Old-Age Pension and Labor Market Outcomes of Younger Individuals: Evidence from Uganda

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Abstract

In most low-income countries, younger individuals play a big role in providing financial support and care needs to the elderly. Do programs that alleviate these responsibilities affect younger individuals' labor market outcomes? I address this question by studying the effects of old-age pension on earnings and labor supply of ineligible adults in the household, in Uganda's context. My results show that one additional year of pension exposure increases monthly sales and profits of self-employed ineligible individuals by 10.2% and 9.5% respectively. No significant effect is found on earnings of individuals engaged as employees. I also find an additional year of pension exposure to increase weekly hours worked by 1.3%, with the effect only significant among self-employed individuals. Women rather than men experience significant effects on earnings and labor supply. The results are largely driven by an increase in working capital. I show that the effects of the pension on earnings are bolstered by the availability of micro credit and informal networks, suggesting complementarities between cash transfers and the local financial and social landscape. Lastly, I show that the gender of the pension recipient matters. In this case, pension received by men rather than women, has a significant effect on the labor market outcomes of younger individuals. My results suggest that old-age pension relaxes the resource constraint of younger individuals.

JEL: H55, J14, J22

Key Words: Old-Age Pension, Earnings, Labor Supply

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1 Introduction

In most low-income countries, the elderly rely on younger individuals for financial support and other care needs.[1] This is particularly the case due to limited coverage of formal retirement schemes, as the majority of the people are engaged in the informal sector during their working life. With the projected rise in population aging, old-age dependence is likely to rise, which may ultimately undermine economic prosperity at the individual level and economic growth in general (Harasty & Ostermeier 2020). In response to this situation, many developing countries have adopted or expanded non-contributory social protection programs to provide income support to the vulnerable elderly who are not covered by formal retirement schemes (Huang & Zhang 2021). Prior research has shown that such programs have spillover effects on children's health outcomes (Duflo 2003). However, it remains unclear whether and how such programs affect adult ineligible individuals. Given the social linkage (mostly through the social support system) between the elderly and the younger individuals, such programs are likely to have spillover effects on the latter, especially in low-income countries. These programs relax the resource constraint of the household, which is likely to have an effect on different margins of the labor market outcomes of other household members. This paper therefore addresses the question: How does old-age pension affect labor market outcomes of ineligible adults in the household? Understanding these spillover effects is important for the proper evaluation of the welfare implications and cost-benefit analysis of such programs.

Old-age pension may affect labor market outcomes of younger individuals through several channels. First, the pension grants reduce financial dependence of the elderly on younger individuals. For instance, Fan (2010) shows that old-age pension reduces the probability that the recipients' children make transfers to their parents. Whereas the paper interprets this result as crowding out of private transfers received by the elderly, it is indicative of a reduction in financial dependence of the elderly on younger individuals. Secondly, the pension grants support consumption expenditures of the household. There is evidence that the elderly invest part of the pension in health and nutrition (Duflo 2003), and education (Moscona & Seck 2024) of the children. A combination of these two channels implies that old-age pension can relieve financial resources for younger individuals, which, if put to productive use, can enhance earnings and labor supply of these individuals. However, reduced financial pressure on younger individuals could also lead them to reduce their labor supply, which makes the effect ambiguous a priori. Third, the younger individuals, especially the self-employed category, could provide an avenue for investing the pension grants. Lastly, previous studies

[1] In the paper, I use younger individuals to refer to pension ineligible adults (aged 18 to 64)

have shown that such pension programs induce direct beneficiaries to reduce labor supply or exit the labor market (Fetter & Lockwood 2018, Unnikrishnan & Imai 2020). To the extent that the elderly reallocate this time to some housework and care activities, this could relieve time for the younger individuals, especially women, to work more. Ardington et al. (2009) provide suggestive evidence that old-age pension reduces childcare constraints on prime-aged individuals, allowing them to migrate for work.

To address the question of interest, I leverage an unusual policy experiment in Uganda which piloted the old-age pension program in 15 districts in 2011—the Senior Citizen Grant (SCG). In the pilot districts, the program provides a monthly grant of \$9.8 (based on 2012 exchange rate) to all individuals aged 65 years and above (60 years and above for Karamoja sub-region). The amount of the monthly transfer was estimated to be equivalent to 20% of the average monthly per-capita consumption at the baseline.

I estimate the causal effect of this pension program on younger individuals' labor market outcomes using difference-in-difference design. Following Moscona & Seck (2024), I begin by constructing a measure of potential pension exposure based on the number of household members who are eligible for the pension at any given time. Cumulatively, this measure captures the number of years of pension grants the household would have received if it were located in the pilot districts. I use this measure as the pension instrument. By construction, this measure varies across households depending on the age composition of the members. I combine this variation with geographical variation induced by the location of the household (pilot vs non-pilot districts). To estimate the regressions, I use data from the Uganda National Household Survey (UNHS) and the Demographic and Health Survey (DHS) for Uganda.

The results indicate that exposure to old-age pension has a positive effect on earnings and labor supply of ineligible adults. One additional year of pension exposure increases monthly sales and profits of self-employed individuals by 10.2% and 9.5%, respectively. Based on the mean in non-pilot districts, the effect on sales is equivalent to \$11.7, while the effect on profits is equivalent to \$4.8. The effects are more salient among women—18.0% increase in sales and 17.4% increase in profits. I also explore whether there are effects on the earnings of individuals in the category of employees. For this group, I find no significant effects of the pension exposure on their wage income. My results also indicate that exposure to the pension has a positive effect on labor supply. One additional year of pension exposure increases weekly hours worked by 1.3% in the full sample, and 1.4% among women. Consistent with earnings, the effects on labor supply are only significant among self-employed individuals. Although not the focus of my paper, I also show that the pension leads to a decline in labor supply of the elderly (pension recipients), which is consistent with previous studies. The effects on earnings and labor supply of ineligible individuals are economically meaningful given that one year of pension is approximately equal to \$118 (based on monthly transfers of \$9.8). I show that these effects are largely driven by an increase in the working capital for self-employed individuals, which increases by 6.4%, equivalent to a \$2.3 increase in working capital, suggesting that old-age pension either relieves, or provides financial resources which are invested by younger individuals. Broadly, the positive effects on ineligible individuals suggest that, in aggregate, the effects of the decline in labor supply of the elderly can be partly offset by an increase in earnings and labor supply of the younger individuals. I show that the impact of pension exposure is bolstered by the availability of micro credit and stronger informal networks, suggesting complementarities between cash transfers and micro credit as well as informal financial groups rather than being substitutes. The results are stronger in the urban areas suggesting that the effect of the pension on earnings is strengthened by other business opportunities that are associated with urban areas.

I further explore if the gender of the recipient matters for the effects of the pension on earnings and labor supply of younger adults. To do this, I construct two measures of potential pension exposure for each household based on the gender of the recipient. Applying the same research design, I find that exposure based on male recipients has a significant effect on earnings and labor supply of younger individuals, compared to female recipients. Following earlier literature (such as Duflo (2003) and Unnikrishnan & Imai (2020)), I also test if the gender of the pension recipient matters for children's outcomes. I find evidence in support of earlier literature that pension exposure based on female recipients rather than male recipients has a significant impact on children's education and health. To the extent that the effects on earnings and labor supply are driven by investment of the pension grants by younger individuals, the differences in the influence of the gender of the recipient on labor market outcomes of adults and children's outcomes reflect gender differences in preferences, with women preferring to invest in children's human capital while men preferring to invest in income generation.

I show that all my results are robust to the exclusion of Kampala (the capital city) from the sample, suggesting that my results are not driven by the features in the capital city. I also show that the results are not affected by controlling for remittances, suggesting that the changes in remittances are not driving results. Furthermore, I conduct placebo experiments where I construct a placebo potential pension exposure based on individuals that are just below the cutoff age. Since this group does not qualify for the pension, I do not expect to see similar effects as the true pension exposure. Indeed, the results show that this placebo measure does not produce significant effects on both earnings and labor supply. I also construct event study plots to examine if individuals in pilot districts are experiencing differential trends. To achieve this, I conduct a cohort-based analysis by grouping households in cohorts based on how far away from the pension they are (for those yet to reach the pension eligibility) and the number of years the household has had the pension (for those that are eligible). The coefficients for cohorts that have not yet reached the pension are all indistinguishable from zero, indicating that pre-trends are parallel. For cohorts that have reached the pension, the coefficients are positive and significant. Further, I leverage multiple waves of the Uganda National Panel Surveys to test for pre-trends in labor supply outcomes. For this analysis, I implement the staggered difference-in-difference design following recent advances by De Chaisemartin & d'Haultfoeuille (2020). The results from this exercise show that the effects of the pension on labor supply are not driven by pre-existing trends. Lastly, I show that my results are not driven by endogenous household formation.

I argue that the effects of the old-age pension on earnings and labor supply of younger individuals are likely to be different compared to direct cash transfers to this group of individuals due to the channels of influence. To the extent that the effects are driven by investment of the pension grants by younger individuals, this creates an additional social accountability layer since the elderly will indirectly have a stake in the business, which would not be the case with direct cash transfers to younger individuals. If the results are driven by expenditure substitution where the pension grants are spent on household consumption, and reduction in financial dependence of the elderly, the younger individuals will find themselves with more resources without necessarily receiving any direct cash transfers.

This paper is related to literature on the impacts of pension on household income (Huang & Zhang 2021), and labor supply of direct beneficiaries (Fetter & Lockwood 2018, Unnikrishnan & Imai 2020). I contribute to this literature by studying how earnings and labor supply of ineligible adults are affected by old-age pension. Causally identifying the spillover effects of old-age pension has been challenging due to the design and implementation of these programs. Most of these programs are universal in nature, leaving eligibility criteria as the only source of variation. In this paper, I take advantage of an unusual policy experiment that introduced old-age pension in staggered roll-out in Uganda, which generates two sources of variation: eligibility and geographical. This allows me to cleanly identify the causal impact of the pension program on ineligible individuals in the household. In addition, due to data limitations, previous studies have not been able to identify the effects on earnings of ineligible individuals. While Huang & Zhang (2021) show that the pension program for seniors in China led to an increase in household income, their paper does not highlight whether the increase in income is solely from the direct transfers or also from other mechanisms through spillovers on activities of younger individuals. My paper therefore differs from Huang & Zhang (2021) by highlighting the sources of the increase in the household incomes. To achieve this, I take advantage of an unusual module in the UNHS 2016/17, which collected detailed information on earnings of household members, including sales and profits for the self-employed individuals, to show that old-age pension has a multiplier effect by increasing earnings of self-employed ineligible individuals. Regarding the spillover effects on labor supply, my paper is close to Ardington et al. (2009) who showed that large cash transfer to the elderly in South Africa led to increased employment rate among prime-aged adults in South Africa, with the effects operating through labor migration. Their paper, however, focuses on the extensive margin of labor supply, while here I examine the effects on both intensive and extensive margins, and show that labor supply responds more on the intensive margin. Unlike Ardington et al. (2009), I focus on individuals residing in the same household with the pension beneficiary.

This paper also relates to literature on the impact of pension programs on the welfare of direct beneficiaries and the household in general. Previous studies have shown that these programs have positive effects on health and well-being of the beneficiaries (Huang & Zhang 2021, Miglino et al. 2023, Alzua et al. 2023), household consumption (Fan 2010, Unnikrishnan & Imai 2020), non-food expenditures, and asset accumulation (Unnikrishnan & Imai 2020), children's health and nutrition (Duflo 2003). In this paper, I provide suggestive evidence that old-age pension programs also improve the welfare of adult ineligible individuals by improving their earnings. Relatedly, I contribute to the debate on whether the gender of the recipient matters for the impact of pension grants and other cash grants. Previous studies have shown that pension grants received by women have a higher impact on children's health (Duflo 2003) and consumption expenditures (Unnikrishnan & Imai 2020), compared to pension received by men. I provide novel evidence that the influence of the gender of the recipient on the effects of the pension depends on the demographic group in consideration. I show that, whereas pension received by women has higher effects on children's outcomes, it's pension received by men that has a significant impact on earnings and labor supply of ineligible individuals.

Lastly, this paper is also related to literature on the impacts of Unconditional Cash Transfers (UCT). Using experimental or quasi-experimental methods, studies have documented positive effects of UCT on children's outcomes (education and health) (Akee et al. 2010, Baird et al. 2014, Shah & Gennetian 2023), economic outcomes (such as consumption) and psychological well-being of poor households (Haushofer & Shapiro 2016), health-seeking behaviors and morbidity (Novignon et al. 2022), and negative effects on labor market participation (Verlaat et al. 2023). While UCTs tend to be short-term or one-off in nature, this paper studies a form of Unconditional Cash Transfer that leads to a permanent increase in incomes of the elderly. The pension program I study provides income to the elderly on a monthly basis, indefinitely.

The rest of the paper is organized as follows. Section 2 describes the Senior Citizen Grant. Section 3 describes the data sources used in the study, and presents the description of the sample. Section 4 describes the identification strategy for the paper. Section 5 presents the main empirical results and their discussion. Section 6 presents the conclusion.

2 The Senior Citizen Grant in Uganda

The Senior Citizen Grant (SCG) is an unconditional cash transfer program in Uganda which targets individuals aged 65 and above (60 and above for Karamoja sub-region).[2] The program was adopted in 2011 with the main goal of reducing old-age poverty by providing a minimum level of income security to the beneficiaries. It is implemented as part of the broader Expanding Social Protection (ESP) program designed in 2009 with the aim of "reducing chronic poverty and improving life chances for poor men, women and children by embedding a national social protection system within Uganda's national planning and budgeting" (Bukuluki & Watson 2012). The SCG provides a monthly cash transfer of about \$9.8 (at the baseline) to the beneficiaries, which is about 20% of average monthly per-capita consumption at the baseline.

Beneficiary selection is based on a registration process by community-based local government structures, involving Local Council I (LC I) leaders. Where possible, the information from local government structures is verified using national civil registry (using national identification data). The program excludes older persons with government-funded pensions as part of their formal retirement package.

The implementation of the program followed a phased approach. Rather than introduce the program in all districts, the government piloted the program in 15 districts for five years—henceforth, Phase I.[3] The pilot phase was launched in March 2011 with the first payout in September 2011. After the completion of the pilot phase in 2015, the government decided to expand the program to an additional 40 districts over the next 5 years, starting with 20 districts in the financial year 2015/16, thereafter to 5 districts every year, until 2019/20—henceforth, Phase II. By the financial year 2016/17, SCG had reached 47 districts—7 of these were new districts carved out of the original pilot districts. In 2017, the Parliament of Uganda passed a resolution to roll out the grants to all districts in the country but this has not yet been fully actualized due to financial constraints.[4] Figure 1 shows the staggered

[2] The age of eligibility is lower in Karamoja because of the region is more economically deprived

[3] It's not clear what criteria was followed in selecting the pilot districts

[4] Information about the roll out was obtained from the ESP website. https://socialprotection.go.ug. Accessed February 2024

roll-out of the program.

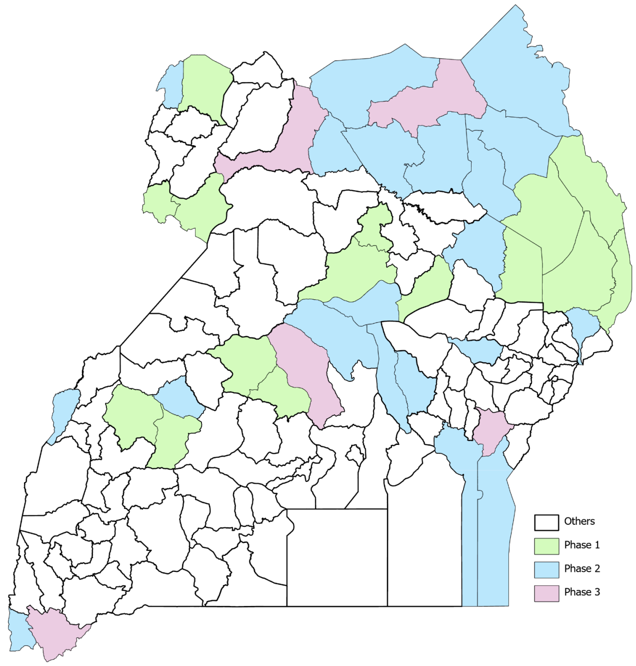


Figure 1: Map of Uganda showing the roll-out of the Senior Citizen Grant

Note: Figure 1 shows the staggered roll-out of the Senior Citizen Grant in different districts in Uganda. The unshaded districts are the not-yet treated districts—where the program has not yet reached as of today. The different colors indicate the different phases of the program. Note that here, Phase 2 and 3 are broadly categorized as Phase II in the rest of the paper since phase 3 happens just one year after phase 2 and both were under the additional 40 districts earlier mentioned.

Phase II came with some administrative and design changes compared to Phase I. First, in Phase I, monthly transfers were sent directly to beneficiaries' mobile money accounts through MTN Mobile Money, and converted into cash by MTN agents at designated pay points. In Phase II, the mode of payment was changed from mobile money to Post Bank Uganda. Under this new arrangement, the bank organizes payout days and delivers the money at designated pay points. Secondly, in Phase II, the program enrolls only 100 oldest persons in each sub-county in the selected districts, while in Phase I all individuals who are aged 65 years and above (or 60 and above in Karamoja) were enrolled in the program and this criteria continues.

The above changes in the design in Phase II make it hard to cleanly define treatment status for households in the selected districts since not all age-eligible individuals in the household were included in the program. One way to circumvent this would be to use administrative data of beneficiaries to define treatment status and then link it to household survey data. This is not possible because I do not have overlapping identifiers in administrative data and household data. For Phase I, it is possible to define treatment status for households in the pilot districts since all age-eligible individuals were included in the program. In this case, the treatment depends on whether the household has age-eligible individuals and the number of such individuals in the household. For this reason, my identification is based on Phase I (pilot districts) and not-yet treated districts. Since my main data set was collected in 2016/17, some Phase II districts had received treatment for at least 1 year. For this reason, I exclude the Phase II districts from the control group in the main analysis. I, however, show that my results are still robust if I use all districts, but slight lower in magnitude, as expected since districts treated in Phase II would bias the effects downwards.

3 Data and Descriptive Statistics

The main dataset for the paper is the Uganda National Household Survey (UNHS) collected in 2016/17. This is a cross-sectional wave which is the sixth in the series of the national household surveys conducted by Uganda Bureau of Statistics (UBOS). The survey is geographically representative and covers all the districts in Uganda. To account for seasonality, the data collection is spread over a 12 months period. I account for this in my analysis by including month of interview fixed effects. The survey administered three key modules; the socioeconomic, the labor force, and the community modules. This paper uses information from the first two modules. The socioeconomic module collects detailed information on household and individual demographic characteristics, including the age of all members. I use information on age composition from this module to define potential exposure to the pension program as discussed in section 4. The labor force module collects detailed information about employment status, job characteristics, hours worked, different measures of unemployment, and household chores (care activities). Unlike the earlier waves and other household surveys, the 2016/17 wave separately classified incomes for self-employed individuals, by collecting information on the sales, operating expenses, and profits. This unusual feature of the data is very important for my paper.[5] It allows me to explore how the earnings of self-employed individuals are affected by the old-age pension.

The other dataset that I use in this study is the Uganda National Panel Surveys (UNPS) 2009/10-2015/16. The UNPS is also a nationally representative household survey which is implemented under the World Bank's Living Standards Measurement Study (LSMS). It's designed to provide statistical information for monitoring changes and transitions in poverty dynamics, trends and related welfare indicators. The survey is carried out over a twelve-month period on a selected sample of households. I leverage the panel structure of this

[5]In this paper I define self-employed to include employers and own-account workers

data to test for pre-trends in labor supply and examine alternative mechanisms which might evolve as a result of the pension program, such as endogenous formation of households. This also serves as robustness check for my results from the UNHS data. I do not use the UNPS for my main results albeit its advantages of the panel structure, for the following reasons. First, the panel is limited in terms of sample size which is compounded by the sample refresh in 2013 where one-third of the sample was replaced. This sample size limitation makes it infeasible to disaggregate the analysis by gender and status in employment. Secondly, the panel does not collect detailed information on the earnings of individuals in self-employment, which is also a major aspect of my paper. For these two reasons, I only use the panel for robustness checks and take the results from UNHS as my main results. Lastly, I use the Demographic and Health Surveys (DHS) for Uganda, collected in 2016 to study outcomes on children's education and health.

Table 1 shows the characteristics of the sample (aged 18 to 64 years). In this table, I summarize the mean values for potential pension exposure, individual characteristics, and household characteristics for pilot and non-pilot districts.[6] The average potential pension exposure is 1.4 years. Notably, the potential pension exposure measure is fairly balanced across pilot and non-pilot districts, suggesting absence of migration of elderly individuals from non-pilot to pilot districts. Figure 2 presents the distribution of the potential pension exposure for pilot and non-pilot districts. It shows that the distribution is similar for pilot and non-pilot, both including individuals with zero potential exposure (Figure 2a), and excluding them (Figure 2a). Figure A.2 in the Appendix also shows that the aggregate potential exposure across the districts is as good as randomly assigned. This suggests that my instrument for pension exposure can be taken to be exogenous. In Table 1, I also show the proportion of individuals potentially exposed to at least one pension year. The table shows that 17% of the individuals in my analysis sample are potentially exposed to at least one pension year. This proportion is slightly higher in pilot districts (20%).

The individual level characteristics show that, on average, individuals work for 36 hours, with pilot districts working about 9.5 hours less compared to non-pilot. Similarly, pilot districts have a lower percentage of individuals engaged in paid employment. While 39% of the individuals in the full sample are engaged in paid employment, only 36% report being engaged in paid employment in pilot districts, and 41% report so in non-pilot districts. The table also shows that individuals in the pilot districts are more likely to be engaged in farm work and skilled agriculture, and less likely to be engaged in elementary occupations, and service and sales work, compared to those in non-pilot. Individuals in pilot districts are less

[6] For the purpose of the discussion I will refer to non-pilot as being non-pilot districts excluding Phase II districts i.e column 3 in Table 1.

Table 1: Descriptive statistics: UNHS 2016/17

	(1) All	(2) Pilot	(3) Non-Pilot (Excl.Phase II)	(4) Non-Pilot (Excl. Kla)	(5) Diff: (2)-(3)
Potential Exposure					
Potential Pension Exposure	1.37	1.46	1.32	1.33	0.14 (0.073)
$\mathbb{I}\{\text{Potential Pension Exposure} > 0\}$	0.17	0.20	0.17	0.17	0.03 (0.007)
Individual characteristics					
Hours worked (weekly)	36.14	28.50	38.08	36.96	-9.58 (0.435)
Paid work	0.39	0.36	0.41	0.39	-0.05 (0.009)
Farm work	0.48	0.59	0.45	0.48	0.14 (0.010)
Skilled agriculture	0.53	0.70	0.48	0.51	0.22 (0.010)
Elementary occupations	0.17	0.11	0.18	0.18	-0.07 (0.008)
Service and sales workers	0.16	0.10	0.18	0.17	-0.08 (0.008)
Employee in main job	0.23	0.15	0.26	0.24	-0.11 (0.009)
Self-Employed in main job	0.47	0.54	0.45	0.45	0.09 (0.010)
Monthly wage (USD)	93.73	78.20	95.93	88.74	-17.73 (6.549)
Sales (USD)	91.44	49.97	107.61	99.27	-57.64 (4.458)
Profits (USD)	42.07	23.08	48.51	45.65	-25.43 (1.850)
Female	0.54	0.54	0.54	0.54	0.00 (0.010)
Age	33.26	33.23	33.28	33.48	-0.05 (0.231)
Years of schooling	7.45	6.28	7.74	7.49	-1.47 (0.084)
Household characteristics					
Household size	4.52	4.73	4.38	4.46	0.36 (0.066)
Urban residence	0.33	0.18	0.38	0.32	-0.20 (0.012)
Age of the head	42.60	42.93	42.56	43.03	0.37 (0.412)
Years of schooling of the head	6.32	4.81	6.75	6.50	-1.94 (0.113)
Poor	0.22	0.29	0.19	0.20	0.11 (0.010)
Male headed	0.69	0.67	0.69	0.69	-0.02 (0.012)

Notes: This table presents the averages of the samples. The sales and profits are self-reported by self-employed individuals (employers and own-account workers). All monetary values are converted into US dollars using end period exchange rate of 2016/17: \$1 =UGX 3590.90. All the employment related indicators are only based on the main job. All individual level characteristics and statistics for potential pension exposure are computed on a sample of individuals aged 18 to 64. Column 5 presents the difference in the means for pilot and non-pilot (excl. Phase II). The standard errors are presented in parenthesis.

likely to be employees but more likely to be self-employed. Regarding earnings, the average monthly wage in the sample is estimated at \$94, with pilot districts having a lower average (\$78) compared to non-pilot (\$96). Earnings in self-employment (sales and profits) follow a similar pattern as wages. While the differences between pilot and non-pilot are apparent, the gap narrows when I exclude Kampala from the sample, suggesting the differences are partly inflated by the capital city. Accordingly, I conduct a robustness test where I exclude Kampala from the sample.

(a) Full sample (b) Non-zero exposure

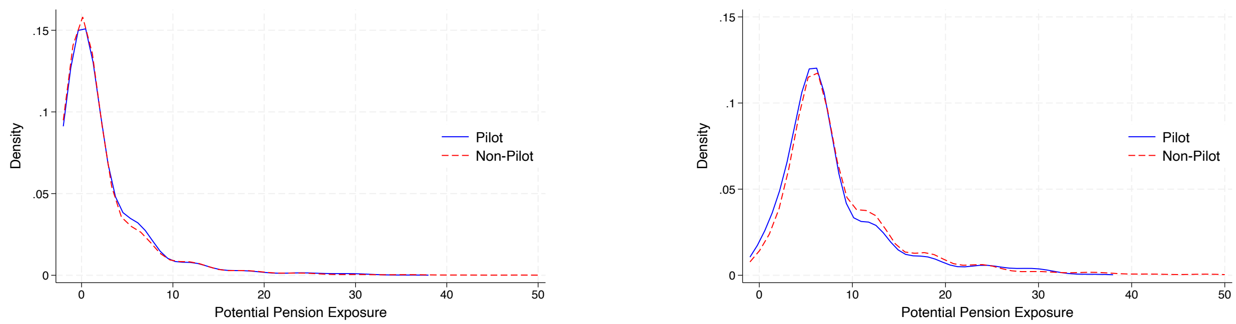


Figure 2: Distribution of potential exposure across pilot and non-pilot districts.

Notes: The figure presents the distribution of potential pension exposure for pilot and non-pilot (excl. Phase II) districts. The measure is computed using the formula in equation 1. Figure 2a presents the distribution while including those in the full sample. Figure 2b presents the distribution while excluding those with zero potential exposures.

On individual demographic characteristics, Table 1 shows that the sample is gender balanced across pilot and non-pilot. There is also no significant difference in the average age of individuals across pilot and non-pilot districts, but the pilot districts have a lower mean for years of schooling compared to non-pilot districts. Household characteristics show differences in residence, education of household head, and poverty status. Households in the pilot districts are less likely to be located in urban areas, their household heads have lower levels of education on average, and are more likely to be poor. There is, however, no difference in household size, age, and sex of the household head. Again, some of the differences are accounted for by the capital city.

The point of Table 1 is not to claim balance between pilot and non-pilot districts but rather to get a sense of the sample characteristics and distribution of potential pension exposure across pilot and non-pilot districts. Indeed, some of the differences in Table 1 could be attributed to exposure to the pension. To get a better sense of how pilot and non-pilot districts are comparable, I conduct a test of the differences in means at the baseline (in 2011) using the UNPS data. The results in Table 2 show that the potential exposure at the baseline is slightly higher in pilot districts. Note that the magnitudes here are smaller compared to Table 1 because this is potential exposure only in 2011 rather than cumulative exposure. The table also shows that pilot districts had lower labor supply, were more likely to be self-employed in the secondary job, earned lower wages, and had lower average years of schooling. While these baseline differences are apparent, when I cluster the standard errors at district level (Table A.1 in Appendix), only differences in wages are significant. This suggests that the standard errors are highly correlated within districts.

Table 2: Differences in means for pilot and non-pilot districts in 2011

	Mean in Non-Pilot	Diff. (Pilot-Non-Pilot)	N
Potential Pension Exposure	0.223	0.051***	11810
	(0.005)	(0.014)	
Hours worked (weekly)	24.966	-3.800***	6582
	(0.263)	(0.731)	
Self-employed in main job	0.114	-0.014	7406
	(0.004)	(0.011)	
Self-employed in secondary job	0.349	0.082*	965
	(0.017)	(0.039)	
Paid work	0.140	-0.009	11809
	(0.003)	(0.010)	
Monthly wage (USD)	89.104	-27.125*	606
	(3.231)	(12.128)	
Female	0.514	0.003	11810
	(0.005)	(0.014)	
Age	24.291	0.315	11778
	(0.177)	(0.501)	
Years of schooling	5.060	-0.805***	10252
	(0.041)	(0.123)	

Notes: This table presents mean differences between pilot and non-pilot (excl. phase II) districts. Each row is a regression of the named variable on a constant and the indicator for pilot. Standard errors are presented in parenthesis. *** p<0.01, ** p< 0.05, * p<0.10.

4 Identification

I use quasi-experimental variation in the roll-out of the Senior Citizen Grant (SCG) in Uganda. My main identification exploits the variation in exposure to the program across households and across districts. Following Moscona & Seck (2024), I use the age composition of the household members to construct a measure for Potential Pension Exposure (PPE) which measures the cumulative number of years of pension grants the household would have received if it was located in pension districts. By construction, this measure varies at household level depending on whether the household has pension-eligible individuals and the number of such individuals. To construct this measure, I first take a count of all pension-eligible individuals in the household at any given point in time and then sum up across time, from the time of the program launch to the time of my data. This measure is constructed as shown below.

$$PPE_h = \sum_{t=2011}^{2016} \left(\sum_{i \in h} \mathbb{I}\{Age_{iht} \geq 65\} \right), \tag{1}$$

where Age_{iht} is the age of individual i in household h at time t. One assumption I make is that the household composition is not endogenously changing. As I show later, this assumption is reasonable in the setting that I study.

The second source of variation in exposure to the program comes from the staggered roll-out of the pension program across districts. I combine the variation in potential exposure and the geographical variation in the allocation of the program using Difference-in-Difference design. The main regression specification is as follows:

$$y_{ihd} = \beta_0 + \beta_1 \left(PPE_h \cdot \mathbb{I}\{Pilot_d\} \right) + \beta_2 PPE_h + \beta_3 Pilot_d + \gamma' X_{ihd} + \tau_m + \tau_r + e_{ihd}, \tag{2}$$

where y_{ihd} is the outcome variable of individual i, living in household h, located in district d, in this case labor supply and earnings. $Pilot_d$ is the indicator variable for being located in pilot district, X_{ihd} is a vector of control variables including age, sex, education, location (urban/rural), marital status, education of the household head, household size, and employment status of the household head. τ_m and τ_r are month of interview and region fixed effects respectively. In the analysis, I cluster the standard errors at district level to account for potential correlation within a district.

My identification comes from comparing outcomes of individuals living in different households with otherwise similar levels of potential pension exposure but located in different districts (pilot versus non-pilot). Given the controls in the model, the identifying assumption is that individuals in households with the same level of potential pension exposure, same age, same level of education, same sex, same location (rural-urban), same region, interviewed in the same month, living in households with similar characteristics are on similar trends absent the pension program. Under the assumption of exogeneity of the pension instrument (Potential Pension Exposure), I interpret β_1 as a causal effect of the pension program.

With the approach described above, it's difficult to test for parallel trends assumption, which is crucial in difference-in-difference design. To deal with this issue and other robustness issues, I extend the analysis by leveraging the panel data earlier described. For the panel analysis, I begin by defining the treatment (access to pension) to follow a staggered design. As indicated in section 2, the design and implementation of the pension program was such that only individuals aged 65 years and above (60 years for Karamoja) were eligible for the pension grant. This implies households without eligible individuals did not access the program until the oldest person crossed the eligibility threshold. This means households where the oldest person was above 64 years at the time of introduction of the program (in 2011) and were located in pilot districts accessed the program at the onset in 2011. Households whose oldest person was 64 years, are treated one year later, and those whose oldest person was 63 years in 2011 are treated 2 years later, and so on. This introduces staggered adoption among households, which I exploit. For the purposes of the estimation, I make a simplifying assumption that keeps the treatment stable. In practice, the intensity of treatment can increase as more people in the household cross the age eligibility threshold, or decrease if the household loses the pension beneficiary. I assume that once the household gets treated, it doesn't lose the treatment. This is necessary for implementing the staggered DID design since it requires that the treatment is in absorbing state. Because of these assumptions and the caveats of the panel data highlighted earlier, I do not explicitly interpret the magnitudes from this exercise but rather use it to study dynamic trends in labor supply.

To estimate the average treatment effect from the staggered treatment described above, I adopt the Difference-in-Difference estimator (DID_M) recently proposed by De Chaisemartin & d'Haultfoeuille (2020). The estimator has been suggested to mitigate the bias arising from negative weights and heterogeneous treatment effects that are not accounted for in earlier estimators such as the Two-Way Fixed Effects (De Chaisemartin & d'Haultfoeuille 2020). The estimator is also the most applicable in cases where, for each pair of consecutive dates, there are groups whose treatment does not change, which is likely the case in my data. I therefore estimate the following regression specification.

$$y_{it} = \alpha_i + \gamma_t + \sum_{\substack{t=2009 \\ t\neq 2011}}^{2015} \beta_t \mathbb{I}\{Pension_{it} = 1\} * \mathbb{I}\{Wave = t\} + \varepsilon_{it}, \tag{3}$$

where $pension_{it}$ is an indicator if the household has a pension eligible individual, $Wave$ indicates the year of observation, α_i is individual fixed effect and γ_t is time fixed effect. ϵ_{it} is the error term.

5 Empirical Results and Discussion

5.1 Old-Age Pension and Earnings of Younger Individuals

Figure 3 shows the summary of the results from equation 2 using sales, profits, and wages as the dependent variables. Table A.2 presents the results in detail. I present the results separately for the full sample, women's sample, and men's sample. The results show that exposure to old-age pension leads to an increase in earnings of younger individuals, but only for those engaged in self-employment. Specifically, Figure 3 shows that, in the full sample, one additional year of pension exposure increases the monthly sales and profits by 10.2% and 9.5%, respectively. Based on the mean in the non-pilot districts, the effect on sales is equivalent to \$11.7, while the effect on profits is equivalent to \$4.8. These effects are economically meaningful given that one year of pension exposure is equivalent to \$118. I do not find significant effect of the pension exposure on earnings of employees (wages) in the full sample, even though the direction suggests negative effects. The effects on sales and profits are only significant among women and substantially higher for this group. I find that one additional year of pension exposure increases sales and profits by 18.0% and 17.4%, respectively, among women. Among men, the effect on sales and profits is positive but not significant. Indeed, Table A.3 in the Appendix shows that the effect of the pension on women is statistically different compared to men.

The positive effect of the old-age pension on earnings is in line with the recent findings by Huang & Zhang (2021) who show that the pension program for seniors in China, the New Rural Pension Scheme (NRPS) significantly increased household income of eligible households in rural China. They show that household income increases by 18% in the full sample but the effect drops to 10% when they exclude the very poor seniors.

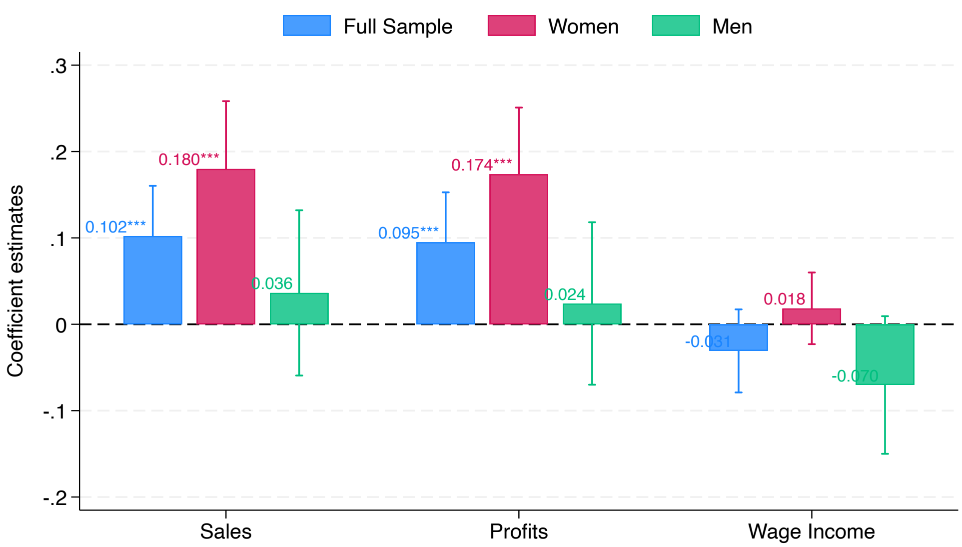


Figure 3: Effects of old-age pension on earnings of ineligible adults.

Notes: The variables at the bottom are the dependent variables, transformed using Inverse Hyperbolic Sine Transformation (IHS). Each of the estimates is from a different regression. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Placebo Experiment: As a validation exercise, I conduct a placebo experiment where I construct a placebo potential pension exposure based on the individuals that are just below the cutoff age (just below 65 years). In this case, my placebo exposure is based on individuals aged 50 to 64 years, as shown in equation 4, where PPE_h^p is the *Placebo Potential Pension Exposure*.

$$PPE_h^p = \sum_{t=2011}^{2016} \sum_{i \in h} \mathbb{I}\{50 \leq Age_{iht} \leq 64\}, \tag{4}$$

With this placebo measure, I re-estimate the regression in equation 2 including this placebo. Specifically, I estimate the following regression:

$$\begin{aligned} y_{ihd} =& \beta_0 + \beta_1 \left(PPE_h \cdot Pilot_d\right) + \beta_2 \left(PPE_h^p \cdot Pilot_d\right) + \beta_3 PPE_h + \beta_4 PPE_h^p + \\ & \beta_5 Pilot_d + \gamma' X_{ihd} + \tau_m + \tau_r + e_{ihd}, \end{aligned} \tag{5}$$

where β_1 is the coefficient of the true exposure while β_2 is the coefficient of the placebo exposure. All the variables are as defined before.

If the original measure of pension exposure is capturing the effects of the pension program, I do not expect the placebo exposure to have the same effects, since individuals just below the cutoff are not eligible and therefore do not receive the pension. Figure 4 presents β_1 and β_2 from the regression in equation 5. The results show that only the true exposure predicts positive and significant effects on sales and profits both in the full sample and women's sample. The coefficients for the placebo exposure are very small in magnitude and not significant. This suggests that the pension instrument is indeed capturing the effects of the pension and not anything else associated with household structure. For robustness, I adjust the interval for the placebo from 50 - 64 to 55 - 64 years. Results in Table A.4 show a similar pattern where only the true exposure produces the true effects.

Event Study Graphs: To examine if individuals in pilot districts are experiencing differential trends, I conduct a cohort-based analysis where I group households in cohorts based on far away from the pension (for those below eligibility age) and the number of years the household has had the pension. I begin by constructing a measure of the distance to the pension in years which captures the difference between the age of the oldest person in the household and the eligibility cutoff age of 64. Since the pension program had existed for five years prior to my data, I restrict the highest number of years the household has spent with the pension to five. Equation 6 shows how the Distance to Pension (DTP) is constructed, where i indexes individuals and h indexes households. I then discretize this measure into cohorts of two-year intervals (I do this for power reasons) and estimate cohort-specific effects using regression in equation 7.

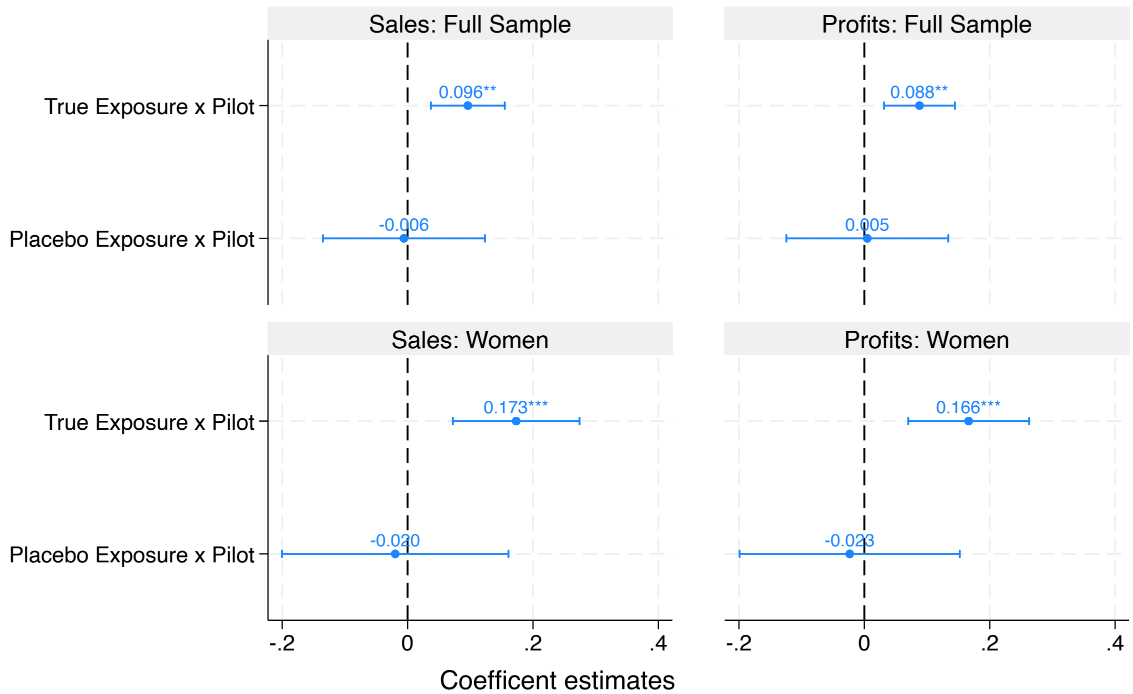


Figure 4: Effects of old-age pension on earnings of ineligible adults: Placebo Experiment

Notes: The dependent variables are sales and profits, transformed using inverse hyperbolic sine transformation. The top-right and top-left plots are for the full sample while the bottom-left and bottom-right plots are for the women's sample. True exposure is constructed from equation 1 while placebo exposure is constructed from equation 4. The coefficients are estimated from regression equation 5. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with exposure measures), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

$$DTP_h = max_i \left\{ \min\{\text{Age}_{ih} - 64, 5\} \right\}, \tag{6}$$

$$y_{ihc} = \beta_0 + \sum_c \beta_c \left(\mathbb{I}_d^{Pilot} * \mathbb{I}_h^c \right) + \beta_p Pilot_d + \gamma' X_{ihd} + \tau_c + \tau_m + \tau_r + e_{ihd}, \tag{7}$$

where $\sum_c$ sums over two-year cohort bins. $\mathbb{I}_d^{Pilot}$ is an indicator of being located in the pilot district, $\mathbb{I}_h^c$ is an indicator for the household's DTP_h falling in cohort c, and τ_c are cohort fixed effects. X_{ihd} contains all the variables defined before but also includes interactions with cohorts. β_c are the coefficients of interest. It captures the differential effect on individuals living in households located in pilot districts by cohort.

Figure 5 presents the coefficients of β_c on sales and profits. As expected, β_c is indistinguishable from 0 for cohorts that have not yet reached the pension, indicating that pre-trends are parallel. For cohorts that have reached the pension, the coefficients are positive and significant. Note that I do not explicitly interpret the magnitudes here since this analysis doesn't account for the intensity of the treatment arising from the fact that some households may have more than one pension eligible individual. Nonetheless, the coefficients are close in magnitude to those in Figure 3.

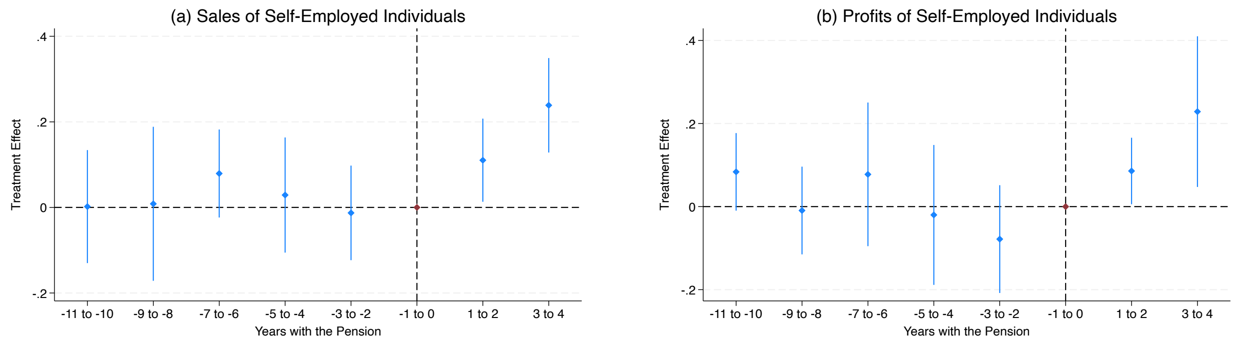


Figure 5: Old-age pension and earnings of ineligible adults in different household cohorts

Notes: The dependent variables are sales and profits, transformed using inverse hyperbolic sine transformation. The coefficients are estimated from regression equation 7, on a sample of individuals aged 18 to 64. The regression includes control variables (which also include interaction terms with cohorts), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level.

The results on earnings beg a question as to what explains the positive effects of old-age pension on the earnings of self-employed individuals. In what follows, I rationalize two interrelated potential channels. First, I test the labor supply channel—does the pension program enable younger individuals to increase their labor supply? Secondly, given that the increase in the earnings is only salient among self-employed individuals, I test whether the pension relieves or provides resources that are invested by younger individuals. For this, I test whether the pension affects the working capital for these individuals. This channel can simultaneously affect both earnings and labor supply of self-employed individuals. If the working capital increases, it means these individuals have more stock and therefore can work longer hours.

5.2 Old-Age Pension and Labor Supply of Younger Individuals

Table 3 shows exposure to old-age pension has a positive effect on labor supply of younger individuals (aged 18 to 64 years). One additional year of pension exposure increases labor supply (weekly hours worked) by 1.3%. Based on the mean in the non-pilot, this represents an increase of 0.49 hours. This effect is also economically meaningful given the size of the pension grant as earlier highlighted. Consistent with the earnings, the effect is more significant among the self-employed individuals (1.6% increase). This suggests that the increase in earnings earlier observed is partly explained by the increase in labor supply. Among women, pension exposure increases labor supply by 1.4% overall, 1.8% among self-employed women. The effect among men is not significant although positive. Table A.9 in the Appendix shows the coefficient estimates for effects on men and women are statistically indistinguishable. I find no significant effect on labor supply among men and women in the category of employees, although the coefficients are also positive.

Table 3: Impact of old-age pension on labor supply

	(1) All	(2) Self-Employed	(3) Employees
Panel A: Full Sample			
Exposure x Pilot	0.013*	0.016**	0.015
	(0.007)	(0.007)	(0.012)
Mean in Non-Pilot	38	35	47
Observations	16626	12930	5050
R^2	0.104	0.098	0.074
Panel B: Women			
Exposure x Pilot	0.014**	0.018**	0.007
	(0.007)	(0.006)	(0.019)
Mean in Non-Pilot	34	32	44
Observations	8286	7058	1659
R^2	0.095	0.085	0.118
Panel C: Men			
Exposure x Pilot	0.013	0.017	0.020
	(0.012)	(0.014)	(0.016)
Observations	8340	5872	3391
Mean in Non-Pilot	42	39	49
R^2	0.102	0.108	0.091

Notes: In all models, the dependent variable is *ihs*(hours worked). All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Similar to earnings, there might be a concern that the pension instrument is capturing effects of other features related to household composition (that might be different across pilot and non-pilot), and not only the pension. To address this concern, I follow the same approach as in section 5.1, using the *placebo pension exposure* in equation 4 and estimate the regression in equation 5. Results in Table 4 show that only "true exposure" predicts positive effects of the pension on labor supply. The coefficients of the "placebo exposure" are not significant, if anything negative. This suggests that my measure of pension exposure is indeed capturing the effect of the pension.

Table 4: Placebo experiment on labor supply of individuals aged 18 to 64

	(1) All	(2) Self-Emp	(3) Women
Exposure x Pilot	0.013*	0.017**	0.014*
	(0.008)	(0.007)	(0.007)
Placebo Exposure x Pilot	-0.018	-0.015	-0.005
	(0.013)	(0.016)	(0.013)
Observations	16626	12930	8286
R^2	0.107	0.102	0.098

Notes: In all models, the dependent variable is *ihs*(hours worked). All the models are estimated on a sample of individuals aged 18 to 64 years. True exposure is constructed from equation 1 while placebo exposure is constructed from equation 4. The coefficients are estimated from regression equation 5. All models include control variables (which include interaction terms between exposure measures and household/individuals characteristics), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

As a further robustness check, I use a staggered difference-in-difference design to estimate the dynamic effects of the pension on labor supply. For this, I use the Uganda National Panel Survey (UNPS). Unlike earnings, the panel collects information about the hours worked for working-age individuals. Figure 6 shows that exposure to the pension program has a positive effect on hours worked, which is significant and persistent after two periods. Note that, the panel data for the period I consider is available every two years. Therefore, I interpret a period to consist of two years. Figure 6 also shows that there are no pre-trends, which suggests that the treated individuals would have been on the same trend as untreated individuals absent the pension.

I also examine the effect of the pension exposure on labor supply at the extensive margin. In Table A.10, I present the effects of the pension exposure on the likelihood of working for pay (Column 1 to 3), and being self-employed in main job (Column 4 to 6). Overall, the results show that pension exposure did not have a significant effect on the likelihood of engaging in paid employment. This result is consistent with (Huang & Zhang 2021) who show that pension exposure in China did not significantly affect the likelihood of working among the rural population. The results also show that pension exposure did not have a significant impact on the likelihood of being self-employed in the main job. Taken together, the results imply that the labor supply of ineligible adults responds more at the intensive

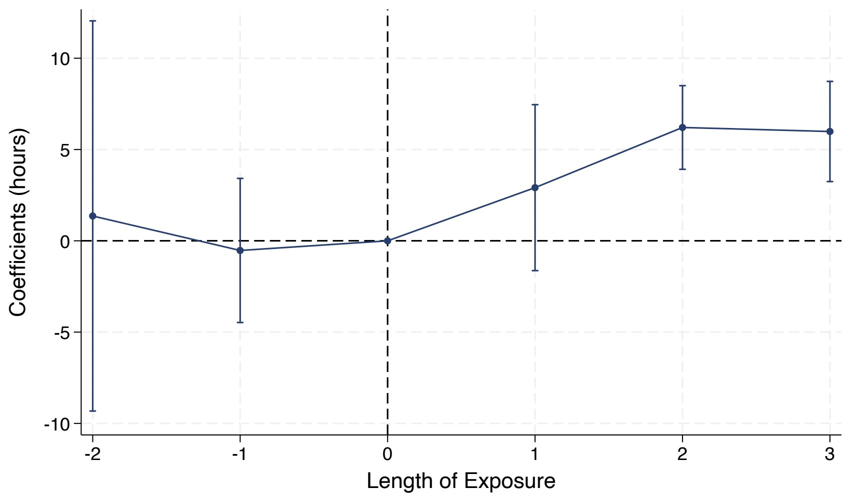


Figure 6: Dynamic effects of old-age pension on labor supply of ineligible adults

Note: This graph is estimated using the staggered Difference-in-Difference approach recently proposed by De Chaisemartin & d'Haultfoeuille (2020). The average treatment effect is 3.9 with standard error of 0.723. One caution to keep in mind while interpreting the graph is that the data is observed after every two years and so each period technically represents two years.

margin rather than the extensive margin, as a result of exposure to the old-age pension. This could be attributed to the small size of the pension grants.

The results that old-age pension increases labor supply of ineligible individuals relates to a few recent papers that have shown positive effects of cash transfers. For instance, Salehi-Isfahani & Mostafavi-Dehzooei (2018) showed that national cash transfer program in Iran had positive effects on the labor supply of women and self-employed men. More recently, Vera-Cossio (2022) showed that nationwide conditional cash transfer program in Bolivian public schools led to an increase in employment among parents of eligible children. The paper shows that the cash transfer increased the likelihood of self-employment by relaxing liquidity constraints. Even though these papers examine the effects on direct beneficiaries, they relate to my findings by showing that there are contexts in which cash transfers increase labor supply rather than decrease it, as widely cited in the literature. More closely related to my results is Ardington et al. (2009) who show that cash transfers in South Africa led to an increase in employment rates among prime-aged individuals in South Africa. They show that the pension allows prime-aged individuals to migrate for work by easing the childcare constraint. However, they only focus on the extensive margin while here, I examine the effects on both the intensive and extensive margin and show that labor supply responds more on the intensive margin. I also show that in this case, the pension did not have a significant effect on the likelihood that any household member migrates for work (Table

A.17)

Effects on the elderly: Previous studies show that pension beneficiaries tend to alter their labor supply when they start receiving pension (de Carvalho Filho 2008, Fetter & Lockwood 2018, Unnikrishnan & Imai 2020). Although the focus of my paper is not on the effects of the pension on direct beneficiaries, I re-estimate this relationship using staggered difference-in-difference, leveraging the panel data. I find results consistent with previous studies. Figure A.1 in the Appendix shows that the elderly (above 64 years) are less likely to engage in paid employment when they start receiving pension grants. The results suggest that on average, exposure to the pension led to a 18.5 percentage point reduction in the likelihood of engaging in paid employment. This effect is larger relative to the effect reported by earlier studies in different contexts. For example, Kaushal (2014) showed that \$11 (in purchasing power parity) provided through India's National Old-Age Pension Scheme was associated with a 1–3 percentage points decline in the employment of the elderly. Similarly, de Carvalho Filho (2008) showed that an increase in old-age benefits by \$95 in Brazil increased the probability of not working by about 38 percentage points. The higher effect I find could be attributed to the sample size limitation highlighted earlier, and relatively longer duration of exposure. Nonetheless, this result provides suggestive evidence that the elderly reduce their labor supply once they start receiving the pension.

Given the above evidence, I investigate whether there is reallocation of some household tasks from younger individuals to the elderly. If the elderly are exiting the labor market and are able to take on some household tasks such as caring for children, this might relieve time for younger individuals to work more. If this is happening, it could explain part of the increase in labor supply observed earlier. This could also enable them to migrate for work outside the household as highlighted by Ardington et al. (2009). To investigate this channel, I examine how the pension exposure affects time spent on household tasks by younger individuals (18 to 64 years). Table A.11 shows that exposure to pension does not significantly affect time spent on household tasks. This suggests that the increase in labor supply earlier observed is not coming from a reduction in household tasks but rather a reduction in leisure or simply from the slack in labor supply. This corroborates my finding that the pension does not induce household members to migrate for work.

5.3 Old-Age Pension and Working Capital of Younger Individuals

To better understand what drives the increase in earnings and labor supply as a result of pension exposure, I test whether the pension program affects the working capital for self-employed individuals. As earlier mentioned, the self-employed individuals could provide an avenue for investment of the pension grants. The pension could also relieve financial resources for the younger individuals by supporting consumption expenditure in the household, and reducing financial dependence of the elderly. If these channels are present, we might expect the younger individuals to have more financial resources to invest in their businesses.

Table 5 shows that the pension exposure increases working capital for self-employed ineligible adults. One additional year of pension exposure increases working capital by 6.4%, which is equivalent to $2.3 increase, based on the mean in non-pilot districts. While the effect is positive for both men and women, it is insignificant among men. In a placebo experiment presented in Table A.12, I show that the effects on working capital are indeed driven by the "True Exposure" and not the "Placebo Exposure". The results on working capital are consistent with results on earnings and labor supply which show that the effects are only significant among women. The positive effect on working capital is in line with Vera-Cossio (2022) who shows that cash transfers can relax liquidity constraints and boost entrepreneurship, although Vera-Cossio (2022) studies effects on direct beneficiaries rather than spillover. I interpret the increase in working capital to be driving the results on labor supply through the complementarity channel, and earnings. More resources can induce self-employed individuals to work longer but also has a direct effect on sales and profits.

Table 5: Impact of old-age pension on working capital for self-employed individuals

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure x Pilot	0.085**	0.064*	0.101**	0.097**	0.077	0.039
	(0.036)	(0.034)	(0.045)	(0.044)	(0.067)	(0.062)
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Interview Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	No	Yes	No	Yes	No	Yes
Mean in Non-Pilot (USD)	36.6	36.6	35.4	35.4	37.6	37.6
Observations	12260	12260	6885	6885	5375	5375
R^2	0.129	0.204	0.148	0.234	0.125	0.188

Notes: The dependent variable is working capital (proxied by investment in raw materials/inputs), transformed using inverse hyperbolic sine transformation. All the models are estimated on a sample of individuals aged 18 to 64 years. All models include control variables (including interaction terms with potential exposure measure), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

5.4 Heterogeneity Analysis

Availability of microfinance and the effects of pension on earnings and working capital: I explore whether the effect of the pension is different depending on the availability of micro credit in the community. In particular, I ask the question: Do pension grants compensate for limited availability of and access to micro credit? or instead, does availability of micro credit bolster the effects of the pension? If the pension grants are compensating for lack of micro credit, we would expect to see higher effects in areas with limited access to micro credit. To answer these questions, I use triple difference-in-difference design estimating the regression equation specified in equation 8.

$$\begin{aligned} y_{ihd} =& \beta_0 + \beta_1 \left(PPE_h \cdot \mathbb{I}\{Pilot_d\} \cdot \mathbb{I}\{\text{MFI} \leq 10\text{km}\} \right) + \beta_2 \left(PPE_h \cdot \mathbb{I}\{Pilot_d\} \cdot \mathbb{I}\{\text{MFI} > 10\text{km}\} \right) \\ &+ \beta_3 PPE_h + \beta_4 PPE_h^p + \beta_5 Pilot_d + \eta \prime Z_{hd} + \gamma' X_{ihd} + \tau_m + \tau_r + e_{ihd}, \end{aligned} \tag{8}$$

where $\mathbb{I}\{\text{MFI} \leq 10\text{km}\}$ is an indicator for a household being located within 10km from a microfinance and $\mathbb{I}\{\text{MFI} > 10\text{km}\}$ is an indicator for a household being located more than 10km from a microfinance. Z_{hd} is a vector of double interactions for PPE, $Pilot$, and distance to a microfinance. The rest of the variables are as defined before. β_1 and β_2 are the coefficients of interest.

Results presented in Table 6 show that the effects of the pension on sales, profits, and working capital are bolstered by the availability of and access to microfinance. In the full sample, the results show that the effect of the pension on the sales is 13 percentage points (β_1-β_2) higher for individuals in households located within 10km from a microfinance compared to those living more than 10km (Column 1). Similarly, the effect on profits and working capital is higher by 13 and 14 percentage points respectively, for individuals located within 10km of a microfinance. Among women, the pattern is the same, with a much higher increase in working capital for those located within 10km of a microfinance. These differences are statistically significant for sales and profits in the full sample, and sales and working capital in the women's sample. Among men, exposure to old-age pension does not produce significant effects regardless of whether they are within or far from microfinance. Broadly, these results suggest that cash transfers cannot be treated as a substitute for micro credit and do not compensate for the limited availability of and access to micro credit. Rather, the results suggest that the impact of cash transfers is bolstered by the availability of micro credit. This complementary relationship can be attributed to cash transfers increasing creditworthiness of the borrowers which stimulates demand for and access to additional capital from microfinance.

Table 6: Distance to a microfinance or bank and the effects of the pension

	Full Sample			Women			Men		
	(1) Sales	(2) Profits	(3) W. Capital	(4) Sales	(5) Profits	(6) W. Capital	(7) Sales	(8) Profits	(9) W. Capital
Exposure x Pilot x $\mathbb{I}\{\text{MFI} \leq 10\text{km}\}$	0.203***	0.193***	0.174**	0.297***	0.274***	0.268**	0.040	0.045	0.035
	(0.043)	(0.039)	(0.082)	(0.072)	(0.069)	(0.103)	(0.085)	(0.076)	(0.156)
Exposure x Pilot x $\mathbb{I}\{\text{MFI} > 10\text{km}\}$	0.072	0.065	0.031	0.137***	0.137***	0.037	0.035	0.019	0.039
	(0.042)	(0.041)	(0.041)	(0.048)	(0.046)	(0.042)	(0.065)	(0.063)	(0.074)
P-values ($\beta_1 = \beta_2$)	0.065	0.054	0.178	0.093	0.122	0.088	0.962	0.782	0.981
Observations	14609	14608	12260	8579	8579	6885	6030	6029	5375
R^2	0.194	0.194	0.204	0.131	0.131	0.235	0.219	0.213	0.188

Notes: The depend variables are sales, profits, and working capital, transformed using inverse hyperbolic sine transformation. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables, double interaction terms, interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Do the pension grants provide informal insurance to the younger individuals? To answer this question, I examine the heterogeneity of the effect of the pension based on the availability of informal safety nets in the community. If the pension grants are providing informal insurance to the younger individuals, this could allow them to take on high-risk but high-return ventures which could increase their earnings. In that case, we would expect to see higher effects in areas with limited access to informal safety nets. To test this, I estimate a regression similar to equation 8, replacing distance to microfinance with the availability of informal safety nets in the community.

In Table 7, Panel A, I shows heterogeneity based on the presence of a Savings and Credit Co-operative (SACCO) in the community and Panel B shows heterogeneity based on the presence of any informal group in the community (including SACCOs, youth groups, women groups, and business associations). Results show that the effects of the pension are higher in locations that have access to informal safety nets. Panel A shows that the pension only has a significant effect on sales, profits, and working capital in communities that have access to SACCOs. In Panel B, results show that having any informal group bolsters the effect of the pension on earnings and working capital. Communities that lack any form of informal safety nets see no effect at all in the full sample. The differences in the effect of the pension are statistically significant for sales and profits in the full sample and for all the three outcomes in the women's sample. These results suggest that the pension enables these younger individuals to leverage the existing informal networks, suggesting cash transfers can be effective in enhancing productive use of the existing social networks.

Heterogeneity across rural and urban locations: Following the same approach as above, I estimate whether the effects of the pension are different across rural and urban locations. I estimate equation 8 replacing distance to MFI with indicators for rural and urban locations.

Table 7: Availability of informal safety nets and the effects of the pension

	Full Sample			Women			Men		
	(1) Sales	(2) Profits	(3) W. Capital	(4) Sales	(5) Profits	(6) W. Capital	(7) Sales	(8) Profits	(9) W. Capital
Panel A: SACCO in the Community									
Exposure x Pilot x $\mathbb{I}$\{SACCO\}	0.137***	0.125***	0.120***	0.241***	0.228***	0.181**	0.090*	0.073	0.083*
	(0.019)	(0.020)	(0.028)	(0.051)	(0.049)	(0.067)	(0.051)	(0.053)	(0.044)
Exposure x Pilot x $\mathbb{I}$\{No SACCO\}	0.046	0.045	0.010	0.112	0.107	0.061	-0.004	-0.008	-0.012
	(0.084)	(0.078)	(0.061)	(0.083)	(0.079)	(0.049)	(0.106)	(0.100)	(0.105)
P-values ($\beta_1 = \beta_2$)	0.270	0.292	0.115	0.181	0.184	0.185	0.443	0.483	0.390
Observations	18914	18915	15337	11243	11243	8598	7671	7672	6739
R^2	0.207	0.206	0.251	0.126	0.126	0.278	0.265	0.258	0.238
Panel B: Informal Group in the Community									
Exposure x Pilot x $\mathbb{I}$\{Any Informal Group\}	0.166***	0.152***	0.136**	0.260***	0.246***	0.218**	0.094**	0.074*	0.064*
	(0.034)	(0.032)	(0.053)	(0.075)	(0.071)	(0.097)	(0.039)	(0.043)	(0.035)
Exposure x Pilot x $\mathbb{I}$\{No Informal Group\}	-0.005	-0.004	-0.015	0.068	0.066	-0.013	-0.004	-0.005	0.030
	(0.048)	(0.046)	(0.062)	(0.062)	(0.062)	(0.062)	(0.082)	(0.078)	(0.113)
P-values ($\beta_1 = \beta_2$)	0.001	0.001	0.150	0.016	0.018	0.091	0.236	0.315	0.772
Observations	18914	18915	15337	11243	11243	8598	7671	7672	6739
R^2	0.207	0.206	0.251	0.126	0.126	0.279	0.264	0.256	0.237

Notes: The depend variables are sales, profits, and working capital, transformed using inverse hyperbolic sine transformation. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables, double interaction terms, interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table 8 shows that effects of the pension are stronger in urban areas compared to rural areas. In the full sample, the impact of old-age pension on sales is 15 percentage points higher in urban areas compared to rural areas. The effect on profits is 11 percentage points higher in urban areas. The difference in the effect on working capital is 13 percentage points. Among women, the differences between urban and rural are 18, 17, 28 percentage points for sales, profits, and working capital. The differences in the effect of the pension are significant for sales and profits in the full sample and women's sample. These results are likely explained by the fact that urban areas offer more opportunities for business growth. To the extent that the pension grants are directly invested or ease the liquidity constraint of younger individuals, the additional capital is complemented by the business opportunities in urban areas to foster a higher impact. These results are consistent with the findings that the effects are higher in areas with access to microfinance since urban locations are likely to be correlated with the availability of microfinance.

Even though this paper does not study a direct cash transfer to the analysis sample (18-64 years), the results in Table 6, 7, and 8 suggest stronger complementarities between cash transfers and the local financial, social, and economic landscape.

5.5 Does the Gender of Pension Recipient Matter?

Earlier literature about the effects of pension programs shows that the effects might depend on the gender of the recipient. In particular Duflo (2003) showed that pensions

Table 8: Location (urban vs rural) and the effects of the pension

	Full Sample			Women			Men		
	(1) Sales	(2) Profits	(3) W. Capital	(4) Sales	(5) Profits	(6) W. Capital	(7) Sales	(8) Profits	(9) W. Capital
Exposure x $\mathbb{I}\{\text{Pilot}\}$ x $\mathbb{I}\{\text{Urban}\}$	0.221***	0.182***	0.176*	0.329***	0.319***	0.325**	0.174***	0.116	0.098
	(0.030)	(0.048)	(0.098)	(0.071)	(0.068)	(0.150)	(0.056)	(0.093)	(0.141)
Exposure x $\mathbb{I}\{\text{Pilot}\}$ x $\mathbb{I}\{\text{Rural}\}$	0.073**	0.072**	0.043	0.149***	0.145***	0.048	-0.007	-0.007	0.035
	(0.035)	(0.034)	(0.040)	(0.052)	(0.051)	(0.048)	(0.061)	(0.058)	(0.080)
P-values ($\beta_1 = \beta_2$)	0.006	0.086	0.260	0.045	0.046	0.112	0.043	0.249	0.712
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interview Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14609	14608	12260	8579	8579	6885	6030	6029	5375
R^2	0.167	0.167	0.156	0.097	0.097	0.167	0.164	0.158	0.152

Notes: The depend variables are sales, profits, and working capital, transformed using inverse hyperbolic sine transformation. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (which include interaction terms between exposure measure and household/individuals characteristics), double interaction terms, interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

received by women rather than men had a large impact on the anthropometric measures of girls. Similarly, Unnikrishnan & Imai (2020) shows the positive effects of old-age pensions on consumption expenditures are only observed when the recipients are women. Accordingly, I also examine whether the gender of the pension recipient matters for labor market outcomes of adult ineligible individuals. To achieve this, I construct two measures of pension exposure for each household, based on the number of eligible male and female individuals in the household as shown in equation 9.

$$FPE_h = \sum_{t=2011}^{2016} \left(\sum_{i \in h} \mathbb{I}\{Age_{iht} \geq 65\} * \mathbb{I}\{Female_{ih}\} \right), \tag{9}$$

where FPE_h is female-based pension exposure, $Female_{ih}$ is an indicator variable if individual i in household h is female. I construct male-based exposure in an analogous way, which I label MPE_h. With these two measures of gender-based pension exposure, I estimate a modified version of equation 2 replacing PPE_h with the two gender-based measures as shown in equation 10.

$$\begin{aligned} y_{ihd} = &\beta_0 + \beta_1(FPE_h \times Pilot_d) + \beta_2(MPE_h \times Pilot_d) + \beta_3 FPE_h + \beta_4 MPE_h \\ &+ \beta_5 Pilot_d + \gamma' X_{ihd} + \tau_m + \tau_r + e_{ihd}, \end{aligned} \tag{10}$$

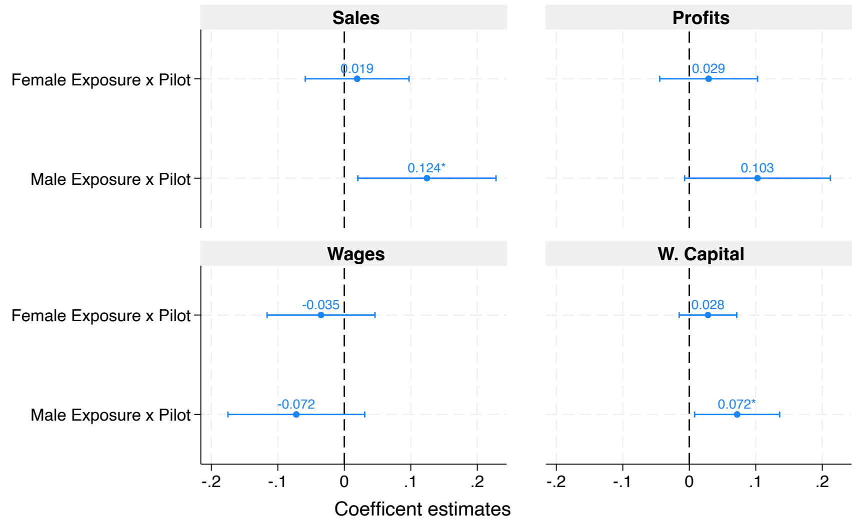
all the other variables are as defined before. The coefficients of interest here are β_1 and β_2 which capture the effects of female-based and male-based pension exposure respectively.

Figure 7 presents the results for this analysis in the full sample and women's sample for outcomes on earnings. The results show that male-based exposure rather than female-based exposure has a significant effect on earnings. As shown in Figure 7a, in the full sample, one additional year of male-based pension exposure increases sales by 12.4%, profits by 10.3% (though not significant), and working capital by 7.2%. All coefficients for female-based exposure are not significant. Even though the effects of female-based and male-based exposure are statistically indistinguishable in the full sample (Table A.15), the differences are more salient in the women's sample (Figure 7b and Table A.15, Panel B). One additional year of male-based pension exposure leads to a 19.7% increase in sales, a 16.7% increase in profits, and 11.5% increase in the working capital, among women while female-based pension exposure has a mute effect. Table A.15 indeed shows that the coefficient estimates for male-based and female-based exposure are statistically different for sales and working capital (see p-values in Panel B). I however find that the gender of the recipient does not differentially influence the impact of the pension on earnings among men (Table A.15). This could be due to the weaker effects of the pension among men, to begin with.

In a similar way, I test whether the gender of the recipient matters for the effects of the pension exposure on labor supply. Results in Figure 8 show that, consistent with earnings, male-based pension exposure rather than female-based has a significant impact on labor supply. In the full sample, one additional year of male-based pension exposure increases labor supply by 2.4%, while female-based exposure has an effect of only 0.5%, which is statistically insignificant. In the women's sample, the effect of male-based pension exposure is even higher (4.1%). However, we can not reject that the effects are statistically indistinguishable (Table A.16). I also find no significant differences in the effects of male-based and female-based pension exposure in men's sample, which is consistent with the preceding evidence that the pension has small effects on men's labor market outcomes. As shown in Table A.16 in Appendix, the effect of male-based pension exposure is only significant among self-employed individuals (column 2). This is consistent with the preceding evidence that pension exposure mostly affects labor market outcomes of self-employed individuals rather than employees.

At first glance, the results that male-based rather than female-based pension exposure has a significant impact on labor market outcomes of adult ineligible individuals seems contrary to earlier evidence by Duflo (2003) and Unnikrishnan & Imai (2020). In particular, (Duflo 2003) shows that pension received by women has a large impact on children's health outcomes, especially girls, while pension received by men does not have such effects. To reconcile my results with this earlier evidence, I examine how the gender of the pension recipient affects children's outcomes in the same context that I study. For this, I use the Demographic and Health Survey for Uganda conducted in 2016. I study the effects of the pension on school attendance for primary school-going age and weight-for-height for children

(a) Full Sample



(b) Women

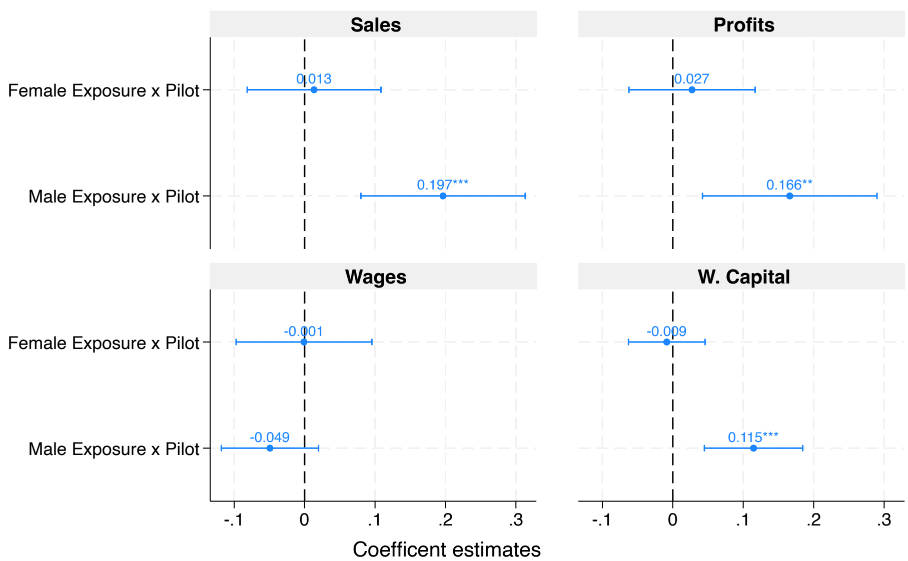


Figure 7: Gender of the recipient and effects of the pension on earnings.

Notes: The dependent variables are sales, profits, and working capital, transformed using IHS. Panel (a) shows effects in the full sample, while Panel (b) shows effects among women. It shows different effects for male-based exposure and female-based exposure. The dependent variable is transformed using inverse hyperbolic sine transformation. All the models are estimated on a sample of individuals aged 18 to 64 years. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** $p<0.01$, ** $p< 0.05$, * $p<0.10$.

aged 0 to 59 months. Weight-for-height is preferred because it tends to respond faster to changes in environmental conditions (Duflo 2003). Therefore, if the pension was affecting

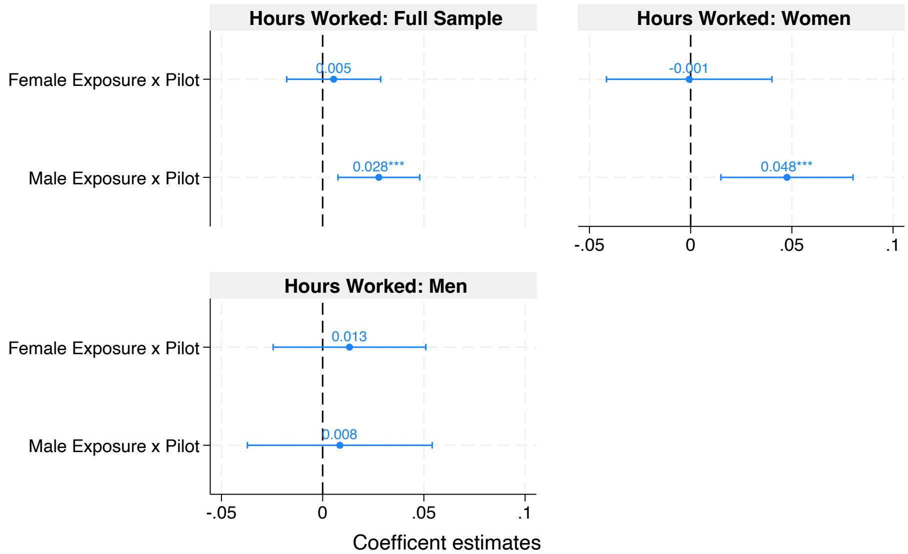


Figure 8: Gender of the recipient and effects of the pension on labor supply

Notes: The figure presents coefficient estimates of the effect of pension exposure on hours worked, transformed using IHS. It shows different effects for male-based exposure and female-based exposure. All the models are estimated on a sample of individuals aged 18 to 64 years. All models include control variables (including interactions with PPE), interview month fixed effects, and location fixed effects. Each plot represents a different sample as indicated in the headings. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

children's health outcomes, its effects would be easily detected on weight-for-height.

Results in Table 9 show that female-based pension exposure rather than male-based exposure has a significant effect on children's outcomes. I find one additional year of female-based pension exposure increases the likelihood of attending school among primary school age children by 1.6 percentage points (equivalent to 4.8% based on the mean in non-pilot districts), reduces the likelihood of being wasted by 2 percentage points (equivalent to 60.6% based on the mean in non-pilot). All the estimates for male-based pension exposure are insignificant. The p-values for the test of the differences in the effects of female-based and male-based pension exposure show that the effect of female-based exposure is significantly different from the effect of male-based exposure on school attendance (column 1 and 2) and likelihood of being wasted (column 3 and 4). This result is consistent with earlier evidence that women's pension exposure rather than men's improves children's outcomes.

In summary, my results suggest that male-based pension exposure has a significant effect on labor market outcomes of ineligible adults while female-based pension exposure has a significant effect on children's outcomes. These results reflect gender differences in preferences, with men preferring to invest in income generation and women preferring to invest

Table 9: Gender of recipient and effect of pension on children's outcomes

	School Attendence		Weight-for-Height			
	(1)	(2)	(3) Wasted (1/0)	(4) Wasted (1/0)	(5) Z-Score (SD)	(6) Z-Score (SD)
Female Exposure x Pilot	0.015*	0.016**	-0.019***	-0.020***	0.087	0.086
	(0.008)	(0.008)	(0.006)	(0.007)	(0.074)	(0.075)
Male Exposure x Pilot	-0.003	-0.007	-0.000	0.001	0.063	0.074
	(0.003)	(0.007)	(0.007)	(0.007)	(0.068)	(0.053)
P-values ($\beta_1 = \beta_2$)	0.090	0.064	0.065	0.058	0.844	0.908
Mean in Non-Pilot	0.335	0.335	0.033	0.033	0.098	0.098
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Interview Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	No	Yes	No	Yes	No	Yes
Observations	4110	4110	3947	3947	3947	3947

Notes: School attendance is a dummy variable taking 1 if the individual of primary school going age is attending primary school, 0 otherwise. Wasted is a dummy variable taking 1 if the weight-for-height z-score is minus 2 (-2.0) standard deviations (SD) below the median on the WHO Child Growth Standards. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

in children's human capital. The fact that pension received by women is largely spent on household consumption and investment in children, the most feasible channel through which it could affect earnings of younger individuals is easing financial pressure on these individuals. Given that female-based pension exposure doesn't have a stronger effect on younger individuals, this channel is likely weaker in this context. Rather, the direct investment of pension grants driven by male-based exposure seems to be the stronger channel in this case.

5.6 Further Robustness Checks

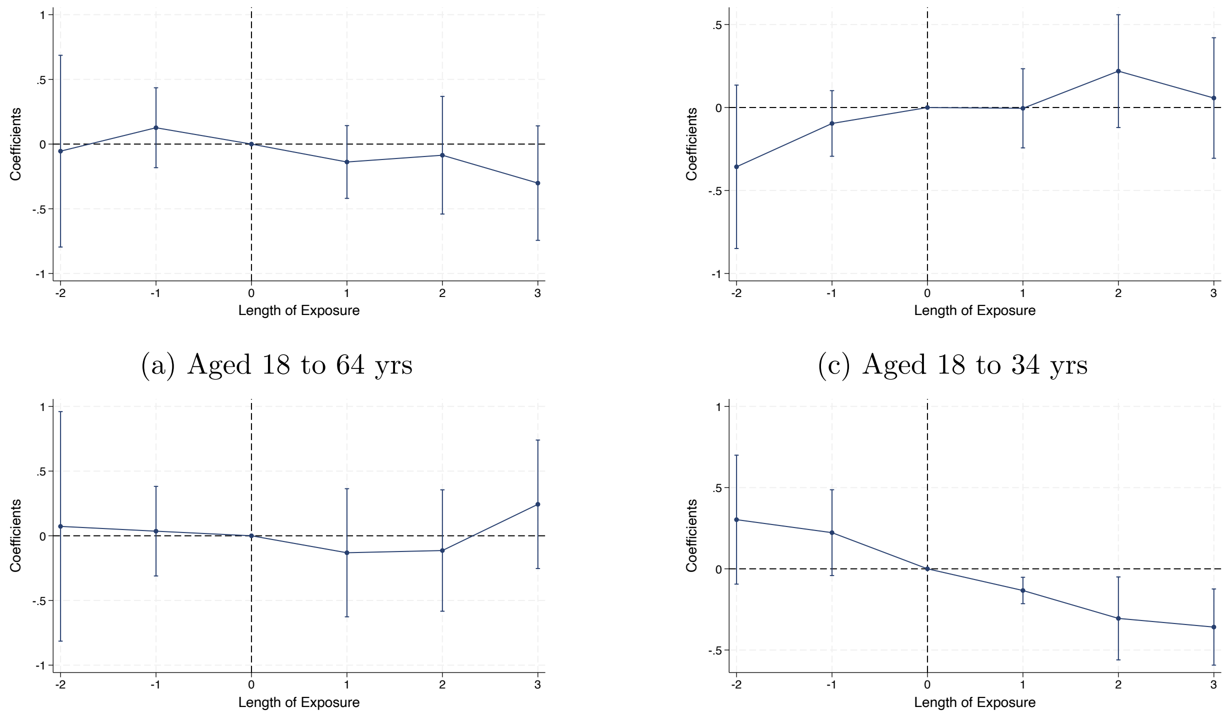
Besides the robustness checks based on the placebo experiment, parallel trends plots, and the panel analysis presented earlier, I conduct the following checks. First, I test whether my results are driven by features in the capital city. To test this, I re-estimate the regressions while excluding Kampala from the sample. The results show that exclusion of Kampala from the sample does not affect the results on both earnings (Table A.5 in Appendix) and labor supply (Table A.7 in Appendix). This suggests that the results are not driven by the capital city. I also test whether the results are sensitive to the control group. For this, I first restrict the control to only districts in Phase II. I then expand the sample to include all the districts. This exercise shows that the results are not sensitive to the control groups although they tend to be slightly smaller in the magnitudes when restricting the control to Phase II districts, which is reasonable since this group was partially treated by the time of my data (see Tables A.6) and A.8.

I then test whether remittances are playing a role in the results on earnings and labor supply. As shown by Fan (2010) pension grants can crowd out private transfers received by the elderly. If this is happening, my analysis might understate the effect of the pension. First, I examine whether old-age pension impacts the amount of remittances received by the household. Table A.13 shows that indeed the pension crowds out remittances. One additional year of pension exposure reduces annual remittances received by the household by 12%. Given this result, I re-estimate the models controlling for remittance, so as to compare households with the same level of remittances. Results in Table A.14 in the Appendix show that the results are still robust when controlling for the monetary value of remittances received by the household.

5.7 Potential Threats to the Identification and Shortcomings

A potential threat of my baseline specification in equation 2 is that it could be biased if the introduction of the pension program induces differential endogenous changes in household composition. I test whether this kind of endogenous formation of households is happening using the panel data. I define the outcome variable to be the number of household individuals in different age brackets at different points in time. If the pension program induces individuals to form around the pension recipients, we would expect to see different trends in treated households compared to untreated households. Evidence in Figure 9 suggests that this kind of endogenous household formation is not happening. For the age group 18 to 64 years (which is the focus of this paper), there is no differential change in the number of household members across treated and non-treated households, before and after the pension program (Figure 9a). I see a similar pattern for age groups less than 18 years (children) (Figure 9b) and 18 to 35 years (the youths) (Figure 9c). I do however see a downward trend in the number of household members aged 35 to 64 years (Figure 9d). This trend is unlikely to be driven by the pension program since there is a pre-trend before the pension program. Relatedly, if the pension induces high ability individuals to migrate for work, then studying the effects on individuals that stay in the household might understate the effects of the pension. In Table A.17 I show that the pension does not lead to migration for work.

One potential shortcoming of my analysis is that I only observe individual's exposure to the pension if they live in the same household as the pension beneficiary. If the channel through which the pension affects earnings of ineligible adults is through reduction in financial dependence by the elderly, then the effects can extend to relatives outside the households. In this study, I am not able to capture those extended effects because of data limitations. My results therefore should be interpreted as spillover effects within the household rather



(a) Aged 18 to 64 yrs

(c) Aged 18 to 34 yrs

(b) Below 18 yrs

(d) Aged 35 to 64 yrs

Figure 9: Dynamic effects of pension on the number of household members

Note: These plots are estimated using the staggered Difference-in-Difference approach recently proposed by De Chaisemartin & d'Haultfoeuille (2020). They show the trends in the number of household members in the age range indicated. One caution to keep in mind while interpreting the graph is that the data is observed after every two years and so each period technically represents two years.

than broader spillovers to extended relatives.

6 Conclusion

This paper examines how old-age pension affects the labor market outcomes of ineligible adults within the household. While many countries have non-contributory old-age pension, the effects on ineligible individuals have been understudied. Prior work has shown spillover effects on children. In this paper, I rationalize the idea that old-age pension might affect resource availability (both financial and time) for younger individuals. For instance, by reducing financial dependence and supporting consumption expenditures, the pension grants might relieve financial resources for younger individuals. In addition, the younger individuals operating small businesses might provide avenues for investment of the pension grants. Finally, if the pension leads the elderly to exit the labor market as prior research has shown, there might be re-allocation of some household tasks to the elderly which might relieve some time for the younger individuals. Through a combination of these forces, old-age pension might have an impact on labor market outcomes of younger individuals.

To address the question of interest, I exploit the quasi-experimental variation in the roll-out of the old-age pension program in Uganda—the Senior Citizen Grant. I construct an instrument for pension exposure based on household age composition and use difference-in-difference design to estimate the causal effects on labor market outcomes of individuals aged 18 to 64 years. I show that the pension exposure has significant positive effects on labor market outcomes of self-employed individuals, more so among women. I find evidence consistent with the idea that old-age pension relaxes the financial resource constraint of younger individuals. In this case, I show that exposure to old-age pension increases working capital of self-employed individuals, leading to higher earnings and labor supply. I show that the impact of pension exposure is bolstered by the availability of micro credit and stronger informal networks, suggesting complementarities between cash transfers and local financial and social landscape. The results are stronger in urban areas, suggesting that the effects of the pension on earnings are strengthened by other business opportunities that are associated with urban areas. I also find evidence that male-based pension exposure rather than female-based has a significant effect on labor market outcomes of ineligible adults. This is contrary to the results on children's outcomes where female-based exposure has a significant effect. To the extent that the effects on earnings are driven by investment of the pension grants by younger individuals, the difference in these results is a reflection of gender preferences, with women preferring to invest in children's human capital and men preferring to invest in income generation. I show that my results are not driven by endogenous household formation, migration, or pre-existing trends. The results are also robust to exclusion of Kampala (the capital city) from the sample, and controlling for remittances received by the household. Taken together, these results suggest that old-age pension programs have a developmental impact even on individuals that are ineligible.

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A Appendix

A.1 Additional Tables and Figures

Table A.1: Differences in means for pilot and non-pilot districts in 2011, clustered

	Mean in Non-Pilot	Diff. (Pilot-Non-Pilot)	N
Potential Pension Exposure	0.224	0.050	11215
	(0.014)	(0.052)	
Hours worked (weekly)	24.752	-3.586	6193
	(1.602)	(2.036)	
Self-employed in main job	0.113	-0.013	6989
	(0.017)	(0.033)	
Self-employed in secondary job	0.350	0.081	900
	(0.018)	(0.048)	
Paid work	0.136	-0.005	11215
	(0.012)	(0.022)	
Running a business	0.099	0.032	11215
	(0.007)	(0.016)	
Monthly wage (USD)	88.623	-26.644*	546
	(5.730)	(11.894)	
Female	0.514	0.003	11215
	(0.004)	(0.010)	
Age	24.389	0.217	11183
	(0.175)	(0.510)	
Years of schooling	5.028	-0.773	9724
	(0.228)	(0.392)	

Note: This table presents mean differences between pilot and non-pilot (excl. Phase II) districts. Each row is a regression of the named variable on a constant and the indicator for pilot. Standard errors are clustered at sub-county level. Standard errors are presented in parenthesis. *** $p<0.01$, ** $p< 0.05$, * $p<0.10$.

Table A.2: Impact of old-age pension on earnings

	(1) *ihs*(Sales)	(2) *ihs*(Profits)	(3) *ihs*(Wages)
Panel A: Full Sample			
Exposure x Pilot	0.102***	0.095***	-0.030
	(0.034)	(0.034)	(0.027)
Mean in Non-Pilot (USD)	115	51	95
Observations	14609	14608	16793
R^2	0.194	0.194	0.212
Panel B: Women			
Exposure x Pilot	0.180***	0.174***	0.017
	(0.046)	(0.045)	(0.024)
Mean in Non-Pilot (USD)	93	36	74
Observations	8579	8579	9376
R^2	0.131	0.131	0.200
Panel C: Men			
Exposure x Pilot	0.036	0.024	-0.070
	(0.056)	(0.055)	(0.041)
Observations	6030	6029	7417
Mean in Non-Pilot (USD)	136	64	109
R^2	0.219	0.213	0.214

Notes: The dependent variables are sales, profits, and wages, transformed using Inverse Hyperbolic Sine Transformation (IHS). All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level *** p<0.01, ** p< 0.05, * p<0.10.

Table A.3: Testing for differences in the effect of pension on earnings among men and women

	Full Sample			Excl. Kampala		
	(1) *ihs*(sales)	(2) *ihs*(profits)	(3) *ihs*(wages)	(4) *ihs*(sales)	(5) *ihs*(profits)	(6) *ihs*(wages)
Exposure x Pilot x Female	0.183***	0.178***	0.038	0.181***	0.177***	0.033
	(0.038)	(0.038)	(0.024)	(0.038)	(0.037)	(0.024)
Exposure x Pilot x Male	0.036	0.024	-0.096*	0.036	0.023	-0.097*
	(0.058)	(0.055)	(0.050)	(0.059)	(0.055)	(0.050)
P-values ($\beta_1 = \beta_2$)	0.079	0.051	0.011	0.083	0.053	0.015
Observations	14609	14608	16995	13843	13842	15781
R^2	0.195	0.195	0.212	0.191	0.191	0.207

Notes: The dependent variables are sales, profits, and wage, transformed using inverse hyperbolic since transformation. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.4: Placebo experiment on Earnings: Based on 55 to 64 years

	ihs(Sales)		*ihs*(Profits)	
	(1) All	(2) Women	(3) All	(4) Women
Exposure x Pilot	0.099**	0.179***	0.091**	0.171***
	(0.038)	(0.059)	(0.037)	(0.056)
Placebo Exposure x Pilot	0.018	0.040	0.027	0.030
	(0.097)	(0.134)	(0.096)	(0.132)
Observations	18914	11243	18915	11243
R^2	0.200	0.113	0.200	0.112

Notes: The dependent variables are *ihs*(sales) and *ihs*(profits). All the models are estimated on a sample of individuals aged 18 to 64 years. True exposure is constructed from equation 1 while placebo exposure is constructed from equation 4 by adjusting the lower threshold to 55. The coefficients are estimated from equation 5. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

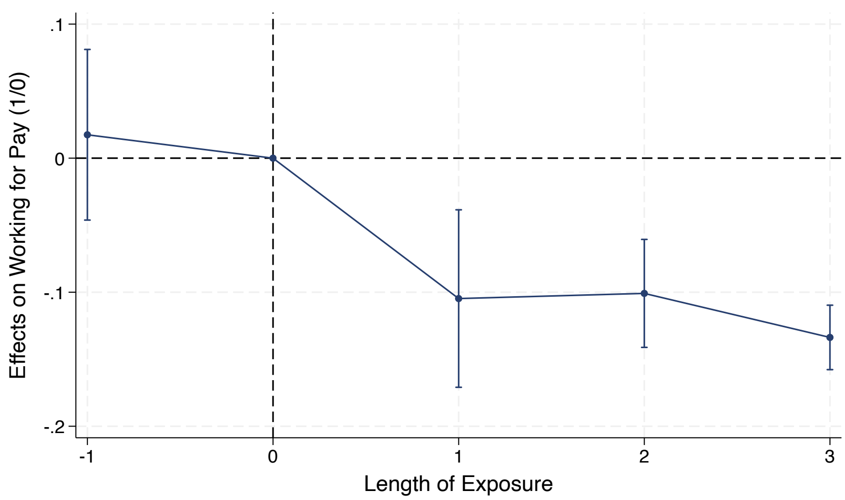

Figure A.1: Effects of old-age pension on the likelihood of working for pay: recipients

Note: This graph is estimated using the staggered Difference-in-Difference approach recently proposed by De Chaisemartin & d'Haultfoeuille (2020). The average treatment effect is -0.185 with standard error of 0.033. The graph shows how old-age pension affects the likelihood that eligible individuals are engaged in work for pay (paid employees). One caution to keep in mind while interpreting the graph is that there are gap years after the first year of treatment which are not depicted on the graph.

Table A.5: Impact of the pension on earnings (Excl. Kampala)

	(1) *ihs*(Sales)	(2) *ihs*(Profits)	(3) *ihs*(Wages)
Panel A: Full Sample			
Exposure x Pilot	0.102***	0.094***	-0.034
	(0.034)	(0.033)	(0.026)
Mean in Non-Pilot (USD)	107	48	89
Observations	13843	13842	15598
R^2	0.190	0.190	0.207
Panel B: Women			
Exposure x Pilot	0.176***	0.170***	0.013
	(0.046)	(0.045)	(0.023)
Mean in Non-Pilot (USD)	86	34	68
Observations	8075	8075	8683
R^2	0.129	0.128	0.198
Panel C: Men			
Exposure x Pilot	0.035	0.023	-0.076*
	(0.058)	(0.056)	(0.040)
Observations	5768	5767	6915
Mean in Non-Pilot (USD)	126	61	102
R^2	0.209	0.204	0.210

Notes: The dependent variables are indicated in the column headings. These are transformed using inverse hyperbolic sine transformation. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.6: Impact of the pension on earnings: Alternative control groups

	Only Phase II as control			Including all districts		
	(1)	(2)	(3)	(4)	(5)	(6)
	ihs(Sales)	*ihs*(Profits)	*ihs*(Wages)	*ihs*(Sales)	*ihs*(Profits)	*ihs*(Wages)
Panel A: Full Sample						
Exposure x Pilot	0.081**	0.078**	-0.057**	0.098***	0.092***	-0.038
	(0.033)	(0.033)	(0.026)	(0.028)	(0.028)	(0.025)
Observations	5868	5867	6358	18589	18588	21207
R^2	0.180	0.179	0.184	0.191	0.190	0.210
Panel B: Women						
Exposure x Pilot	0.168**	0.166**	-0.000	0.175***	0.170***	0.013
	(0.061)	(0.060)	(0.036)	(0.044)	(0.043)	(0.024)
Observations	3254	3254	3372	10792	10792	11717
R^2	0.133	0.133	0.142	0.130	0.130	0.193
Panel B: Men						
Exposure x Pilot	0.029	0.021	-0.116***	0.038	0.026	-0.089**
	(0.064)	(0.060)	(0.036)	(0.055)	(0.053)	(0.037)
Observations	2614	2613	2986	7797	7796	9490
R^2	0.197	0.196	0.193	0.210	0.205	0.213

Notes: The dependent variables are indicated in the column headings. These are transformed using IHS. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.7: Impact of old-age pension on labor supply: Excluding Kampala

	(1) All	(2) Self-Employed	(3) Employees
Panel A: Full Sample			
Exposure x Pilot	0.013*	0.016**	0.018
	(0.007)	(0.007)	(0.012)
Mean in Non-Pilot	37	34	46
Observations	15654	12523	4460
R^2	0.097	0.092	0.074
Panel B: Women			
Exposure x Pilot	0.014**	0.018***	0.008
	(0.006)	(0.006)	(0.021)
Mean in Non-Pilot	33	31	42
Observations	7839	6846	1415
R^2	0.085	0.079	0.123
Panel C: Men			
Exposure x Pilot	0.013	0.017	0.023
	(0.012)	(0.014)	(0.017)
Observations	7815	5677	3045
Mean in Non-Pilot	41	38	48
R^2	0.095	0.101	0.090

Notes: In all models, the dependent variable is *ihs*(hours worked). All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.8: Impact of old-age pension on labor supply: All districts

	(1) All	(2) Self-Employed	(3) Employees
Panel A: Full Sample			
Exposure x Pilot	0.009	0.013**	0.015
	(0.006)	(0.006)	(0.010)
Mean in Non-Pilot	37	34	46
Observations	21025	16589	6169
R^2	0.096	0.091	0.067
Panel B: Women			
Exposure x Pilot	0.012	0.015**	0.007
	(0.007)	(0.007)	(0.017)
Mean in Non-Pilot	33	31	43
Observations	10438	8998	2001
R^2	0.090	0.081	0.113
Panel C: Men			
Exposure x Pilot	0.008	0.012	0.016
	(0.012)	(0.013)	(0.015)
Observations	10587	7591	4168
Mean in Non-Pilot	41	38	48
R^2	0.088	0.092	0.077

Notes: In all models, the dependent variable is *ihs*(hours worked). All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.9: Testing for differences in the effect of pension on labor supply among men and women

	Full Sample			Excl. Kampala		
	(1)	(2)	(3)	(4)	(5)	(6)
	All Categories	Self-Employed	Employee	All Categories	Self-Employed	Employee
Exposure x Pilot x Female	0.019**	0.021***	0.010	0.020**	0.021***	0.013
	(0.007)	(0.007)	(0.017)	(0.007)	(0.007)	(0.017)
Exposure x Pilot x Male	0.005	0.011	0.013	0.005	0.011	0.015
	(0.014)	(0.015)	(0.014)	(0.015)	(0.015)	(0.015)
P-values ($\beta_1 = \beta_2$)	0.446	0.613	0.892	0.436	0.595	0.933
Observations	16626	12930	5050	15654	12523	4460
R^2	0.105	0.099	0.075	0.097	0.092	0.074

Notes: In all models, the dependent variable is *ihs*(hours worked). The coefficients in the first row indicate the effects of the pension on women. The coefficients in second row are the effects on men. All the models are estimated on a sample of individuals age 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.10: Effects of old-age pension on likelihood of working for pay and being self-employed

	Working for pay			Self-Employed in main job		
	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Women	Men	Full Sample	Women	Men
Exposure x Pilot	-0.000	0.002	-0.001	0.006	0.008	0.000
	(0.003)	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Interview Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Non-Pilot	0.431	0.342	0.534	0.455	0.426	0.485
Observations	25663	13548	12115	21828	10771	11057
R^2	0.173	0.109	0.175	0.126	0.114	0.172

Notes: The dependent variable is a dummy variable taking 1 if the individual is working for pay or self-employed, 0 otherwise. All the models are estimated on a sample of individuals aged 18 to 64 years. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.11: Effects of old-age pension on time spent on household task

	(1) All	(2) Women	(3) Men
Exposure x Pilot	0.008	0.009	0.001
	(0.007)	(0.009)	(0.012)
Additional Controls	Yes	Yes	Yes
Interview Month FE	Yes	Yes	Yes
Location FE	Yes	Yes	Yes
Mean in Non-Pilot	14	20	7
Observations	25661	13548	12113
R^2	0.319	0.048	0.128

Note: In all the models,the dependent variable is *ihs*(hours). All the models are estimated on a sample of individuals aged 18 to 64 years. All models include control variables (including interaction terms with PPE), month of interview and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.12: Placebo on Working Capital

	(1) All	(2) Women	(3) Men
Exposure x Pilot	0.063*	0.091*	0.037
	(0.034)	(0.045)	(0.064)
Placebo Exposure x Pilot	-0.032	0.040	-0.089*
	(0.050)	(0.080)	(0.044)
Additional Controls	Yes	Yes	Yes
Interview Month FE	Yes	Yes	Yes
Location FE	Yes	Yes	Yes
Mean in Non-Pilot (USD)	33.2	33.0	33.4
Observations	12260	6885	5375
R^2	0.213	0.243	0.199

Notes: The dependent variable is working capital, transformed using IHS. All the models are estimated on a sample of individuals aged 18 to 64 years. True exposure is constructed from equation 1 while placebo exposure is constructed from equation 4. The coefficients are estimated from regression equation 5. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.13: Impact of old-age pension on remittances received by the household

	(1)	(2)
Exposure x Pilot	-0.125**	-0.117**
	(0.052)	(0.050)
Additional Controls	Yes	Yes
Interview Month FE	Yes	Yes
Location FE	No	Yes
Mean in Non-Pilot (USD)	69	69
Observations	12168	12168
R^2	0.070	0.097

Notes: The dependent variable is remittance transformed using Inverse Hyperbolic Sine transformation. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.14: Old-age pension and labor market outcomes: Controlling for remittances

	Earnings			Hours worked		
	(1)	(2)	(3)	(4)	(5)	(6)
	ihs(Sales)	*ihs*(Profits)	*ihs*(Wage)	All	Self-Employed	Employees
Panel A: Full Sample						
Exposure x Pilot	0.102***	0.095***	-0.030	0.013*	0.016**	0.016
	(0.034)	(0.034)	(0.026)	(0.007)	(0.007)	(0.012)
Observations	14609	14608	16793	16626	12930	5050
R^2	0.194	0.194	0.212	0.104	0.099	0.075
Panel B: Women						
Exposure x Pilot	0.180***	0.174***	0.017	0.014**	0.018***	0.009
	(0.046)	(0.045)	(0.024)	(0.007)	(0.006)	(0.020)
Observations	8579	8579	9376	8286	7058	1659
R^2	0.131	0.131	0.200	0.095	0.085	0.119
Panel C: Men						
Exposure x Pilot	0.033	0.021	-0.069*	0.013	0.017	0.020
	(0.053)	(0.052)	(0.040)	(0.012)	(0.014)	(0.016)
Observations	6030	6029	7417	8340	5872	3391
R^2	0.220	0.213	0.214	0.102	0.109	0.092

Notes: In columns 1 to 3, the dependent variables are *ihs*(sales), *ihs*(profits) and *ihs*(wages). In columns 4 to 6, the dependent variable is *ihs*(hours worked). All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. In this particular table, I also control for the monetary value of remittances received by the households. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.15: Gender of the recipient and effect of pension on earnings and working capital

	(1) *ihs*(Sales)	(2) *ihs*(Profits)	(3) *ihs*(Wages)	(4) *ihs*(W. Capital)
Panel A: Full Sample				
Female-Based Exposure x Pilot	0.019	0.029	-0.035	0.028
	(0.047)	(0.044)	(0.049)	(0.026)
Male-Based Exposure x Pilot	0.124*	0.103	-0.072	0.072*
	(0.063)	(0.066)	(0.062)	(0.038)
P-values ($\beta_1 = \beta_2$)	0.248	0.418	0.605	0.389
Mean in Non-Pilot (USD)	107.6	48.5	95.9	45.6
Observations	14628	14627	9544	18696
R^2	0.232	0.230	0.476	0.213
Panel B: Women				
Female-Based Exposure x Pilot	0.013	0.027	-0.001	-0.009
	(0.057)	(0.054)	(0.058)	(0.033)
Male-Based Exposure x Pilot	0.197***	0.166**	-0.049	0.115***
	(0.070)	(0.074)	(0.041)	(0.042)
P-values ($\beta_1 = \beta_2$)	0.068	0.150	0.516	0.045
Mean in Non-Pilot (USD)	87.6	35.3	78.9	41.5
Observations	8595	8595	6111	11045
R^2	0.151	0.148	0.334	0.152
Panel C: Men				
Female-Based Exposure x Pilot	0.034	0.040	-0.043	0.065
	(0.063)	(0.059)	(0.078)	(0.047)
Male-Based Exposure x Pilot	0.060	0.048	-0.063	0.011
	(0.085)	(0.087)	(0.130)	(0.053)
P-values ($\beta_1 = \beta_2$)	0.829	0.943	0.897	0.478
Mean in Non-Pilot (USD)	125.3	60.2	106.4	49.2
Observations	6033	6032	3433	7651
R^2	0.284	0.275	0.600	0.295

Notes: The dependent variables are indicated in respective columns. All dependent variables are transformed using IHS. Column 4 is the working capital for self-employed individuals. The coefficients are estimated from equation 10. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.16: Gender of the recipient and effect of pension on labor supply

	(1) All Categories	(2) Self-Employed	(3) Employees
Panel A: Full Sample			
Female Exposure x Pilot	0.005	0.013	-0.010
	(0.012)	(0.011)	(0.037)
Male Exposure x Pilot	0.028***	0.026**	0.035
	(0.010)	(0.011)	(0.025)
P-values ($\beta_1 = \beta_2$)	0.176	0.470	0.403
Mean in Non-Pilot	36.8	33.6	45.7
Observations	16652	12944	5062
R^2	0.094	0.083	0.076
Panel B: Women			
Female Exposure x Pilot	-0.001	0.001	0.019
	(0.021)	(0.021)	(0.021)
Male Exposure x Pilot	0.048***	0.047***	0.030
	(0.016)	(0.017)	(0.042)
P-values ($\beta_1 = \beta_2$)	0.146	0.174	0.850
Mean in Non-Pilot	32.9	30.6	42.7
Observations	8302	7070	1663
R^2	0.077	0.066	0.103
Panel C: Men			
Female Exposure x Pilot	0.013	0.032*	-0.038
	(0.019)	(0.016)	(0.062)
Male Exposure x Pilot	0.008	0.003	0.029
	(0.023)	(0.025)	(0.034)
P-values ($\beta_1 = \beta_2$)	0.896	0.438	0.455
Observations	8350	5874	3399
Mean in Non-Pilot	40.9	37.3	47.3
R^2	0.100	0.101	0.087

Notes: In all models, the dependent variable is *ihs*(hours worked). The coefficients are estimated from equation 10. All the models are estimated on a sample of individuals aged 18 to 64. All models include control variables (including interaction terms with PPE), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

Table A.17: Effects of old-age pension on the likelihood of a household member migrating for work

	(1)	(2)
Exposure x Pilot	0.002	0.002
	(0.003)	(0.003)
Additional Controls	Yes	Yes
Interview Month FE	Yes	Yes
Location FE	No	Yes
Mean in Non-Pilot	0.065	0.065
Observations	12168	12168
R^2	0.033	0.046

Note: The dependent variable is a dummy variable taking 1 if the household has at least one member who migrated for work, 0 otherwise. All models include control variables (which include interaction terms between PPE and household/individuals characteristics), interview month fixed effects, and location fixed effects. Standard errors are clustered at sub-county level. *** p<0.01, ** p< 0.05, * p<0.10.

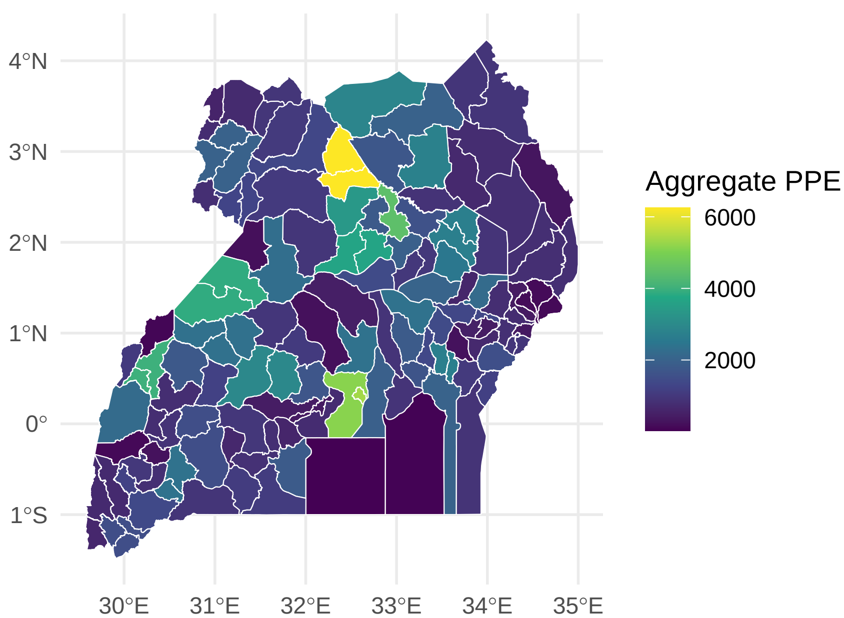

Figure A.2: **Variation in aggregate potential pension exposure by districts**

Note: This map shows variation in aggregate potential pension exposure across all the districts in Uganda. It is generated by aggregating the exposure computed using equation 1 for all the households in each district