Elasticity of Labor Supply in Rural Malawi: Evidence from a Field Experiment

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Abstract

I use panel data from a unique field experiment to estimate the elasticity of working for a sample of adults who participate in the day labor market in rural Malawi. Once a week for 12 consecutive weeks, I make job offers to a pre-defined sample of 530 adults. This approach provides exogenous variation in wages, allows me to observe the full distribution of wage offers rather than the censored distribution of accepted wages, and permits the inclusion of time and village or time and individual fixed effects. I estimate that the elasticity of labor force participation is between 0.15 and 0.17, with no significant differences between men and women. I demonstrate that collapsing my data into a censored cross section that mimics the data used in the previous literature yields estimates of the intensive margin elasticity that are substantially higher than previous estimates developing countries. Further, I find that shocks to individuals' endowments lead to increased probability of working and less elastic supply, results consistent with the hypothesis that wage labor is an ex post coping strategy in developing countries.

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1 Introduction

It is widely appreciated that labor is the most abundant resource of the poor. In agricultural economies the poor may work on their own land and produce goods for home consumption or market sale, and work for other people for wages. This paid employment often takes the form of casual day labor rather than longer-term arrangements governed by contracts, and can be an important source of cash as well as a mechanism for coping with negative shocks that reduce non-labor income. The importance of this sort of labor is highlighted by public sector employment programs with dual goals of infrastructure development and income support. Malawi has invested $40 million in it's Community Livelihoods Support fund, and in India in 2008-2009, almost 45 million households were employed to do day labor through the National Rural Employment Guarantee Act. Despite the importance of day labor to rural households and the large scale investments in programs to employ day laborers by governments in developing countries, little is known about the elasticity of employment for day laborers.

In fact, there is fairly scant evidence on the elasticity of labor supply in any sort of labor market in developing countries. The early literature about economic growth in developing countries followed Lewis (1954) in assuming that the supply of labor was perfectly elastic. More recently, empirical estimates of labor supply elasticities in developing countries have generally supported an upward-sloping labor supply curve. Bardhan [3] estimated upward sloping labor supply curves with what he characterizes as "very small" elasticities for rural households in West Bengal; Abdulai and Delgado [1] estimate somewhat greater elasticities for husbands and wives in Ghana; and Rosenzweig [16] estimates that the labor supply curve for women in India slopes up, while the labor supply curve for men is backward bending. Kochar [11] and Rose [15] study the response of labor supply to weather shocks in India, supporting the hypothesis that poor households increase the level of their wage labor to cope with negative shocks to non-labor income. These papers, like the research about labor supply in developed countries, rely on econometric identification strategies (or a structural model for Rosenzweig) to identify plausibly exogenous variation in wages.

I overcome this identification problem by randomizing wages for 530 individuals who are offered employment on community agricultural development projects in rural Malawi. I estimate on the probability of accepting employment in the day labor market, the relevant market for millions of individuals in poor, rural communities. I work in ten villages in central Malawi. Village leaders and government agricultural extension workers identify households where at least one adult has done casual day labor, known as "ganyu," within the last year. In these households, the head of the household and his wife are included in the sample and offered employment one day per week for 12 consecutive weeks. Wages vary by village-week, ranging from MK 30 ($US 0.21) to MK 140 ($US 1.00), and wages for each work-day are announced one week in advance. I estimate the elasticity of working on a given day using administrative attendance records, and employ surveys conducted at baseline and after weeks four, eight, and 12 to study changes in labor supply in response to household shocks. I find that a ten percent increase in wages leads to a 1.5 to 1.7 percent increase in the probability of working, with no differences between men and women. Individuals exhibit higher levels but lower elasticities of working in weeks following a negative shock to non-labor income. My results are consistent with previous evidence that wage labor is a form of ex-post coping for poor individuals, but stand in contrast to the common finding in developing and developed countries that women's labor supply is more elastic than men's.

To my knowledge, the only previous study to randomize wages is Fehr and Goette [9], which randomly assigns bicycle messengers to receive a 25% increase in commissions for deliveries for four weeks. My experiment, which includes a larger sample and a much wider range of wages, not only provides a unique source of exogenous variation in wages for the most common labor arrangement in Malawi and other developing countries with large rural populations, but also connects the development literature to the more recent literature about day labor markets in developed countries. Oettinger [13] studies the attendance decisions of registered stadium vendors, and finds that the elasticity of working on a given day with respect to that day's expected wage is between 0.55 and 0.65. Barmby and Dolton [4] estimate that the elasticity of working on a given day of a 1938 archeological dig in Syria was 0.035.

Related papers by Camerer et al. [5], Chou [7], and Farber [8] study the relationship between hours worked and implied hourly wage for taxi cab drivers. Camerer et al. and Chou find a puzzling result: taxi drivers seem to work fewer hours on more profitable days, implying a downward-sloping supply curve. They explain this result through so-called "target earning" behavior: taxi drivers set a goal for daily earnings, and stop work when they reach their goal. Using a richer data set and a different approach to imputing hourly wages, Farber finds that taxi drivers work longer hours when hourly wages are higher, the standard upward-sloping labor supply curve. Ashenfelter et al. [2] return to the taxi cab driver puzzle and study changes in hours worked in response to exogenous changes in fares. They estimate the elasticity of the supply of labor in response to a long run change in wages to be -0.20. Though these papers are based on data from the United States and Singapore, they focus on a situation where labor supply is extremely flexible on a short-term basis, which is a key characteristic of ganyu in Malawi as well as wage labor markets in many other developing countries.

The paper proceeds as follows. In section 2, I discuss the theoretical framework. I describe the experiment in section 3, and the data in section 4. In section 5, I present results from individual level regressions; results for village-level regressions are available in the appendix. Section 6 concludes.

2 Theoretical framework

Three key dimensions of labor supply elasticities discussed in the literature are the margin of choice of labor supply, the anticipation of the wage change, and the persistence of the wage change. Heckman provides a useful taxonomy of the different labor supply margins in his 1993 review of the literature; the most important consideration is whether variation in labor supply is at the intensive or extensive margin. Each of the labor supply functions that Heckman describes can be estimated for different types of variation in wages: anticipated or unanticipated changes, and permanent or temporary changes. The standard intertemporal elasticity of substitution applies to trade-offs between labor and leisure in response to an anticipated, temporary change in wages. In section 3, I will argue that the wage changes induced by my experiment are anticipated, temporary changes, and that my estimates should be interpreted as intertemporal elasticities of working for individuals in a daily labor market.

2.1 Intensive versus extensive margin of labor supply

Heckman [10] describes four different labor supply functions, where H represents labor supply, W represents wages, Y represents non-labor income, and ν represents other variables that affect labor supply.

$$E(H|W, Y, \nu) \tag{1}$$

$$E(H|W, Y, H > 0) \tag{2}$$

$$E(H|W, Y) = E(H|W, Y, H > 0) \times Pr(H > 0|W, Y) \tag{3}$$

$$Pr(H > 0|W, Y) \tag{4}$$

When H is properly defined to represent a margin at which individuals can choose to adjust their labor supply, the elasticity of labor supply at the intensive margin comes from the derivative of equation (1) with respect to W: $\epsilon_{intensive} = \frac{\partial E(H|W,Y,\nu)}{\partial W} \frac{W}{H}$. In situations where individuals cannot adjust their supply of labor at the intensive margin and instead have to choose between working a fixed number of hours (or days, or weeks) and not working, or when only the binary participation decision is observed, we may estimate the *extensive* margin elasticity or the elasticity of participation from the derivative of equation (4) with respect to W: $\epsilon_{extensive} = \frac{\partial Pr(H>0|W,Y)}{\partial W} \frac{W}{H}$. Theoretically, the marginal effect of wages on labor supply at the intensive margin may be larger or smaller than the marginal effect of wages on labor supply at the extensive margin. Empirically, "Participation (or employment) decisions generally manifest greater responsiveness to wage and income variation than do hours-of-work equations for workers," (Heckman [10]) based on empirical estimates for developed countries.

While the elasticity of labor supply at the intensive margin has received more attention in the empirical literature in developed countries, there are many instances where the extensive margin elasticity is the policy relevant parameter. For example, the change in aggregate supply of labor by single women due to the expansion of the EITC in the 1990s was dominated by an increase in labor force participation (Meyer [12]). Understanding the impact of the EITC expansion, then, requires an estimate of the increase in labor force participation due to the policy change. In developing countries with large-scale public works programs, including Malawi's $40 million Community Livelihoods Support fund and India's National Rural Employment Guarantee Act, which makes over a billion people eligible for up to 100 days of work per year, the change in the fraction of the population who would work under the program at different wages is of crucial importance.

The market for day labor, where individuals can work or not work for the prevailing wage each day, blurs the distinction between the intensive and extensive margin at the same time it makes clear the separation of participation versus employment. In a daily labor market the decision of $H = 0$ or $H > 0$ is made each day, and reflects movement between employment and unemployment but not between labor force participation and non-participation. Some people choose not to work on a given day because the prevailing wage is less than their opportunity cost, but would have worked had the day's wage been higher. Thus, they are *in the market* for day labor even though they are not *employed* on a given day. Empirical estimates of the probability of working in a day labor market should condition on a different participation indicator than $H > 0$, and estimate a labor supply function that combines elements of equations (2) and (4) above:

$$Pr(H > 0|W, Y, \text{in daily labor market}) \tag{5}$$

This labor supply function combines elements of the intensive margin elasticity of hours worked for participants in Heckman's equation (2) by conditioning on participation, and of the extensive margin probability of participating in Heckman's equation (4) since the outcome of interest is the probability of positive hours of work. The corresponding elasticity, which I will call "the elasticity of working" is $\frac{\partial Pr(H>0|W,Y,\text{in daily labor market})}{\partial W} \times \frac{W}{H}$. Oettinger [13] calls this parameter the elasticity of participation in a daily labor market in his study of the labor supply of stadium vendors. He finds that the elasticity of working on a given day for registered stadium vendors is between 0.55 and 0.65. Barmby and Dolton (2010) also estimate the wage elasticity implied by equation (5) for workers on an archeological dig in Syria in the 1930s, and find an elasticity of 0.035.

Both Oettinger and Barmby and Dolton interpret their estimates as intertemporal elasticities of substitution, where workers experience anticipated, transitory shocks to wages and substitute between labor and leisure accordingly. Oettinger assumes that stadium vendors form expectations about future wages based on the popularity of the visiting team. Barmby and Dolton assume that serial correlation in the probability of unearthing valuable objects for which bonus payments are made allows archeological workers to form expectations based past work.

Like Oettinger and Barmby and Dolton, I estimate changes in the probability of working on a given day among a sample of individuals who are known to be participants in the relevant labor market. My sample is restricted to households that have performed ganyu in the recent past, which satisfies the conditioning on labor market participation in equation (5). The margin of choice is at the level of a day, and because each participant is offered one day's employment at each wage, the only possible values of H are 0 or 1.

2.2 Missing data

In cross sectional data, observations are censored on the employment outcome. The distribution of wages in the data represent the distribution of wages for those who accepted employment, but do not represent the full distribution of wage offers. Lower wages are disproportionately likely to have been refused. Wage elasticities are typically computed from regressions of the form $labor = \alpha + \beta w + \nu$. The elasticity is defined as $\epsilon \equiv \beta \times \frac{\overline{w}}{\overline{h}}$. The sign of the bias in the estimate of this elasticity is ambiguous, but we can make some progress in understanding how it enters.

Let $\alpha \equiv \frac{\overline{w}}{\overline{h}}$. Then, $\epsilon \equiv \beta\alpha$. The observed regression coefficient β_{obs} can biased up or down. The ratio of observed mean wages to observed labor α_{obs}, though, is always lower than the true ratio in the underlying distribution.

In the full distribution of wages for n individuals, assume that $c < n$ individuals get low wage offers and do not work at all. These are the individuals whose wages are unobserved in standard cross sectional data about employment and wages. If individuals were arrayed in ascending order by their wage offers, then we could compute the average wage in the observed portion of the distribution,

$$\overline{w_{obs}} \equiv \frac{1}{n-(c+1)} \sum_{i=c+1}^{n} w_i \tag{6}$$

Similarly, average employment in the observed portion of the distribution is

$$\overline{h_{obs}} \equiv \frac{1}{n-(c+1)} \times \sum_{i=c+1}^{n} h_i \tag{7}$$

The average wage in full distribution is the weighted average of the average wage for the censored portion of the distribution and the average wage for the observed portion of the distribution,

$$\overline{w} \equiv \frac{1}{n}\sum_{i=1}^{n} w_i = \frac{c}{n} \times \frac{1}{c}\sum_{j=1}^{c} wage_j + \frac{n-c}{n}\frac{1}{n-(c+1)} \times \sum_{i=c+1}^{n} h_i \tag{8}$$

Equation (8) can be re-written as

$$\overline{w} = \frac{1}{n}\sum_{j=1}^{c} w_j + \frac{n-c}{n} \times \overline{w_{obs}} \tag{9}$$

Recall that employment in the censored portion of the wage distribution is zero by definition. Therefore, average employment in the full distribution

$$\overline{h} \equiv \frac{1}{n}\sum_{i=1}^{n} h_i = \frac{c}{n} \times 0 + \frac{n-c}{n} \times \overline{h_{obs}} = \frac{n-c}{n}\overline{h_{obs}} \tag{10}$$

Recall that we defined $\alpha \equiv \frac{w}{h}$. In the censored portion of the distribution, we have $\alpha_{obs} \equiv \frac{\overline{w_{obs}}}{\overline{h_{obs}}}$. We can rewrite the expression for the full distribution in terms of α_{obs} in order to sign the bias:

$$\alpha = \frac{\frac{1}{n}\sum_{j=1}^{c} w_j + \frac{n-c}{n}\overline{w_obs}}{\frac{n-c}{n}\overline{h_{obs}}} \tag{11}$$

We can simplify the right hand side of equation (11) to $\frac{\frac{1}{n}\sum_{j=1}^{c} w_j}{\frac{n-c}{n}\overline{l_{obs}}} + \alpha_{obs}$, which reduces to $\alpha = \frac{\sum_{j=1}^{c} w_j}{(n-c)\times\overline{h_{obs}}} + \alpha_{obs}$. The first term is positive and the second term represents the ratio of average wages to average employment in the observed fraction of the distribution. Therefore, $\alpha_{obs} < \alpha$, which induces downward bias in the observed elasticity relative to the elasticity of the full distribution when low wage offers are refused and the distribution of wages is censored on wage offers being accepted.

Recall that β_{obs} from the censored data may be smaller or larger than β^* from the full distribution. Moving from the regression coefficient β_{obs} to the estimated elasticity ϵ_{obs} introduces additional bias through α_{obs}. The bias affects estimates of the intensive or extensive margin elasticities.

Econometric strategies such as instrumental variables (see Bardhan [3], Kochar [11], and Rose [15]) are used to address endogeneity in wages. However, it is more difficult to reconstruct the censored wage offers. It would be possible to use a the bivariate normal model to estimate selection and wage equations separately in order to address selection into employment, but that approach is not common in the literature about labor supply in developing countries. My data, which come from a randomized experiment with exogenous variation in wages and no censoring, thus differ from data used in previous estimates in three important ways. I have exogenous variation in wages, I observe the full distribution of wage offers, and I have panel data with within-person variation in wage offers and labor supply.

3 Experimental Design

Malawi is a small, extremely poor country in southeastern Africa. Fifty-two percent of Malawians consume less than a minimum subsistence level of food and non-food items, according to the 2006 World Bank Poverty and Vulnerability Assessment, and 28 percent fall below the PPP-adjusted $1/day threshold. While on-farm production is the dominant source of income and use of time for the rural poor, ganyu, or casual wage labor, can play an important role in bringing in cash and coping with shocks. In the 2004 Integrated Household Survey (IHS) 28 percent of those living in rural areas report doing some ganyu within the last year and 21 percent reported doing some ganyu in the previous seven days. Wages vary seasonally and geographically, ranging from around MK 40 ($US 0.29) to MK 200 ($US 1.43) per day. My study takes place in Lobi, a rural area in in the Central Region, along Malawi's western border with Mozambique. Lobi was chosen as the study area because local officials report a high incidence of ganyu for both private and public employers, including the national Public Works Programme. Working in an area where some people already perform ganyu helps in defining a sample of individuals already participating in the relevant market and makes it more likely that people will treat the work offered through the project as a routine business decision rather than a special opportunity subject to non-economic considerations.

I partnered with a local community-based organization called the Lobi Horticultural Association (LHA) to identify a sample and appropriate work activities and, in a cross-cutting randomization, provide access to savings accounts with LHA's savings and credit cooperative (SACCO) for half of the participating households. In cooperation with local leaders and government extension workers in Dedza, Malawi, I identified 10 villages that were within 20 KM of LHA's headquarters, situated at the Lobi Extension Planning Area offices. The villages were chosen to be near enough to LHA's office to make it feasible for people who received savings accounts to access those accounts easily. To minimize the chance that participants in one village would learn about wages in other villages, only one village per "group village headman" was included in the project.

Within each village, LHA leaders and extension workers chose a work activity. These activities were by design labor intensive, unskilled, and had public rather than private benefits. To be consistent with local standards, "one ganyu" or day's work lasted for four hours.

Activities included clearing and preparing communal land for planting, digging shallow wells to be used for irrigation, and building compost heaps to be used to fertilize communal land. Within each village, the activity was the same for all 12 weeks. The amount of effort was held constant by objective standards from week to week: participants had to dig the same number of cubic feet or hoe the same number of linear feet each week. Since all analysis uses village fixed effects, differences between activities across villages do not affect the results.

Up to 30 households in each village were invited to participate in the project. Qualifying households had to have at least one adult member who had performed ganyu within the last year. The head of household and his spouse were invited to participate. While having multiple participants per household complicates the analysis of an individual's response to a change in his own wages because household income is not held constant, it allows me to identify the parameter that is relevant in this context. Much of the literature in labor economics considers changes in wages for a single member of a household, holding constant income for other household members. This is the relevant parameter in developed countries or urban areas, where members of the household participate in different job markets. However, it is not relevant in rural areas in developing countries, where all adults in the household have the same work opportunities. In Malawi, men and women perform similar on- and off-farm labor. Men and women may participate in the government's Public Works Programme, which pays individuals in poor households to work on community infrastructure projects such as road construction. Allowing both spouses to participate in this project is akin to studying the effect of a transitory change in the prevailing village wage for unskilled labor.

Participating households were given the opportunity to work for pay on their village's activity one day per week for 12 consecutive weeks. The work-day was the same each week for each village, so that village fixed effects also control for day-of-week effects. Participants were told at the outset that the project would last 12 weeks, that the work would be the same each week, that the wage would be different each week, and that they could work as many or as few days as they chose without penalty. Work was supervised by government agricultural extension agents. Wages were announced one week in advance, and in each village, a foreman was responsible for communicating the wage to all participants in the village. Participants were paid immediately, in cash, after they worked. Payments were made by a three person team that included one Chichewa-speaking research assistant who handled money and recorded attendance, one government extension worker who supervised the community project, and one local foreman who helped identify participants to ensure that only pre-selected participants were included. Work activities were carefully monitored to ensure that within village, the intensity and duration of work was the same from week to week.

The once-a-week design of the project was intended to minimize general equilibrium effects and to ensure that regular village activities were not unduly disrupted. Also, spreading the project over 12 weeks, rather than 12 consecutive days, allowed additional time for participants to experience positive and negative shocks, and thus for me to observe supply of labor in response to these shocks. A disadvantage of the design is that the six-day gap between each employment offer gives individuals substantial opportunity to rearrange their other obligations in order to be able to work on this project without reducing their time in other productive activities.

Intertemporal elasticities of substitution are typically interpreted as substitution between labor and leisure. Because my experiment offers employment for one out of seven days, individuals could instead substitute work on my project for other wage employment. I argue that respondents' behavior is more consistent with substitution between labor and leisure than labor for different employers. First, the effect of wages in my project on the probability of outside employment is very small, though it is statistically significant in some specifications (see Appendix Table 18). Second, using an alternate definition of labor supply that counts individuals as working if they work either for my project or for another employer during the week does not change the the estimated elasticity of working (see Appendix Table 19). If individuals were substituting away from other wage work into employment on my project, we would expect that the effect of project wages would be lower on the more comprehensive definition of employment. This explanation is consistent with the notion that demand for labor is scarce during the dry, unproductive season. Finally, despite similar gaps between employment opportunities for stadium vendors, Oettinger interprets his estimates as intertemporal elasticities of substitution of labor for leisure.

The project took place in June, July, and August, months that fall between the harvest and planting seasons in Malawi. This is a time of year with low marginal productivity either on- or off-farm. It is the time of year when individuals have the most food and most cash.

Importantly, I can be confident that the opportunity cost of time was constant throughout the experimental period. Labor supply elasticities may vary seasonally, and the estimates from this experiment are not necessarily valid for a different time of year, when the opportunity cost of time is higher.

Wages for this project range from MK 30/day ($US 0.21) to MK 140/day ($US 1.00), in increments of MK 10.[1] Table 1 shows the schedule of wages, which alternated high and low wages over the 12-week duration of the project, then shifted the schedule forward in order to have 10 separate schedules that followed the same pattern of increases and decreases. Using 10 different wage schedules creates $village \times week$ variation that allows me to control for village and time fixed effects separately. The shifted schedule (as opposed to iid randomized wages) means that each village has the same total earnings potential, and that averages across villages, within week are approximately constant. Since it is possible that participants will consider relative wages, the schedule is designed such that each village faces the same number of wage increases and decreases. After randomly allocating each village to a wage-schedule, I allowed LHA leaders and government extension workers to determine the day of the week on which villages would be visited.[2]

Randomizing the villages' starting points in the wage schedule rather than separately assigning wages for each village-week was ultimately a trade off that insured against poorly distributed wages in a small sample at the cost of introducing serial correlation in the wages. Participants did not detect the negative serial correlation in wages, however. A survey conducted after week eight asked participants, "what do you think the wage will be next week?" and "what do you think the wage will be in two weeks?" Eighty percent of participants knew the correct wage for their village in week nine; three percent answered but gave an incorrect wage; 17 percent said that they did not know the wage for week nine. This is clear evidence that wage changes were anticipated one week in advance. In contrast, fewer than

[1] The wages are based on outcomes from a pilot study I conducted in March 2009, where 77 percent of participants worked for the lowest offered wage of MK 70, and 96 percent worked for the highest offered wage of MK 120.

[2] The list of villages given to LHA leaders and extension workers reflected the randomization, i.e. the village randomly selected as "village one" was listed first, the village randomly selected as "village two" was second, etc. The LHA leaders and extension workers retained that ordering in many cases when deciding which villages to visit on which days of the week. Since I use village fixed effects, and since the wage schedule is exogenous in each village, the relationship between day-of-week and wage schedule does not compromise the results.

one percent of those surveyed in week eight knew the correct wage for week 10. When asked, "what will the wage be in two weeks?" eight percent answered but gave an incorrect wage; 92 percent said that they did not know the wage for week 10. It seems reasonable to assume that participants' expectations of wages after the anticipated change in week $t+1$ would revert to some constant level, perhaps the government rate for day labor (MK 110) or the local market rate. To further explore the hypothesis that only the announced change affects employment and participants did not detect the negative serial correlation in the wage schedule, I run specification tests of my baseline specification of labor on wages where I include future wages. I check the effect of including wages for one week in the future (which limits the sample to data from weeks one to 11) and one, two, three, and four weeks in the future (which limits the sample further, to weeks one to eight). The coefficients on future wages are never statistically different from zero and do not significantly change the point estimate of the coefficient on concurrent wages. See Appendix Table 20 for these results.

4 Data

In total, the project includes 530 individuals[3] in 298 households. I follow these individuals for 12 weeks, recording their participation in each week's work activity. This gives me 6333 binary observations of individual labor supply. Additionally, I have records of major community events that may affect participation, especially funerals held in the village.

To complement the administrative data, I use data from four surveys: a baseline survey and three follow-up surveys. The baseline survey was conducted at the outset, before participants were told about the nature of the project or the activities involved. It contains demographic and socioeconomic characteristics of respondents and information about their previous work history. The three follow-ups were conducted after the fourth, eighth, and 12th weeks of the project (with each village surveyed 6 days following its 4th, 8th, and 12th assigned work day). These follow-up surveys first ask respondents to recall their own participation and the wages over the previous four weeks, then ask about positive and negative shocks experienced in the same time period. The recall questions verify that participants are reasonably accurate in describing their participation in the project, and enhance my

[3]One individual died after week six of the project, so the sample size in weeks 7-12 is 529.

confidence in their memory of shocks experienced in specific weeks.

5 Results from individual level data

5.1 Baseline Characteristics

The final sample includes 530 individuals in 298 households. Of those, 464 respondents are spouses living in 232 households, and 66 are individuals whose spouses were not available to participate in this project. The survey team was able to interview 514 participants the week before the project began. Respondents in pre-selected households who were not available during the survey period were nonetheless allowed to participate in the study, to avoid creating a sample biased towards those with low opportunity cost of time. Table 2 presents baseline characteristics for participants in this project. The majority of the sample are married women. Participants have attended an average of four years of school and live in households with approximately two adults and three children. Respondents own an average of 1.8 acres of land; their houses have an average of two rooms; and only 16 percent of respondents have tin roofs on their houses.

Respondents report being paid an average of MK 236 ($US 1.69) per day the most recent time they did ganyu. They worked an average of one day in the week before the survey or 2.7 days in the month before the survey. The reported wage is outside of the range included in the study, and is higher than the average in the IHS.

When asked the lowest wage they would accept for a day of ganyu each month of the year, respondents quote similarly high numbers. Interestingly, there is little variation across month: the median lowest acceptable wage is MK 200 per day for all months except for February and April, when it is MK 150/day. The constant stated reservation wage is not consistent with the notion that marginal productivity, and therefore opportunity cost, varies across the season in an agricultural economy. Further, it is not consistent with respondents' own behavior in the subsequent experiment, where all participants accept work at wages substantially lower than MK 200/day.

5.2 Elasticity of working

In order to estimate the elasticity of working, I run regressions of the form $labor_{itv} = \alpha + \beta ln(wage_{tv}) + \nu$. The coefficient β is the marginal effect of a one log-point, or approximately one-percent, change in wages on the probability that an individual works. The marginal effect is not an elasticity, but it is easily transformed into one using the standard formula, $\epsilon_e = \frac{\partial Q}{\partial P} \times \frac{P}{Q}$. Because I am using log-wages as the independent variable, I compute $\epsilon_e = \frac{\beta}{mean(labor)}$. This elasticity corresponds to the extensive margin elasticity from labor supply equation 5 above.[4]

In Table 3, I begin by pooling observations across weeks and villages without any additional controls. I find that a one-percent increase in wages is associated with a 12.8 percentage-point increase in the probability of working. This effect is significantly different from zero at the 99 percent confidence level. The elasticity corresponding to the estimate from the pooled data in Column (1) is 0.15. In columns (2), (3), and (4) respectively, I add fixed effects for village, week, and village and week together. Controlling for village and week separately or together does not change the magnitude of the coefficient or associated elasticity much. The elasticity in the specifications with week effects increases slightly, to 0.17. In Column (5), I replace village and week fixed effects with individual fixed effects, controlling for unobserved time-invariant characteristics that are commonly thought to affect labor supply. Finally, I include individual and week fixed effects in Column (6). As before, this specification does not substantially alter the results: a one-percent increase in wages is associated with a 12.8 percentage-point increase in the probability of working, for an implied labor force participation elasticity of 0.17. I report bootstrapped standard errors for the specifications in this and all subsequent tables.[5]

A long literature suggests that men may supply labor more elastically than women in developed countries (e.g. Killingsworth 1983, Heckman [10]). Previous work in developing countries is also consistent with women supplying labor more elastically than men in India

[4] I discuss results for aggregate analysis at the village-week level in the appendix.

[5] I calculate standard errors for regressions of the form $labor_{itv} = \alpha + \beta ln(wage_{tv}) + \nu$ from 500 bootstrap replications, where I re-sample villages from the original data. Cameron et al. [6] demonstrate that a wild boostrap is preferable with a small number of clusters, but a wild boostrap is valid for t-statistics rather than standard errors, and thus requires a single null hypothesis. Zero is a poor choice for the null hypothesis since there is nothing in the previous literature that seriously suggests the supply of wage labor is perfectly inelactic, but there is no other obvious choice of a null hypothesis. Future drafts of this paper will present graphs of wild bootstrapped t-statistics against a range of null hypotheses for key estimates.

and Ghana. In Tables 4 and 5, I look at the samples of men and women separately. On average, 81 percent of men worked when offered employment. The estimated elasticity for men ranges between 0.16 and 0.19, with fixed effects added across columns in Table 4 as in Table 3. Results for women are strikingly similar. Some 86 percent of women work across the entire sample. Their elasticity with respect to wages falls between 0.14 and 0.15, estimates that are not statistically different from the elasticity for men.

The most serious challenge to the internal validity of the estimates, given the experimental design, would be that there was some unobserved "treatment" that was correlated with wages and affected labor supply. One such would be pressure applied by village headmen or other local leaders to increase participation during low-wage weeks. Survey data collected after weeks four, eight, and 12 indicate that pressure to work affected the participation decisions of about 20 percent of those who worked for MK 30, and had negligible effects at all other wages. Getting cash to spend immediately was the dominant self-reported reason for working at all wage levels. More detailed analysis of self-reported reasons for working and not working will be presented in a subsequent section of this paper. Another challenge to the internal validity of the results would be that individuals anticipated future wages, making each week's decision a response not only to that week's wages, but also to the prediction of the following week's wages. However, as reported in the description of the wage schedule, participants were unable to make predictions about wages, and including future wages does not affect the estimated elasticity.

The estimates themselves are robust to many alternative specifications. In addition to the specifications in Table 3, I have estimated the elasticity without the first week of data; without the last week of data; and separately for the first and last halves of the project. I have limited the sample to individuals in households with two participants and to individuals who responded to the baseline survey. Results are available in the appendix. None of these specifications lead to statistically or substantively different results from those presented in Table 3. I also repeat the analysis removing the 95 always-takers and limiting the sample to the 434 individuals who worked 11 days or fewer. As expected, the elasticity among this subsample is somewhat higher, but it never exceeds 0.20.

The best case for the generalizability of my results comes from the invariance of estimates to the inclusion of village and time fixed effects. Village specific factors are not driving results within the 10 villages in the sample. Without overgeneralizing this finding to the rest of Malawi or to other countries, I can offer evidence that Dedza (the district in which the project took place) has a similar market for casual day labor as the rest of Malawi. Using IHS data, I plot the distributions of wages[6] in Dedza and in the rest of Malawi in Figure 1. The distributions are well-aligned. Additionally, average wages are not statistically different in Dedza from the rest of Malawi. Similarly, in Figure 2 the distributions of ganyu worked in the previous week (conditional on doing some ganyu) in Dedza and the rest of the country are similar.

One serious question about my results is why labor force participation is as high as it is for even very low wages. Specifically, I am concerned about a John Henry effect of the experiment, where participants work in order to please the experimenter, rather than because of the treatment. The project was designed to minimize any such effect by working with a known local organization, having jobs supervised by government extension officers who routinely hire villagers for day labor, and making the end date of the project explicit. Nonetheless, one may wonder why seventy-three percent of participants chose to work for a wage lower than one third of the government's rate for similar labor. While this is striking, working for very low wages is not uncommon in Malawi. In the 2004 IHS, adult respondents report wages as low as MK 10/day. One quarter of adults in rural areas report an average wage of MK 40 or less, and the median daily wage was MK 60. The phenomena of working for very low wages is common throughout Malawi and not specific to my experiment.

5.3 Elasticity at the intensive margin from simulated cross sectional data

My data differ from data used in previous estimates of labor supply in three important ways. First, wages are randomly assigned. Second, I observe the full distribution of wage offers, rather than only the average wage *accepted* by each individual. Third, I have panel rather than cross sectional data. Additionally, I estimate the elasticity of labor supply at the extensive, rather than the intensive, margin. In order to understand how my results fit into the longer literature on the supply of labor in developing countries, it is useful to collapse my panel into a cross section that mimics the limitations of the commonly available data,

[6] Wages are censored at the 99th percentile.

and then use this comparable data set in order to calculate the intensive margin elasticity. For the dependent variable, I add up the total number of days worked (which ranges from 0 to 12). This is the concept that Bardhan uses by taking the total number of days worked in the seven-day period covered by the survey of households in West Bengal that he analyzes. Note that this measure in my cross section is already more precise than normal in survey data, because it comes from administrative records rather than self-reports. Every individual in the sample worked at least two days, and, on average, individuals worked 10 days. Since every individual worked at least once, it is not possible to estimate the elasticity of labor force participation using the cross sectional data in this sample.

I construct three different measures of wages. First, I use the common "average wage" measure by taking the within-person across-week average *accepted* wage. Second, following Bardhan, I compute the "village average wage" as the within-village across-week average accepted wage. Third, I use baseline demographic and socioeconomic measures as instruments for accepted wages. Because all wages that were offered in this experiment were accepted by at least some participants (and in practice, even the lowest wage was accepted 73 percent of the time it was offered) and all participants had the same distribution of wage offers, the average wage measures in the simulated cross section are endogenous but not censored.

I present the results from this exercise in Table 6. The dependent variables in this table are the scalar number of days worked during the project. The elasticity is interpreted as the percentage increase in days worked for a one-percent increase in wages and comes from equation (1) in Section 2.1. Column (1) is a baseline specification with no additional controls. In this specification, a one percent increase in wages is associated with an 8.64 increase in days worked, for an elasticity of 0.86 (because average days worked is close to 10). Despite lack of individual covariates, the r-squared for this specification is very high, 0.81. In column (2), I add village fixed effects. In column (3), I add individual controls for gender, marriage status, age, and three measures of wealth: acres of land owned by the household, number of rooms in the house, and whether or not the house has a tin roof. The elasticities estimated in these two specifications are 0.85 and 0.86, respectively, and are not statistically different from the baseline specification. In column (4), the regressor of interest is average village wages. Fixed effects are not separately identified with this measure of wages, but the same individual covariates as in column (3) are included. The elasticity is 0.89, not statistically different from estimates using person-specific average wages. Finally, I use the individual characteristics as instruments for wage in column (5), and individual characteristics and village dummies as instruments for wages in column (6). In both IV specifications, the first stage f-statistic strongly exceeds 10. The estimated elasticities from the IV specifications are 1.04 using individual characteristics as instruments, and 1.01 using individual characteristics and village dummies. I cannot reject perfectly elastic labor supply at the intensive margin ($\epsilon = 1$) in any of estimates using my simulated cross sectional data.

Tables 7 and 8 analyze the simulated cross sectional data separately for men and women. As in the results for extensive margin elasticities using panel data, these intensive margin elasticities for men are substantively and statistically similar. One exception is the results when using village average wages as in column (4) of each table. Women appear less elastic than men using this measure. The estimated elasticities for women are of similar magnitude to those in Rosenzweig [17] in India and Abdulai and Delgado [1] in Ghana. However, the elasticities for men in my data exceed the elasticities for men in these previous papers.

5.4 Non-linear models of employment

The estimates I have presented thus far come from log-linear specifications of a continuous labor supply function. Figure 3 plots the fraction of the sample working at each wage. The graph suggests possible trend breaks at MK 50 and MK 100. I estimate several non-parametric and semi-parametric models in Table 9, allowing for individual and week fixed effects. Column (1) repeats the log-linear specification from Table 3 column (6) and is included for reference. In the fully non-paremetric model in column (2), I estimate $labor_{itv} = \alpha + \beta_w 1(wage_{tv} = w) + \nu$, with separate indicators for each wage of MK 40 through MK 140. The omitted indicator is for MK 30, so α represents the fraction of the sample who work for the lowest wage and each coefficient represents the increase in employment at a wage above MK 30. Labor supply at wages of MK 100 and higher is significantly different from labor supply at MK 30. I show p-values for f-tests of each non or semi parametric model against other, more restricted models. In column (2), I reject the linear restriction imposed in column (1) as well as the models with a single discontinuity at MK 100 that are estimated on columns (4) and (5).

In column (3), I move from a full set of dummy variables for wages to a single indicator for wages of MK 100 or higher. I run the regression $labor_{itv} = \alpha + \beta 1(wage_{tv} \geq MK100) + \nu$, with the coefficient β representing the difference between average employment for wages less than MK 100 and wages MK 100 and higher. A positive coefficient on the indicator for wages of MK 100 or higher is consistent with a linear increase in employment and therefore the significance of the coefficient on $\beta 1(wage_{tv} \geq MK100) + \nu$ is not evidence of a discontinuity. I test for a discontinuity at MK 100 in column (4) with the regression $labor_{itv} = \alpha + \beta_1 ln(wage_{tv}) + \beta_2 1(wage_{tv} \geq MK100) + \nu$. The positive, significant coefficient β_2 indicates a non-linear increase in labor force participation at the wage MK 100. Nearly seven percent more respondents work when wages reach MK 100 than we would expect from a linear trend. I reject the linear restriction imposed in column (1) against this model that allows for a discontinuity at MK 100. Finally, in column (5) I include an interaction term between the indicator for high wages and the continuous measure of wages: $labor_{itv} = \alpha + \beta_1 ln(wage_{tv}) + \beta_2 1(wage_{tv} \geq MK100) + \beta_3(1(wage_{tv} \geq MK100) \times ln(wage_{tv})) + \nu$. I cannot reject the model in column (4), which restricts the slope of labor force participation to be the same above and below MK 100.

The non linearity of labor supply is a more dominant pattern for women than men. Table 10 presents the results for men only. In the non parametric specification in column (2), labor force participation at wages as low as MK 50 is significantly different from labor force participation for MK 30. I marginally reject the linear restriction against the fully non-parametric model, but cannot reject the semi parametric models from columns (4) and (5) against the non parametric model in column (2). For men, models with a break point at MK 100 appear to fit the data better than either the linear or the fully non parametric model.

In Table 11 I analyze women's labor force participation. Here, differences from labor supply for wages of MK 30 become statistically significant at MK 100. I reject the linear restriction against the non parametric model in column (2), find that labor supply is higher above than below MK 100 in column (3), and determine that labor force participation jumps by a statistically significant 7.9 percentage points at MK 100.

There is no obvious rational explanation for a discontinuity at MK 100. No outside employer offered participants MK 100 as an alternative to working through this project. The government's set rate is MK 110, but employment increases discontinuously before MK 110. One plausible explanation is of "benchmarking" to the round number 100.

5.5 Household employment

Recall that my experiment made work available to up to two adults in a household, in order to simulate the effect of an increase in the prevailing wage available to all adults. Members of the household may have coordinated their decisions about working, making the household's labor supply, rather than the labor supply of individuals, the appropriate outcome. To examine household labor supply, I limit the sample to the 232 households with two participating members, and create an indicator for whether *both* of those members worked in a given week. Overall, both members of two-person households worked 74 percent of the time. I present results from this specification in Table 12. As in the analyses of individuals' labor supply, I add fixed effects for village, week, and household as I move across the table. The resulting estimates suggest that a 10 percent increase in wages leads to a 20 percentage-point increase in the probability that both members of a household will work. While these estimates are not transformable into standard elasticities, the statistically significant differences between the magnitudes of these coefficients and those for individuals' labor supply in Table 3 do provide information about household decision-making about labor supply. An increase in wages increases the probability that both members of a household will work by twice as much as it increases the probability an individual will work.[7] This could be due to correlated marginal utility of consumption (or opportunity cost of work) within a household, or could have behavioral explanations.

5.6 Savings accounts

The estimates shown in Tables 3, 4 and 5 indicate highly inelastic decisions about working for participants in a daily labor market. One hypothesis for inelastic labor supply is that individuals have difficulties saving money, making labor supply more responsive to marginal utility of consumption than to wages. If so, the ability to save should result in more elastic supply of labor. To test this hypothesis, I implemented a cross-cutting randomization of savings accounts. Since individuals within a household are likely to share resources, I randomized savings accounts at the household level. Within each village, respondents in half of the households were offered the chance to open a savings account with the LHA Savings and

[7] Using only individuals in the 232 two-member households to compute individuals' response to changes in wages yields results identical to those presented in Table 3.

Credit Cooperative (SACCO), an affiliate of the regional government's SACCO. Randomization was conducted in the field, with one representative per household drawing a bottle cap from an envelope. No participants had accounts before this project, apparently because of lack of information about account availability and account opening procedures. All "winners" chose to open accounts. To be eligible for an account, individuals must be members of LHA. I paid the MK 150 ($US 1.10) membership dues for all participants in this project, including those who were not assigned to receive a savings account. I collect information about deposits into SACCO accounts in the 7 days following the respondent's assigned work day for all 12 weeks. The data about deposits come from the LHA SACCO files.

I run regressions of the form $labor_{itv} = \alpha + \beta_1 ln(wage_{tv}) + \beta_2 save_i + \beta_3 (save_i \times ln(wage_{tv})) + \nu$, where "save" in an indicator variable that equals one for individuals in households that were randomly assigned to receive an account with the LHA SACCO, and equals zero otherwise. If access to savings accounts leads to more elastic labor force participation, then β_3 will be positive.

In columns (1) and (2) of Table 13, I use individual labor supply as the outcome of interest. The binary "save" equals one for all members of households who were assigned to receive savings accounts. All of the specifications in Table 13 use village and week fixed effects, as in columns (4) of the previous tables. Using individual fixed effects precludes separately identifying the effect of savings accounts on the outcomes of interest. Neither the main effect of having an account, in column (1), nor the interaction between savings accounts and wages, in column (2) is statistically different from zero. Note that for those assigned to receive savings accounts, the elasticity of supply of labor includes the main and interaction effects, so $\epsilon_e = \frac{\beta_1 + \beta_3}{mean(labor)}$. The estimated elasticity when accounting for savings accounts is 0.17, similar to results in Tables 3 and 12. In columns (3) and (4), the dependent variable is an indicator for whether both members of two-person households worked. Again, neither the main nor the interaction effects of savings accounts are statistically significant.

If being assigned to receive a savings account does not actually increase savings, then the results in Table 13 would not be not surprising. In Table 14, I examine the relationship between savings accumulated up to but not including week t on the effect of labor supply in week t. The sample is limited to those assigned to receive savings accounts. Accumulated savings are zero by definition in all weeks for all respondents who did not receive accounts, so including the full sample introduces substantial noise. Focusing on the subsample of those who received accounts should yield a more precise estimate of accumulated savings among the relevant population. Ninety-four of the 147 households assigned to receive savings accounts had made at least one deposit as of week 12 (and 87 of those households had made at least one deposit by week six). Households that made at least one deposit had deposited an average of MK 752 as of week 12.

I estimate $labor_{itv} = \alpha + \beta_1 ln(wage_{tv}) + \beta_2 savings_{it-1} + \beta_3 (savings_{it-1} \times ln(wage_{tv})) + \nu$. Note that "$savings_{it-1}$" is predetermined as of week t. As in Table 13, columns (1) and (2) refer to individuals' supply of labor, and columns (3) and (4) to an indicator for both members of two-person households working. The elasticity for savers in Column (2) includes the main and interaction effects of wages, and $\epsilon_e = \frac{\beta_1+\beta_3}{mean(labor)}$, and the coefficients in the household regressions are not transformed into standard elasticities. I use individual or household fixed effects to control for omitted characteristics of respondents that might simultaneously determine past savings and present labor supply.

Neither the main effect of accumulated savings or the interaction between accumulated savings and wages are statistically different from zero in any of the specifications. The estimated elasticity of individual labor supply controlling for accumulated savings is 0.15, consistent with the results for individuals in Table 3. When including the interaction between accumulated savings and wages, the estimated elasticity of individual labor supply is 0.24. While this is substantively higher than my earlier estimates for individuals, the difference between this and earlier estimates is not statistically significant. Together, the results in Tables 13 and 14 do not support the hypothesis that lack of access to savings technology constrains the elasticity of the supply of casual wage labor.

5.7 Funerals

In Malawi, attending funerals is a serious social obligation. Bodies are not typically embalmed, so funerals occur soon after deaths. Thus, in the context of my experiment, funerals can be though of as exogenous village-level shocks that increase the opportunity cost of working. While funerals are a specific and perhaps particularly important increase in the opportunity cost of work, the results in Table 15 may provide a bound on the elasticity of labor supply during other periods when the opportunity cost of working is high, such as when individuals are planing or harvesting their own fields. I begin by running regressions of the form $labor_{itv} = \alpha + \beta_1 ln(wage_{tv}) + \beta_2 funeral_{tv} + \beta_3(funeral_{tv} \times ln(wage_{tv})) + \nu$. I include week and individual or household fixed effects in all specifications. In Table 15, we see that the main effect of funerals is to sharply reduce the probability of individuals (column 1) or households (column 3) working at any given wage. Interacting funerals with wages, as in columns (2) and (4), makes the effect of funerals on labor supply not statistically different from zero. The negative coefficient on the interaction between funerals and wages tells us that the effect of wages on supply of individual labor is reduced when there is a funeral. In other words, individuals are less elastic in their supply of labor when there is a funeral in their village. The elasticity when there is a funeral in the village is computed as $\epsilon_e = \frac{\beta_1+\beta_3}{mean(labor)}$. For individuals, elasticity of labor force participation with respect to wages is 0.15 with no funerals, but 0.10 when work conflicts with funerals.

5.8 Reasons for working or not working

In follow up surveys administered after weeks four, eight, and 12, I first ask respondents to recall the wages in each of the past four weeks, and to report whether or not they worked in each of those weeks. While these data duplicate administrative records, they provide a useful benchmark for interpreting self-reported reasons for working or not working. If individuals cannot accurately report whether or not they worked, or do not correctly recall their own wages, then their explanations for why they worked may be questioned. However, recall of wages and work history is relatively accurate in this sample, with 83 percent and 86 percent of the self-reported person*week observations for both wages and labor supply respectively matching the administrative data. In 77 percent of observations, work and wages were reported accurately. In subsequent analysis of reasons for working or not working, and reports of shocks experienced by the household, I use only data corresponding to weeks in which the self-reported wage and work history were accurate, though results do not change when using data corresponding to weeks with imperfectly recalled information.

Respondents listed up to three reasons for working in weeks that they worked, or three reasons for not working in weeks they did not work. Wages do not appear to be a major factor in the decision either to work, or not to work. Reasons for working were grouped into four categories: because of the wage, to get money to spend immediately, to get money to save, or because of social pressure. Figure 4 shows the fraction of individuals who mentioned each reason, aggregated across weeks for individuals who worked at each wage. Earning money to spend immediately is the dominant factor at all wage levels, and is mentioned by over 70 percent of respondents no matter what the wage. Social pressure to work, which includes being told to work by a local leader or government extension worker, seems relevant only at the lowest wage, MK 30. The wage itself is rarely mentioned by fewer than two percent of respondents for all wages less than MK 100, but by 30 or more of respondents at wages of MK 100 or higher.

Reasons for not working were grouped into six categories: because of the wage, because the respondent was occupied with other work, because money was not needed, because of a funeral, because of illness (to the respondent or someone he/she was caring for), and because of social pressure not to work. Figure 5 shows the reasons for not working at each wage. Illnesses and funerals were the dominant causes of not working, which is consistent with the strong negative effect of funerals on labor supply in the administrative data used in Table 15. Wages were mentioned by fewer than 20 percent of respondents at all wage levels except for the lowest two, MK 30 and MK 40, and a spike at MK 80.

These self-reported data are consistent with the highly inelastic labor supply curve estimated in previous sections. Wages do not seem to drive either the decision to work, or the decision not to work. Other factors dominate wages, even at very high or very low wage levels.

5.9 Shocks to the endowment

Models of day labor as an ex post strategy for mitigating the effect of negative shocks have been substantiated by research in India [11] and suggestive evidence exists for Malawi [14]. I use the survey data collected every four weeks to measure the response of labor supply to positive and negative shocks in the previous week. If working is an ex-post coping strategy, we would expect higher supply of labor in weeks following a negative shock, and lower supply of labor in weeks following a positive shock. Further, we would anticipate a lower wage elasticity following a negative shock. The effect of a positive shock on wage elasticity is less obvious. Elasticity would be higher if the income effect of a positive shock dominates the substitution effect, and lower if the substitution effect dominates.

In analyzing response to shocks, I limit the sample to observations corresponding to correct recollection of wages and labor supply. I include individual fixed effects in all models, and use individuals as the unit of analysis. Though shocks are not randomized, using within-person variation increases my confidence that the "effect" of shocks has a causal interpretation. I first examine the effect of shocks in the linear framework, running regressions of the form $labor_{itv} = \alpha + \beta_1 shock_{it-1,v} + \beta_2 ln(wage_{tv}) + \nu$. Note that because of the lagged dependent variable, I use labor supply outcomes beginning in week 2. The first three columns in Table 16 examine the effect of negative shocks. A negative shock in one week increases the supply of labor in the following week by about one percentage point, or 1.1 percentage points after controlling for wages. Neither result is statistically different from zero. Adding the interaction between shocks and wages in column (3), however, the effect of a negative shock becomes a highly significant 21 percentage-point increase in subsequent labor supply. The elasticity is indeed lower with a shock ($\epsilon_e = 0.09$) than without a shock ($\epsilon_e = 0.14$), and the difference between the two elasticities at the margin of being statistically significant. The result in column (3) is consistent with the hypothesis that ganyu is used as an ex post strategy for coping with negative shocks.

The fourth through sixth columns in Table 16 consider positive shocks. The effect of positive shocks on the supply of labor is small and not statistically different from zero, with or without controlling for wages. In column (6), we see that the elasticity is smaller after a positive shock than after no shock, but the difference is not statistically significant at standard confidence levels.

Next, I test whether shocks explain behavior in the non linear model, with a single discontinuity at MK 100. In column (1) of Table 17, I confirm that the difference between employment at MK 90 and less remains statistically different from employment at MK 100 or more in the subsample of respondents with accurate recall. In column (2) we are reminded that the slopes above and below MK 100 are not statistically distinguishable. I add an indicator for negative shocks in the previous week in column (3). As in the linear model, negative shocks are strong positive predictors of labor. In column (4), I interact shocks with an indicator for wages of MK 100 or higher. The coefficient on the interaction term is negative as predicted, but it is not statistically different from zero.

5.10 Funerals

Recall that having a funeral in the village sharply reduced the probability that an individual would work. In Table 27 I run regressions of the form $labor_{tv} = \alpha + \beta_1 ln(wage_{tv}) + \beta_2 funeral_{tv} + \beta_3(funeral_{tv} \times ln(wage_t))$ in order to test the effect of funerals on total village labor supply. The specification in column (1) omits the interaction term; the main effect of funerals β_2 is highly significant; having a funeral in the village reduces total employment by more than 20 percent on the day of the funeral. Column (2) includes the interaction term. There, the main effect of funerals large and negative but not statistically significant, and the coefficient on the interaction term is positive and not significant. The standard errors are too large to draw meaningful inference about the effect of funerals on the elasticity of aggregate labor supply.

6 Conclusion

I use experimental variation in wages to study the effect of wages on the probability of working in the daily labor market in rural Malawi. This unique field experiment allows me to estimate a causal effect of wages on the probability of employment and to avoid the standard problems associated with simultaneous determination of supply and demand in cross sectional data about employment. I randomize wages at the village*week level, then offer employment to up to two adult members of pre-selected households in participating villages for one day per week for 12 weeks. The final sample consists of 530 individuals in 298 households, across ten villages. The panel of administrative outcomes allows me to use individual fixed effects in most specifications. I estimate that the elasticity of employment for individuals in this sample is between 0.15 and 0.17. The elasticity of labor supply at the village level is 0.15 to 0.16. These estimates are remarkably consistent across a variety of specifications. I robustly reject perfectly inelastic supply of labor in all specifications, and find some evidence of non linearities in the supply of labor.

One potential explanation for highly inelastic supply of labor is that people face obstacles to saving their wages. If income cannot be transferred from one period to another, it is rational to supply labor in response to marginal utility of consumption rather than wages. I test this hypothesis randomly assigning half of participating households to receive savings accounts with a local savings and credit cooperative. The effect of access to savings on the supply of labor and on the elasticity of the supply of labor is a precisely estimated zero. This suggests that inability to save wages does not cause the highly inelastic employment patterns observed in this sample.

After weeks four, eight, and 12, I collect survey data about recollection of wages and work history, as well as reasons for working and positive and negative shocks experienced by the household. The data about recollection of wages and work history confirm that respondents are accurate in their memory of the events, reporting both wages and past work accurately in 83 percent of the cases. I then use information from weeks in which respondents remembered the wage and whether or not they worked to examine self-reported reasons for working. At all wage levels, earning money to spend immediately is the most frequently reported reason for working, and funerals and illnesses are the dominant reasons for not working. Wages are cited by more than 20 percent of respondents as a reason for not working predominantly at very low wages (MK 30 and MK 40), and as a reason for working only at high wages of MK 100 or higher. These survey responses are consistent with the inelastic supply of labor observed in the administrative data.

I also use survey data about positive and negative shocks to examine the use of ganyu as an ex post coping strategy. Controlling for wages, I find that individuals are 21 percentage points more likely to work in the week following a negative shock. The elasticity of employment is 50 percent higher in weeks that do not follow a negative shock as in weeks that do follow a negative shock. In the non linear specification, negative shocks predict deviations from a reservation wage of MK 100. At the village level, elasticity of labor supply following a week with no shocks is 0.19 compared to 0.14 following a week with an average frequency of shocks. More labor is supplied and labor supply is less responsive to wages following negative shocks. These findings are consistent with the notion of supplying wage labor as an ex post coping strategy.

Understanding the labor supply behavior of poor individuals is crucial for the design of public employment projects in Malawi and other developing countries. The Government of Malawi and the World Bank are investing $40 million in a Community Livelihoods Support fund that uses public sector employment to meet dual goals: providing a safety net for poor individuals by offering employment, and improving infrastructure in the communities where those individuals live. Inelastic labor force participation makes it clear that there are stark tradeoffs between these goals when determining wage levels for the program. Malawi is not the only developing country with an interest in public employment programs: India passed the National Rural Employment Guarantee Act, which assures all adults who live in rural households up to 100 days of work at a guaranteed minimum wage. The estimates I obtain from my experiment in Malawi not only contribute to the long and evolving literature about labor supply in developing countries, but also provide important parameters for understanding the impact of government and NGO programs that are already reaching millions of people.

Tables

Table 1: Weekly Wage Schedule (MK)

	1	2	3	4	5	6	7	8	9	10	11	12	Total
Kafotokoza	40	100	60	120	30	110	70	140	80	130	90	50	1020
Chimowa	100	60	120	30	110	70	140	80	130	90	50	40	1020
Manase	60	120	30	110	70	140	80	130	90	50	40	100	1020
Lasani	120	30	110	70	140	80	130	90	50	40	100	60	1020
Njonja	30	110	70	140	80	130	90	50	40	100	60	120	1020
Hashamu	110	70	140	80	130	90	50	40	100	60	120	30	1020
Kachule	70	140	80	130	90	50	40	100	60	120	30	110	1020
Msangu/Kalute	140	80	130	90	50	40	100	60	120	30	110	70	1020
Kamwendo	80	130	90	50	40	100	60	120	30	110	70	140	1020
Kunfunda	130	90	50	40	100	60	120	30	110	70	140	80	1020
Average	88	93	88	86	84	87	88	84	81	80	81	80	

Table 2: Baseline Characteristics

	Mean	SD	N	10th	Median	90th
Male	0.38	0.48	512			
Married	0.80	0.40	512			
Years of education	4.34	3.16	510	0	4	8
Number of adults in HH	2.25	0.97	512	1	2	3
Number of children in HH	3.15	1.91	512	1	3	6
Tin roof	0.16	0.37	512			
Number of rooms	2.02	0.92	507	1	2	3
Acres of land	1.81	0.87	512	1	1.5	3
Last wage for ganyu	235.94	277.94	496	50	200	425
Days of paid work last week	1.02	1.57	512	0	0	3
Days of paid work last month	2.71	4.59	512	0	1	7

Table 3: Elasticity of individual labor force participation w.r.t. wages

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Individual*day indicator for working					
Ln(wage)	0.127***	0.127***	0.140***	0.140***	0.127***	0.140***
	(0.033)	(0.033)	(0.032)	(0.032)	(0.033)	(0.032)
Village effects		x		x		
Week effects			x	x		x
Individual effects					x	x
Observations	6333	6333	6333	6333	6333	6333
Mean of dependent variable	0.84	0.84	0.84	0.84	0.84	0.84
Elasticity	0.15	0.15	0.17	0.17	0.15	0.17
	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is individual*week, sample is all individuals.
* p<0.10, ** p<0.05, *** p<0.001

Table 4: Elasticity of men's labor force participation w.r.t. wages

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Individual*day indicator for working					
Ln(wage)	0.133***	0.133***	0.151***	0.151***	0.133***	0.151***
	(0.032)	(0.032)	(0.035)	(0.035)	(0.032)	(0.035)
Village effects		x		x		
Week effects			x	x		x
Individual effects					x	x
Observations	2544	2544	2544	2544	2544	2544
Mean of dependent variable	0.81	0.81	0.81	0.81	0.81	0.81
Elasticity	0.16	0.16	0.19	0.19	0.16	0.19
	(0.042)	(0.042)	(0.046)	(0.046)	(0.042)	(0.046)

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is individual*week, sample is all men.
* p<0.10, ** p<0.05, *** p<0.001

Table 5: Elasticity of women's labor force participation w.r.t. wages

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Individual*day indicator for working					
Ln(wage)	0.122***	0.122***	0.132***	0.132***	0.123***	0.132***
	(0.035)	(0.035)	(0.032)	(0.032)	(0.035)	(0.032)
Village effects		x		x		
Week effects			x	x		x
Individual effects					x	x
Observations	3789	3789	3789	3789	3789	3789
Mean of dependent variable	0.86	0.86	0.86	0.86	0.86	0.86
Elasticity	0.14	0.14	0.15	0.15	0.14	0.15
	(0.041)	(0.041)	(0.039)	(0.039)	(0.041)	(0.039)

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is individual*week, sample is all women.
* p<0.10, ** p<0.05, *** p<0.001

Table 6: Elasticity of labor supply from simulated cross sectional data

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Number of days worked					
Ln(average accepted wage)	8.641***	8.527***	8.795***		10.607***	10.231***
	(0.436)	(0.478)	(0.574)		(1.211)	(0.852)
Ln(village average accepted wage)				9.059***		
				(1.167)		
Village effects		x	x		x	x
Individual controls			x	x	x	x
Observations	529	529	488	488	488	488
Mean of dep. var.	10.09	10.09	10.18	10.18	10.18	10.18
Elasticity	0.86	0.85	0.86	0.89	1.04	1.01

OLS estimates in columns (1)-(4). IV estimates in columns (5) and (6).
All standard errors are clustered at the village level.
Additional controls are gender, marital status, age, number of rooms, acres owned, and having a tin roof.
Sample in columns (1) and (2) is all individuals. Sample in columns (3)-(6) is all individuals who answered baseline survey.
* p<0.10, ** p<0.05, *** p<0.001

Table 7: Elasticity of men's labor supply from simulated cross sectional data

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:			Number of days worked			
Ln(average accepted wage)	8.336***	8.118***	8.574***		9.746***	10.529***
	(0.291)	(0.318)	(0.274)		(1.619)	(0.629)
Ln(village average accepted wage)				12.004***		
				(2.001)		
Village effects		x	x		x	x
Individual controls			x	x	x	x
Observations	212	212	182	182	182	182
Mean of dep. var.	9.78	9.78	9.95	9.95	9.95	9.95
Elasticity	0.85	0.83	0.86	1.21	0.98	1.06

OLS estimates in columns (1)-(4). IV estimates in columns (5) and (6).
All standard errors are clustered at the village level.
Additional controls are marital status, age, number of rooms, acres owned, and having a tin roof.
Sample in columns (1) and (2) is all men. Sample in columns (3)-(6) is all men who answered baseline survey.
* p<0.10, ** p<0.05, *** p<0.001

Table 8: Elasticity of women's labor supply from simulated cross sectional data

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:			Number of days worked			
Ln(average accepted wage)	9.202***	9.212***	9.082***		13.411***	9.366***
	(1.082)	(1.193)	(1.194)		(2.604)	(1.253)
Ln(village average accepted wage)				7.495***		
				(0.930)		
Village effects		x	x		x	x
Individual controls			x	x	x	x
Observations	317	317	306	306	306	306
Mean of dep. var.	10.29	10.29	10.32	10.32	10.32	10.32
Elasticity	0.89	0.89	0.88	0.73	1.30	0.91

OLS estimates in columns (1)-(4). IV estimates in columns (5) and (6).
All standard errors are clustered at the village level.
Additional controls are marital status, age, number of rooms, acres owned, and having a tin roof.
Sample in columns (1) and (2) is all women. Sample in columns (3)-(6) is all women who answered baseline survey.
* p<0.10, ** p<0.05, *** p<0.001

Table 9: Non linear effects of wages on labor force participation

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Individual*day indicator for working				
Ln(wage)	0.140***			0.080**	0.076*
	(0.032)			(0.040)	(0.041)
Wage = MK 40		-0.002			
		(0.063)			
Wage = MK 50		0.085			
		(0.062)			
Wage = MK 60		0.068			
		(0.040)			
Wage = MK 70		0.076			
		(0.061)			
Wage = MK 80		0.061			
		(0.76)			
Wage = MK 90		0.083			
		(0.054)			
Wage = MK 100		0.178**			
		(0.060)			
Wage = MK 110		0.170**			
		(0.056)			
Wage = MK 120		0.142			
		(0.104)			
Wage = MK 130		0.219***			
		(0.062)			
Wage = MK 140		0.213***			
		(0.064)			
Wage MK 100 or more			0.132***	0.072**	-0.210
			(0.029)	(0.034)	(0.657)
Wage MK 100 or above*Ln(wage)					0.060
					(0.141)
Week effects	x	x	x	x	x
Individual effects	x	x	x	x	x
Observations	6333	6333	6333	6333	6333
Mean of dep. var.	0.84	0.84	0.84	0.84	0.84
P-value against Col. (1)		0.00		0.00	0.00
P-value against Col. (4)		0.00			0.29
P-value against Col. (5)		0.00			
P-value for $\beta_{above100} + ln(100)\beta_{interaction} = 0$					0.08

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is individual*week, sample is all individuals.
* p<0.10, ** p<0.05, *** p<0.001

Table 10: Non linear effects of wages on men's labor force participation

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Individual*day indicator for working				
Ln(wage)	0.151***			0.100**	0.101**
	(0.035)			(0.043)	(0.045)
Wage = MK 40		-0.006			
		(0.059)			
Wage = MK 50		0.085*			
		(0.043)			
Wage = MK 60		0.079**			
		(0.038)			
Wage = MK 70		0.064			
		(0.066)			
Wage = MK 80		0.068			
		(0.075)			
Wage = MK 90		0.128**			
		(0.050)			
Wage = MK 100		0.192**			
		(0.056)			
Wage = MK 110		0.191**			
		(0.046)			
Wage = MK 120		0.165			
		(0.089)			
Wage = MK 130		0.220***			
		(0.051)			
Wage = MK 140		0.211**			
		(0.063)			
Wage MK 100 or more			0.136**	0.062*	0.179
			(0.029)	(0.032)	(0.590)
Wage MK 100 or above*Ln(wage)					-0.025
					(0.128)
Week effects	x	x	x	x	x
Individual effects	x	x	x	x	x
Observations	2544	2544	2544	2544	2544
Mean of dep. var.	0.81	0.81	0.81	0.81	0.81
P-value against Col. (1)		0.09		0.01	0.03
P-value against Col. (4)		0.41			0.79
P-value against Col. (5)		0.33			
P-value for $\beta_{above100} + ln(100)\beta_{interaction} = 0$					0.14

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is individual*week, sample is all men.
* p<0.10, ** p<0.05, *** p<0.001

Table 11: Non linear effects of wages on women's labor force participation

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Individual*day indicator for working				
Ln(wage)	0.132***			0.066	0.058
	(0.032)			(0.040)	(0.041)
Wage = MK 40		-0.002			
		(0.069)			
Wage = MK 50		0.083			
		(0.078)			
Wage = MK 60		0.059			
		(0.047)			
Wage = MK 70		0.082			
		(0.059)			
Wage = MK 80		0.054			
		(0.080)			
Wage = MK 90		0.049			
		(0.061)			
Wage = MK 100		0.167**			
		(0.066)			
Wage = MK 110		0.154**			
		(0.065)			
Wage = MK 120		0.125			
		(0.117)			
Wage = MK 130		0.218**			
		(0.072)			
Wage = MK 140		0.212**			
		(0.067)			
Wage MK 100 or more			0.129***	0.079**	-0.487
			(0.030)	(0.037)	(0.741)
Wage MK 100 or above*Ln(wage)					0.120
					(0.158)
Week effects	x	x	x	x	x
Individual effects	x	x	x	x	x
Observations	3789	3789	3789	3789	3789
Mean of dep. var.	0.86	0.86	0.86	0.86	0.86
P-value against Col. (1)		0.00		0.00	0.00
P-value against Col. (4)		0.00			0.09
P-value against Col. (5)		0.00			
P-value for $\beta_{above100} + ln(100)\beta_{interaction} = 0$					0.06

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is individual*week, sample is all women.
* p<0.10, ** p<0.05, *** p<0.001

Table 12: Effect of wages on household labor force participation

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Household*day indicator both members of a 2-person household working					
Ln(wage)	0.212***	0.212***	0.231***	0.231***	0.212***	0.231***
	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)
Village effects		x		x		
Week effects			x	x		x
Household effects					x	x
Observations	2772	2772	2772	2772	2772	2772
Mean of dependent variable	0.74	0.74	0.74	0.74	0.74	0.74

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is household*week, sample is households with two participants.
* p<0.10, ** p<0.05, *** p<0.001

Table 13: Effect of savings accounts on elasticity of labor force participation w.r.t. wages

	(1)	(2)	(3)	(4)
Dependent variable:	Individual	Individual	Household	Household
Ln(wage)	0.141***	0.140***	0.232***	0.212***
	(0.033)	(0.027)	(0.044)	(0.038)
Account	-0.017	-0.026	-0.032	-0.197
	(0.013)	(0.097)	(0.021)	(0.183)
Account*Ln(wage)		0.002		0.038
		(0.020)		(0.038)
Village effects	x	x	x	x
Week effects	x	x	x	x
Observations	6285	6285	2748	2748
Mean of dependent variable	0.84	0.84	0.74	0.74
Elasticity	0.17			
	(0.041)			
Elasticity (no account)		0.17		
		(0.034)		
Elasticity (account)		0.17		
		(0.049)		

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
In columns (1) and (2), unit of observation is individual*week, sample is all individuals.
In columns (3) and (4), unit of observation is household*week,
sample is households with two participants.
* p<0.10, ** p<0.05, *** p<0.001

Table 14: Effect of accumulated savings on elasticity of labor supply w.r.t. wages

	(1)	(2)	(3)	(4)
Dependent variable:	Individual	Individual	Household	Household
Ln(wage)	0.122**	0.203	0.225***	0.392**
	(0.032)	(0.131)	(0.043)	(0.161)
Ln(savings)	0.011	0.075	0.009	0.140
	(0.011)	(0.104)	(0.016)	(0.135)
Ln(savings)*Ln(wage)		-0.015		-0.030
		(0.023)		(0.030)
Individual effects	x	x		
Household effects			x	x
Observations	1666	1666	730	730
Mean of dep. variable	0.83	0.83	0.73	0.73
Elasticity	0.15			
Elasticity (no savings)		0.24		
Elasticity (with savings)		0.23		

OLS estimates. All standard errors are clustered at the village level.
Sample restricted to households that received savings accounts.
In columns (1) and (2), unit of observation is individual*week.
In columns (3) and (4), unit of observation is household*week,
sample is restricted to households with two participants.
* p<0.10, ** p<0.05, *** p<0.001

Table 15: Effect of funerals on elasticity of labor force participation w.r.t. wages

	(1)	(2)	(3)	(4)
Dependent variable:	Individual	Individual	Household	Household
Ln(wage)	0.126***	0.128***	0.211***	0.211***
	(0.035)	(0.033)	(0.046)	(0.045)
Funeral	-0.194*	-0.013	-0.261*	-0.187
	(0.092)	(1.319)	(0.136)	(1.912)
Funeral*Ln(wage)		-0.044		-0.018
		(0.316)		(0.453)
Week effects	x	x	x	x
Individual effects	x	x		
Household effects			x	x
Observations	6333	6333	2772	2772
Mean of dependent variable	0.84	0.84	0.74	0.74
Elasticity	0.15			
	(0.043)			
Elasticity (no funeral)		0.15		
		(0.041)		
Elasticity (funeral)		0.10		
		(0.376)		

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is individual*week, sample is all individuals.
* p<0.10, ** p<0.05, *** p<0.001

Table 16: Effect of shocks on elasticity of labor force participation w.r.t. wages

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Individual*day indicator for working					
Type of shock:	Negative	Negative	Negative	Positive	Positive	Positive
Shock_{t-1}	0.007	0.008	0.212	-0.034	-0.034	0.195
	(0.012)	(0.011)	(0.161)	(0.031)	(0.033)	(0.304)
Ln(wage)		0.106**	0.120***		0.106**	0.110***
		(0.033)	(0.034)		(0.033)	(0.031)
Shock_{t-1}*Ln(wage)			-0.047			-0.053
			(0.036)			(0.070)
Village effects	x	x	x	x	x	x
Week effects	x	x	x	x	x	x
Observations	4758	4758	4758	4758	4758	4758
Mean of dependent variable	0.84	0.84	0.84	0.84	0.84	0.84
Elasticity		0.12			0.12	
		(0.039)			(0.039)	
Elasticity (no shock)			0.14			0.12
			(0.041)			(0.037)
Elasticity (shock)			0.08			0.07
			(0.048)			(0.099)

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is individual*week, sample is all individual who report wage and work accurately.
* p<0.10, ** p<0.05, *** p<0.001

Table 17: Effect of negative shocks in a non linear model

	(1)	(2)	(3)	(4)
Dependent variable:	Individual*day indicator for working			
Wage MK 100 or more	0.105**	-0.013	0.104**	0.108**
	(0.032)	(0.451)	(0.032)	(0.032)
Below 100*Ln(wage)		0.072**		
		(0.028)		
Wage MK 100 or above*Ln(wage)		0.085		
		(0.103)		
Shock_{t-1}			0.029**	0.035
			(0.011)	(0.026)
Shock_{t-1}*MK 100 or above				-0.014
				(0.037)
Week effects	x	x	x	x
Individual effects	x	x	x	x
Observations	4758	4758	4758	4758
Mean of dep. var.	0.88	0.88	0.88	0.88

OLS estimates. Cluster bootstrapped standard errors (clustered at the village level).
Unit of observation is individual*week, sample is all individual who report wage and work accurately.
* p<0.10, ** p<0.05, *** p<0.001

Figures

Figure 1: Distribution of wages (MK) from IHS

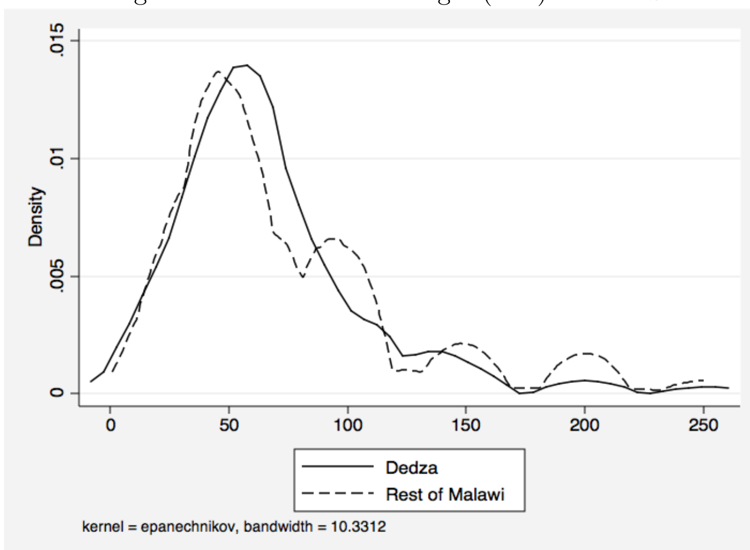


Figure 2: Distribution of hours worked from IHS

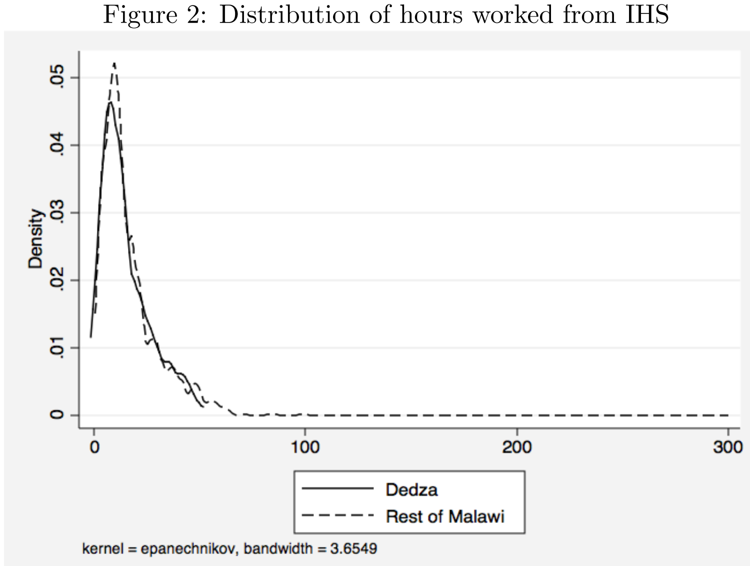


Figure 3: Fraction working at each wage (wages in MK)

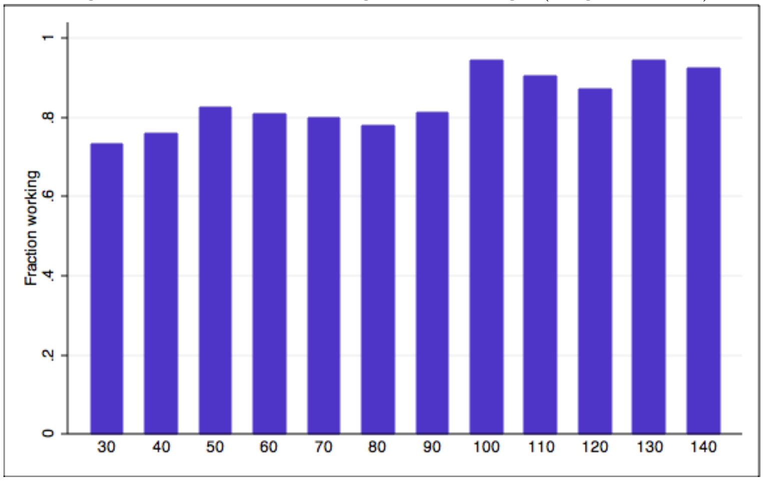


Figure 4: Self-Reported Reasons for Working

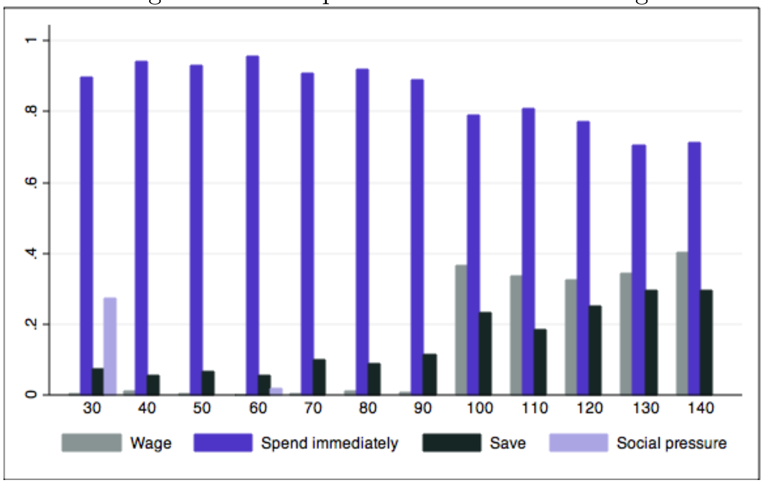
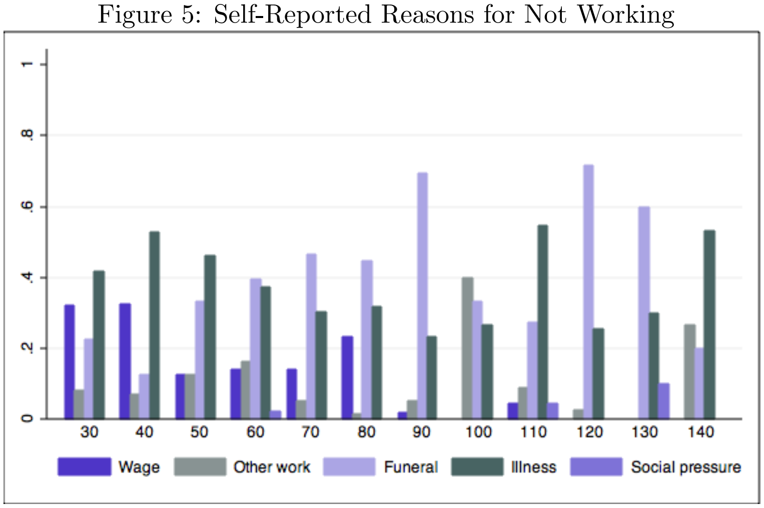


Figure 5: Self-Reported Reasons for Not Working



Appendices

A Results from village level data

An alternative to using individual observations is to collapse data to the village-week level. The study design induces variation in wages is at the village week level, so collapsing data to that level makes the source of variation and the construction of standard errors transparent relative to individual level regressions. Further, the change in average labor supply is the policy parameter of interest in determining the aggregate effect of a change in labor market policies such as a change in a minimum wage or the wage paid as part of a public works program.

Using data at the village-week level, the dependent variable is the fraction of eligible individuals in village v who worked in week t. This outcome is a continuous variable bounded by 0 and 1. The contrast to the binary outcome used in the individual level regressions has important implications for interpreting the estimates. The basic linear estimating equation is $labor_{tv} = \alpha + \beta ln(wage_{tv}) + \nu$. The coefficient β now represents the percentage-point increase in total village employment for a one log-point increase in the prevailing village wage. The aggregation shifts the interpretation of the parameter from the extensive margin estimate of a change in the *probability* of an individual working on a given day for a given wage, to an intensive margin estimate of a change in the *level* of employment in a village on a given day for a given wage. The elasticity is constructed in section 5, $\epsilon = \frac{\beta}{mean(labor)}$, but captures a different margin.

The collapsed data set consists of 120 observations (12 observations each for 10 villages).[8] The Durbin-Watson statistic for the main specification in Table 12 column (1) is 1.813, which exceeds the upper bound critical value of 1.331 and allows me to reject the null hypothesis that there is AR-1 serial correlation in the residuals. All tables include heteroskedastic-robust standard errors.

A.1 Aggregate elasticity of working

The primary results for village level analysis are unchanged from models with individual level data. Table 21 corresponds to Table 3. In the basic specification in column (1) with no covariates, a one percent increase in wages leads to a 12.4 percentage-point increase in village employment, which corresponds to an elasticity of 0.15. As before, adding village or week fixed effects has minimal impact on the estimates. The effect of wages on aggregate employment is unchanged in the model in column (2) that includes village effects and is identified by within-village, across-week variation. Using week fixed effects and identifying off of within-week, across village variation, a one percent increase in wages leads to a 13.5 percentage-point increase in village employment, for an elasticity of 0.16. Tables 22 and 23 repeat the exercise separately for men and women. The increase in average labor supply for

[8] For regressions when the sample is restricted, such as regressions for men only, women only, or regressions that include only people who accurately reported wage and work history, village means correspond to the means for the sample in the individual regressions. In individual level regressions, the number of observations changed as restrictions were imposed. In village level regressions, the number of observations remains the same but the cells represent fewer individuals as additional restrictions are imposed.

a one percent increase in wages is 13.3 to 14.3 percentage points for men, compared to 11.8 to 12.9 percentage points for women.

A.2 Aggregate non-linear labor supply

Next, I turn to the non linear model of labor supply. Table 24 replicates Table 9, with average village employment replacing the individual's binary employment as the dependent variable. Column (1), the basic specification from the linear model, is included for reference. Column (2) is the fully non parametric model, where $labor_{tv} = \alpha + \beta_w 1(wage_{tv} = w) + \nu$. As in Table 9, the indicator for a wage of MK 30 is omitted, so α is the average of village employment for the wage MK 30, and the coefficients β_w represent the increase in employment for wage w relative to MK 30. Average village employment differs significantly from its level at MK 30 starting at MK 100. To confirm the presence of a trend break at MK 100, I first confirm that average village employment is higher when wages are MK 100 or above than when wages are less than MK 100. Column (3) presents results from the regression $labor_{tv} = \alpha + \beta 1(wage_{tv} \geq MK100) + \nu$, indicating that total employment is 12.7 percentage points higher for wages of MK 100 and above than for wages of MK 90 and below. A linear model would also be consistent with significantly higher employment at higher wages, so column (4) tests explicitly for a discontinuity at MK 100. I run the regression $labor_{tv} = \alpha + \beta_1 ln(wage_{tv}) + \beta_2 1(wage_{tv} \geq MK100) + \nu$. The coefficient β_2 is positive but not statistically different from zero. Lack of support for the non linear model persists in column (5), where I allow for a different slope above and below MK 100. Further, I fail to reject the linear restriction imposed by the specification in column (1) against any of the alternative non-linear models. The data do not reject that total employment is linear with respect to wages.

In Tables 25 and 26, I estimate the non linear specifications separately for the average employment for all men in the village and for all women in the village. The results for men mimic those for the pooled sample: total labor supply by men is higher for wages above MK 100 than below, but the linear restriction cannot be rejected in favor of a model with a discontinuity at MK 100. For women, there is evidence of non linear labor supply. In column (4), total employment increases discontinuously by 8.5 percentage points for wages of MK 100 and above. In column (5), neither the coefficient on the indicator for wages of MK 100 or higher nor the coefficient on the interaction term between the indicator for high wages and the linear measure of wages are statistically significant, but they are jointly significant.

A.3 Effect of village-average shocks on aggregate labor supply

Wages and funerals are village-level variables by design and definition, respectively. Shocks to the endowment are individual level phenomena, so I aggregate to the village level. The measure of shocks used in this section is the fraction of individuals in each village meeting the previously described recall-accuracy criteria who experience a negative (or positive) shock to their endowments in the previous week. The percentage of villagers experiencing negative shocks each week ranges from 1.9 percent to 90.7. The percentage experiencing positive shocks ranges from 0 to 40.0. In regressions of the form $labor_{tv} = \alpha + \beta_1 shock_{t-1,v} + \beta_2 ln(wage_{tv}) + \nu$, β_1 represents the percentage point increase in average village employment for a one percentage point increase in the frequency of shocks within the village. Models estimating the effect of shocks include week fixed effects, because while the levels of shocks are plausibly iid, the effects of shocks may persist from one week to the next. Including week fixed effects uses the variation across villages, where shocks are assumed to be uncorrelated.[9]

Table 28 follows Table 16. The first three columns examine negative shocks, and fourth through sixth columns examine positive shocks. Column (1) shows that a one percentage point increase in the frequency of negative shocks leads to a 5.1 percentage point increase in labor supply in the subsequent week. This basic finding is consistent with the results using individual level dataControlling for wages, as in column (2), the effect of the frequency of negative shocks becomes statistically insignificant. Column three includes the main effect of wages and of shocks, and the interaction between the two: $labor_{tv} = \alpha + \beta_1 shock_{t-1,v} + \beta_2 ln(wage_{tv}) + \beta_3(shock_{t-1,v} \times ln(wage_{tv})) + \nu$. A higher frequency of shocks the previous week is associated with significantly higher labor supply, and higher wages cause significantly higher labor supply. However, the effect of wages on labor supply is mitigated by shocks: the coefficient on the interaction term is large, negative, and significant. In individual specifications with a binary measure of shocks for individuals interacted with the continuous log wage variable, the elasticity was computed with and without shocks, $\epsilon_{noshock} = \frac{\beta_2}{mean(labor)}$ and $\epsilon_{shock} = \frac{\beta_2+\beta_3}{mean(labor)}$. Using a continuous measure of the frequency of shocks in a village, we can compute $\epsilon_{noshock}$ as before, and compare it to the elasticity in a week following an average level of shocks in the village, $\epsilon_{averageshock} \equiv \frac{mean(shock)\times\beta_2+\beta_3}{mean(labor)}$. From column (3), we compute the elasticity of labor supply following a week with no shocks to be 0.21, but the elasticity following a week with an average level to be 0.14. Labor supply is both higher and less responsive to wages following an increased frequency of negative shocks. This finding is consistent with results from individual level data and from previous work on wage labor as an ex post coping strategy.

The fourth through sixth columns of table 28 analyze the effect of positive shocks on labor supply. In a specification without wages in column (4), the main effect of positive shocks is to reduce labor supply. Though the magnitude of the coefficient is large, it is imprecisely estimated. The large but imprecise effect of positive shocks persists when controlling for wages in column (5), but the coefficient changes signs when adding an interaction term in column (6). Positive shocks are rare phenomena in this sample. On average, seven percent of people per village experienced a positive shock each week, and almost 20 percent of village-week observations contained no positive shocks. Given this lack of variation, the imprecise estimates are not surprising.

Finally, Table 29 estimates the effect of negative shocks in models allowing a non linear response to wages. This table corresponds to Table 17 for individual level data and finds similar results. The only notable difference is that though the magnitude of the effect of shocks in column (3) using village level data is the same as the magnitude for using individual level data, the larger standard errors with the village level data mean that the effect not statistically significant in the village level regressions.

[9]This project took place during Malawi's dry season. As is usual for the June-August season, there was no precipitation in any of the villages during the project, and temperatures were seasonal.

B Appendix Tables

Table 18: Effect of wages on the probability of working for an outside employer

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Individual*week indicator for working for an outside employer					
Ln(wage)	-0.014	-0.014	-0.017**	-0.017**	-0.015	-0.017**
	(0.009)	(0.009)	(0.006)	(0.006)	(0.009)	(0.006)
Village effects		x		x		
Week effects			x	x		x
Individual effects					x	x
Observations	6157	6157	6157	6157	6157	6157
Mean of dependent variable	0.05	0.05	0.05	0.05	0.05	0.05

OLS estimates. Standard errors are clustered at the village level.
Unit of observation is individual*week, sample is all individuals.
* p<0.10, ** p<0.05, *** p<0.001

Table 19: Effect of wages on the elasticity of working for the project or an outside employer

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Individual*day indicator for working					
Ln(wage)	0.110**	0.110**	0.123**	0.123**	0.110**	0.123**
	(0.031)	(0.031)	(0.027)	(0.027)	(0.033)	(0.029)
Village effects		x		x		
Week effects			x	x		x
Individual effects					x	x
Observations	6333	6333	6333	6333	6333	6333
Mean of dependent variable	0.86	0.86	0.86	0.86	0.86	0.86
Elasticity	0.13	0.13	0.14	0.14	0.13	0.14

OLS estimates. Standard errors are clustered at the village level.
Unit of observation is individual*week, sample is all individuals.
* p<0.10, ** p<0.05, *** p<0.001

Table 20: Effect of future wages on the elasticity of working

	(1)	(2)	(3)	(4)	(5)	(6)
		Weeks 1 to 11			Weeks 1 to 8	
Dependent variable:			Individual*day indicator for working			
Ln(wage)	0.131**	0.125**	0.142**	0.149**	0.120**	0.133**
	(0.040)	(0.038)	(0.032)	(0.050)	(0.046)	(0.050)
Ln($wage_{t+1}$)		-0.018	-0.010		0.027	0.029
		(0.044)	(0.034)		(0.079)	(0.063)
Ln($wage_{t+2}$)					0.016	0.013
					(0.051)	(0.023)
Ln($wage_{t+3}$)					-0.047	-0.028
					(0.038)	(0.039)
Ln($wage_{t+4}$)					0.029	0.039
					(0.039)	(0.035)
Village effects	x	x	x	x	x	x
Week effects	x	x	x	x	x	x
Observations	5805	5804	5804	4221	4217	4217
Mean of dependent variable	0.84	0.84	0.84	0.81	0.81	0.81
Elasticity	0.16	0.15	0.17	0.18	0.15	0.16

OLS estimates. Standard errors are clustered at the village level.
Unit of observation is individual*week, sample is all individuals.
* p<0.10, ** p<0.05, *** p<0.001

Table 21: Elasticity of aggregate labor supply w.r.t. wages

	(1)	(2)	(3)
Dependent variable:	Village*week average employment		
	(1)	(2)	(3)
Ln(wage)	0.124***	0.124***	0.135***
	(0.031)	(0.030)	(0.029)
Village effects		x	
Week effects			x
Observations	120	120	120
Mean of dependent variable	0.85	0.85	0.85
Elasticity	0.15	0.15	0.16

OLS estimates. Robust standard errors.
Unit of observation is village*week.
* p<0.10, ** p<0.05, *** p<0.001

Table 22: Elasticity of men's aggregate labor supply w.r.t. wages

	(1)	(2)	(3)
Dependent variable:	Village*week average employment		
	(1)	(2)	(3)
Ln(wage)	0.133***	0.133***	0.143***
	(0.033)	(0.029)	(0.030)
Village effects		x	
Week effects			x
Observations	120	120	120
Mean of dependent variable	0.82	0.82	0.82
Elasticity	0.16	0.16	0.17

OLS estimates. Robust standard errors.
Unit of observation is village*week. Sample is all men.
* p<0.10, ** p<0.05, *** p<0.001

Table 23: Elasticity of women's aggregate labor supply w.r.t. wages

	(1)	(2)	(3)
Dependent variable:	Village*week average employment		
	(1)	(2)	(3)
Ln(wage)	0.118***	0.118***	0.129***
	(0.032)	(0.032)	(0.029)
Village effects		x	
Week effects			x
Observations	120	120	120
Mean of dependent variable	0.86	0.86	0.86
Elasticity	0.14	0.14	0.15

OLS estimates. Robust standard errors.
Unit of observation is village*week. Sample is all women.
* p<0.10, ** p<0.05, *** p<0.001

Table 24: Non linear effects of wages on aggregate labor supply

	(1)	(2)	(3)	(4)	(5)
Dependent variable:		Village*week average employment			
Ln(wage)	0.135***			0.077	0.074
	(0.029)			(0.052)	(0.055)
Wage = MK 40		-0.003			
		(0.071)			
Wage = MK 50		0.088			
		(0.071)			
Wage = MK 60		0.064			
		(0.068)			
Wage = MK 70		0.069			
		(0.070)			
Wage = MK 80		0.066			
		(0.079)			
Wage = MK 90		0.077			
		(0.086)			
Wage = MK 100		0.170**			
		(0.065)			
Wage = MK 110		0.173**			
		(0.077)			
Wage = MK 120		0.130			
		(0.097)			
Wage = MK 130		0.217**			
		(0.066)			
Wage = MK 140		0.200**			
		(0.066)			
Wage MK 100 or more			0.127***	0.070	-0.137
			(0.026)	(0.048)	(0.521)
Wage MK 100 or above*Ln(wage)					0.044
					(0.112)
Week effects	x	x	x	x	x
Observations	120	120	120	120	120
Mean of dep. var.	0.85	0.85	0.85	0.85	0.85
P-value against Col. (1)		1.00		0.34	0.63
P-value against Col. (4)		1.00			0.88
P-value against Col. (5)		1.00			
P-value for $\beta_{above100} + ln(100)\beta_{interaction} = 0$					0.15

OLS estimates. Robust standard errors.

Unit of observation is village*week, sample is all individuals.

* p<0.10, ** p<0.05, *** p<0.001

Table 25: Non linear effects of wages on men's aggregate labor supply

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Village*week average employment				
Ln(wage)	0.143***			0.103*	0.106*
	(0.030)			(0.056)	(0.059)
Wage = MK 40		-0.007			
		(0.082)			
Wage = MK 50		0.088			
		(0.076)			
Wage = MK 60		0.082			
		(0.075)			
Wage = MK 70		0.071			
		(0.075)			
Wage = MK 80		0.086			
		(0.083)			
Wage = MK 90		0.120			
		(0.090)			
Wage = MK 100		0.180**			
		(0.070)			
Wage = MK 110		0.201**			
		(0.078)			
Wage = MK 120		0.139			
		(0.096)			
Wage = MK 130		0.219**			
		(0.067)			
Wage = MK 140		0.200**			
		(0.067)			
Wage MK 100 or more			0.125***	0.048	0.234
			(0.028)	(0.051)	(0.573)
Wage MK 100 or above*Ln(wage)					-0.039
					(0.122)
Week effects	x	x	x	x	x
Observations	120	120	120	120	120
Mean of dep. var.	0.82	0.82	0.82	0.82	0.82
P-value against Col. (1)		1.00		0.54	0.83
P-value against Col. (4)		1.00			0.90
P-value against Col. (5)		1.00			
P-value for $\beta_{above100} + ln(100)\beta_{interaction} = 0$					0.39

OLS estimates. Robust standard errors.
Unit of observation is village*week, sample is all men.
* p<0.10, ** p<0.05, *** p<0.001

Table 26: Non linear effects of wages on women's aggregate labor supply

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Village*week average employment				
Ln(wage)	0.129***			0.058	0.050
	(0.029)			(0.053)	(0.056)
Wage = MK 40		-0.004			
		(0.069)			
Wage = MK 50		0.082			
		(0.074)			
Wage = MK 60		0.056			
		(0.068)			
Wage = MK 70		0.068			
		(0.072)			
Wage = MK 80		0.050			
		(0.080)			
Wage = MK 90		0.042			
		(0.088)			
Wage = MK 100		0.161**			
		(0.066)			
Wage = MK 110		0.155*			
		(0.079)			
Wage = MK 120		0.121			
		(0.100)			
Wage = MK 130		0.214**			
		(0.070)			
Wage = MK 140		0.199**			
		(0.068)			
Wage MK 100 or more			0.128***	0.085*	-0.404
			(0.027)	(0.049)	(0.538)
Wage MK 100 or above*Ln(wage)					0.104
					(0.116)
Week effects	x	x	x	x	x
Observations	120	120	120	120	120
Mean of dep. var.	0.86	0.86	0.86	0.86	0.86
P-value against Col. (1)		0.99		0.25	0.50
P-value against Col. (4)		1.00			0.74
P-value against Col. (5)		1.00			
P-value for $\beta_{above100} + ln(100)\beta_{interaction} = 0$					0.08

OLS estimates. Robust standard errors.
Unit of observation is village*week, sample is all women.
* p<0.10, ** p<0.05, *** p<0.001

Table 27: Elasticity of funerals on elasticity of aggregate labor supply w.r.t. wages

	(1)	(2)
Dependent variable:	Village*week average employment	
Ln(wage)	0.119***	0.118***
	(0.027)	(0.027)
Funeral	-0.213**	-0.360
	(0.071)	(0.824)
Funeral*Ln(wage)		0.035
		(0.210)
Week effects	x	x
Observations	120	120
Mean of dependent variable	0.85	0.85
Elasticity	0.14	
Elasticity (no funeral)		0.14
Elasticity (funeral)		0.18

OLS estimates. Robust standard errors.
Unit of observation is village*week.
* p<0.10, ** p<0.05, *** p<0.001

Table 28: Effect of shocks on elasticity of aggregate labor supply w.r.t. wages

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Village*week average employment					
Type of shock:	Negative	Negative	Negative	Positive	Positive	Positive
Shock_{t-1}	0.051	0.040	1.052*	-0.275	-0.289	2.507
	(0.099)	(0.096)	(0.554)	(0.323)	(0.340)	(2.168)
Ln(wage)		0.117***	0.185***		0.118***	0.171***
		(0.029)	(0.047)		(0.029)	(0.043)
Shock_{t-1}*Ln(wage)			-0.233*			-0.644
			(0.128)			(0.542)
Week effects	x	x	x	x	x	x
Observations	110	110	110	110	110	110
Mean of dependent variable	0.88	0.88	0.88	0.88	0.88	0.88
Elasticity		0.13			0.13	
Elasticity (no shock)			0.21			0.19
Elasticity (shock)			0.14			0.14

OLS estimates. Robust standard errors.
Unit of observation is village*week, sample is all individual who report wage and work accurately.
* p<0.10, ** p<0.05, *** p<0.001

Table 29: Effect of negative shocks in a non linear model

	(1)	(2)	(3)	(4)
Dependent variable:		Village*week average employment		
Wage MK 100 or more	0.100***	-0.270	0.105***	0.151***
	(0.026)	(0.480)	(0.027)	(0.043)
Below 100*Ln(wage)		0.055		
		(0.054)		
Wage MK 100 or above*Ln(wage)		0.124		
		(0.092)		
Shock_{t-1}			0.034	0.111
			(0.091)	(0.117)
Shock_{t-1}*MK 100 or above				-0.166
				(0.150)
Week effects	x	x	x	x
Observations	120	120	110	110
Mean of dep. var.	0.88	0.88	0.88	0.88

OLS estimates. Robust standard errors.
Unit of observation is village*week, sample is all individual who report wage and work accurately.
* p<0.10, ** p<0.05, *** p<0.001

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