tau_i)\left(\chi_{i,s}n_s\frac{\partial w_i(\cdot)}{\partial n_s} + \chi_{i,u}n_u\frac{\partial w_i(\cdot)}{\partial n_u}\right)\right] \tag{10}$$

where

$$\chi_{i,j} = \frac{a_i(1+\tau_j)}{a_j(1+\tau_i)}$$

Following Cahuc, Marque and Wasmer (2008) (henceforth CMW), we first solve the equation for the case in which $\chi_{i,j} = 1$. Later, we generalize the solution. The insight in CMW is to perform a change of coordinates that allows us to express the term multiplying $(1+\tau_i)$ in equation 10 in a simpler manner, effectively obtaining a univariate differential equation as the result. The transformation we need is:

$$\begin{aligned} n_s &= \rho\cos\phi \\ n_u &= \rho\sin\phi \end{aligned}$$

Now if we let $\hat{w}_i(\rho,\phi) = w_i(\rho\cos\phi, \rho\sin\phi)$, we can find that:

$$\begin{aligned} \rho\frac{\partial\hat{w}_i(\rho,\phi)}{\partial\rho} &= \rho\left[\cos\phi\frac{\partial w_i(\cdot)}{\partial n_s} + \sin\theta\frac{\partial w_i(\cdot)}{\partial n_s}\right] \\ &= n_s\frac{\partial w_i(\cdot)}{\partial n_s} + n_u\frac{\partial w_i(\cdot)}{\partial n_u} \end{aligned}$$

Which is the term multiplying $(1+\tau_i)$ in equations 10 if $\chi_{i,j} = 1$. Following the same notation, let $\hat{F_{n_i}}(\rho,\phi) = \frac{\partial F(\rho\cos\phi,\rho\sin\phi)}{\partial n_i}$ denote the marginal product function in the new coordinate system. We can then rewrite the differential equations as:

$$\frac{\partial\hat{w}_i(\rho,\phi)}{\partial\rho} + \frac{c_i}{\sigma(1+\tau_i)\rho}\hat{w}_i(\rho,\phi) = \frac{1-\sigma}{\sigma(1+\tau_i)\rho}(rU_i - b_i) + \frac{1}{(1+\tau_i)\rho}\hat{F_{n_i}}(\rho,\phi) \tag{11}$$

We guess the following form for the solution:

$$\begin{aligned} \hat{w}_i(\rho,\phi) &= C(\rho)\rho^{-\frac{c_i}{\sigma(1+\tau)}} + D \\ \frac{\partial\hat{w}_i(\rho,\phi)}{\partial\rho} &= C'(\rho)\rho^{-\frac{c_i}{\sigma(1+\tau)}} - C(\rho)\frac{c_i}{\sigma(1+\tau)}\rho^{-\frac{c_i}{\sigma(1+\tau)}-1} \end{aligned} \tag{12}$$

Plugging these expressions back in differential equation, we get:

$$\begin{aligned} D &= \frac{1-\sigma}{c_i}(rU_i - b_i) \\ C'(\rho) &= \rho^{\frac{c_i}{\sigma(1+\tau_i)}-1}\frac{1}{1+\tau_i}\hat{F_{n_i}}(\rho,\phi) = \rho^{\frac{1-\sigma}{\sigma}\frac{a_i}{1+\tau_i}}\frac{1}{1+\tau_i}\hat{F_{n_i}}(\rho,\phi) \end{aligned}$$

Which we can integrate to:

$$C(\rho) = \frac{1}{1+\tau_i}\int_0^\rho x^{\frac{1-\sigma}{\sigma}\frac{a_i}{1+\tau_i}}\hat{F_{n_i}}(x,\phi)dx + \kappa(\phi)$$

Replacing in 12, we get:

$$\hat{w}_i(\rho,\phi) \quad = \quad \frac{1-\sigma}{c_i}(rU_i - b_i) + \frac{\rho^{-\frac{1-\sigma}{\sigma}\frac{a_i}{1+\tau_i}-1}}{1+\tau_i}\left[\int_0^\rho x^{\frac{1-\sigma}{\sigma}\frac{a_i}{1+\tau_i}}\hat{F_{n_i}}(x,\phi)dx + \kappa(\phi)\right]$$

We assume that $\lim_{\rho\to 0}\rho w_i(\rho,\phi) = 0$ – that is, payroll tends to zero as firm size decreases while keeping the ratio of skilled to unskilled workers constant. This condition can be satisfied if marginal productivities do not increase too fast as the number of worker decreases, which is the case in our application. Then, the equation above implies $\kappa(\phi) = 0$. In addition, we change the integration variable to $z = x/\rho$. With that modification, we can easily change back to the rectangular coordinates by noting that $\hat{F_{n_i}}(x,\phi) = \hat{F_{n_i}}(z\rho,\phi) = F_{n_i}(zn_s, zn_u)$. The solution is given by:

$$w_i(n_s,n_u) \quad = \quad \frac{1-\sigma}{c_i}(rU_i - b_i) + \frac{1}{1+\tau_i}\int_0^1 z^{\frac{1-\sigma}{\sigma}\frac{a_i}{1+\tau_i}}\frac{\partial F(zn_s,zn_u)}{\partial n_i}dz$$

Now we consider the case in which $\chi_{i,j} = \frac{a_i(1+\tau_j)}{a_j(1+\tau_i)} \neq 1$. We perform another coordinate change, introducing a new set of variables $M_i = (M_{is}, M_{iu})$, with the goal of writing:

$$\sum_{j=s,u} M_{ij}\frac{\partial \tilde{w}_j(M_i)}{\partial M_{ij}} = \sum_{j=s,u}\chi_{ij}n_j\frac{\partial w_i(n_s,n_u)}{\partial n_j}$$

with $\tilde{w}_i(M_i) = w_i(n_s,n_u)$. Denote by $\tilde{F}(M_i) = F(n_s,n_u)$ the production function in the new coordinate system. To find M_i as a function of n_s and n_u, we assume that M_{ij} only depends on n_j. In this case,

$$\frac{\partial w_i(\cdot)}{\partial n_j} = \frac{\partial \tilde{w}_i(\cdot)}{\partial M_{ij}}\frac{\partial M_{ij}}{\partial n_j}$$

Also, we further impose that

$$M_{ij}\frac{\partial \tilde{w}_i(\cdot)}{\partial M_{ij}} = \chi_{ij}n_j\frac{\partial w_i(n_s,n_u)}{\partial n_j}$$

in order to fulfill the initial requirement on the M_i variables. Combining these expressions, we find a differential equation for M_{ij}:

$$M_{ij} = \chi_{ij}n_j\frac{\partial M_{ij}}{\partial n_j}$$

We only need one solution, the simplest being

$$M_{ij} = n_j^{\frac{1}{\chi_{i,j}}} = n_j^{\chi_{j,1}}$$

since $1/\chi_{i,j} = \chi_{j,i}$. Then, using $\partial F/\partial n_j = \chi_{j,i}n_j^{\chi_{j,i}-1}\partial\tilde{F}/\partial M_{i,j}$ and $\partial F/\partial n_i = \partial\tilde{F}/\partial M_{i,i}$ as $\chi_{i,i} = 1$, the system 10 can be rewritten as

$$c_i\tilde{w}_i(M_{is},M_{iu}) = (1-\sigma)(rU_i - b_i) + \sigma\left[\frac{\partial\tilde{F}(M_i)}{\partial M_{ii}} - (1+\tau_i)\left(M_{is}\frac{\partial\tilde{w}_i(M_i)}{\partial M_{is}} - M_{iu}\frac{\partial\tilde{w}_i(M_i)}{\partial M_{iu}}\right)\right] \tag{13}$$

System 13 is equivalent to system 10 in the case where $\chi_{i,j} = 1$. Thus, the solution for $\tilde{w}_i(M_{is}, M_{iu})$ is known:

$$\tilde{w}_i(M_{is}, M_{iu}) \quad = \quad \frac{1-\sigma}{c_i}(rU_i - b_i) + \frac{1}{1+\tau_i}\int_0^1 z^{\frac{1-\sigma}{\sigma}\frac{a_i}{1+\tau_i}} \tilde{F}_i(zM_{is}, zM_{iu})dz$$

where $\tilde{F}_i$ is the derivative of function $\tilde{F}$ with respect to its argument $i = 1, ..., n$. Switching back to the original coordinate system, we obtain:

$$w_i(n_s, n_u) = \frac{1-\sigma}{c_i}(rU_i - b_i) + \frac{1}{1+\tau_i}\int_0^1 z^{\frac{1-\sigma}{\sigma}\frac{a_i}{1+\tau_i}} \frac{\partial F\left(z^{\frac{1+\tau_s}{a_s}\frac{a_i}{1+\tau_i}} n_s, z^{\frac{1+\tau_u}{a_u}\frac{a_i}{1+\tau_i}} n_u\right)}{\partial n_i} dz \tag{14}$$

This wage equation is easily differentiable with regard to the number of employed workers of any type:

$$\frac{\partial w_i(n_s, n_u)}{\partial n_j} = \frac{1}{1+\tau_i}\int_0^1 z^{\frac{a_i}{1+\tau_i}\left(\frac{1-\sigma}{\sigma}+\frac{1+\tau_j}{a_j}\right)} \frac{\partial^2 F\left(z^{\frac{1+\tau_s}{a_s}\frac{a_i}{1+\tau_i}} n_s, z^{\frac{1+\tau_u}{a_u}\frac{a_i}{1+\tau_i}} n_u\right)}{\partial n_i \partial n_j} dz \tag{15}$$

To compare the solution we found to that in CMW, write $\tilde{\sigma}_i = \frac{\sigma(1+\tau_i)}{\sigma(1+\tau_i)+(1-\sigma)a_i} = \frac{\sigma(1+\tau_i)}{c_i}$. Then, equation 14 can be stated as:

$$a_i w_i(n_s, n_u) = (1-\tilde{\sigma}_i)(rU_i - b_i) + \frac{a_i}{1+\tau_i}\int_0^1 z^{\frac{1-\tilde{\sigma}_i}{\tilde{\sigma}_i}} \frac{\partial F\left(z^{\frac{1+\tilde{\sigma}_i}{\tilde{\sigma}_i}\frac{\tilde{\sigma}_s}{1-\tilde{\sigma}_s}} n_s, z^{\frac{1+\tilde{\sigma}_i}{\tilde{\sigma}_i}\frac{\tilde{\sigma}_u}{1-\tilde{\sigma}_u}} n_u\right)}{\partial n_i} dz \tag{16}$$

This expression is very similar to the solution in CMW, except for the terms a_i and $a_i/(1+\tau_i)$. Consider the case where $\alpha_i = 1 + \tau_i$: the valuation of formal benefits by workers is exactly equal to the total costs incurred by firms. In this case, $\tilde{\sigma}_i = \sigma$ and the only difference between our solution and that in CMW is a term a_i multiplying w_i on the left-hand side. This factor accounts for the fact that the "true" wage in this economy is $(1+\tau_i)w_i = a_i w_i$, which is both the value that firms pay and how workers value total compensation.

If $\tau_i \neq a_i - 1$, then there is a wedge between firm disbursements and the valuation of total pay by workers, and $\tilde{\sigma}_i \neq \sigma$. Note that this does not mean that the share of rents appropriated by workers is different; instead, this is an adjustment inside the integral term to compensate for the term $a_i/(1+\tau_i)$ outside the integral, keeping the Nash bargaining equation valid. However, even in the case where σ is the same for all workers, we can have $\tilde{\sigma}_i \neq \tilde{\sigma}_j$. This would lead to non-trivial interactions between different types of labor in a similar manner to how heterogeneity in bargaining power affects wages in CMW.

Finally, note that, although we have assumed the same bargaining power for all workers, it is immediate to extend it to the more general case with type-specific bargaining power. This would lead to an expression similar to 16, but with $\tilde{\sigma}_i = \frac{\sigma_i(1+\tau_i)}{\sigma_i(1+\tau_i)+(1-\sigma_i)a_i}$. Similarly, extending the solution to more than two types of workers would be trivial, requiring essentially a change in notation. See CMW, in particular how they define the matrix $\mathbf{NA}_i(z)$.

Minimum wages and wage bargaining

The solution we found above for the wage bargaining differential equation, $w_i(n_s, n_u)$, does not take into account the possibility of a minimum wage. If we set a rule that constrains wages to be no less than a constant value, then

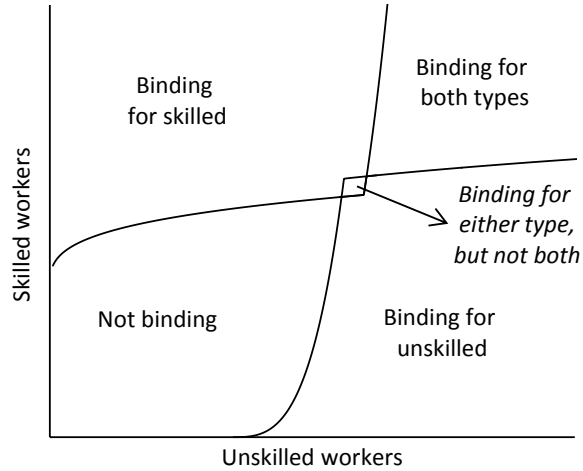

Figure D.1 – Minimum Wage Status According to Firm Size

the previous solution is only correct in the interior of the subset of the (n_s, n_u) space in which the minimum wage is less than the freely bargained wage. For other values of (n_s, n_u), the minimum wage binds for the skilled, unskilled, or both.

Figure D.1 shows an example of how wages can be affected by the minimum wage according to firm size. For small values of n_s and n_u, marginal productivities are high and bargained wages are above the minimum wage. As the quantity of either type of worker increases, it is possible that marginal productivities decrease so much that the minimum wage binds. For high values of both of inputs, it is possible that all wages equal the minimum wage. In this example, the curves are upward sloping because there is complementarity between labor types ($\frac{\partial^2 F^k(n_s,n_u)}{\partial n_s \partial n_u} > 0$). They would be straight or downward sloping if that cross derivative was null or negative, respectively.

It is also possible that, for certain values of (n_s, n_u), there is multiplicity of wages satisfying the bargaining conditions: either type of worker might receive the minimum wage, but not both. This pathology is caused by discontinuities in the marginal value of workers which we discuss below. In our applications, there is no possibility that the minimum wage binds for the skilled, no matter how many workers of this type are hired. The reason is that the first term in the wage equation 14, related to the reservation wage, is strictly greater than the minimum wage in all simulations. Hence, we are not concerned about this multiplicity problem.

If the minimum wage binds for only one type of worker, the unrestricted solution for the other type is no longer adequate. This is because, contrary to what is implied in the wage bargaining differential equation, marginal changes in the amount of the unconstrained type do not affect wages of the constrained type. From now on, for ease of exposition and focusing on our empirical application, we restrict attention to the case in which the minimum wage binds for unskilled workers, but not for skilled workers.

To find the correct skilled wage function in this case, we observe that the differential equation 9 simplifies to:

$$c_i w_s(n_s, n_u) = (1-\sigma)(rU_i - b_i) + \sigma \left[F_s(n_s, n_u) - (1+\tau_s) n_s \frac{\partial w_s(n_s, n_u)}{\partial n_s} \right] \tag{17}$$

as the term $\frac{\partial w_u(n_s,n_u)}{\partial n_s}$ is set to zero. This is a univariate differential equation in n_s, similar to 11. The solution is

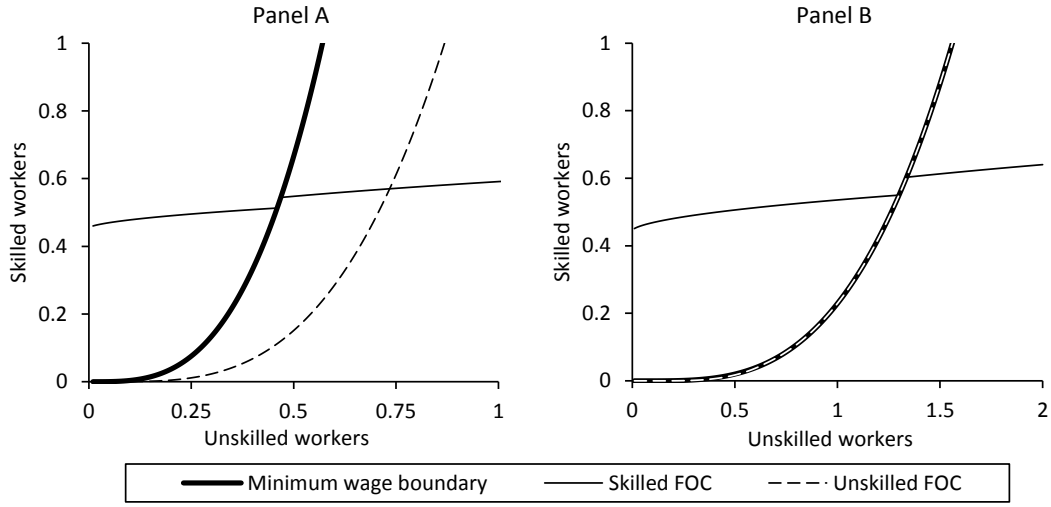

Figure D.2 – Problem of the Firm and Minimum Wages

analogous:

$$w_s^{k,for}(n_s, n_u) = \frac{1-\sigma}{c_s}(rU_s - b_s) + \frac{1}{1+\tau_s}\int_0^1 z^{\frac{1-\sigma}{\sigma}\frac{a_s}{1+\tau_s}} \frac{\partial F^k(zn_s, n_u)}{\partial n_i} dz$$

Note that skilled wages are still a function of the number of unskilled workers, but not the same function as before. When the cross derivative of the production function $\frac{\partial^2 F^k(n_s,n_u)}{\partial n_s \partial n_u}$ is positive, as in our quantitative exercises, then we should expect this new wage function to be strictly greater than the unrestricted one for the same values of n_s and n_u. The reason is that, in the unrestricted case, hiring an additional skilled worker leads to an increase in unskilled wages due to the effect in the unskilled marginal productivities, but this effect does not exist (at the margin) when the minimum wage binds. Note that this will lead to a discontinuity in the wage function at the points that separate the regions where the minimum wage is or is not binding.

Minimum wages and the solution to the problem of the firm

Finally, we discuss how the existence of the minimum wage might change the problem of choosing the optimal firm size. The discontinuity in the wage function, discussed above, is caused by discrete changes in the net marginal value of workers $J_i^{for}(\cdot)$ (see equation 2) at the boundary of region of the (n_s, n_u) space where the minimum wage is binding. This discontinuity might lead to cases in which there is no exact solution to the firm's first order condition, equation 3. We continue to restrict attention to the case in which the minimum wage binds only for unskilled workers.

In figure D.2, we show how the minimum wage can affect the problem of the firm. In Panel A, we illustrate the problem of a formal firm with average productivity ($k = 1$) in our baseline calibration. The heavy solid line marks the transition between a non-binding and a binding minimum wage for the unskilled as the number of unskilled workers increases – that is, it is the vertical line in figure D.1. The other lines are the optimality conditions for the number of skilled and unskilled workers (equation 3). The solid line marks the combinations of (n_s, n_u) in which the marginal value of a skilled worker, $J_s^{for}(n_s, n_u)$, is equal to the expected search cost $\frac{(r+s^{for})\xi}{q(\theta_s)}$. Above this line, there are too many skilled workers, which drives down their marginal productivity and makes the marginal value be less than the search cost. The same reasoning is valid for the dashed line: to the right of it, the marginal value

of unskilled workers is less than the expected search cost, and the converse is true to the left of the line. As before, the upward slope of all curves comes from complementarity between labor inputs.

The unique solution to the problem of the firm in Panel A is the point where the two first order conditions are satisfied. Since this point is to the right of the heavy solid line, the minimum wage is binding at the optimal firm size. Note that there is a discontinuity in the skilled worker's first order condition as it crosses the minimum wage boundary. Since the marginal value of skilled workers increases when the minimum wage binds for the unskilled, it becomes optimal to hire more skilled workers immediately to the right of the boundary. There is a similar discontinuity in the value of the unskilled worker, but in the opposite direction: to the right of the boundary, hiring an additional unskilled worker no longer benefits the firm by bringing down unskilled wages. However, in this case, the discrete decrease is not enough to reduce the marginal value of the unskilled to below the search cost. This is why the dashed line lies to the right of the minimum wage boundary.

Panel B describes a case in which there is no solution to the problem of the firm because of the discontinuities associated with the minimum wage. It follows from a change in the baseline model that increases overall productivity (parameter A in the quantitative experiments section). The difference between Panel B and Panel A is that the discrete fall in the value of the unskilled workers is enough to cause it to drop from a number strictly greater than the expected search costs to another strictly less than it. As a consequence, there is no point in the graph in which the unskilled first order condition is satisfied. The skilled first order condition is not satisfied either at the intersection of the three lines.

In such situation, the firm would strategically choose a point to the left of that intersection, as bargained wages for skilled workers would be discontinuously lower. This is optimal because there is no similar discontinuity in unskilled wage, since it cannot discretely drop below the minimum wage, and thus unskilled wages are approximately equal on both sides of the boundary. In our numerical applications, the optimal firm size in those situations is chosen by finding the point (n_s^*, n_u^*) that satisfies the first order condition for skilled workers and lies immediately to the left of the discontinuity.

Note that, in the absence of the minimum wage, we would expect the firm to hire more unskilled workers, since the dashed line would lie to the right of the heavy solid line. Whether the firm would hire more or less skilled workers depend on the degree of complementarity between the two types of labor in the production function.

Minimum Wage Effects at Different Enforcement Levels: Evidence from Employment Surveys in India*

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Abstract

I present the first piece of empirical evidence on the effects of minimum wage legislation throughout the minimum wage distribution across different enforcement levels. Instrumental variable estimates indicate a hump-shaped relationship between employment and minimum wage at median and higher enforcement levels, but a negative relationship at lower levels of enforcement. Between wages and minimum wage, a positive relationship uniformly emerges at median and higher levels of enforcement but only at the upper tail of the minimum wage distribution at low levels of enforcement. Results are consistent with a model of imperfect competition and imperfect enforcement.

JEL Classification: J30, J38.

Keywords: Minimum wage, enforcement, employment, monopsony

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1 Introduction

How do the effects of minimum wages on the labor market vary according to the level of enforcement? To date, no empirical study in the minimum wage literature has addressed this question. Empirical studies consistent with the standard competitive neo-classical model and monopsonisitc or oligopsonistic models assume perfect enforcement of the minimum wage legislation (Card and Krueger, 1994; Card and Krueger, 2000; Neumark and Wascher, 2000; Machin and Wilson, 2004; Dube, Lester and Reich, 2010). However, this assumption does not accord with the growing empirical evidence of non-compliance of labor regulations (including minimum wage) in both developed and developing countries. Important studies in this regard include Ashenfelter and Smith (1979) who found that compliance with the minimum wage law during the early 1970s in the United States was just 64%. Also, Ronconi (2010) reports that compliance with employment regulations in Argentina between 1995 and 2002 was just 48.26%.This evidence underscores that the enforcement of the minimum wage legislation is as important as the level of minimum wage itself.

With perfect enforcement, the standard competitive labor market model predicts that the response of employment to a binding minimum wage hike is uniformly negative. Contrarily, models of imperfect competition predict a positive response of employment, as long as the minimum wage is below a threshold (Stigler, 1946). However, recent theoretical work by Basu, Chau and Kanbur (2010), henceforth BCK, incorporating elements of imperfect enforcement in an imperfectly competitive labor market model predicts that the equilibrium response to minimum wages depends intricately on the interaction between enforcement and the minimum wage.

The above discussed theoretical results have empirical implications that beg to be tested, and the present study precisely investigates those implications in the Indian context. Specifically, it asks two questions: First, how does a minimum wage affect the level of employment, wage, and days of work across the minimum wage distribution? Second, do these relationships vary across the level of enforcement? Using a repeated-cross sectional dataset from the nationally representative employment surveys of India (administered by the National Sample Survey) for the years 2004, 2004-05, 2005-06, 2007-08, 2009-10, and 2011-12, this study estimates the interactive effect of minimum wages and enforcement on employment, wages and days of work

in the construction industry.

This paper contributes to the minimum wage literature in three key ways. First, evidence in the empirical minimum wage literature supports competitive labor market models as well as imperfectly competitive models and the issue still remains open for debate. Many recent and older studies based on developed countries and developing countries find negative employment effects supporting the competitive theory (Burkhauser, Couch and Wittenburg, 2000, Neumark and Wascher, 2000, Neumark, Schweitzer and Wascher, 2000, for the US; Machin, Manning and Rahman, 2002, for the UK; Abowd et al., 2000, for France; Bell (1997) for Mexico and Colombia, Montenegro and Pags (2004) for a group of Latin American countries). Positive or insignificant employment effects, supporting imperfectly competitive models, are also found in a number of old and new studies alike, both in developed and in developing countries (see Card and Krueger (1994) and Dube et al (2010) for United States, Lemos (2004) for Brazil; Dickens, Machin and Manning (1999) for the United Kingdom; Abowd et al., 2000, for United States). The nature of minimum wage effects (sign and significance of coefficients) on employment observed in this paper can point towards one labor market model versus the other, contributing directly to the above debate.

Second, this paper addresses a key weakness in the above literature - the lack of studies accounting for the imperfect nature of labor enforcement and non-compliance with labor laws. In developing countries, and to an extent in developed countries, there is high non-compliance with labor laws, and the de facto level of regulation is lower than the de jure level of regulation (Ronconi, 2005). Studies find non-compliance in United States (Ashenfelter and Smith, 1979), Argentina (Ronconi, 2010), South Africa (Bhorat, Kanbur, and Mayet, 2012), Brazil (Lemos, 2004, 2006), Costa Rica (Gindling and Terrell, 1995), Mexico (Bell, 1997), Trinidad and Tobago (Strobl and Walsh, 2001), Chile (Kanbur, Ronconi, and Wedenoja, 2013) and a selection of Latin American countries (Maloney and Nunez, 2004). The present study directly addresses this above weakness by controlling for enforcement and enforcement interacted with minimum wage in its empirical models.

Third, only few studies estimate minimum wage effects throughout the minimum wage distribution (Neumark, Schweitzer and Wascher, 200; Dickens, Machin and Manning, 1999) although theories predict non-linear effects (e.g. Stigler, 1946). This paper, in that spirit, without binding relationships to be linear, employs flexible form models to estimate minimum

wage effects throughout the distribution.

Gauging the effects of minimum wage increase throughout the minimum wage distribution at different levels of enforcement presented a few empirical challenges. First, the level of enforcement at the state level is possibly endogenous because factors determining labor market outcomes may also affect how strictly states enforce the minimum wage law. A candidate measure for the level of enforcement of minimum wages is the number of inspectors at the state level under The Minimum Wages Act, 1948. To address the endogeneity in this variable, number of inspectors under The Factories Act, another state-level regulation, is used as an instrument. The Factories Act, 1948, concerns health and safety violations in factories in the registered manufacturing industry. This is a relevant instrument because both factories and the minimum wage divisions, falling under the same state labor department, are subjected to similar shocks. Also, exclusionary criteria are plausibly satisfied because factories inspectors check health and safety violations of factory workers and do not deal with minimum wages in the construction industry.

The second challenge is in estimating non-linear minimum wages effects and interactive effects of minimum wages and enforcement as suggested by theories. Non-linear effects, particularly hump-shaped effects of minimum wages on employment, are suggested by Stigler (1946)s model of imperfect competition. The interaction effects capturing cross elasticities of labor market outcomes with respect to minimum wages and enforcement, are suggested by BCK who incorporated imperfect enforcement in Stiglers model of imperfect competition. BCK show that the effects of minimum wage depends intricately both on the level of minimum wage and its interaction with the level of enforcement. In this paper, I capture non-linear minimum wage effects by dummy variables representing various quartiles of minimum wages and interactive effects by explicitly interacting the minimum wage dummy variables with the continuous enforcement variable.

The present study focuses on the construction industry in India, the second largest employer (after agriculture) employing 32 million workers in 2009-10. It is a dynamic industry that contributed to 8% of the countrys Gross Domestic Product in 2012-13 (approximately $124 billion) and grew at 14.58% on average between 2000-01 and 2011-12 (a rise of $104 in the current U.S dollars or 6475 billion Indian rupees). Despite the growth and employment generation in the construction industry, a majority of workers receive wage payments below the

minimum wage. In 2009-10, 52% of the construction workers nationwide received wages below the minimum and state specific noncompliance varied from as low as 4% to as high as 90%. There is qualitative evidence that contractors employing workers exert considerable monopsony power in payment of wages (Self-Employed Womens Association, 2005).

Studying minimum wage effects across enforcement regimes in the Indian context, is worthwhile for a number of reasons. First, there is state-time variation in minimum wages in India. Minimum wages, under the Minimum Wage Act, 1949, are set and revised by the state governments and revisions occur once or at most twice every year. Second, there is evidence of imperfect enforcement in India. A comparison of minimum wage violations estimated from worker reported National Sample Survey data and government reports on detection of violation reveals the starkness of this phenomenon. According to the National Sample Survey, 37% of the workers working in all industries throughout India received wages below the minimum wage in 2009-10. In contrast, only 2.1% of inspections lead to discoveries of violations in the same year. Remarkably, only about one-fifth of violations are detected by the government. Further, enforcement also varies across state and time, a setting unique to India which provides a platform to study the interactive effects of minimum wages and enforcement.

Ordinary Least Squares regression and Instrumental Variables two-stage least squares regression methods are employed. Additionally, Probit and Instrumental Variable Probit regressions are employed to model binary employment outcomes. Two sets of results are striking. First, there is a hump-shaped relationship between employment (as measured by participation in the construction industry) and minimum wage at median and higher levels of enforcement (at the 50th and 75th percentiles). However, at lower levels of enforcement (the 25th percentile), there is a negative relationship between employment and minimum wage. Second, there is a positive and an increasing relationship between wages and minimum wages at median and higher levels of enforcement (at the 50th and 75th percentiles). However, at low levels of enforcement (the 25th percentile), there is a positive effect on wages but only at the upper tail of the minimum wage distribution. The non-linearly in the minimum wage effects and the role of enforcement in above estimated relationships is striking, particularly for employment effects.

The empirical results are largely consistent with a model of imperfect competition and imperfect enforcement (BCK) and contrary to the neoclassical model which predicts a uniform negative effect on employment. Stigler's model predicts that employment responses to mini-

mum wage are positive until a threshold (the competitive wage equilibrium in this case) and negative beyond that. BCK's model of imperfect competition and imperfect enforcement, built on Stiglers model, predicts that the turnaround threshold of the minimum wage at which employment response changes from positive to negative,changes based on the level of enforcement. The lower the level of enforcement, the smaller the threshold. This theory has clean testable implications. At high levels of enforcement, the upward sloping part of the employment response to minimum wage is to be observed for a relatively long interval of the minimum wage distribution. Consequently, the hump shape is very distinct at higher levels of enforcement. However, at low levels of enforcement, comparatively, the upward sloping part of employment response to minimum wages is to be observed for a relatively short interval. This could even be approximately observed as uniform negative effects at very low levels of enforcement, depending on the estimation methodology. [1] This is precisely what is observed in the empirical results. Uniform negative employment effects are observed in low levels of enforcement but a hump-shape emerges at higher levels of enforcement.

The rest of the paper is organized as follows. Section 2 describes the data; Section 3 provides institutional details on minimum wages and enforcement; Section 4 presents the econometric methodology; Section 5 presents the results and their interpretation; Section 6 presents robustness checks and results for specific demographic groups; Section 7 concludes the paper and discusses further research possibilities.

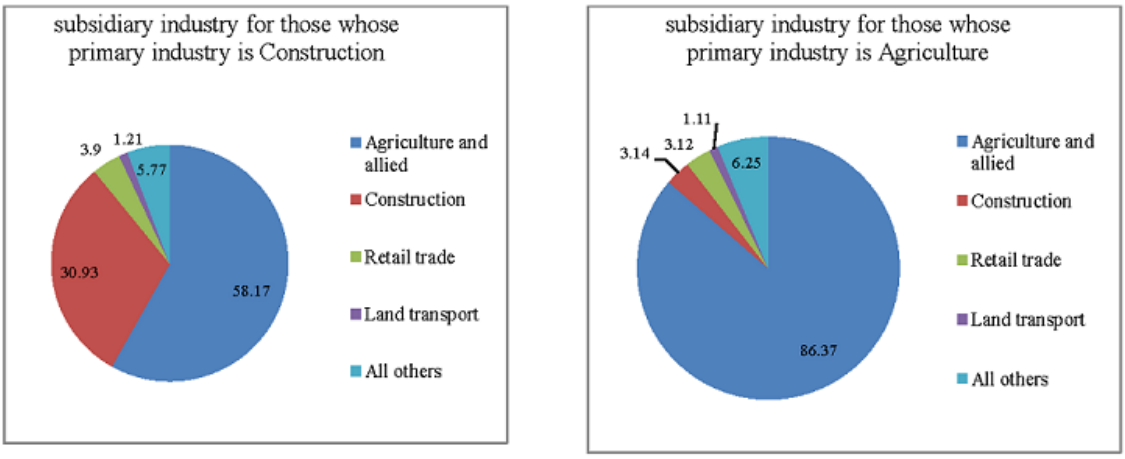
2 Data Description

The primary data source for this study are six rounds of the National Sample Surveys (NSS) administered in the years 2004, 2004-05, 2005-06, 2007-08, 2009-10, 2011-12. These surveys are conducted from July to June. For example, the 2004-05 survey is conducted from July 2004 to June 2005. The exception is the survey in 2004 which took place from January to June 2004. These are cross section surveys conducted at the household level, inquiring on characteristics of the household, the numerous demographic particulars of all individuals, their

[1] In Stigler's model of perfect enforcement, as long as the minimum wage is below the turnaround threshold, an increase in minimum wage decreases the marginal cost of labor. Hence, employment responses are positive below the threshold. Above the threshold, an increase in minimum wage increases marginal labor cost; consequently, employment responses are negative. The same argument holds in the case of imperfect enforcement in BCK, except now that we are looking at how expected marginal cost of labor changes below and above the threshold and consequently affects employment responses. The threshold itself is a function of enforcement, which is measured by the probability of detection of violation.

employment status and characteristics. Among other things, every member of the household is asked to report up to four activities they did in the last seven days, which can include looking for work (unemployed), not looking for work (not in the labor force), or working (employed), and if employed, the industry and occupation of the industry they were employed in. Additionally, the number of days spent in each activity and earnings from the previous week for wage earners are reported for the last week. The key outcomes variables considered in this paper are employment, wages, and days of work in the construction sector [2]. I describe these key variables below.

Figure 1: Principal and Subsidiary industry based on a weekly recall



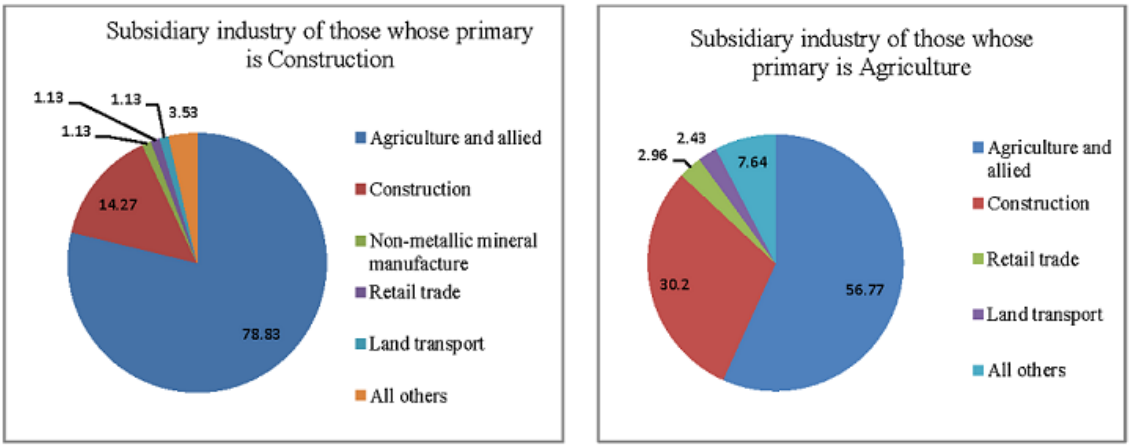
Note: Data from National Sample Survey for the years 2004, 2004-05, 2005-06, 2007-08, 2009-10, and 2011-12. Principal and subsidiary industries based on a weekly recall are determined based on a time criterion. Industry in which the most time was spent, is the principal industry.

A defining characteristic of the Indian low-wage labor force is that workers tend to be employed in multiple low paying jobs over the course of the year and within a week. For the purpose of this paper, I employ a neighbor criterion to measure employment in the construction industry. Employment in the construction industry is defined as a binary variable taking a value 1 if the worker works in the construction industry and 0 if the worker works in agriculture, the closest neighbor to the construction industry. Industry B as a neighbor to industry A if most workers working in A for their principal work, work in B for their subsidiary work, and vice-versa. Employment, defined this way captures extensive margin, not in the classical sense

[2]Household members also report labor market activities during the reference period of 365 days preceding the date of the survey (that is, a yearly recall period). They report principal and subsidiary employment in the last one year, but they do not report earnings or number of days of work from this recall.

of working versus not working, but rather working in the construction industry versus working in the neighboring industries. Figures 1 and 2 present pie-charts of employment in subsidiary industries for wage earners whose primary industry is construction or agriculture, based on a weekly and yearly recall period respectively. It is seen that, those engaged in the construction industry for their primary job, work predominantly in agriculture for their subsidiary job. Similarly, those engaged in agriculture as their primary job, tend to work in agriculture as their secondary job (perhaps plant another crop in the lean season) but a good majority of them are also engaged in construction (this is more obvious from the yearly recall).

Figure 2: Principal and Subsidiary industry based on a yearly recall



Note: Data from National Sample Survey for the years 2004, 2004-05, 2005-06, 2007-08, 2009-10, and 2011-12. Principal and subsidiary industries based on a weekly recall are determined based on a majority criterion. Industry in which the most time was spent, is the principal industry.

The final dataset consists of a homogenous group of workers who share similar social and demographic characteristics and for whom minimum wages are potentially binding. I consider unskilled construction and agriculture workers (classified based on the National Industrial Classification and National Classification of Occupation [3]), and who are educated below middle school, or illiterates. There are 37, 339 observations for all years and states altogether. 48% of the overall sample consists of construction workers and the rest are agriculture workers.

[3]Semi-skilled and unskilled workers are defined based on the occupational classification reported by the workers in the National Sample Survey. In this paper, occupational categories, 712, 713, 714 and 931, under Indias National Occupational Classification, 2004 are classified as unskilled and semi-skilled construction workers. Under NCO 1968 for survey years before 2007-08, occupational classifications 871, 931, 951 to 959 are considered unskilled. For Agriculture: 611 to 620 and 920 under NCO 2004 and 610 to 662, and 670 to 681 are considered unskilled.

3 Minimum wages and the enforcement machinery

The Minimum Wages Act 1948 of India legally grants a minimum wage (MW) for workers in many industries and they are defined in Rupees per day at the state level for each covered industry [4]. They are set, implemented and enforced by state (and a few cases, the central) governments [5]. Existence of a large number of minimum wages for different industries/occupation in each state across years makes Indias system of minimum wages complicated [6]. Further, it makes enforcement cumbersome, even in theory. State governments enforce the minimum wage law through a cadre of inspectors who randomly inspect construction sites within their jurisdiction. Assuming that a higher number of inspectors implies higher enforcement level or in other words higher likelihood of inspection and discovery (as in BCK), I measure enforcement by the number of minimum wage inspectors and this varies across state and time. This may not the most accurate measurement of enforcement because a quantitative measure as this might not reveal aspects of corruption and collusive agreements that could potentially exist between employers and inspectors (Basu, Chau, and Kanbur, 2010). However, assuming the quality and effectiveness of enforcement is uniform through the country and over time, number of inspectors could give a fair sense of enforcement.

Minimum wage and enforcement data are obtained from the Reports on the Working of the Minimum Wage Law published yearly by the Labor Bureau, Ministry of Labor & Employment, Government of India. These reports provide state-specific information on minimum wages set in different industries and on the enforcement machinery of the minimum wage legislation for all years[7].

This paper exploits variation in minimum wages across state and time to estimate its effects on labor market outcomes. Figure 3 presents spatial variation in minimum wages for construction industry in 2011-12. The lowest minimum wage is in Orissa (Rs. 93/day) and the

[4] Minimum wages are defined only for employments listed under the employment schedule of the Minimum Wages Act under the concerned government. Employments other than those listed are not covered under the law.

[5] The concerned government is either the state government or the central government depending on the industry and sector of work. Government owned enterprises and firms in the mining and railway sector belongs to the central sphere; all other firms fall under the state sphere.

[6] Besler and Rani (2011) report that the central government sets 48 minimum wages for different categories including mining, agriculture and oil extraction, or any corporation under its ownership. State governments altogether set minimum wages for 1,123 job categories making a grand total of about 1,171 different minimum wage rates in India.

[7] The minimum wage data are available in table 3 and the enforcement data are available in annexure II of the report.

highest is in Maharashtra (Rs. 229/day). Additionally, to provide a sense of level and variation in minimum wages and its change over time, Table 1 provides the mean and standard deviation of minimum wages across years. The state-time varying minimum wage data were mapped to the worker level dataset (described in section 2). Workers in the current year were mapped to MW effective as on December 31 of the preceding year[8]. For example, workers surveyed in 2004 (July to December) are mapped to the MW as on December 31, 2003; workers surveyed in 2005 (January to July 2005) are mapped to MW effective as on December 31, 2004. Using MW effective in the year proceeding the year of survey (rather than say after the year of survey), addresses endogeneity concerns because in this case, minimum wages were set before labor market outcomes were realized.

Table 1: Summary statistics of minimum wages across years

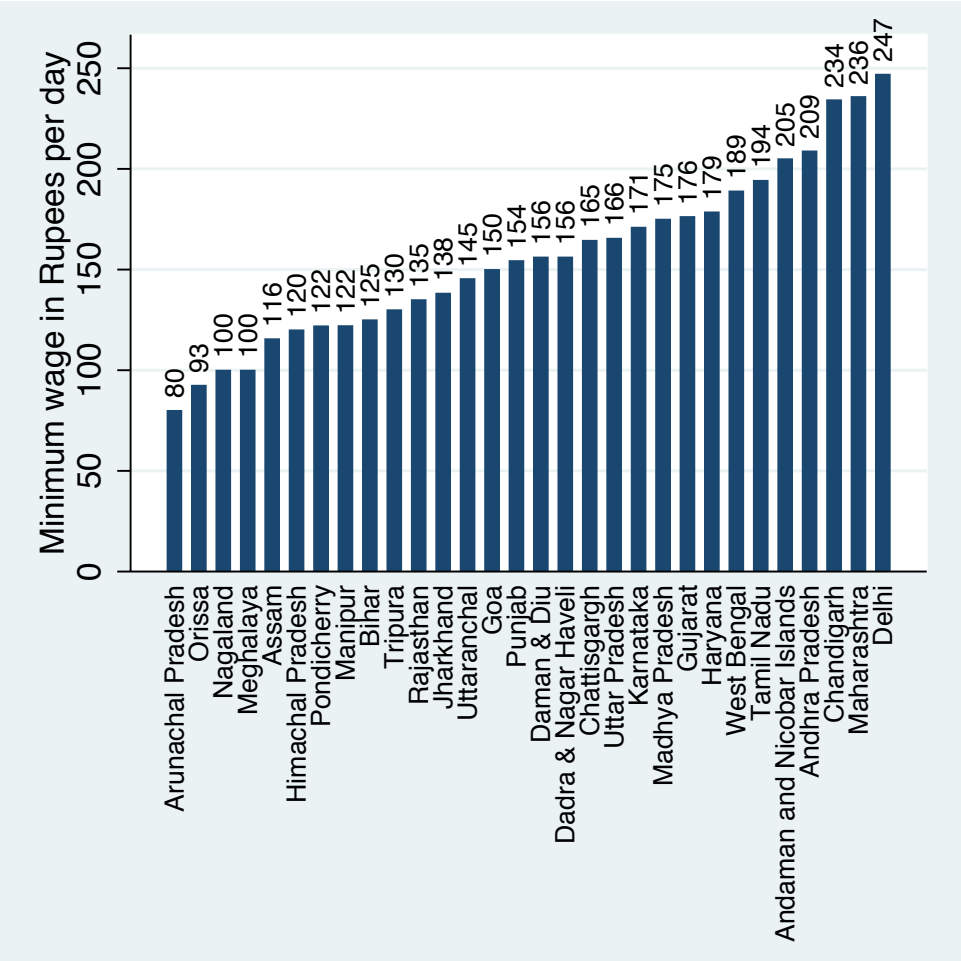
Year	Mean	Std. Deviation
2003	81.07	26.11
2004	85.41	24.02
2005	86.51	23.06
2006	94.86	32.23
2007	100.59	29.75
2008	117.38	39.3
2009	122.02	27.78
2010	149.32	38.4
2011	156.26	43.18

Source: Reports on the Working of the Minimum Wage Law, various years

Table 2 shows the extent of variability across time and states in the enforcement variable. Enforcement data for a survey year (which are parts of full year) is an average of number of inspectors corresponding to the two years constituting the survey. The average (across states) number of inspectors all of India has declined from 187 in 2003-04 to 183 in 2011-12. Further, the number of inspectors at the 25th percentile is at 33 inspectors, at the median is 123, and at 75th percentile is 361, giving a well spread out distribution of enforcement regimes across

[8]In each state, MW for an industry could change multiple times within a year. Tracking the details of each MW change could be challenging because revisions are done decentrally by state governments and such detailed documentation are not available digitally. Sometimes they are available only in a regional language.

Figure 3: State specific minimum wages in 2011



Source: Reports on the Working of the Minimum Wage Law, 2011

Table 2: Summary statistics of minimum wage inspectors across years

Year	Mean	Std. Deviation
2003-04	187.01	221.50
2004-05	189.25	212.60
2005-06	193.06	209.58
2007-08	198.80	207.66
2009-10	174.84	211.38
2011-12	183	214.13

Source: Reports on the Working of the Minimum Wage Law, various years

different states. Number of inspectors at the state level is obviously endogenous to labor market outcomes. An instrumental variable strategy is used to address this and is presented in section 4 below.

4 Econometric approach

As a starting point, I estimate a non-parametric bivariate model to obtain a descriptive picture of the relationship between employment and log minimum wages (Figure 4). The graph presents a non-linear picture with two humps, indicating that a linear Ordinary Least Squares regression model will be far from sufficient. A similar graph for real log daily wages (figure 5) and log of days of work (Figure 6) also indicate non-linear relationships with log minimum wages.

Figure 4: Lowess smoothing estimate of employment on log real minimum wage

Note:Blue dots are scatter plots; red lines are estimated relationships

Figure 5: Lowess smoothing estimate of log real wages on log real minimum wage

Note: Blue dots are scatter plots; red lines are estimated relationships

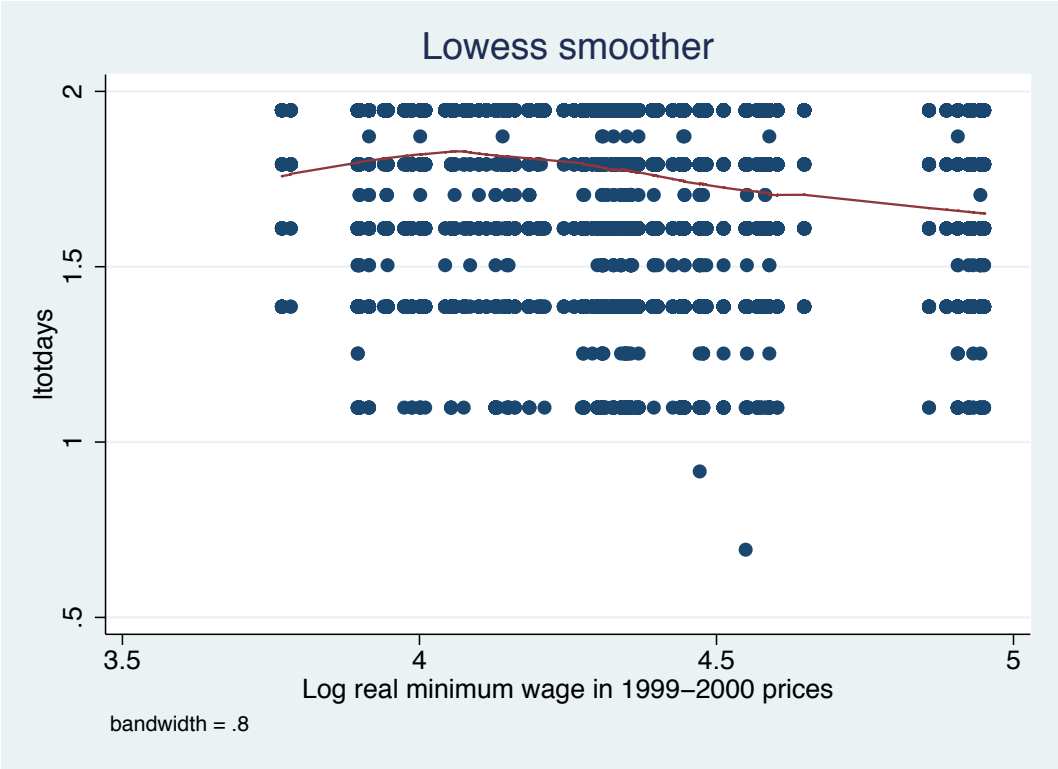
Taking cues from these preliminary diagnostics, I specify a flexible regression model allowing for these non-linearities as follows:

$$Y_{ist} = f(MW_{s(t-1)}, E_{st}, MW_{s(t-1)} * E_{st}) + \alpha * LGDP + \beta * X_{ist} + D_s + D_t$$

Y_{ist} , the outcome variable represents the individual level outcome for worker i working in state s at time t and could be either (1) Employment taking the value 1 if the worker is employed in the construction industry and 0 if in agriculture (neighbor industry); (2) log daily wage of a worker, conditional on working in the construction industry; or (3) log days of employment in the construction industry in the preceding week, conditional on working in the construction industry.

$f(.)$ is a nonlinear function of minimum wage, $MW_{s(t-1)}$ (the real minimum wage in state s in time $t-1$) and enforcement at state s at time t, E_{st}, measured by the number of inspectors, and the interaction of both. In the above specification, minimum wage appears as

Figure 6: Lowess smoothing estimate of log days on log real minimum wage



Note: Blue dots are scatter plots; red lines are estimated relationships

dummies representing various levels of minimum wages. Here, I consider four dummy variables representing four quartiles of the minimum wage distribution. The binary variable quartile 1 takes a value 1 if the minimum wage falls in the first quartile of the distribution and 0 otherwise. The binary variable quartile 2 takes a value 1 if the minimum wage falls in the second quartile of the distribution and 0 otherwise. The binary variable quartile 3 takes a value 1 if the minimum wage falls in the third quartile of the distribution and 0 otherwise. The binary variable quartile 4 takes a value 1 if the minimum wage falls in the last quartile of the distribution and 0 otherwise.

$LGDP_{st}$ is log per-worker real construction GDP in state s at time t and controls for aggregate demand conditions. X_{ist} represents individual demographic characteristics such as age, square of age, gender, social group, and sector. Gender is coded as dummy variable and the base category is females. Sector is either rural or urban and the base category in this case is rural. In India, social groups are classified into four major categories the scheduled caste,

scheduled tribes, other backward classes and other castes[9]. Social groups are coded as dummy variables, and the base category is Scheduled Tribes. The model also controls for year fixed effects (D_t) and state fixed effects (D_s).

The dummy variables (of MW) model for all three outcome variables, was estimated using an Ordinary Least Squared (OLS) and Instrumental Variable Two Stage Least Squares (IV-TS) regressions. Additionally, employment variable was also studied using Probit and Instrumental Variable Probit (IV Probit) regressions. The entire sample including construction workers and agriculture workers was used for the employment regression. Wage and days of work regression was based on a sample of workers, conditional on working in the construction industry. Table 2 provides the list of endogenous regressors and instruments for the dummy variables model with 4 dummies each taking the value 1 when the log real minimum wages falls in the first quartile, second quartile, third quartile, and fourth quartile of the distribution respectively, and 0 otherwise. Note that this is an exactly identified model with four endogenous regressors and four instruments. Table 3 lists down all the endogenous regressors and instruments in the dummy variables model considered here.

As a first step, instruments are tested for relevance. In a model with multiple endogenous regressors, Angrist and Pichske (2008) provide for the Angrist-Pischke multivariate F-test of excluded instruments, which corresponds to a test based on F-statistic from each first stage regression after netting out the effect of the remaining endogenous regressors. As a rule of thumb, an F-value above 10 is considered significant.

Table 3: Approach for the Instrumental variable strategy

Model	Endogenous regressors	Instruments
Dummy variables model A case of 4 dummies representing each quartile.	MW inspectors, MW inspectors * quartile 2, MW inspectors * quartile 3, MW inspectors * quartile 4.	Factories inspectors, Factories inspectors * quartile 2, Factories inspectors * quartile 3, Factories inspectors * quartile 4.

[9]The Scheduled Castes (SC) and Scheduled Tribes (STs) are two groups of historically-disadvantaged people recognized in the Constitution of India. Other Backward Class (OBC) is a collective term used by the Government of India to classify castes which are educationally and socially disadvantaged, but not as acutely as SCs and STs. All other castes are grouped as Forward caste. The lists of Forward, Other Backward and Scheduled castes, and Scheduled tribes are compiled by the government of India irrespective of religion.

5 Results and Interpretation

5.1 Main Results

Table 4 presents the statistics for instrument relevance from the first stage regressions for the dummy variables model based on quartiles of minimum wage distribution. The p-values for the Angrist-Pischke F-test in the employment model and wage/days of work model for each of the five endogenous regressors are reported. All p-values are 0.0, implying each of these regressors are individually identified[10].

Table 4: Tests for instrument relevance in the dummy-variables model

Endogenous regressors	P-value for employment regression	P-value for wage/days regression
(1)	(2)	(3)
MW Inspectors	0.0	0.0
MW Inspectors*quartile 1	0.00	0.00
MW Inspectors*quartile 2	0.00	0.00
MW Inspectors*quartile 3	0.00	0.00
MW Inspectors*quartile 4	0.00	0.00

Table 5 presents the effect of minimum wages on employment at different levels of enforcement using the linear probability model (OLS and IV two-stage method) in panel 1 and probit and IV probit models in panel 2. Ordinary Least Squares (OLS) regression results from columns 1 and 2 (panel 1) indicate a negative relationship between employment and minimum wages at low level of enforcement, say the 25th percentile. Compared to the base category of first quartile (0 to 25th percentile log MW), the likelihood of employment significantly declines by .23 in the second quartile, by .25 in the third quartile and by .18 in the fourth quartile. The Instrumental Variables two-state least squares regression (IV 2SLS) , which is my preferred specification, confirms these results, although the magnitude of the effect is different, especially in higher quartiles. Column 3 and 4 (panel 1) present the effects at the median level of enforcement, which are positive unlike at lower level of enforcement. The OLS results in column 3 shows that compared to the base category of 1st quartile, the likelihood of employment

[10] A linear-quadratic model was also estimated using minimum wage and a minimum wage squared term. This model has three endogenous regressors (MW Inspectors, MW Inspectors* log minimum wage, MW Inspectors * log minimum wage* log minimum wage) and three instruments (Factories inspectors, Factories inspectors * log minimum wage, Factories inspectors * log minimum wage * log minimum wage). The p-value obtained from the AP F-test for each of the three regressors is above .1, implying they are not individually identified by the instruments. Hence, the linear quadratic model was dropped from the main specification.

significantly increases by .28 in the second quartile, by .26 in the third quartile, and by .33 in the fourth quartile. But IV 2SLS results indicate that compared to the base category of 1st quartile, the likelihood of employment significantly increases by .25 in quartile 2, .27 in quartile 3, but drops to -.07 in quartile 4 (although the results are not significant at the fourth quartile). This indicates a hump-shaped relationship between employment and minimum wages. At very high levels of enforcement, say 75th percentile, OLS results (column 5) indicate that compared to quartile 1, the likelihood of employment in quartile 2, quartile 3 and quartile 4 are positive and increasing over the distribution of log minimum wages. But IV 2SLS results (column 6), indicate a clear and significant hump shaped relationship. Compared to the base category of quartile 1, the likelihood of employment in quartile 2 significantly increases by .93 in quartile 2, .92 in quartile 3, and .85 in quartile 4.

These results are robust to alternate specifications. Panel 2 in Table 5 presents the results using probit and IV probit regressions. The IV probit regressions, which are my preferred specifications because the predicted probabilities in this case are between between 0 and 1 (unlike the IV 2SLS model), indicate a uniform negative relationship at 25th percentile enforcement, a hump-shaped relationship at the median level of enforcement and higher levels of enforcement.

Table 6 presents minimum wage effects on log wages, conditional on working in the construction industry. Wage effects at 25th percentile of enforcement from both OLS and IV 2SLS indicates a negative effect in the second and third quartile and a positive effect in the fourth quartile. The IV 2SLS regression results (my preferred specification), indicates that compared to the base category of quartile 1, log wages in quartile 2 significantly decreased by .45 points in quartile 2, .42 points in quartile 3, and increased by .50 points in quartile 4. At the median level of enforcement, positive and significant effects are observed from OLS and IV regression results. The effects from IV regression are higher in magnitude, compared to OLS. Column 4 indicates that compared to the base category of quartile 1, log wages in quartile 2 are higher by .46 points, in quartile 3 by .39 points and in quartile 4 by 1.15 points. At even higher levels of enforcement (75th percentile), the wage effects are positive but are higher in magnitude compared to lower levels of enforcement. IV 2SLS results in column 6 indicates that compared to the base category of quartile 1, log wages in quartile 2 are higher by 1.77 points, in quartile 3 by 1.56 points and in quartile 4 by 2.09 points[11].

[11] OLS and IV 2SLS regressions were also estimated between between days of work and minimum wages using

Table 5: Effects on employment at different level of enforcement

	LINEAR PROBABILITY MODEL					
Enforcement	25th percentile		50th percentile		75th percentile	
	(1)	(2)	(3)	(4)	(5)	(6)
Log minimum wage	OLS	IV 2SLS	OLS	IV 2SLS	OLS	IV 2SLS
Quartile 1	-	-	-	-	-	-
(base category)						
Quartile 2	-0.23***	-0.21***	0.28***	0.25***	1.02***	0.93***
	(0.03)	(0.06)	(0.03)	(0.06)	(0.07)	(0.21)
Quartile 3	-0.27***	-0.18**	0.26***	0.27***	1.02***	0.92***
	(0.03)	(0.07)	(0.03)	(0.06)	(0.07)	(0.22)
Quartile 4	-0.18***	-0.72***	0.33***	-0.07	1.07***	0.85***
	(0.03)	(0.10)	(0.03)	(0.10)	(0.07)	(0.23)
	PROBIT AND IV PROBIT REGRESSIONS					
Enforcement	25th percentile		50th percentile		75th percentile	
Log minimum wage	Probit	IV Probit	Probit	IV probit	Probit	IV Probit
Quartile 1	-	-	-	-	-	-
(base category)						
Quartile 2	-0.09	-0.28*	0.32***	0.35***	0.51***	0.65***
	(0.07)	(0.16)	(0.03)	(0.05)	(0.03)	(0.09)
Quartile 3	-0.22***	-0.29	0.24***	0.32***	0.50***	0.60***
	(0.06)	(0.18)	(0.03)	(0.05)	(0.04)	(0.08)
Quartile 4	0.01	-0.40**	0.42***	0.23**	0.60***	0.61***
	(0.08)	(0.17)	(0.04)	(0.11)	(0.03)	(0.06)

Note: *** - statistical significance at 1%; **- statistical significance at 5%; *- statistical significance at 10%; Robust standard errors in parentheses for all models; bootstrap standard errors are reported for IV probit regressions; controls in all regressions include (1) at the individual level: age, age-squared, social group, and sector (Rural/urban); (2) at the state level: per worker construction sector state net domestic product, time dummies and state dummies. Quartile i is a dummy for belonging to the i_{th} quartile of minimum wages and the base category is quartile 1. Effects in quartile 'i is the simply difference of predicted log wages at quartile i from quartile 1. For the probit and IV probit models, effects were calculated by differencing the probit index function at quartile i from quartile 0.

5.2 Interpretation of results

Results in section 5.1 indicates that the relationship between employment and minimum wage and between wages and minimum wages in the construction sector are distinctly different across enforcement levels, clarifying the importance of enforcement in this relationship. At high levels of enforcement (50th percentile and above),the likelihood of employment in the construction industry rises with an increase in minimum wage (quartiles) but declines at the upper tail.

the same specifications. However, insignificant results were obtained in the IV 2SLS regression throughout the minimum wage distribution.

Table 6: Effects on log wages at different level of enforcement

Enforcement	25th percentile		50th percentile		75th percentile	
	(1)	(2)	(3)	(4)	(5)	(6)
Log minimum wage	OLS	IV	OLS	OLS	IV	OLS
Quartile 1	-	-	-	-	-	-
(base category)						
Quartile 2	-0.02	-0.45***	0.18***	0.46***	0.48***	1.77***
	(0.04)	(0.08)	(0.04)	(0.09)	(0.09)	(0.31)
Quartile 3	-0.04	-0.42***	0.19***	0.39***	0.54***	1.56***
	(0.04)	(0.10)	(0.04)	(0.08)	(0.09)	(0.32)
Quartile 4	0.13***	0.50***	0.34***	1.15***	0.64***	2.09***
	(0.04)	(0.11)	(0.04)	(0.13)	(0.09)	(0.33)

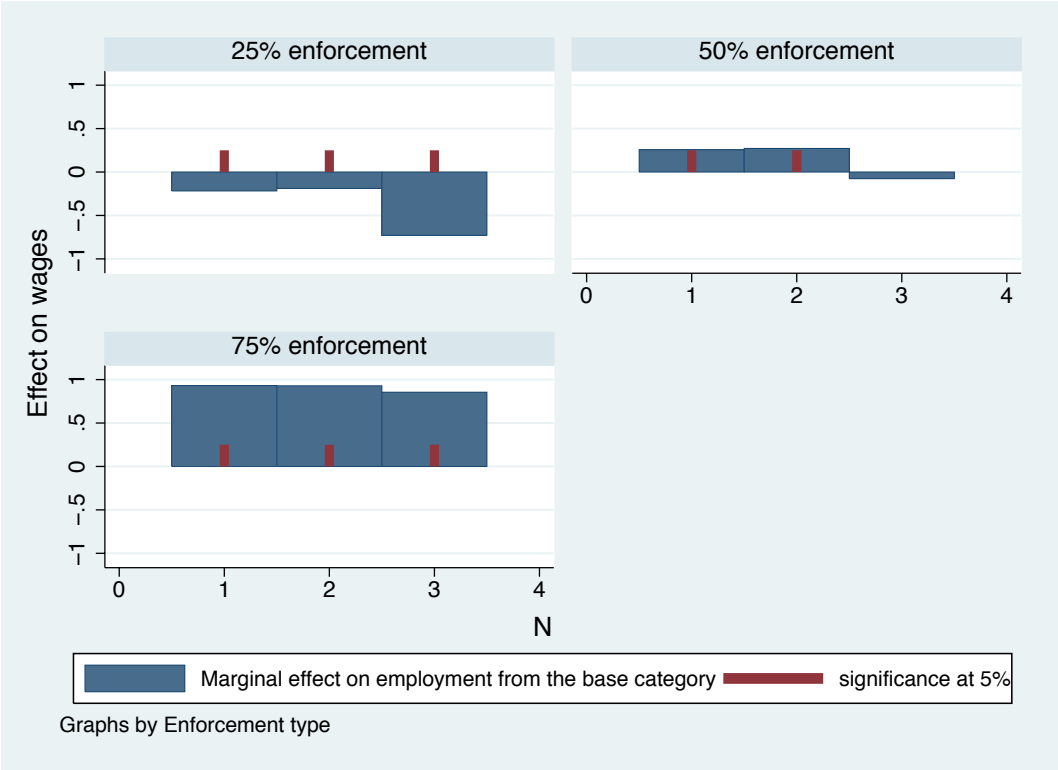
Note: *** - statistical significance at 1%; **- statistical significance at 5%; *- statistical significance at 10%; Robust standard errors in parentheses for all models; bootstrap standard errors are reported for IV probit regressions; controls in all regressions include (1) at the individual level: age, age-squared, social group, and sector (Rural/urban); (2) at the state level: per worker construction sector state net domestic product, time dummies and state dummies. Quartile i is a dummy for belonging to the i_{th} quartile of minimum wages and the base category is quartile 1. Effects in quartile 'i is the simply difference of predicted log wages at quartile i from quartile 1. For the probit and IV probit models, effects were calculated by differencing the probit index function at quartile i from quartile 0.

But at lower levels of enforcement (say 25th percentile), a rise in minimum wage decreases the likelihood of employment across all quartiles with a mild dent. Wage effects are negative at 25th percentile enforcement and at lower quartiles but are positive at 4th quartile. At higher levels of enforcement, wages effects are uniformly positive although with a mild dent in the third quartile. These results are summarized in a bar graph in figures 7 and 8 for employment and wages, respectively.

As mentioned earier, labor market model with imperfect competition and imperfect enforcement as in Basu, Chau and Kanbur (2010) provides a consistent theoretical explanation to these empirical results. BCKs model incorporates imperfect enforcement to Stigler (1946)s labor market model of imperfect competition.

In BCKs model, imperfect enforcement is modelled as the likelihood λ of inspection and discovery. Under perfect enforcement ($\lambda = 1$), comparative static responses in this model is exactly the same as Stiglers model, which is a hump shaped relationship with the turnaround threshold at the competitive wage equilibrium. The hump shape is predicted in Stigler(1946) and BCK because below the threshold, a perfectly enforced binding minimum wage decreases the marginal cost of labor, which causes employment to increase. However, above the threshold, a perfectly enforced binding minimum wage increases the marginal cost of labor and hence

Figure 7: Effect on employment at different levels of enforcement from IV-2SLS regressions

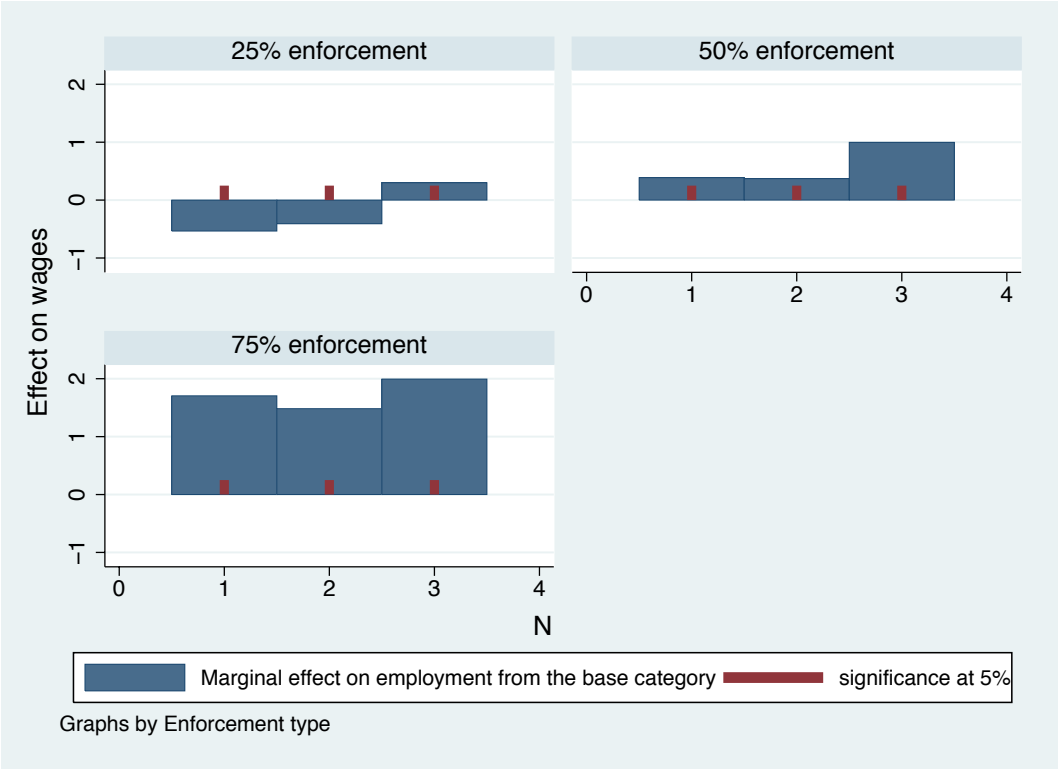


Red spike indicates that the estimate is significant at 5% level

causes employment to decline. With imperfect enforcement, the hump shape between employment and minimum wages are retained but the threshold for sign reversal is lower than the competitive-wage threshold(as in the case of perfect enforcement) and depends uniquely on λ, the enforcement level. With lower enforcement, the threshold at which the expected marginal cost changes from positive to negative with an increase in minimum wage, is lower. The threshold increases with an increase in enforcement. This implies that lower the level of enforcement, the shorter the interval of minimum wage for which employment responses to minimum wages are positive. It also implies that higher the enforcement, longer the interval of minimum wage for which employment responses are positive or more prominent is the hump shape.

This is precisely what we see in the results. The employment response at 25th percentile (low level) enforcement indicates a negative effect, indicating possibly that the upward sloping part of employment response is for a very short interval of minimum wage and that the downward-sloping response is for the longer interval. For higher levels of enforcement (50th

Figure 8: Effect on wages at different levels of enforcement from IV 2SLS regressions



Red spike indicates that the estimate is significant at 5% level

and 75th percentile in the Figure 7), the upward sloping part of employment is over a larger interval of minimum wage and is more pronounced, creating the hump shape.

Wage responses at higher levels of enforcement (50th and 75th percentile) are positive and increasing through the minimum wage distribution (Figure 8). At 25th percentile enforcement, wage effects are negative but very low in magnitude in quartile 2 and 3, but positive and low in magnitude in quartile 4. Firms tend to shirk complying with the law when there is low enforcement, and even reduce wages slightly by a marginal amount. But at higher levels of enforcement wage responses to minimum wage are comparatively more compliant and the magnitude of wage response are higher in the higher tail of the minimum wage distribution.

6 Robustness and results for specific demographic groups

6.1 Robustness

It is important to check if the results are robust to an alternate definition of the employment variable. In the main results in section 5.1, the employment variable was defined to take

the value 1 if the worker worked in the construction industry and 0 if the worker worked in agriculture. In the alternate definition, the 0 category now includes workers in agriculture, retail trade and land transport industries (next closest neighbors to construction after agriculture as in Figure 1 and 2). Table 7 shows the result, at 25th,50th and 75th percentile enforcement levels. At 50th percentile enforcement level both IV 2SLS (column 3) and IV probit (column 4) models indicate that the hump shape is retained and is significant. At 25th percentile enforcement, there is a uniform negative effect similar to the main results in table 5 (columns 1 and 2), although with a slight dent, which is again similar to the main results. At 75th percentile enforcement, there is a hump shaped relationship from the IV 2SLS model as seen in column 5, which mimics the IV-2SLS results in table 6. IV Probit results in column 6 indicate an increasing and tapering effect again similar to that obtained in table 6 (panel 2 and column 6). This implies that the results are robust to alternate definitions of the employment variable.

Table 7: Effects on employment - altering the neighbor industry to include retail trade and land transport

Enforcement	25th percentile		50th percentile		75th percentile	
	(1)	(2)	(3)	(4)	(5)	(6)
Log minimum wage	IV 2SLS	IVProbit	IV 2SLS	IVProbit	IV 2SLS	IVProbit
Quartile 1	-	-	-	-	-	-
(base category)						
Quartile 2	-0.22***	-0.24**	0.40***	0.26***	1.29***	0.43***
	(0.08)	(0.10)	(0.08)	(0.03)	(0.28)	(0.06)
Quartile 3	-0.15	-0.16	0.43***	0.30***	1.28***	0.40***
	(0.09)	(0.12)	(0.07)	(0.03)	(0.29)	(0.06)
Quartile 4	-0.34***	-0.30**	0.32**	0.21***	1.27***	0.42***
	(0.11)	(0.13)	(0.12)	(0.08)	(0.30)	(0.05)

Note: *** - statistical significance at 1%; **- statistical significance at 5%; *- statistical significance at 10%; Robust standard errors in parentheses for all models; bootstrap standard errors are reported for IV probit regressions; controls in all regressions include (1) at the individual level: age, age-squared, social group, and sector (Rural/urban); (2) at the state level: per worker construction sector state net domestic product, time dummies and state dummies. Quartile i is a dummy for belonging to the i_{th} quartile of minimum wages and the base category is quartile 1. Effects in quartile i is the simply difference of predicted wages at quartile i from quartile 1. For the probit and IV probit models, effects were calculated by differencing the probit index function at quartile i from quartile 0.

6.2 Results for specific demographic groups

Table A1 and table A2 in the Appendix presents employment and wage effects respectively for a sample of workers who belong to the social group called scheduled tribes and scheduled castes.

The Scheduled Castes and Scheduled Tribes (STs) are two groups of historically-disadvantaged people recognized in the Constitution of India. Due to the relative disadvantages they face, employers could potentially exert market power on workers belonging to these groups. Results based on this sample mimic the main results for the entire sample in table 5 and table 6. Another group that can potentially face employers power are workers who reside in rural areas and commute or migrate for a short term to work in urban areas. Assuming, that a large majority of construction activity takes place in urban areas, traveling to work and incomplete information will be a defining factor for workers residing in rural areas. Employment and wage effects estimated using from a sample of rural workers are presented in table A3 and table A4 in the Appendix respectively. Here again, results are similar to those in table 5 and table 6.

7 Conclusion

There is growing empirical evidence of imperfect enforcement and high non-compliance of the minimum wage law in both developed and developing country settings. Despite this evidence, studies that estimate the effects of the minimum wage legislation accounting for imperfect enforcement, are missing. The present study addresses this gap by estimating the interactive effects of minimum wage and enforcement among construction industry workers in the Indian context. Regional and time varying minimum wage and enforcement in India provides a unique platform to study these effects.

Enforcement in this paper, is measured by the number of inspectors under the minimum wage law and is endogenous because of unobserved heterogeneity affecting enforcement and labor market outcomes at the state level. Further, reverse causality could exist that is, employment and wage levels can also drive the levels of enforcement. A unique instrument number of inspectors under the Factories Act, another law implemented and enforced by the states is employed to address this endogeneity.

The results from this paper strongly indicate that response of employment and wages to minimum wages vary starkly with the levels of enforcement. At low levels of enforcement, employment responses are uniformly negative, and at higher levels, there emerges a hump shaped relationship. These findings underscore the role of enforcement in studying the minimum law and the importance of enforcement as an institution in itself. Further, these results are consistent with models of imperfect competition and imperfect enforcement (Basu, Chau

and Kanbur, 2010).

These results raise a number of research and policy questions for further research. While the present paper studies minimum wage effects at different enforcement levels on average levels of employment, wages and days of work for all workers, it brings up an interesting question of whether and how minimum wage effects vary across different enforcements levels for sub-minimum wage workers. This is an important policy question because it strikes the heart of the matter by asking if the minimum wage policy benefits those workers whom it was intended to benefit. Another key issue in the realm of enforcement is whether enforcement by itself and/or in interaction with minimum wage affects the level of non-compliance at all in the Indian context. A few papers have addressed this question in other developing countries (Bhorat et al. (2012) and Ronconi (2010)). Additionally, enforcement could potentially affect the depth of non-compliance and the square of depth of non-compliance; these classes of measures would be similar to the Foster-Greer-Thorbecke generalized measures of poverty. A key issue in these type of research questions, as in the present paper, is to address endogeneity in the allocation of enforcement by the government.

With an understanding of how enforcement affects average and sub minimum wage labor market outcomes as well as generalized measures of non-compliance, it may be worthwhile to theoretically explore the optimal level of enforcement, and empirically test if the levels of enforcement are optimal in the Indian context (or other developing countries depending on the types of availability of data).

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Appendix

Table A1: Effects on employment for scheduled tribes and scheduled caste

Enforcement	25th percentile		50th percentile		75th percentile	
	(1)	(2)	(3)	(4)	(5)	(6)
Log minimum wage	IV 2SLS	IVProbit	IV 2SLS	IVProbit	IV 2SLS	IVProbit
Quartile 1	-	-	-	-	-	-
(base category)						
Quartile 2	-0.06	-0.003	0.30***	0.45***	0.81***	0.60***
	(0.07)	(0.21)	(0.07)	(0.06)	(0.21)	(0.11)
Quartile 3	-0.10	-0.06	0.34***	0.48***	0.98***	0.74***
	(0.08)	(0.22)	(0.07)	(0.07)	(0.24)	(0.12)
Quartile 4	-0.9***	-0.47***	-0.18	-0.02	0.84***	0.57***
	(0.17)	(0.16)	(0.13)	(0.11)	(0.24)	(0.07)

Note: *** - statistical significance at 1%; **- statistical significance at 5%; *- statistical significance at 10%; Robust standard errors in parentheses for all models; bootstrap standard errors are reported for IV probit regressions; controls in all regressions include (1) at the individual level: age, age-squared, social group, and sector (Rural/urban); (2) at the state level: per worker construction sector state net domestic product, time dummies and state dummies. Quartile i is a dummy for belonging to the i_{th} quartile of minimum wages and the base category is quartile 1. Effects in quartile i is the simply difference of predicted wages at quartile i from quartile 1. For the probit and IV probit models, effects were calculated by differencing the probit index function at quartile i from quartile 0.

Table A2: Effects on wages for scheduled tribes and scheduled caste

Enforcement	25th percentile (1)	50th percentile (2)	75th percentile (3)
Log minimum wage	IV 2SLS	IV 2SLS	IV 2SLS
Quartile 1 (base category)	-	-	-
Quartile 2	-0.31***	-0.18	0.89***
	(0.11)	(0.12)	(0.38)
Quartile 3	-0.09	-0.15	0.50
	(0.14)	(0.10)	(0.39)
Quartile 4	0.63***	0.79***	1.02**
	(0.23)	(0.24)	(0.43)

Note: *** - statistical significance at 1%; **- statistical significance at 5%; *- statistical significance at 10%; Robust standard errors in parentheses for all models; bootstrap standard errors are reported for IV probit regressions; controls in all regressions include (1) at the individual level: age, age-squared, social group, and sector (Rural/urban); (2) at the state level: per worker construction sector state net domestic product, time dummies and state dummies. Quartile i is a dummy for belonging to the i_{th} quartile of minimum wages and the base category is quartile 1. Effects in quartile i is the simply difference of predicted wages at quartile i from quartile 1. For the probit and IV probit models, effects were calculated by differencing the probit index function at quartile i from quartile 0.

Table A3: Effects on employment for rural residents

Enforcement	25th percentile		50th percentile		75th percentile	
	(1)	(2)	(3)	(4)	(5)	(6)
Log minimum wage	IV 2SLS	IVProbit	IV 2SLS	IVProbit	IV 2SLS	IVProbit
Quartile 1	-	-	-	-	-	-
(base category)						
Quartile 2	-0.15*	-0.23	0.27***	0.34***	0.90***	0.49***
	(0.08)	(0.2)	(0.07)	(0.05)	(0.25)	(0.10)
Quartile 3	-0.13	-0.25	0.28***	0.31***	0.87***	0.42***
	(0.09)	(0.22)	(0.07)	(0.05)	(0.26)	(0.08)
Quartile 4	-0.58***	-0.41*	0.0005	0.21*	0.84***	0.46***
	(0.12)	(0.22)	(0.11)	(0.11)	(0.27)	(0.07)

Note: *** - statistical significance at 1%; **- statistical significance at 5%; *- statistical significance at 10%; Robust standard errors in parentheses for all models; bootstrap standard errors are reported for IV probit regressions; controls in all regressions include (1) at the individual level: age, age-squared, social group, and sector (Rural/urban); (2) at the state level: per worker construction sector state net domestic product, time dummies and state dummies. Quartile i is a dummy for belonging to the i_{th} quartile of minimum wages and the base category is quartile 1. Effects in quartile i is the simply difference of predicted wages at quartile i from quartile 1. For the probit and IV probit models, effects were calculated by differencing the probit index function at quartile i from quartile 0.

Table A4: Effects on wages for rural residents

Enforcement	25th percentile (1)	50th percentile (2)	75th percentile (3)
Log minimum wage	IV 2SLS	IV 2SLS	IV 2SLS
Quartile 1 (base category)	-	-	-
Quartile 2	-0.53***	0.38***	1.7***
	(0.10)	(0.12)	(0.4)
Quartile 3	-0.4***	0.37***	1.48***
	(0.14)	(0.10)	(0.41)
Quartile 4	0.29**	0.99***	1.99***
	(0.13)	(0.15)	(0.42)

Note: *** - statistical significance at 1%; **- statistical significance at 5%; *- statistical significance at 10%; Robust standard errors in parentheses for all models; bootstrap standard errors are reported for IV probit regressions; controls in all regressions include (1) at the individual level: age, age-squared, social group, and sector (Rural/urban); (2) at the state level: per worker construction sector state net domestic product, time dummies and state dummies. Quartile i is a dummy for belonging to the i_{th} quartile of minimum wages and the base category is quartile 1. Effects in quartile i is the simply difference of predicted wages at quartile i from quartile 1. For the probit and IV probit models, effects were calculated by differencing the probit index function at quartile i from quartile 0.