Mitigating the Consequences of Job Loss in Low-Income Countries: Evidence from Ethiopia*

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Abstract

Job loss is an understudied risk for formal workers in low-income countries, and it is unclear how to optimally insure workers against it. We provide evidence on the impacts of job loss among female factory workers in Ethiopia and on how these impacts can be mitigated. We leverage quasi-experimental variation in job loss, experimental variation in job-loss support payments, and high-frequency data spanning a period of 13 months after displacement. We find that job loss is a persistent shock that reduces employment and consumption spending for longer than one year, and almost doubles the rate of poverty. An additional lump-sum payment encourages early spending and reduces both overall and manufacturing employment. In contrast, providing an equivalent amount in monthly tranches – a payment modality preferred by a majority of workers – enables workers to better smooth consumption expenditures without negative employment effects. We show that workers have high willingness to pay for additional job-loss insurance, but also heterogeneous preferences over the payment modality. This generates a key trade-off between workers' private welfare and the government industrialization objectives: allowing workers to choose their preferred insurance product maximises their surplus, but at the cost of reducing future manufacturing employment.

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1 Introduction

Job loss is an understudied risk for formal workers in low-income countries. An expanding literature studies the barriers that prevent the creation of productive formal jobs in low-income settings (Caria et al., 2024; Ulyssea, 2020). However, once such productive matches are formed, workers are often at significant risk of being laid off (Donovan et al., 2023). Further, in low-income countries, existing insurance against job loss is limited. The only source of income support after job loss comes from Severance Pay (SP) policies, which mandate employers to pay an unconditional lump-sum to displaced formal workers (Gerard et al., 2025). Unemployment Insurance (UI) programs — programs that offer regular payments conditional on remaining without a formal job —- are non-existent.[1]

Should formal workers in low-income countries be offered additional financial support after layoff? We currently lack the evidence necessary to answer this question (Gerard et al., 2025). In high-income countries, job loss has high and persistent impacts on workers' long-term consumption and earnings (Ganong and Noel, 2019; Rose and Shem-Tov, 2023); and job-loss insurance policies play a key role in mitigating welfare losses (Landais and Spinnewijn, 2021). Whether these effects are larger or smaller for formal workers in low-income countries remains unclear. On the one hand, many formal jobs in low-income countries are likely to be low skill, and may thus be replaced easily. Workers may also have access to significant informal insurance opportunities. This would suggest a modest welfare loss from formal job displacement, and hence a limited need for additional job-loss insurance. On the other hand, worker search frictions and firm demand constraints may slow-down re-employment, exacerbating the welfare cost of job-loss and strengthening the need for additional job-loss support. Which of these two alternatives holds more truth remains unclear.

Further, if there is a case for expanding job-loss support, how should this support be provided? In low-income countries, all financial support after job loss comes in a single, lump-sum transfer. However, Gerard and Naritomi (2021) show that consumption after job loss can be over-sensitive to cash-on-hand. This would suggest that forms of insurance that spread payments over-time may allow workers to achieve better consumption smoothing in the aftermath of a shock. Spreading payments may also reduce workers' ability to make productive investments, e.g., starting a business or migrating to a different labor market, that facilitate remployment. In the absence of credible evidence on the impacts of different forms of job-loss support, it is impossible to design appropriate policies.

In this paper, we provide experimental and quasi-experimental evidence on these questions for a sample of female, formal factory workers in Ethiopia. This setting is particularly

[1]We will use the term 'job-loss insurance' and 'job displacement insurance' interchangeably to refer to all government policies aimed at providing financial support to formal workers after job loss. Thus, we will consider (government-mandated) SP and (government-run) UI as different forms of job-loss insurance.

interesting for two reasons. First, the blue collar, industrial jobs that are the focus of our study have been central to the growth processes of many of today's rich economies in America, Asia, and Europe (Chang, 2009; Smil, 2013; Tsurumi, 1990; Zamagni, 1990), and are key to the development strategies of several low-income countries. Second, governments often believe that factory jobs have strong development externalities and thus invest significant resources to increase their availability (Juhász et al., 2023; Oqubay, 2015). Many of these investments — e.g., those related to migration, training, and life-style adjustments (Bick et al., 2018; Blattman and Dercon, 2018; Lagakos et al., 2023; Thompson, 2017) — are worker specific, and are lost if a worker leaves the sector and migrates away. The potential negative employment effects of job-loss insurance policies are thus of particular concern in these settings.

Our design — a field experiment where one firm suffered a trade shock — enables us to tackle the two main constraints that have hampered research on job loss in low-income countries: the lack of credible research designs, and the lack of longitudinal high-frequency data. First, we *quasi-experimentally* estimate the impacts of job loss by comparing the outcomes of two samples: 1,410 workers laid off by the one firm affected by the shock (the *displaced* sample), and 403 comparable workers employed in a nearby unaffected firm (the *non-displaced* sample), who remained employed with very high rates over the entire period.[2] Our methods are similar to those employed in the recent literature on job loss in high- and middle-income countries (Gerard and Naritomi, 2021; Kolsrud et al., 2018). Second, we *experimentally* estimate the impacts of offering additional financial support after job loss. All workers in the displaced sample receive a severance pay package worth about 2.5 monthly wages from their employer. We offer additional financial support to randomly chosen workers either in a single lump-sum or in monthly tranches. The lump-sum treatment provides evidence on the impacts of lump-sum transfers at job loss, which is the only form of job displacement insurance available to formal workers in low-income countries. We calibrate this transfer to approximate the difference in job-loss support available to individuals in middle-income countries as South Africa and that available to Ethiopian workers. As shown in Gerard and Naritomi (2021), however, consumption after job loss can be over-sensitive to cash-on-hand. We thus compare lump-sum payments to an equivalent (inflation-adjusted) monthly transfer. The monthly treatment spreads payments over time like a UI program — and may thus enhance consumption-smoothing — but, unlike UI, does not condition payments on reemployment and can thus be implemented in a low-capacity environment.

Ethiopia offers an ideal setting to study these questions. The country has invested heavily in the promotion of export-based industrialization, in particular through the creation of several industrial parks. The Hawassa Industrial Park, where our study takes place, focuses

[2]We describe this shock and the selection of the two samples in more detail in section 2.1. Note that all firms in the Hawassa Industrial Park specialize in garment manufacturing and that the socio-demographic characteristic of these two samples are highly balanced.

on garment manufacturing, hosts twenty international investors, and employs over 25,000 workers. Further, Ethiopia has been subject to several shocks that have affected firm growth, and in particular exporting firms. These include internal conflict, exchange-rate shocks, and climate shocks. Mass layoffs are thus a real threat to workers in this setting.

We have three central findings. First, we show that in our context job loss is a persistent shock, which reduces long-run employment and consumption expenditure and generates strong reverse migration towards rural areas. Second, we show that lump-sum payments induce large early spending, reduce employment, including employment in the industrial park, and generate additional migration towards workers' original rural locations. In contrast, monthly payments enable workers to substantially smooth their consumption expenditure, and do not affect employment or migration outcomes. Moreover, the consumption-smoothing gains are driven by workers with a revealed preference for monthly payments at baseline. These findings highlight the benefits of providing job loss insurance payments in tranches. Yet, just under half of workers would prefer a lump-sum payment. This creates a tradeoff that we evaluate quantitatively: allowing workers to choose their preferred job-loss insurance scheme maximises workers' welfare, at the cost of reducing industrial employment and reversing some rural-to-urban migration. Third, we show that workers' willingness to pay for job-loss insurance is high, and would be sufficient to offer an actuarially fair product in our setting.

What are economists' priors on these results? In a prediction survey administered to 84 economists, they correctly expect large negative impacts from job loss and the short-term consumption spending benefits from job-loss payments, including the sensitivity of consumption to cash-on-hand. However, they underestimate the employment effects, as well as the longer-run effects, of these payments. They also predict that willingness-to-pay for job loss payments will be higher than an actuarially fair insurance premium, which they view as the key criterion to decide whether to roll out the policy or not. So, economists believe that there is a case for expanding job-loss insurance in our context, but underestimate some of the key effects of such a policy.

In the next paragraphs, we describe our results in further detail. First, we document large impacts of job loss on employment and consumption spending. Most displaced control workers are unable to secure a new job right after the shock, and thus search for work (mostly blue-collar jobs in the industrial park) or migrate out of the city (mostly towards small home towns and rural locations). Employment rates grow for about six months and then stabilize, while migration rates increase gradually. Thirteen months after job loss, the displaced controls are 27 percentage points less likely to be employed than the workers in the non-displaced sample, and 30 percentage points less likely to be living in Hawassa. Over this period, labor income is halved. These losses are partly mitigated by formal insurance — statutory SP — and informal insurance — transfers from family and friends — which, combined, provide resources that are about half as large as the loss in labor income. Overall, over this period of

time, total consumption expenditure drops by 9 percent on average, and is 20 percent lower by month thirteen. Further, extreme consumption poverty almost doubles. Overall, these results suggest that job-loss generates a large fall in welfare for the workers in our sample.

Second, monthly payments significantly raise total consumption expenditure over the thirteen months after job loss, while lump sum payments do not have a significant average impact on expenditure. On average, monthly payments halve the consumption expenditure drop induced by job loss. Lump-sum payments, on the other hand, raise consumptions expenditure strongly just after the shock, but impacts on consumption shrink quickly. On average, the boost to consumption expenditure is 75 percent lower than that generated by monthly payments and is not significant. How do these results come about? The most important mechanism revolves around the employment response to job-loss support. In particular, lump-sum payments reduce employment by about 7.1 percentage points on average and by 8.5 percentage points at endline. Work in the Hawassa Industrial Park drops by 8.3 percentage points. Also, migration increases by 7.6 percentage points after 13 months. Monthly payments, on the other hand, do not affect employment or migration in the long run.

Two of these results are hard to reconcile with canonical models of consumption smoothing and job search (Chetty, 2008): the short-run spike in consumption spending generated by lump-sum transfers, and the fact that lump-sum payments reduce employment while monthly payments do not. We shed light on the mechanisms underlying these two results by leveraging an incentivized policy preference measure collected at baseline. First, we show that the short-run excess consumption induced by lump-sum payments compared to the monthly treatment is driven by workers who wanted to be allocated to the monthly treatment in order to control their spending. This finding is consistent with models of temptation spending or sophisticated present bias (Gerard and Naritomi, 2021). Second, we show that the reduction in industrial park employment and the increase in migration that lump-sum payments generate compared to monthly payments are instead driven by workers who preferred to be allocated to lump-sum payments at baseline. These workers use the liquidity provided by the lump-sum treatment to finance reverse migration and a transition away from industrial work.

Third, we find substantial willingness to pay for additional job-loss insurance. On average, workers are willing to pay 2.4 percent of the insurance payout every month as premium for additional coverage. Importantly, this figure is well above the actuarially fair price that we calculate for this sample (using either administrative data on separations for all formal workers or data on realized separations in our sample). These conclusions hold irrespective of whether the additional support is provided in the form of a lump-sum or in tranches: the two payment modalities generate similar WTPs (with monthly payments generating slightly larger WTP), although there is substantial heterogeneity in preferences between the two payment modalities across workers.

Further, we shed light on the optimal payment structure, i.e., whether payments should be lump-sum, monthly, or whether individuals should be allowed to choose their preferred payment modality. To do this, we compare the worker surplus from the policy – their marginal rate of substitution between employment and after job loss for a given payment policy – to the two externalities it generates: (i) a fiscal externality (tax payments are lower if workers delay re-entry in the formal labor market), and (ii) an 'industrial policy' externality (coming from the reduction in industrial employment). This analysis generates three insights. First, mandated monthly payments dominate mandated lump-sum payments, since they generate slightly higher worker surplus (i.e., on average workers prefer monthly payments) and lower externalities. Second, surplus can be raised substantially by allowing workers to choose their preferred scheme because of the heterogeneity in workers' preferences over the payment method (i.e., 42% prefer a lump-sum payment over monthly payments). However, compared to a scenario where the governments mandates monthly payments, this also generates larger fiscal and industrialization externalities. Third, we quantify these externalities and find that allowing for choice is optimal in our setting, a conclusion that is robust to a wide range of assumption about the size of the externalities generated by jobs in the Hawassa Industrial Park.

Our results make three key contributions to the literature. First, we provide unique evidence on the persistent employment and consumption expenditure impacts of job loss in a low-income country. Job loss in higher-income countries has received remarkable attention in the literature over the last thirty years (e.g., Ganong and Noel, 2019; Jacobson et al., 1993; Rose and Shem-Tov, 2023). More recently, several papers identify the impacts of job loss in middle-income countries such as Brazil (e.g., Britto et al., 2022; Gerard and Gonzaga, 2021; Gerard and Naritomi, 2021). Whether job loss has more or less impactful consequences in a low-income country is an open question. On the one hand, less productive matches may be easier to replace, since they do not involve high levels of firm-specific human capital (Rose and Shem-Tov, 2023). This would suggest that job loss may generate a less persistent employment shock in low-income, lower-productivity settings. However, search frictions may reverse this conclusion (Caria et al., 2024). Similarly, the ability of workers to smooth consumption may be hampered by the limited provision of formal insurance, or supported by informal insurance mechanisms unavailable to worker in higher-income settings. Overall, our results suggest that, one year after job loss, the workers in our sample experience employment losses that are about twice as large as those of low-wage workers in the U.S. (Rose and Shem-Tov, 2023), and consumption expenditure losses that are similar to those experienced by Brazilian workers ineligible for job loss insurance payments (Gerard and Naritomi, 2021).

To our knowledge, the only other paper that estimates the impacts of the loss of a formal job in a low-income country is Hardy et al. (2024), which tracks the outcomes of Hawassa Industrial Park workers who lost their job during the early days of the COVID-19 pandemic.

Hardy et al. (2024) provide evidence that job-loss is associated with deteriorating food security and that migration is a common coping strategy. We complement their analysis with a more robust causal identification strategy, detailed consumption expenditure data, and a focus on a time period where no other major layoffs occurred in the industrial park.

Second, we present unique experimental evidence on the impacts of providing more generous financial support after job loss. This contributes to a nascent literature that studies social insurance programs in low-income countries and in particular job-loss insurance (Gerard et al., 2025). Most importantly, we provide the first empirical evidence for the promise of unconditional payments in tranches at job loss, a policy proposed by Gerard and Naritomi (2021) but not implemented by any government around the world. Moreover, our experimental design provides direct support for the type of models (e.g., sophisticated present-bias) used by Gerard and Naritomi (2021) to rationalize their consumption expenditure findings in the Brazilian setting and motivate such a policy. In a context where workers' consumption may be over-sensitive to cash-on-hand, sophistication is key for a policy offering a choice of payment modality to increase welfare. Our empirical findings also point to an additional and unexpected benefit in terms of reemployment of payments in tranches over lump-sum payments, which is the only modality of payment for job-loss insurance policies in low-income countries. Indeed, standard models of consumption smoothing and job search would predict similar employment effects between these two payment modalities (Chetty, 2008).

More generally, no study to date has been able to use experimental variation in the size or modality of financial support to assess the desirability of a more generous system of job-loss insurance. A recent paper provides diff-in-diff evidence from Mauritius that UI encourages informal employment at the expense of formal work (Liepmann and Pignatti, 2024). This strong substitution effect is not relevant in our context, since our payments are not conditional on formal employment. However, while our results on the negative employment effects of lump-sum payments at job loss are in line with existing evidence from Brazil (Britto et al., 2022), they are perhaps surprising in light of the contrasting evidence that is emerging from the literature on cash transfers to the rural poor and the literature on microfinance for entrepreneurs. These studies often find that larger, lumpy transfers encourage employment (Bari et al., 2024; Kansikas et al., 2023). The urban workers in our sample, on the other hand, may have fewer opportunities for short-term productive investments in agriculture or urban entrepreneurship, which may reconcile the different results.

Third, we provide the first experimental evidence on workers' willingness to pay for job-loss insurance. Methodologically, our strategy is close to Landais and Spinnewijn (2021), who combine a discrete choice embedded in the Swedish social security system with predicted unemployemnt risk to measure the value of UI. We extend the approach of Landais and Spinnewijn (2021) by presenting a method that relies exclusively on experimental variation

to measure WTP for additional job loss insurance. Our results generate several new lessons. Most importantly, we show that, in our context, WTP for additional job-loss insurance is higher than actuarially fair insurance premia, but also that there is a crucial tradeoff between insurance designs that maximise worker welfare through choice, and designs that favor the maximisation of industrial employment. These results contribute to a growing literature on behavioral mechanism design (e.g., Dizon-Ross and Zucker, 2023). Further, they advance our understanding of social protection in low-income countries by documenting a hitherto poorly documented trade-off between insurance and industrialization objectives.

2 The RCT and the Natural Experiment

2.1 Context

Over the past decade, the Government of Ethiopia has actively promoted industrialization by establishing eighteen Special Economic Zones, commonly known as "industrial parks," focusing on export-oriented light manufacturing. Among these, Hawassa Industrial Park, located in Hawassa, Sidama Region, is one of the largest and was one of the first to be created. As of 2022, the park housed over 20 firms in the ready-made garment (RMG) industry, employing nearly 30,000 workers (see Figure A1 in the Appendix), with approximately 90% being female. At full capacity, Hawassa Industrial Park can employ up to 60,000 individuals.

To attract foreign investment, Ethiopia offers various incentives for industrial parks, including tax exemptions and high-quality infrastructure. Prior to 2021, under the Africa Growth and Opportunity Act (AGOA), investors were able to export Ethiopian goods to the United States with preferential tariffs. However, Ethiopia lost its AGOA beneficiary status on December 31, 2021, due to concerns related to the ongoing civil war in the Tigray region. While many investors primarily exported to the European Union or the Middle East and were largely unaffected by this change, those linked to the U.S. market faced a decline in orders and competitiveness.

The mass layoff examined in this study was directly triggered by the loss of Ethiopia's AGOA status. The RMG company involved—our partner in a previous project, henceforth the "study firm" —was significantly reliant on U.S. orders, which dramatically decreased by mid-2022. In June 2022, the study firm announced plans to lay off nearly two-thirds of its workforce, effective at the end of August 2022. Layoffs began with employees who had less than six months of tenure, followed by those with up to 18 months of experience, and finally included workers with 24 months of tenure. These three waves of layoffs occurred within a four-week period.

It is important to note that, to our knowledge, no other mass layoff event occurred at a similar time in the Hawassa Industrial Park. In fact, as shown in Figure A1, the level of employment in the park was fairly stable in the lead-up and the aftermath of the mass layoff.

This suggests that the study firm was particularly affected by the termination of AGOA, and that, at the time of the mass layoff event, labour market conditions were otherwise not unusual.

2.2 The Displaced Sample and the Non-Displaced Sample

We collect data from two separate samples of workers. First, a random sample of 1,410 factory-floor workers, drawn from a list 2,000 workers that were employed by the study firm in June 2022, and were made redundant at the end of August 2022. We call this the *displaced sample*, since every worker in this sample was displaced from their job in August 2022. We conduct our experiment with the displaced sample and randomly allocate individuals to one of three groups: the control group, the monthly payment group and the lump sum group, stratifying on four tenure categories and five job types.

Second, we survey a random sample of 403 workers employed by a neighboring RMG factory located in the Hawassa Industrial Park. These workers are drawn from a list of workers with fewer than 18 months of tenure at this factory. We call this the *non-displaced sample*, as there was no mass displacement of workers in this second factory at the time of our study.

Overall, we interview 1,813 workers. Their demographics are comparable to that of the typical garment manufacturing workers in Ethiopia. Our sample is 99.9 percent female and has a median age of 22 years old. 86.0 percent of our sample are single and 88.5 percent do not have children. The vast majority of the sample are protestant (91.5 percent), speak Sidama as their mother tongue (83.0 percent) and most are also fluent in Amharic (72.3 percent). 94.8 percent of the sample have at least high school education and 25.4 percent have some university education. Over 60 percent of the sample have moved to Hawassa from a rural area and 32 percent have moved from a small town.

The average worker in the non-displaced sample has a monthly income of about 1887 Birr per month and spends about 1817 Birr per month (see Figure A2). Consumption expenditure is thus 24% above the international extreme poverty line. Thus, while the average worker is not in extreme poverty, a moderate drop in consumption expenditure would push them below the threshold of extreme poverty.

2.3 Intervention: Job-loss payments

We study the impacts of two types of job-loss payments. Workers in the monthly payment group (488 workers) receive a monthly payment from our partner organization, EconInsight, for a period of 5 months after layoff, starting in October 2022. The payment is equal to 810 Ethiopian Birr (37.22 USD PPP) per month, which corresponds to about 60% of the median worker's salary in June 2022. Workers in the lump sum payment group (451 workers) receive a one-off payment from EconInsight in October 2022. The payment is equal to 3850 Ethiopian Birr (176.93 USD PPP), which is equivalent to the value of the monthly payments

discounted for expected inflation. Workers in the control group and in the non-displaced sample do not receive any payment from EconInsight.

These payments are announced at the end of our baseline interview, in July 2022. They are transferred directly to workers' bank accounts, minimizing transaction costs and enabling workers to collect their payments from any ATM in the country.[3] The transfers are presented as having the objective of supporting workers after job loss.

It is important to keep in mind that all workers in the displaced sample — including those in the control group — also receive a statutory job-loss insurance payment. This consists of a one-off severance payment from the study firm equal to about two or three months of their salary.[4] The majority of displaced workers receive their severance payment from the study firm in September 2022. The non-displaced sample does not receive any severance pay.

2.4 Randomization, Data Collection and Measurement of Primary Outcomes

Figure 1 shows the timeline for the project. Data was collected at seven different points in time: the baseline survey was conducted face-to-face in August 2022 and asked questions about the preceding three months; then, five rounds of high frequency surveys were conducted over the phone between November 2022 and August 2023, with each survey having a two-month recall period so that we cover outcomes between September 2022 and July 2022; the endline survey was conducted face-to-face in September and October 2023. The baseline survey collected detailed information about socio-demographic characteristics of study participants, their education, consumption and housing, autonomy from their partner and parents, their life satisfaction, physical and mental health, risk and time preferences, job-search, wage and self- employment, finances (income, saving and debt). Prior to treatment assignment, we collected an incentivized measure of policy preference. Additionally, following treatment assignment, we collected information about participants' expectations of their income, consumption, savings and debt over the next 8 months.

We measure the policy preferences of treated individuals using an incentivized choice task.[5] Workers in the lump-sum and monthly payments groups are asked to express their preference for either the lump-sum or the monthly payments. The task is incentivized: five workers will get their preference implement for real. Furthermore, the question is repeated

[3] All workers have a bank account since monthly salaries at the Hawassa Industrial Parks are generally paid through bank transfers.

[4] This payment was calculated as (1) workers who had been at the firm for over a year were paid 4000 birr (183.82 USD PPP); (2) workers who had been at the firm for less than a year were paid 3000 birr (137.87 USD PPP). In addition, workers were paid for unpaid leave days, which is commonly between 300 to 500 Birr (13-79-22.98 USD PPP).

[5] Workers in the control and non-displaced groups were not included in this elicitation exercise to avoid having to disclose the exact nature of the treatment received by other workers and potentially generating disappointment (all workers are aware that they are being randomized in different treatment groups, but are not given precise information about the different treatments).

for different values of the lump-sum transfer, to identify the strength of the preference for either payment modality. In the analysis below, we will study heterogeneity with respect to a dummy for having a strong preference for the monthly payment, which captures the quarter of the sample that most strongly prefers monthly payments. This sample would prefer monthly payments to a lump-sum alternative that pays 10 percent more. In order words, workers in this sample have a sizable positive willingness to pay for monthly payments.

The high-frequency survey collected information on the subset of these outcomes that we expected to change rapidly over time: consumption and housing, job-search, wage and self- employment and finances (income, saving and debt), life satisfaction and mental health, autonomy from their partner and parents. Table A1 and Table A2 give more information about the measurement of our primary and secondary outcome variables.

We randomized individuals into treatment groups stratifying over tenure and job type. We created three tenure groups, defined as tenure below six months, tenure between six and twelve month, and tenure greater than twelve months. We distinguished job-type using the following categories: cutters and helpers, printing and quality control, production workers, and other positions.

2.5 Balance

We run two separate sets of balance tests. First, we study balance between different experimental groups in the displaced sample. Second, we study balance between the displaced and the non-displaced sample.

The three experimental groups which compose the displaced sample are fairly balanced in terms of demographics and baseline measures of key outcome variables. We show this in Table A3. For each variable of interest, we report the p-values for a test of the null hypothesis that the variable is balanced (i) when comparing each treatment to the control condition, and (ii) when testing for balance across all experimental groups. From the overall test of balance across treatment groups, there are two significant differences at five percent level: the number of children (the lump sum group has a somewhat higher value compared to the control group) and the number of job applications in August (the lump sum group has a somewhat lower to the control group). At a ten percent significance level, there are three additional differences: savings in June and wages one and three months before baseline. It should be noted that there is limited variation in many baseline outcomes, so quantitatively small differences are often statistically significant.

The displaced and non-displaced sample are also largely balanced in terms of demographics. There are some differences in terms of baseline consumption and savings, which are not surprising given that the non-displaced sample was interviewed after the layoff announcement, and is likely to have hence have reduced spending and to have requested pay for unused

holidays. For a subset of the non-displaced sample (N = 403) we do observe one variable prior to the announcement: earnings. We find, reassuringly, that prior to the layoff announcement average earnings among displaced individuals were 1364.39 ETB per month, in line with those of the non-displaced sample.

2.6 Attrition

Table A15 shows the relationship between attrition, treatment assignment and respondents' baseline characteristics. We have low attrition: 92.4% of baseline respondents were re-interviewed at endline and 99.4 percent of individuals were contacted at least once after lay-off. We see no differential attrition by treatment group, with coefficients being small (between 0.2 and 2.1 percentage points) and insignificant. We similarly observe no difference between the non-displaced sample and the displaced control group for both measures of attrition. Only two co-variates predict attrition, having above median baseline savings and speaking Sidemegna, the native language in the area around Hawassa. The interactions of treatment dummies with covariates also does not significantly predict either attrition outcome (one out of 34 coefficients is significant at the 10 percent level). Overall, we conclude that attrition is low and non-differential and we are not concerned that this affects our main results.

2.7 Analysis

In this paper, we carry out two main empirical exercises.[6] First, we compare the outcomes of the individuals in the non-displaced sample to those of the displaced controls, re-weighted to ensure full balance on a large set of observable characteristics.[7]

Specifically, we estimate the following regression model using all observations from the non-displaced sample and the displaced control sample:

$$Y_i = \alpha + \beta \text{LaidOff}_i + \epsilon_i \tag{1}$$

where LaidOff_i is a dummy variable capturing the displaced controls, and Y_i is an outcome for worker i averaged over one of five pre-specified time periods: (i) the entire post-treatment period, (ii) the first two months after treatment (capturing a time when both the lump sum and the monthly group receive financial support), (iii) months three to five after treatment (capturing a time when only the monthly group receives support, and (iv) months 7-9 (capturing the period immediately following the withdrawal of support from the monthly treatment), (v)

[6] Our analysis was pre-registered here: https://www.socialscienceregistry.org/trials/10551.

[7] We re-weight based on covariates that capture (i) demographic characteristics (age, marital status, number of children, number of children in school, protestant dummy, mother language Sidamegna dummy, rural origin, and a housing quality index), (ii) education outcomes (tertiary and secondary education dummies, current enrollement dummies (any, university, diploma), dummies indicating future education plans (any, university, diploma)), (iii) preferences (life and job satisfaction, risk preferences, wage perceptions), (iv) labor income, and (v) expenditure in the three months before lay-off (core expenditure, total expenditure, net transfers, food expenditure).

month 13 (capturing outcomes at endline). This comparison captures the effect of the job loss event in September 2022. Note that 17 percent of individuals in the displaced sample are not employed at endline. We include their outcomes in the calculation of the counterfactual, since some displaced controls would have been laid-off or would have quit, had they not lost their job in September 2022. We also offer estimates that only use the non-displaced sample workers that remain employed throughout the period. This second comparison captures the effect of the job loss compared to a scenario where the worker remains in the employment throughout the period. This second benchmark is conceptually interesting, but is confounded by selection into job termination among workers in the comparison firm, and should thus not be over-interpreted.

Second, we compare the outcomes of treated displaced workers to those of the displaced controls. Specifically, we estimate the following regression model using all observations from the displaced sample:

$$Y_i = \alpha + \beta_1 \text{Lump sum}_i + \beta_2 \text{Monthly}_i + d_i + X_i + \epsilon_i \tag{2}$$

where Lump sum_i is a dummy variable capturing the lump sum group, Monthly_i is a dummy variable capturing the monthly group, d_i is a strata dummy, and X_i is a vector of controls chosen with a LASSO regression, including, when available, the baseline level of the outcome.[8] As before, Y_i is an outcome for worker i averaged over one of five pre-specified time periods.

3 Results

3.1 What Do Economists Expect?

We elicit economists' priors on the impacts of job loss for our sample, and the extent to which job-loss payments can mitigate these impacts. In the summer of 2023, before we started presenting our results, we posted a belief-elicitation survey on the Social Science Prediction Platform. To publicize the survey, we sent out personal email invitations to X economists selected from the list of individuals who would attend the NBER 2023 summer meetings in development and public economics. We also sent out an invite to complete our survey to a list of researchers affiliated to the Institute for Fiscal Studies in London. We obtained 84 responses. 31 of these are from development economists, 26 from public or labor economists, 17 from experimental or behavioral economists, and 10 from individuals in other fields. Four central results emerge from this survey.

First, economists expect imperfect consumption smoothing. We ask economists to predict the difference in core consumption expenditure between the non-displaced sample and the

[8] LASSO selects our controls from a long list of variables measured at baseline.

displaced controls, over a period of 6 months after layoff (Figure A4).[9] The median forecast is a drop in core consumption expenditure just under 20 percent. While some individuals report to expect an increase in consumption expenditure, most forecasts (9.3%) indicate a drop in consumption. The bulk of forecasts (the interquartile range) ranges from a drop in consumption of 7% and one of 70%.

Second, economists expect job-loss payments to provide a major boost to consumption expenditure in the short-run, but they also expect that these impacts will decay quickly. In the two months immediately following layoff, economists expect the monthly and lump sum payments to raise core consumption expenditure by 14.8% and 24.3% percent, respectively, essentially closing the entire drop in consumption expenditure caused by job loss (Figure A5). These treatment effects drop to 11.04% and 6.4% percent four months after job loss, and to 2.9% and 2.2% percent 10 months after job loss.

Third, economists do not expect job-loss payments to impact employment. Eight to twelve months after job loss, the median forecast of the treatment effect on employment is an exact zero for both interventions (Figure A6). The monthly treatment is expected to have a short-lived, small negative impact on employment, four months after treatment. On the other hand, the lump sum treatment is not expected to have any negative impacts on employment, possibly because economists expect it to fund migration (Figure A7) to a high-employment location.

Fourth, we find that most economists (65.2%) expect that willingness-to-pay for job-loss insurance will be sufficient to generate demand for insurance at an actuarially fair premium (Figure A8). And they report that the most important consideration to decide whether to support the policy is whether WTP is sufficient to pay for the policy or not (followed by whether the policy impacts mental health and the allocation of talent).

3.2 What Are the Impacts of Job Loss?

We next document the impacts of job loss on workers, over a period of approximately thirteen months after displacement. We have seven main findings, which show that job loss meaningfully reduces employment, income and consumption expenditure, but does not worsen mental well-being or autonomy from family.

First, job loss has a large and persistent impact on employment and labor income. We document impacts on employment in Figure 2a and Table 1[10]. Right after job loss, most individuals transition to non-employment, opening an employment gap between the displaced and non-displaced of about 70 percentage points. Employment rates among the displaced controls then grow steadily for about eight months, peaking at close to 60 percent, more than 30 percentage points below the employment rate in the non-displaced group. After the eighth month,

[9] Core consumptions expenditure includes expenditure on food and personal hygiene.

[10] The comparison between displaced controls and non-displaced is reported in the first row of coefficients in the second panel of the table

we do not observe further meaningful employment growth among the displaced controls, while the employment rates among the non-displaced remain approximately constant, so that the employment gap between the two groups stabilizes. Thirteen months after job loss, the employment gap between the non-displaced and the displaced controls is about 27 percentage points. We observe similar patterns if we focus on wage employment, which accounts for the bulk of paid work in our sample (Table A4 in the appendix). We do observe a small increase in self-employment among the non-displaced controls (Table A5 in the appendix), of about 3.6 percentage points on average, which is too small to meaningfully mitigate the large (47 percentage point on average) drop in wage employment over the thirteen months period. Impacts on labor income track those of employment, with an average reduction in monthly earnings about about 45 percent (629 ETB) over the thirteen months after displacement (Table A6).

Second, a large share of the displaced workers that are not employed actively search for a job. Overall, 80 percent of the displaced controls report to have searched for work in at least one of the thirteen months after job loss, and 66 percent of the sample searched for longer than two months. Among those that remain out of work for the entire period, 88 percent search for work at least once and 66 percent for longer than two months. Further, search rates remain substantial over the course of the study. One month after job loss, 48 percent of the non-employed displaced controls search for work. Thirteen months after job loss, 28 percent of the non-employed displaced controls search for work.[11] When they are searching, displaced controls spend at least 20 hours per week on job-search related activities, which is relatively high compared to other recent studies (Caria et al., 2024).

Third, job search and employment are largely focused on industrial work. Just after job loss, 87 percent of displaced jobseekers focus their search on industrial jobs. At endline, 89 percent of those who found employment work in an industrial job, and 79 percent in the Hawassa industrial park.[12] Also, among those that still search for work at endline, 80 percent focus their search on industrial jobs. This is striking in light of the fact that the majority of workers aspire to be either white-collar employees (38 percent) or service-sector employees (46 percent) five years after the endline. When asked why they do not currently search for jobs outside of the industrial park, workers most frequently answer that 'HIP jobs offers regular and reasonable hours' (39 percent), 'The location of HIP is convenient' (24 percent), 'The working environment at HIP is better' (23 percent), 'I do not know how to find other jobs' (23 percent). These results illustrate some of the frictions that slow down the reallocation of labor in the aftermath of trade shocks, and have been the subject of recent interest among trade economists (Caliendo et al., 2019; Dix-Carneiro et al., 2023, 2021).

[11]The substantial search rates of the non-employed at endline are not driven by individuals who found another job and then lost it. If we restrict the analysis to displaced controls that have been non-employed for at least six months, the proportion that searches for work at endline is unchanged at 28 percent.

[12]However, only 1 percent work for the same factory that laid them off the previous year.

Fourth, job loss causes a large increase in out-migration. Two months after job loss, 20 percent of the sample has left Hawassa (Table A7 and Figure 2b). This goes up to almost 30 percent of the sample at endline, thirteen months after displacement. This migration is mostly directed to rural areas or secondary towns smaller than Hawassa: at endline only 14 percent of the migrants are in Addis Ababa and migration to other large cities is minimal. Also, this migration is largely directed towards the location where workers used to live before moving to Hawassa: 70 percent of the workers that are not in Hawassa at endline live in the same town where they used to live before starting work at the industrial park. When asked why the migrated, 30 percent of the sample reports they wanted to live with their parents, and 25 percent of the sample that housing costs in Hawassa were unaffordable. In contrast, workers in the non-displaced sample have much lower migration rates: overall, more than 90 percent of the non-displaced sample lives in Hawassa at endline, and among those who remained employed for the entire period, migration is close to zero.

While migration is associated with a lower propensity to work, the rise in migration after job loss does not fully explain the employment gap between the non-displaced and the displaced controls. Among those displaced controls that stay in Hawassa, employment rates at endline are about 63 percent. This rules out a scenario where everybody who stays in the city finds a new job, or secures employment at a rate similar to that of the non-displaced. However, among those displaced controls that leave Hawassa, endline employment is 21 percent, which suggests that out-migration may exacerbate the drop in employment after job loss. If we control for migration in a regression of endline employment on the displacement shock — an exercise that identifies the mediation role of migration under the assumption that migrants are not selected on potential employment outcomes — we find that migration can explain 41 percent of the drop in employment after job loss (columns 1 and 2 in Table A8). However, if workers with poor employment prospects in Hawassa migrate at disproportionately high rates, the role of migration in driving employment outcomes would be more limited than what the analysis in Table A8 suggests.

Fifth, the drop in labor income is partially mitigated by severance pay and by an increase in informal transfers. The average displaced control worker received severance pay after job loss. Averaged over the thirteen month period we observe, this transfers corresponds to about 136 ETB per month. Further, informal transfers increase by about 236 ETB per month (Table A9). Thus, overall, formal and informal insurance mechanism provide resources worth 372 ETB per month, which mitigate more than half of the 629 ETB loss in labor income associated with job displacement. Further, we note that the informal insurance response is quite robust: it provides resources 72 percent larger in magnitude than the current entitlement of severance pay. However, informal transfers are likely to impose a number of costs on respondents. 90 percent of the transfers that workers receive come from the workers' own

family, which may generate tensions across generations. 70 percent of workers report that the more transfers they have received in the past, the less transfers they would receive today, indicating a tight budget constraint. And about 20 percent of the workers report that they will have to repay the money they received. As a result of this, about half of the displaced controls sample reports that they would accept a payment worth 70 percent of their informal transfer to forgo all assistance from family and friends. This suggests that, for these workers, the cost of informal insurance reduces the value of the transfers by at least 30 percent.

Sixth, consumption expenditure drops, and consumption poverty increases. On average, core consumption expenditure – which includes food and basic necessities — drops by 8.5 percent (Table A10) and total expenditure drops by 11 percent (Table 2 and Figure 2c). These effects increase over time. By month thirteen, the core consumption of the displaced controls is 16 percent lower than the core consumption of the non-displaced. And total consumption is 20 percent lower. If we use as counterfactual the non-displaced workers who remain employed over the period, the gap in core consumption expenditure grows to 19 percent and the gap in total consumption expenditure to 32 percent. Further, by month thirteen, 15 percent of the displaced controls are in extreme consumption poverty (Table A11; we define extreme poverty using the World Bank 2.15 USD per day threshold). These changes are generally larger than those reported in studies of the impacts of job loss on consumption expenditure in middle income and or high-income countries. For example, Gerard et al. (2025) find that job loss in Brazil is associated with a drop in consumption expenditure of about 14 percent, 12 months after displacement (and using as counterfactual the consumption expenditure of a sample that is continuously employed). However, these results also suggest that existing insurance mechanisms likely mitigate a meaningful share of the consumption impacts of job loss, since workers suffer a drop in labor income that is proportionally much larger and fairly stable over the second part of the period (thirteen month after displacement, labor income is about 43 percent lower than in the non-displaced sample).

3.3 What Are the Impacts of Job-Loss Insurance Payments?

We next document the impacts of job-loss payments on workers. We first consider effects on the full sample of workers. We then study the heterogeneity of impacts with respect to an incentivized policy choice measure. Overall, we find that lump-sum payments reduces employment, raises migration, leads to a spike in short-run consumption expenditure, and ultimately does not affect consumption at endline. Conversely, we find that monthly payments do not have employment or migration, and boost long term consumption.

3.3.1 Main Effects

The fundamental objective of job-loss payments is to sustain consumption expenditure after a shock. We thus start our discussion by presenting impacts on consumption expenditure. We then move to analyse the dimension of impacts that underlie these effects: employment, migration and transfers.

Our central result is that monthly payments significantly raise total consumption expenditure over the thirteen months after job loss, while lump sum payments do not have a significant average impact on expenditure. Monthly payments raise total consumption expenditure by about 78 ETB (4.5 percent) per month, or about half of the gap between the non-displaced and the displaced controls (Table 2). Effect sizes grow over time and, by month thirteen, they reach 119 ETB (a noisily estimated effect), which corresponds to 6.5 percent of the control mean, or about one third of the gap between the non-displaced and the displaced controls. Conversely, lump-sum payments raise average consumption expenditure by 21 ETB – an insignificant effect, that is about a quarter of the size of the effect we estimate for the monthly group ($p=0.063$). These average effects mask considerable variation over time. In the first two months after job loss, lump-sum payments raise consumption expenditure by a large and significant 231 ETB. However, at this point in time, there is no significant difference in expenditure between the controls and the non-displaced sample. Thus, the treatment raises consumption expenditure over the level observed for the group that was not subject to job loss – Gerard and Naritomi (2021) find a similar spike in consumption expenditure after job loss for a sample of Brazilian workers who receive statutory severance pay. Impacts of lump-sum payments on consumption expenditure are insignificant (and slightly negative) in later time frames. For each specific time period, we can reject the null hypothesis that the two interventions have the same impact. Results on core consumption expenditure (Table A10) are qualitatively similar: monthly payments significantly halve the expenditure drop associated with job loss, while lump sum payments have smaller and insignificant impacts.

Further, monthly payments reduce consumption volatility and risk, while lump-sum payments exacerbate it. This is the case if one considers consumption smoothing over time (Figure 2c). Lump sum payments raise consumption expenditure just after job loss, when it is already quite high, and reduce it towards the end of the period, when it is has fallen considerably with respect to the non-displaced benchmark. Conversely, the effects of monthly payments grow over time, supporting consumption expenditure precisely at the time when it starts to fall with respect to the non-displaced workers. Further, monthly payments significantly lower the risk of expenditure poverty, by 3.6 percentage points on average (Table A11). Lump-sum payments, on the the other have, have an average effect on the risk of poverty that is close to zero. And, by month thirteen, they are associated with a noisily estimated, insignificant increase in poverty of about 4.8 percentage points.

How do these results come about? The most important mechanism is related to employment. We find that lump-sum payments reduce employment rates by a significant 7.1 percentage points on average (Table 1). At endline, the reduction in employment amounts to about 8.5 percentage points. Reductions in wage employment are even more pronounces (8.8 percentage points on average and 9.9 percentage points at endline) and are somewhat mitigated by a significant 2-3 percentage point increase in self-employment (Table A5). Overall, labor income declines by 11.2 percent on average and by 19.5 percent at endline. Monthly payments, on the other hand, do not significantly affect average or endline employment rates — both effects are statistically different from those of lump-sum payments with p-values of .095 and .03, respectively.

We also find that lump-sum payments increase migration at endline (Table A7). The effect takes several months to materialize: up to month 9, impacts on migration hover around 3 percentage points, and are not significant. But, by endline, is sizable: migration increases by 7.3 percentage points, or 25 percent of the control mean. In contrast, monthly payments do not raise endline migration (an effect that is statistically different from those of lump-sum payments).

It is important to note that, while migration likely reduces employment in the lump-sum payment group, it cannot account for the bulk of the negative employment effect of this treatment. In particular, if we control for migration in the regressions studying treatment effects on endline employment, we find that the coefficient on the lump-sum treatment drops by about a third, while the coefficient on monthly payments remains unchanged (columns 3 and 4 in Table A8). As noted before, this exercise amounts to a mediation analysis under the assumption that migrants are not selected on potential employment outcomes. If migrants instead have worse-than-average employment outcomes in Hawassa (a plausible assumption), this exercise would overstate the mediation role of migration. This means that we should consider the results of Table A8 as a likely upper bound to the true mediation effect of migration. The migration response thus cannot offer a conclusive explanation for the reduction in employment that we observe among those workers that receive the lump-sum payment.

Finally, neither treatment significantly changes average informal transfers. Individuals in the lump-sum group give larger transfers to others at the start of the period (Table **??**, and receive larger transfers from others later on (Table A9), resulting in an effect that cannot be distinguished from zero.

3.3.2 Discussion

The results above motivate two main questions: (i) why do lump-sum payments raise consumption expenditure so starkly just after job loss? and (ii) why do lump-sum payments lower employment and raise migration? In this section, we will provide some answers to these

questions by presenting analysis of additional outcomes and treatment effect heterogeneity with respect to policy preference (using the incentivized task described in Section2.4).[13]

[14]

We document substantial heterogeneity in whether people prefer lump-sum or monthly payments, and this heterogeneity is crucial to understand the different effects that these interventions have on expenditure and employment. When asked to choose between a monthly transfer and a lump-sum transfer of the same amount, 58 percent of individuals prefer the monthly transfer. The most common reason for preferring monthly payments is to 'control expenditure' (Table A3) — a result strongly suggestive of the presence of self-control issues. In contrast, the most common reason for preferring lump-sum payments is the desire to purchase a lumpy asset or good — a result that indicates that a second group of workers may be interested to invest in costly projects such as starting an enterprise or migrating.

With regard to the first question, our evidence suggests that lump-sum payments generate a spike in consumption expenditure due to self-control issues. We have two results that support this hypothesis against the most plausible alternative explanation — that these initial expenses are time-consistent economic or social investments. First, as explained above, many workers themselves report to have self control problems and a desired for commitment, since they would prefer monthly payments to lump-sum payments to help them control their own spending. Second, among those who strongly prefer monthly payments, those that are assigned to the lump-sum treatment consume 426 ETB more per month immediately after job loss compared to those that are assigned to their preferred monthly treatment. In contrast, among those that did not strongly prefer the monthly treatment, lump-sum payments are associated with with a increase in consumption expenditure that is a significant 75 percent smaller than that experienced by those workers who strongly preferred monthly. The results for core consumption expenditure paint a similar picture (Table A12). In sum, a large number of workers report a desire to control their expenditure, and, when they are not offered a

[13] Recall that we split the sample based on a dummy that captures the 25 percent of workers that have the strongest preference for monthly payments. See section 2.4 for a precise definition of this dummy variable. We will refer to the group that does not have a strong preference for monthly transfers as the group that prefers the lump sum treatment, since this is true of the average person of this group. Further, note that we do not observe policy preference for the control group. This means that we can provide a direct comparison between the two treatment conditions for each group, but we cannot identify the treatment effects of each treatment with respect to the control condition for a given group. Also, note that the difference in average outcomes between those that strongly prefer monthly and those that do not is identical to the difference in treatment effects between the two groups. Let TL be a dummy for people assigned to the lump-sum treatment, TM a dummy for people assigned to the monthly treatment, TC a dummy for the control group, and PM a dummy capturing individuals that strongly prefer monthly payments. Formally, the difference in the impacts of the two treatments for the group that strongly prefers monthly payments is given by

$$(E[Y|TL,PM]-E[Y|TC,PM])-(E[Y|TM,PM]-E[Y|TC,PM])$$

which is equal to

$$E[Y|TL,PM]-E[Y|TM,PM]$$

Hence, while we cannot break down the contribution of different policy preference groups in generating a given treatment effect (since we do not observe policy preferences for the control group), we can pin down which policy preference group drives the difference in impacts between the two treatments.

[14]

commitment device, they display high sensitivity to cash-on-hand. These results are consistent with models of imperfect self-control based on preferences, such as models of present bias or temptation (Gul and Pesendorfer, 2001; Laibson, 1997). They would also be consistent with models where there is a social pressure to over-spend, however only 10.5 percent of workers report to have been under such pressure, and trafers to others are generally limited.

With regard to the second question, our evidence suggests that lump-sum payments enable a different set of workers — those that preferred lump-sum payments to start with — to leave the Hawassa Industrial Park (HIP) and to leave the city of Hawassa. We summarise this evidence in Table A14 (the top panel reports treatment effects for a set of variables capturing employment outcomes and employment preferences at endline; the bottom panel reports the difference in outcomes between the lump-sum and monthly conditions for (i) the group that strongly prefers monthly, and (ii) for the group that does not. We have three pieces of evidence. First, we find that lump-sum payments are associated with a decrease in having any factory work (12.9 percentage points) and in working at HIP (8.3 percentage points). Monthly payments, on the other hand, are associated with smaller and insignificant decreases of, respectively, 4.7 and 2.5 percentage points. Importantly, the additional reduction in HIP work precipitated by lump-sum payments over monthly payments is driven exclusively by the group that prefers lump-sum payments. Those workers that want lump-sum payments use them to reduce their employment in HIP. Second, we find that lump-sum payments reduce workers desire to work in HIP or in the textile sector in the long run (columns 6 and 7). Again, the difference in these effects is driven by the group that wants lump-sum payments. Workers in this group change their long-run goals if they are given a large boost in liquidity. Third, we show that the difference in endline migration between the two treatments is driven by those workers that prefer lump-sum payments (column 10). A clear picture emerges from these results: lump-sum payments lead workers away from work at HIP and away from the city of Hawassa, while monthly payments do not generate these effects. And the difference between these two treatments is generally driven by those workers that preferred lump-sum payments at baseline, presumably since they wanted to access the liquidity to make these transitions possible.

4 Welfare implications and policy design

We now shed light on the welfare implications of our results for policy design. First, our data collection is particularly well-suited to evaluate policies changing the payment modality of the additional support received by displaced workers, compared to a benchmark where all workers receive a lump-sum payment: (i) mandating all workers to receive monthly payments and (ii) offering workers the choice between a lump-sum payment and monthly payments. Although this exercise applies to additional support only, it highlights the potential benefits

of moving away from using lump-sum payments as the sole payment modality for job-loss insurance policies in low-income countries. Second, we provide a complementary analysis assessing the potential welfare gains from increasing job displacement insurance coverage, i.e., increasing the support that workers are eligible for at layoff (for a given payment modality).

Throughout this section, we evaluate policy designs through an insurance-efficiency trade-off à la Baily (1978) and Chetty (2006). Specifically, we assess the welfare effect of increasing job displacement insurance coverage and changing payment modality as follows:

$$dW(x) = E[MRS_i(x) - 1] - FE(x) + IndPolicy(x) \tag{3}$$

$$\begin{aligned}[dW(x_2) - dW(x_1)] = E[MRS_i(x_2) - MRS_i(x_1)] - [FE(x_2) - FE(x_1)] \\ + [IndPolicy(x_2) - IndPolicy(x_1)],\end{aligned} \tag{4}$$

where the welfare effects are expressed per unit of additional support received at job loss and $x = \{lump-sum, monthly, choice\}$ indicates the payment modality of the additional support. The first component is the value of additional job-loss insurance for workers, which depends on their marginal rate of substitution (MRS_i) between employment and after job loss (Landais et al., 2021). The second component is the fiscal externality ($-FE$), i.e., the impact on the government budget due to behavioral responses, which is a standard measure of efficiency cost in the literature (Finkelstein and Hendren, 2020). In our application, however, another relevant consideration for efficiency comes from the industrial policy objective of the government to retain workers employed in the industrial park ($IndPolicy$), which we model as a separate externality arising from changes in HIP employment ($dEmpHIP$) multiplied by an external marginal value of HIP employment for our workers (κ): $IndPolicy(x) = \kappa \times dEmpHIP$. This external value (beyond any wage received by workers and any tax revenue received by the government) is difficult to quantify, so we present results from a range of values in our analysis.

We compute the fiscal and industrial policy externalities directly from our estimated treatment effects, and we rely on a revealed preference assumption for the values of insurance. Specifically, under that assumption, the elicited willingness-to-pay (WTP) for different payment modalities at baseline captures $[MRS_i(x_2) - MRS_i(x_1)]$. Yet, it does not provide information on the value of additional job displacement insurance per se. To shed light on this, we designed an additional data collection effort at endline that we describe below.

4.1 Changing payment modality

Table 3 uses equation (4) and presents the welfare effect of mandating workers to receive the additional support through monthly payments rather than a lump-sum payment (top panel) and of offering workers the choice between the two payment modalities (bottom panel).

In column (1), we begin by considering the impact of the change in payment modality on

workers' value of insurance ($E[\Delta MRS]$). The elicitation exercise conducted at the end of the baseline survey, just prior to treatment assignment, provides us with the distribution $F(\overline{S_i})$ in our sample, where $\overline{S_i}$ is the maximum lump-sum value at which a worker would still prefer the monthly treatment, which (as a reminder) provided payments equivalent to a lump-sum of $S_0 = 3850$ Birr. Given this distribution, a worker's surplus or WTP for receiving the monthly treatment rather than the lump-sum S_0 is captured by the difference $\overline{S_i} - S_0$, and the average change in workers' value of insurance would be $E\left[\overline{S_i} - S_0\right]$. Specifically, we find that mandating the monthly payments would lead to $E[\Delta MRS] = E\left[\overline{S_i} - S_0\right]/3850 = 112/3850 = 0.029$ per Birr received, although the value of insurance would decrease for 42% of our sample. By contrast, offering the possibility to receive payments in monthly tranches would not decrease the MRS of any worker and would striclty increase it for 58% of workers with an average surplus in that sample of $746/3850 = 0.194$ per Birr received. Overall, offering the choice in payment modality would thus lead to $E[\Delta MRS] = 0.58 \times 0.194 = 0.112$ per Birr received.

In column (2), we consider the impact on the fiscal externality ($-\Delta FE$). By contrast to unemployment insurance programs, behavioral responses do not affect the benefits paid whey payments are unconditional on reemployment. However, behavioral responses can still affect the government budget through their impact on formal wage employment and the associated income taxes paid. Considering a two-year horizon after job loss,[15] treatment effects imply that providing additional support would lead to a decrease in average formal wage employment by 2.569 months and 1.215 month, for the lump-sum treatment and the monthly treatment, respectively. Mandating workers to receive monthly payments rather than a lump-sum payment would thus increase average formal wage employment by 1.354 month. Assuming a wage of 1500 Birr (the median in our sample), the average income tax rate would be 6%, so that the impact on the fiscal externality would be: $-\Delta FE = 1.354 * 1500 * 0.06/3850 = 0.032$ per Birr received. Similarly, treatment effects among the sub-sample of workers who prefer monthly payments (those with $\overline{S_i} - S_0 > 0$ at baseline) imply that offering the choice of payment modality would increase average formal wage employment by 0.785 month, for an impact on the fiscal externality of $-\Delta FE = 0.785 * 1500 * 0.06/3850 = 0.018$ per Birr received.

In column (3), we consider the industrial policy externality from retaining workers in HIP employment. Treatment effects imply that providing additional support would lead to a decrease in average HIP employment in the two years after job loss by 1.735 months and 0.268 month, for the lump-sum treatment and the monthly treatment, respectively. Mandating workers to receive monthly payments rather than a lump-sum payment would thus increase average HIP employment by 1.467 month. Similarly, treatment effects among the sub-sample of workers who prefer monthly payments imply that offering the choice of

[15] We consider a two-year horizon because we still find persistent effects 14 months after job loss and no sign that the effect is decreasing over time (so considering a two-year horizon might be conservative). In practice, we assume that the treatment effects at endline persist until the end of the 24-month period.

payment modality would increase average HIP employment by 0.851 month. To provide some perspective, Table 3 presents results for three levels of the external marginal value of HIP employment (κ). For instance, if we use a value of 6% of the median wage (equivalent to the average tax rate), the impact on the industrial policy externality would be $\Delta IndPolicy = 1.467 * .06 * 1500/3850 = 0.034$ per Birr for mandating the monthly payments and $\Delta IndPolicy = 0.851 * .06 * 1500/3850 = 0.020$ per Birr for offering the choice of payment modality, respectively. For such a value, the overall welfare effect (ΔdW) displayed in column (4) would thus reach 0.095 per Birr in the first case and 0.151 per Birr in the second case. These welfare effects would increase for higher values of κ, but more so for a policy that mandates the monthly payments given the larger impact on HIP employment. Yet, the welfare effect of offering the choice of payment modality would remain larger as long as the external value of HIP employment does not exceed 29% of the median wage, or about five times the average tax rate. In other words, the external value of HIP employment would need to be quite high to justify a policy that mandates the monthly payments rather than offers the possibility to receive monthly payments, and makes many workers strictly worse off.

In sum, rather simple changes in payment modality away from the lump-sum default for job displacement insurance policies in low-income countries can have large welfare effects. Even using the low external value of HIP employment in Table 3, we find that offering the possibility to receive financial support in tranches increases the social value of the payment by 15.1%.

4.2 Increasing job displacement insurance coverage

The previous analysis leaves an important question for the experimental results presented in this paper unanswered: would an increase in financial support at job loss increase welfare? To answer this question, we can evaluate the last two components in equation (3) using our estimated treatment effects (for a given value of κ). The challenge, however, is to quantify workers' value of additional job displacement insurance, i.e., their MRS between employment and after job loss for a given payment modality. To shed light on this important question, we conducted a separate data collection effort at endline.

i. Workers' value of additional job displacement insurance. – Specifically, we elicited workers' WTP for an insurance product that provided the same increase in financial support at layoff as in our experiment (3850 Birr) for the same two payment modalities. For practical reasons, this insurance product only provided such higher coverage for one specific formal employment spell that would start during a six-month "activation window" following the endline survey (or that had already started before the start of the activation period), and for a six-month "insurance window" from the start of the employment spell (or from the start of the activation window). To elicit WTP, we asked how much of a 250 Birr wage subsidy offered by the researchers during the insurance window workers would be willing to give up

to be eligible for the additional financial support in case of layoff during that period.[16] We used multiple price lists ranging from 5 to 250 Birr with an enforced unique switch point. We define WTP as the midpoint between the switch point and the previous price. To be conservative, we code WTP as 250 Birr for individuals willing to pay at least 250 Birr. To incentivize the elicitation, we implement a randomly selected choices for 15 randomly selected jobseekers. Appendix C provides more details on the WTP elicitation and insurance schemes.

Figure 3 displays the demand curve for the additional job displacement insurance coverage resulting from conducting this elicitation exercise in the control group. To be consistent with equation (3), the monthly premium on the y-axis is expressed per unit of benefit payout, i.e., we divide the elicited WTP by 3850. The demand curves for the lump-sum and the monthly schemes are very similar: in both cases, workers are on average willing to pay 0.024 every month per Birr of additional support at layoff. However, workers are not indifferent between the two payment modalities. Figure 3 shows a marked right shift in the demand curve if workers could choose their preferred scheme; the average WTP would increase by 12.5 percent.

We find no significant effect on WTP if we compare treatment and control groups or if we compare the non-displaced and displaced control groups (see Figure A9b and Table A16). This suggests that neither the experience of receiving additional support at job loss nor the experience of job loss itself influences demand for job displacement insurance in our setting.

Formally, workers' MRS is the ratio of the maximum price at which they would be willing to transfer resources between employment and after job loss (π^{max}), which corresponds to their WTP per Birr of additional support at layoff discussed above, to the actuarially fair price to transfer resources between these two states of the world (π^{fair}), which equates expected contributions and payouts (Landais and Spinnewijn, 2021). This actuarially fair price depends on the monthly hazard rates of layoff (p_l) and quit (p_q), and the monthly discount factor δ in the following way (see Appendix C.1 for details):

$$\pi^{fair} = p_l \times (1 - p_q) \times \delta \tag{5}$$

We calibrate $\delta = 0.976$ based on an annual inflation estimate of 34% to make the lump sum and monthly payments fiscally equivalent. It is hard to find estimates of hazard rates of layoff and quit in our sample. To make progress, we compute the average layoff and quit probabilities based on existing evidence from Ethiopia more broadly. Shiferaw and Söderbom (2023) document a 6-month separation rate of 17.95%. We assume that 20% of these separations are due to layoffs based on data on the nature of separations in a representative sample of large firms in Addis Ababa (Abebe and Caria, 2018). We then recover the monthly lay-off and quit hazards as 0.0065 and 0.0259, respectively. These are

[16] The 250 Birr make up 16.7 percent of the median income of formally employed individuals in our endline sample.

broadly in line with realized separations in our sample following the endline survey.[17]

Table 4 summarizes these calibrations and shows that the actuarially fair price – 0.0061 per Birr of additional support at job loss – is substantially smaller than the average WTP elicited at endline, implying an average MRS around 4 in our sample. This is at the upper end of estimates in the job displacement insurance literature, although one of the rare study using actual insurance choices obtain a lower-bound for the average MRS between employment and unemployment around 3 (Landais and Spinnewijn, 2021). Moreover, Table 4 shows that most workers in our displaced sample would be willing to pay the actuarially fair price: 82.6% for the lump-sum scheme, 83% for the monthly scheme, and 88.4% if we allow workers to choose their payment modality. Finally, column (1) in Table 5 shows that workers' implied value of additional job displacement insurance ($E[MRS-1]$) reaches 3.011 per Birr of additional support for the lump-sum scheme, 3.033 for the monthly scheme, and 3.477 if we allow workers to choose their payment modality.[18]

ii. Welfare effects. – We can now compare these estimates to estimates of the fiscal and industrial policy externalities computed as in the previous subsection to evaluate welfare effects. Column (2) in Table 5 shows that the fiscal externality ($-FE$) amounts to $-.060$ per Birr received for the lump-sum scheme, -0.028 for the monthly scheme, and -0.042 if we allow workers to choose their payment modality. Column (3) shows that the industrial policy externality, which is always weakly negative and thus decreasing in κ, also remains orders of magnitudes smaller in absolute value than the implied value of insurance even for high external marginal values of HIP employment (we show results up to a value corresponding to 50% of the median wage). As a result, the welfare effect of increasing job displacement insurance coverage appears strongly positive in our setting irrespective of the payment modality. These welfare effects are always larger with monthly payments rather than a lump-sum because the value of insurance is slightly higher and the negative impacts on formal and HIP employment are systematically smaller. However, given the higher implied value of insurance if we allow workers to choose their payment modality, column (4) shows that offering such a choice leads to to the largest welfare gains even for high external marginal values of HIP employment.

[17]We conducted a short follow-up survey four to nine months after the endline survey to compute realized quit and layoff rates. We sampled all participants who had a formal job at endline and a subsample stratified by WTP of those who did not have a formal job at endline. In total, we attempted to contact 939 workers and reached 880 pf them for a response rate of 93.7 percent. For our analysis, we focus on the 764 workers who were formally employed at the time of the endline survey or at some point during the equivalent of the activation window in our elicitation exercise. We estimate monthly quit and lay-off hazards using maximum likelihood estimation and observed separation rates. The estimated monthly hazard rates are 0.0040 and 0.0206 for layoff and quit, respectively. We note that these are substantially smaller than average subjective layoff and quit rates provided by workers as part of the endline survey. However, it is notoriously difficult to measure workers' belief about such probabilities accurately. This is not to say that these beliefs are uninformative: for instance, the correlation between WTP and subjective layoff rate and the correlation between WTP and subjective quit rate are of the expected sign in our data (positive in the first case and negative in the second case).

[18]Table 4 shows that the actuarially fair price would be even smaller if we were to use layoff and quit rates based on realized separations, so that the implied value of additional job displacement insurance would be even higher.

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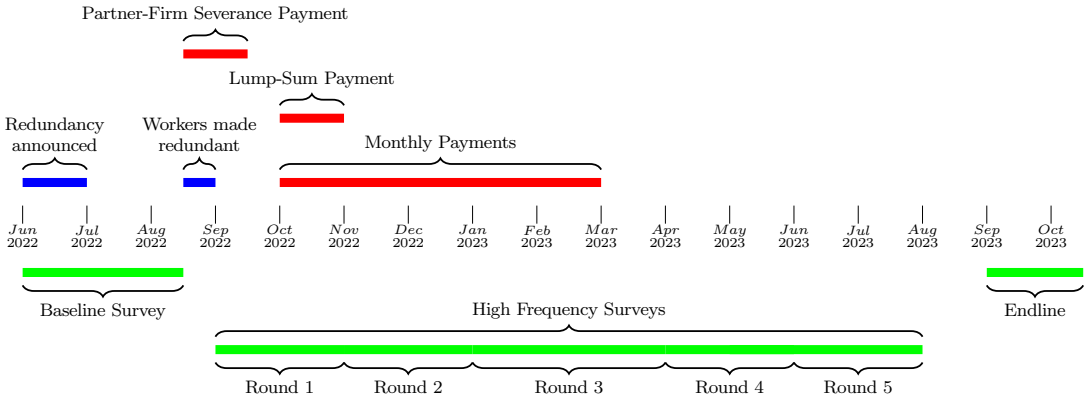
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Main Figures and Tables

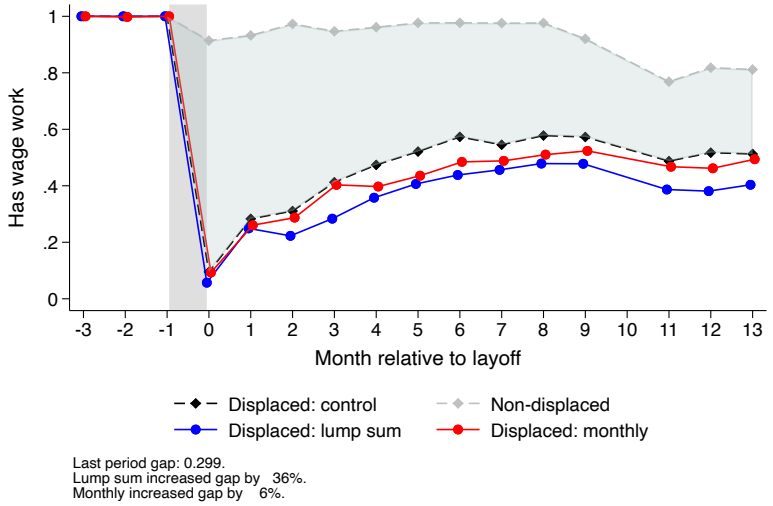
Figure 1: Project Timeline



Notes: **Figure 1 displays the project timeline.** Green bars indicate which survey covered which period.

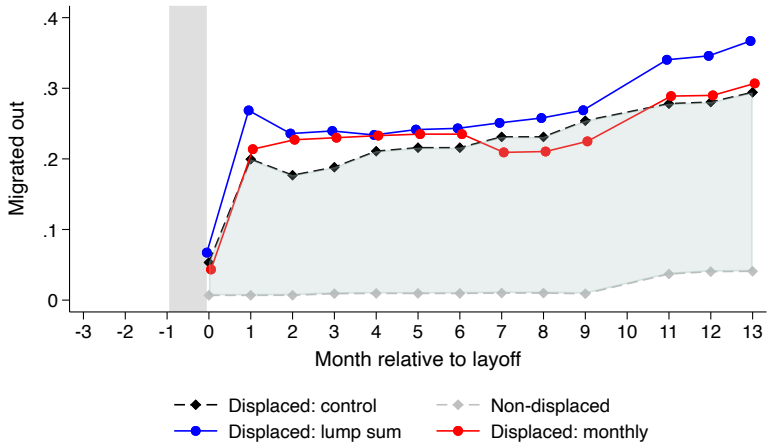
Figure 2: The evolution of key outcomes over time

(a) Employment



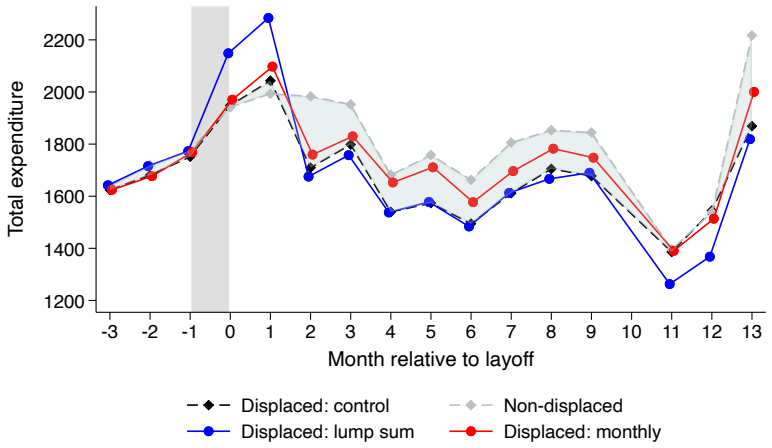
Last period gap: 0.299.
Lump sum increased gap by 36%.
Monthly increased gap by 6%.

(b) Migration



Last period gap: -0.253.
Lump sum increased gap by 29%.
Monthly increased gap by 5%.

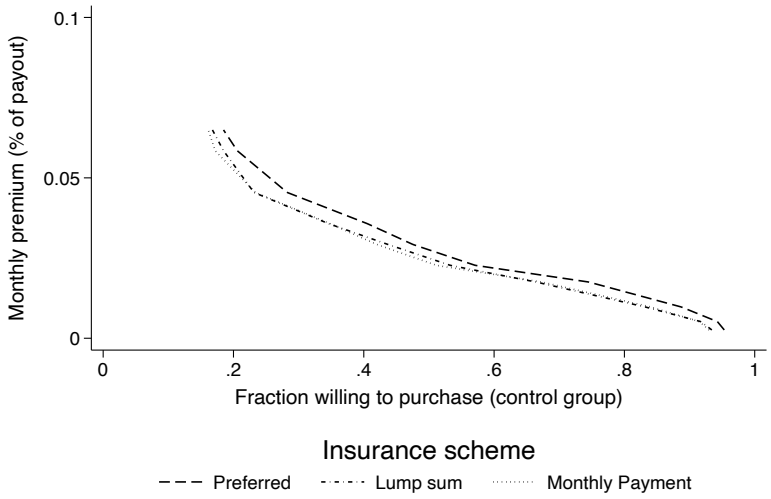
(c) Total consumption expenditure



Last period gap: 348.603.
Lump sum increased gap by 14%.
Monthly closed gap by 38%.

Notes: **Figure 2 displays the evolution of key outcomes over time by treatment group.** Figure 2a shows the development for wage employment rates. Figure 2b shows the development for wage migration rates. Figure 2b shows the development for total consumption expenditure. Observations in the non-displaced group are weighted to match the displaced control group (see Footnote 7).

Figure 3: Demand for job displacement insurance in the control group



Notes: **Figure 3 displays the demand for displacement insurance in the control group resulting from the elicited WTP exercise at endline.**

Table 1: The impacts of job-loss payments on employment

	Economically active				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Lump sum	-0.071***	-0.003	-0.089***	-0.075**	-0.085**
	(0.021)	(0.023)	(0.026)	(0.031)	(0.034)
Monthly	-0.027	-0.001	-0.036	-0.032	-0.009
	(0.021)	(0.023)	(0.026)	(0.031)	(0.033)
Δ Control - Non-displaced	-0.440***	-0.708***	-0.506***	-0.361***	-0.267***
Control mean	0.482	0.218	0.464	0.599	0.547
Lump sum = monthly (p)	0.034	0.931	0.039	0.165	0.023
Observations	1400	1314	1350	1330	1312

Notes: **Table 1 shows that the lump sum treatment reduces economic activity.** The dependent variable is (the average) of a dummy indicating any economic activity during the specified time period. Column 1 shows impacts during the 14 months since lay-off. Column 2 shows impacts in the month of lay-off and the following month. Column 3 shows impacts during months 2 to 5 after lay-off (when the monthly payments were paid). Column 4 shows impact during months 7 to 9 after lay-off (after the end of the monthly payments). Column 13 shows impacts in month 13 after lay-off. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 2: Impacts of job-loss payments on total consumption expenditure

	Total expenditure				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Lump sum	21.268	231.777***	-24.335	-14.614	-27.619
	(35.126)	(63.256)	(36.898)	(42.123)	(79.950)
Monthly	86.220***	28.581	67.967*	57.771	117.658
	(33.458)	(60.046)	(35.282)	(39.684)	(79.977)
Δ Control - Non-displaced	-183.900**	28.636	-189.285**	-161.701*	-370.025**
Control mean	1738.107	1995.846	1654.600	1666.292	1846.832
Lump sum = monthly (p)	0.063	0.001	0.012	0.078	0.079
Observations	1400	1314	1350	1330	1312

Notes: **Table 2 shows treatment effects on total consumption expenditure.** The dependent variable is (the average) total monthly expenditure during the specified time period. Column 1 shows impacts during the 14 months since lay-off. Column 2 shows impacts in the month of lay-off and the following month. Column 3 shows impacts during month 2 to 5 after lay-off (when the monthly payments were paid). Column 4 shows impact during month 7 to 9 after lay-off (after the end of the monthly payments). Column 13 shows impacts in month 13 after lay-off. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 3: The welfare effect of changing payment modality

		(1) $E[\Delta MRS]$	(2) $-\Delta FE$	(3) $\Delta IndPolicy$	(4) ΔdW
Mandating monthly	$\kappa = 0.06 * w$	0.029	0.032	0.034	0.095
	$\kappa = 0.29 * w$	0.029	0.032	0.166	0.226
	$\kappa = 0.50 * w$	0.029	0.032	0.286	0.347
Offering choice	$\kappa = 0.06 * w$	0.112	0.018	0.020	0.151
	$\kappa = 0.29 * w$	0.112	0.018	0.096	0.227
	$\kappa = 0.50 * w$	0.112	0.018	0.166	0.296

Notes: **Table 3 displays estimates of the welfare effect of changing the payment modality of additional support at job loss away from the lump-sum benchmark for job displacement insurance policies in low-income countries.** The first panel considers a policy mandating workers to receive the additional support in five monthly tranches rathen than in a lump-sum payment at job loss. The second panel considers a policy offering workers the choice between the two payment modalities. Columns (1)-(3) displays the three components of the welfare effects displayed in column (4) following equation (4). The industrial policy externality in column (3) depends on the unknown external marginal value of HIP employment κ (see text for details). We thus report estimates for a range of values going from 6% of the median wage (equivalent to the average tax rate) to 50% of the median wage.

Table 4: Actuarially fair price and demand for additional job displacement insurance

	Calibration			Fraction willing to purchase		
	(1)	(2)	(3)	(4)	(5)	(6)
Data source	p_l	p_q	π^{fair}	Preferred	Lump sum	Monthly
Firm data	0.0065	0.0259	0.0061	0.884	0.826	0.830
Realized separations	0.0040	0.0206	0.0037	0.934	0.908	0.910

Notes: **Table 4 assesses the fiscal viability of providing additional support to workers at job loss by comparing the actuarially fair price for transferring resources between employment and after job loss to the maximum price that workers would be willing to pay for it.** Columns 1 and 2 displays the monthly hazard rates of layoff and quit, respectively hazard. In the first row, they are calibrated based on *firm data* from Shiferaw and Söderbom (2023) and Abebe and Caria (2018) on separation rates and the share of lay-offs, respectively; in the second row, they are estimated based on *realized separations* six to nine months after the endline survey in our sample (see text for details). Column 3 displays the resulting actuarially fair price given a calibrated monthly discount rate of $\delta = 0.976$. Columns 4 to 6 display the fraction of workers' willing to pay this actuarially fair price based on their WTP at endline.

Table 5: The welfare effect of increasing job displacement insurance

		(1) $E[MRS-1]$	(2) $-FE$	(3) $IndPolicy$	(4) dW
Lump-sum payment	$\kappa=0.06*w$	3.011	-.060	-0.041	2.911
	$\kappa=0.29*w$	3.011	-.060	-0.196	2.755
	$\kappa=0.50*w$	3.011	-.060	-0.338	2.61
Monthly payments	$\kappa=0.06*w$	3.033	-.028	-0.006	2.999
	$\kappa=0.29*w$	3.033	-.028	-0.030	2.975
	$\kappa=0.50*w$	3.033	-.028	-0.052	2.95
Choice of payment modality	$\kappa=0.06*w$	3.477	-.042	-0.021	3.415
	$\kappa=0.29*w$	3.477	-.042	-0.100	3.335
	$\kappa=0.50*w$	3.477	-.042	-0.172	3.26

Notes: **Table 5 displays estimates of the welfare effect of increasing job displacement insurance coverage by the same magnitude as in our experiment (an additional 3850 Birr at job loss).** The three panels correspond to three payment modality for the additional support: a lump-sum payment, five monthly payments, and offering workers a choice between the two payment modalities. Columns (1)-(3) displays the three components of the welfare effects displayed in column (4) following equation (3). The industrial policy externality in column (3) depends on the unknown external marginal value of HIP employment κ (see text for details). We thus report estimates for a range of values going from 6% of the median wage (equivalent to the average tax rate) to 50% of the median wage.

Online Appendix

A Figures and Tables

A.1 Appendix Tables

Table A1: Details of Measurement of Primary Outcomes

Order	Outcome	Survey Round(s)	Details
Primary	Expenditure plans	Baseline	Expected expenditure on groceries and basic necessities for each of the eight months from September 2022 to April 2023.
Primary	Savings plans	Baseline	Expected savings at the beginning of the month for each of the eight months from September 2022 to April 2023.
Primary	Job-Search plans	Baseline	Expected hours of job-search at the beginning of the month for each of the eight months from September 2022 to April 2023.
Primary	Expenditure Outcomes	High Frequency Rounds 1 to 5	Expenditure on groceries and basic necessities for each of the ten months from September 2022 to June 2023.
Primary	Savings Outcomes	High Frequency Rounds 1 to 5	Savings for each of the ten months from September 2022 to June 2023
Primary	Job-Search Outcomes	High Frequency Rounds 1 to 5	The number of applications for each of the ten months from September 2022 to June 2023.
Primary	Employment Outcomes	High Frequency Rounds 1 to 5	Any self-employment dummy and any wage employment dummy for each of the ten months from September 2022 to June 2023.
Primary	Employment Quality	High Frequency Rounds 1 to 5	A work quality index for each high frequency round. The work quality index is constructed according to Anderson (2008) out of (1) a dummy for whether the respondent has a written contract, (2) a dummy for if they are in permanent work, (3) their earnings, (4) a measure of worker surplus, (5) their job-satisfaction and (6) their expected tenure.
Primary	Migration Outcomes	High Frequency Rounds 1 to 5	Monthly dummies for living in an urban area for each high frequency survey round.

Primary	Welfare Outcomes	High Frequency Rounds 1 to 5	A welfare index for each high frequency round. The welfare index is constructed according to Anderson (2008) out of (1a) index of depression index (constructed according to Anderson (2008) out of the 10 CESD-10 depression scale questions) or (1b) an index of anxiety (constructed according to Anderson (2008) out of the 7 HADS-A anxiety questions) and (2) life satisfaction, in each high frequency round. Note that we measured only one of depression and anxiety in each high frequency round so the index is constructed out of one not both of these measures.
Primary	Empowerment Outcomes	High Frequency Rounds 1 to 5	An empowerment index for each high frequency round. The empowerment index is constructed according to Anderson (2008) out of (1) an index of autonomy from their parents (constructed according to Anderson (2008)) (2) an index of autonomy from partner (constructed according to Anderson (2008)) (3) number of friends (4) number of friends they can talk to about important personal decisions (5) number of friends they can seek comfort from when they are unhappy or feeling down.

Notes: **Table A1 describes our prespecified primary outcomes.**

Table A2: Details of Measurement of Secondary Outcomes

Order	Outcome	Survey Round(s)	Details
Secondary	Expenditure Outcomes	High Frequency Rounds 1 to 5	Expenditure sub-categories: Total expenditure, Food expenditure, Non-food non-durable expenditure, Rent expenditure, Expenditure on durables, Expenditure on investments
Secondary	Job-search expectations	Baseline	Reservation wage for formal jobs for September 2022 and reservation wage for informal jobs for September 2022.
Secondary	Job-search outcomes	Baseline	Reservation wage for formal work for each high frequency survey round; Reservation wage for informal work for each high frequency survey round; Time spent on job-search in an average week; Expected time to find formal job (weeks) for each high frequency round; Expected earnings in formal job for each high frequency survey round.
Secondary	Employment outcomes	High Frequency Rounds 1 to 5	Time to first formal job; Number of job-offers per application for each high frequency round; Current number of jobs for each high frequency survey round.
Secondary	Employment characteristics	High Frequency Rounds 1 to 5	Expected tenure in main job; Job-satisfaction; Dummy indicating the use of skills acquired in Hawassa Industrial Park in their new employment. All for each high frequency round.
Secondary	Self-employment	High Frequency Rounds 1 to 5	Total sales, number of employees, profits - all for each high frequency round.
Secondary	Household finances	High Frequency Rounds 1 to 5	Total amount of outstanding loans; Maximum amount of money that respondent can borrow within 3 months; Able to borrow 4000 Birr if needed; Able to access formal sources to borrow 4000 Birr if needed. All for each high frequency round
Secondary	Welfare	High Frequency Rounds 1 to 5	Number of usual meals per day; Number of times the respondent went to bed hungry in the last 7 days; Dummy whether the respondent has worried about not having enough food in the last 7 days. All for each high frequency round

Notes: **Table A2 describes our prespecified secondary outcomes.**

Table A3: Balance Table: Full Sample

	N	Control Mean	SD	Lump Sum	Monthly Payment	F-Test
Respondent's age (at baseline)	1410	22.115	2.325	-0.104	-0.068	0.803
				(0.515)	(0.665)	
Respondent is married (at baseline)	1412	0.125	0.331	0.041*	0.008	0.167
				(0.074)	(0.724)	
Number of children (at baseline)	1410	0.142	0.504	0.079**	0.003	0.036**
				(0.022)	(0.924)	
Retention at BEST (months) (at baseline)	1410	12.868	4.921	-0.447	-0.366	0.322
				(0.161)	(0.241)	
Respondent can speak Sidemegma (at baseline)	1412	0.860	0.347	-0.048**	-0.024	0.140
				(0.048)	(0.319)	
Respondent's is from a rural area (at baseline)	1412	0.597	0.491	-0.022	0.004	0.684
				(0.491)	(0.905)	
Respondent has university education (at baseline)	1412	0.229	0.421	-0.003	0.015	0.789
				(0.911)	(0.595)	
Respondent has secondary education	1412	0.958	0.202	-0.011	-0.023	0.285
				(0.470)	(0.113)	
Core expenditure in June	1410	851.476	313.692	24.943	22.387	0.443
				(0.250)	(0.293)	
Savings in June (flow)	1410	190.011	513.484	-64.751**	-18.800	0.094*
				(0.034)	(0.530)	
Total income in June	1412	1921.089	714.460	1.357	18.452	0.908
				(0.977)	(0.694)	
Income from wage work in June	1410	1570.380	308.794	-35.628**	-24.855	0.120
				(0.046)	(0.155)	

Notes: **Table A3 shows the balance table for the main experiment.** Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A4: Impacts of job-loss payments on wage employment

	Wage employment				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Lump sum	-0.088***	-0.041**	-0.108***	-0.099***	-0.099***
	(0.021)	(0.021)	(0.026)	(0.032)	(0.034)
Monthly	-0.036*	-0.012	-0.043	-0.040	-0.021
	(0.021)	(0.021)	(0.026)	(0.031)	(0.033)
Δ Control - Non-displaced	-0.473***	-0.734***	-0.542***	-0.397***	-0.299***
Control mean	0.445	0.189	0.422	0.558	0.513
Lump sum = monthly (p)	0.013	0.149	0.013	0.062	0.021
Observations	1400	1314	1350	1330	1312

Notes: **Table A4 shows treatment effects on wage employment.** Depended variable is (the average) wage employment rate during the specified time period. Column 1 shows impacts during the 14 months since lay-off. Column 2 shows impacts in the month of lay-off and the following month. Column 3 shows impacts during month 2 to 5 after lay-off (when the monthly payments were paid). Column 4 shows impact during month 7 to 9 after lay-off (after the end of the monthly payments). Column 13 shows impacts in month 13 after lay-off. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A5: Impacts of job-loss payments on self-employment

	Self employment				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Lump sum	0.027***	0.035**	0.021*	0.025*	0.024
	(0.010)	(0.014)	(0.013)	(0.014)	(0.017)
Monthly	0.012	0.015	0.009	0.013	0.006
	(0.009)	(0.012)	(0.011)	(0.013)	(0.016)
Δ Control - Non-displaced	0.036***	0.031***	0.036***	0.036***	0.050***
Control mean	0.041	0.034	0.041	0.041	0.053
Lump sum = monthly (p)	0.128	0.166	0.324	0.400	0.314
Observations	1400	1314	1350	1330	1312

Notes: **Table A5 shows treatment effects on self-employment.** Depended variable is (the average) self employment rate during the specified time period. Column 1 shows impacts during the 14 months since lay-off. Column 2 shows impacts in the month of lay-off and the following month. Column 3 shows impacts during month 2 to 5 after lay-off (when the monthly payments were paid). Column 4 shows impact during month 7 to 9 after lay-off (after the end of the monthly payments). Column 13 shows impacts in month 13 after lay-off. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A6: Impacts of job-loss payments on labor income

	Labor income				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Lump sum	-84.370**	53.432	-83.612*	-76.961	-201.106***
	(37.957)	(43.321)	(44.112)	(55.411)	(71.381)
Monthly	-37.300	32.085	-14.869	-36.139	-109.366
	(37.554)	(41.070)	(44.539)	(52.925)	(71.313)
Δ Control - Non-displaced	-628.734***	-943.578***	-734.498***	-547.537***	-429.137***
Control mean	750.915	274.415	661.817	923.177	985.568
Lump sum = monthly (p)	0.223	0.645	0.141	0.467	0.183
Observations	1400	1314	1350	1330	1312

Notes: **Table A6 shows treatment effects on total labor income.** Depended variable is total labor income (wage earnings + self-employment profits). Column 1 shows impacts during the 14 months since lay-off. Column 2 shows impacts in the month of lay-off and the following month. Column 3 shows impacts during month 2 to 5 after lay-off (when the monthly payments were paid). Column 4 shows impact during month 7 to 9 after lay-off (after the end of the monthly payments). Column 13 shows impacts in month 13 after lay-off. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A7: Impacts of job-loss payments on migration

	Migrated				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Lump sum	0.030	0.031	0.038	0.027	0.076**
	(0.021)	(0.019)	(0.025)	(0.028)	(0.032)
Monthly	-0.005	0.003	0.021	-0.034	-0.000
	(0.021)	(0.018)	(0.024)	(0.026)	(0.030)
Δ Control - Non-displaced	0.216***	0.120***	0.192***	0.239***	0.253***
Control mean	0.231	0.126	0.200	0.251	0.294
Lump sum = monthly (p)	0.092	0.132	0.498	0.024	0.015
Observations	1400	1314	1350	1330	1312

Notes: **Table A7 shows treatment effects on migration.** Depended variable is a migration dummy for the specified time period (defines as not living in Hawassa in that period). Column 1 shows impacts during the 14 months since lay-off. Column 2 shows impacts in the month of lay-off and the following month. Column 3 shows impacts during month 2 to 5 after lay-off (when the monthly payments were paid). Column 4 shows impact during month 7 to 9 after lay-off (after the end of the monthly payments). Column 13 shows impacts in month 13 after lay-off. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A8: Migration and effects on employment

	Dep. var.: Wage work in month 13			
	Job loss effect		Treatment effect	
	(1)	(2)	(3)	(4)
Displaced control	-0.299***	-0.176***		
	(0.057)	(0.054)		
Lump sum			-0.095***	-0.061*
			(0.034)	(0.032)
Monthly			-0.021	-0.018
			(0.033)	(0.031)
Migrated		-0.487***		-0.405***
		(0.044)		(0.026)
Lump sum = monthly (p)			0.028	0.171
Migrated mean	0.230	0.230	0.197	0.197
Stayed mean	0.770	0.770	0.605	0.605
Observations	817	817	1312	1312
Sample	No treatment		Displaced only	

Notes: **Table A8 displays the extent to which migration mediates the employment effects.** Columns 1 and 2 show estimates of the effect of job-loss on wage employment 13 months after lay-off with and without controlling for migration status. Estimates compare the experimental control group to the non-displaced group using weight to match the non-displaced group on observables. Columns 3 and 4 show the treatment effects on wage employment 13 months after lay-off with and without controlling for migration status. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A9: Impacts of job-loss payments on informal transfers received from others

	Informal transfers				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Lump sum	49.665	17.035	99.981**	74.665*	55.436
	(30.826)	(46.383)	(40.360)	(44.478)	(59.553)
Monthly	12.482	21.712	-50.992	79.267*	55.759
	(31.772)	(44.038)	(38.060)	(44.967)	(63.415)
Δ Control - Non-displaced	235.953***	34.572	165.195**	314.155***	346.161***
Control mean	578.073	439.559	566.721	592.917	672.552
Lump sum = monthly (p)	0.248	0.919	0.000	0.921	0.996
Observations	1400	1314	1350	1330	1312

Notes: **Table A9 shows treatment effects on informal transfers received.** Depended variable is (the average) transfer received from other people during the specified time period. Column 1 shows impacts during the 14 months since lay-off. Column 2 shows impacts in the month of lay-off and the following month. Column 3 shows impacts during month 2 to 5 after lay-off (when the monthly payments were paid). Column 4 shows impact during month 7 to 9 after lay-off (after the end of the monthly payments). Column 13 shows impacts in month 13 after lay-off. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A10: Impacts of job-loss payments on core consumption expenditure

	Core expenditure				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Lump sum	0.306	66.167***	6.186	-4.478	-33.160
	(15.614)	(23.661)	(16.756)	(20.974)	(33.877)
Monthly	28.282*	6.233	36.515**	19.324	27.468
	(15.019)	(22.462)	(15.977)	(19.253)	(33.737)
Δ Control - Non-displaced	-63.581***	-8.378	-76.318***	-57.221*	-134.574**
Control mean	818.628	809.762	802.643	840.480	825.862
Lump sum = monthly (p)	0.068	0.011	0.073	0.242	0.078
Observations	1400	1314	1350	1330	1312

Notes: **Table A10 shows treatment effects on core consumption expenditure.** Depended variable is (the average) total monthly core expenditure during the specified time period. Column 1 shows impacts during the 14 months since lay-off. Column 2 shows impacts in the month of lay-off and the following month. Column 3 shows impacts during month 2 to 5 after lay-off (when the monthly payments were paid). Column 4 shows impact during month 7 to 9 after lay-off (after the end of the monthly payments). Column 13 shows impacts in month 13 after lay-off. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A11: Impacts of job-loss payments on poverty

	In absolute poverty				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Lump sum	0.001	-0.070***	0.025	-0.013	0.048
	(0.020)	(0.025)	(0.025)	(0.028)	(0.033)
Monthly	-0.036**	-0.027	-0.048**	-0.050*	0.017
	(0.018)	(0.025)	(0.024)	(0.027)	(0.032)
Δ Control - Non-displaced	0.125**	0.085	0.143***	0.086	0.153**
Control mean	0.311	0.262	0.316	0.308	0.322
Lump sum = monthly (p)	0.049	0.074	0.003	0.170	0.338
Observations	1400	1314	1350	1330	1312

Notes: **Table A11 shows treatment effects on poverty.** Depended variable is (the average) poverty rate during the specified time period. Column 1 shows impacts during the 14 months since lay-off. Column 2 shows impacts in the month of lay-off and the following month. Column 3 shows impacts during month 2 to 5 after lay-off (when the monthly payments were paid). Column 4 shows impact during month 7 to 9 after lay-off (after the end of the monthly payments). Column 13 shows impacts in month 13 after lay-off. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A12: Impacts of job-loss payments on total consumption expenditure: break-down by policy preference

	Total expenditure				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Strongly preferred monthly					
Lump sum treatment	1769.94	2342.35	1758.41	1781.63	1458.07
	(107.90)	(183.97)	(101.59)	(116.78)	(212.77)
Monthly treatment	1799.68	1916.08	1796.48	1889.43	1586.49
	(106.15)	(191.27)	(101.35)	(115.57)	(210.08)
Not strongly preferred monthly					
Lump sum treatment	1757.39	2201.52	1720.95	1722.19	1528.01
	(101.00)	(170.14)	(96.70)	(110.09)	(196.54)
Monthly treatment	1815.62	2095.38	1837.00	1775.38	1644.83
	(101.27)	(169.29)	(94.71)	(108.68)	(197.39)
Δ | Strongly preferred monthly	-29.74	426.27***	-38.07	-107.80	-128.41
	(65.87)	(114.94)	(69.40)	(77.67)	(153.94)
Δ | Not strongly preferred monthly	-58.23	106.14	-116.05***	-53.19	-116.83
	(42.94)	(74.54)	(43.73)	(48.83)	(99.81)
Δ | Str. preferred monthly - Δ | Not str. preferred monthly	28.49	320.13**	77.98	-54.61	-11.59
	(78.38)	(137.18)	(81.94)	(91.60)	(183.69)
Control mean	1738.107	1995.846	1654.600	1666.292	1846.832
Observations	932	883	904	890	877

Table A13: Impacts of job-loss payments on core consumption expenditure: break-down by policy preference

	Core expenditure				
	(1) Mean	(2) Months 0-1	(3) Months 2-5	(4) Months 7-9	(5) Month 13
Strongly preferred monthly					
Lump sum treatment	893.15	935.39	897.51	981.96	809.34
	(58.20)	(74.47)	(52.08)	(72.21)	(114.06)
Monthly treatment	882.71	790.18	897.43	1002.33	797.89
	(57.73)	(77.95)	(52.57)	(70.58)	(113.00)
Not strongly preferred monthly					
Lump sum treatment	872.76	883.81	872.55	967.23	811.18
	(57.34)	(71.17)	(51.43)	(71.69)	(112.26)
Monthly treatment	901.18	847.89	906.00	973.85	873.35
	(56.99)	(69.91)	(50.18)	(69.34)	(112.38)
Δ | Strongly preferred monthly	10.44	145.21***	0.08	-20.38	11.45
	(30.23)	(43.12)	(33.99)	(39.88)	(65.41)
Δ | Not strongly preferred monthly	-28.42	35.91	-33.45*	-6.62	-62.17
	(19.77)	(29.14)	(20.30)	(25.03)	(43.96)
Δ | Str. preferred monthly - Δ | Not str. preferred monthly	38.87	109.29**	33.53	-13.76	73.63
	(35.98)	(52.25)	(39.44)	(46.86)	(78.71)
Control mean	818.628	809.762	802.643	840.480	825.862
Observations	932	883	904	890	877

Table A14: Impacts of job-loss payments on preferences and other labor market outcomes

	Employment status					Job search	Job aspirations				Migration
	(1) Wage work	(2) Formal work	(3) Factory work	(4) HIP work	(5) Self-emp	(6) # Apps	(7) HIP	(8) Textile	(9) Trade	(10) Self-emp.	(11) Migrated
Lump sum	-0.122***	-0.108***	-0.129***	-0.083**	0.008	-0.012	-0.037*	-0.050**	0.074**	0.069**	0.076**
	(0.034)	(0.034)	(0.034)	(0.032)	(0.016)	(0.081)	(0.021)	(0.021)	(0.032)	(0.034)	(0.032)
Monthly	-0.044	-0.059*	-0.047	-0.025	0.010	0.146*	0.005	-0.019	0.032	0.052	-0.000
	(0.033)	(0.033)	(0.034)	(0.032)	(0.015)	(0.086)	(0.022)	(0.022)	(0.031)	(0.034)	(0.030)
Δ Control - Non-displaced	-0.253***	-0.191**	-0.299***	-0.301***	0.043***	0.255**	-0.018	0.076***	-0.112	-0.201**	0.253***
Control mean	0.593	0.513	0.524	0.395	0.050	0.464	0.122	0.131	0.290	0.467	0.294
Lump sum = monthly (p)	0.023	0.136	0.014	0.068	0.930	0.055	0.043	0.121	0.200	0.603	0.015
Observations	1312	1312	1312	1312	1246	1312	1312	1312	1312	1312	1312
Δ lump sum vs monthly											
| If strongly preferred monthly	-0.086	-0.043	-0.080	0.014	-0.042	-0.042	0.010	0.007	0.023	-0.008	0.028
	(0.062)	(0.062)	(0.062)	(0.059)	(0.031)	(0.147)	(0.038)	(0.035)	(0.059)	(0.063)	(0.059)
| If not strongly preferred monthly	-0.075*	-0.063	-0.088**	-0.094**	0.016	-0.189*	-0.063***	-0.049**	0.049	0.026	0.102***
	(0.040)	(0.040)	(0.040)	(0.038)	(0.019)	(0.100)	(0.024)	(0.024)	(0.039)	(0.040)	(0.038)
Observations	877	877	877	877	843	877	877	877	877	877	877

Table A15: Attrition analysis

	Overall attrition						Endline attrition					
	(1) Coeff.	(2) se	(3) Coeff.	(4) se	(5) Coeff.	(6) se	(7) Coeff.	(8) se	(9) Coeff.	(10) se	(11) Coeff.	(12) se
Lump sum	0.005	(0.006)			0.014	(0.071)	0.001	(0.018)			-0.007	(0.209)
Monthly	-0.002	(0.005)			0.023	(0.049)	-0.021	(0.016)			-0.049	(0.194)
Age			0.000	(0.001)	0.000	(0.001)			0.003	(0.003)	0.005	(0.006)
Married			-0.002	(0.007)	-0.004	(0.005)			0.002	(0.021)	0.023	(0.044)
Above med. retention			-0.002	(0.003)	-0.002	(0.008)			-0.017	(0.021)	-0.001	(0.032)
Sidemegna = first language			-0.019*	(0.010)	-0.011	(0.015)			-0.040*	(0.022)	-0.064	(0.043)
From rural area			-0.004	(0.006)	-0.008	(0.008)			-0.014	(0.014)	-0.042	(0.026)
Has university education			0.001	(0.006)	-0.002	(0.012)			0.005	(0.017)	0.005	(0.032)
Has secondary education			-0.010	(0.014)	0.002	(0.005)			-0.048	(0.039)	-0.070	(0.079)
Baseline depression index			0.004	(0.005)	-0.000	(0.009)			-0.019	(0.012)	-0.009	(0.022)
Above med. savings			-0.010**	(0.005)	-0.007	(0.007)			0.001	(0.014)	-0.005	(0.025)
Lump sum× Age					0.001	(0.004)					-0.005	(0.009)
Monthly× Age					-0.000	(0.002)					-0.000	(0.008)
Lump sum× Married					-0.004	(0.008)					-0.020	(0.054)
Monthly× Married					-0.004	(0.008)					-0.020	(0.054)
Lump sum × Above med. retention					0.007	(0.014)					-0.014	(0.036)
Monthly× Above med. retention					-0.006	(0.011)					-0.037	(0.034)
Lump sum× Sidemegna = first language					-0.034	(0.028)					0.018	(0.058)
Monthly× Sidemegna = first language					0.015	(0.015)					0.058	(0.051)
Lump sum× From rural area					0.002	(0.015)					0.067*	(0.036)
Monthly× From rural area					0.006	(0.011)					0.013	(0.034)
Lump sum× Has university education					0.010	(0.016)					0.024	(0.045)
Monthly× Has university education					-0.003	(0.012)					-0.029	(0.039)
Lump sum× Has secondary education					0.003	(0.007)					0.055	(0.095)
Monthly × Has secondary education					-0.032	(0.033)					0.005	(0.101)
Lump sum × Baseline depression index					0.013	(0.015)					-0.007	(0.034)
Monthly × Baseline depression index					0.000	(0.010)					-0.022	(0.029)
Lump sum× Above med. savings					-0.009	(0.014)					0.028	(0.036)
Monthly× Above med. savings					-0.002	(0.010)					-0.000	(0.033)
Lump sum = monthly (p)	0.509						0.289					
Lump sum = monthly = 0 (p)	0.246						0.182					
Δ Control - Non-displaced	0.006*	(0.004)					0.024	(0.017)				
Control mean	0.006						0.076					
Observations	1410						1410					

Notes: **Table A15 shows that attrition at endline is unrelated to treatment.** Columns 1 to 6 show effects on not having been contacted for any follow-up data collection. Columns 7 to 12 show effects on attrition at endline. All specification include Strata fixed effects. Robust standard errors are given in odd columns in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

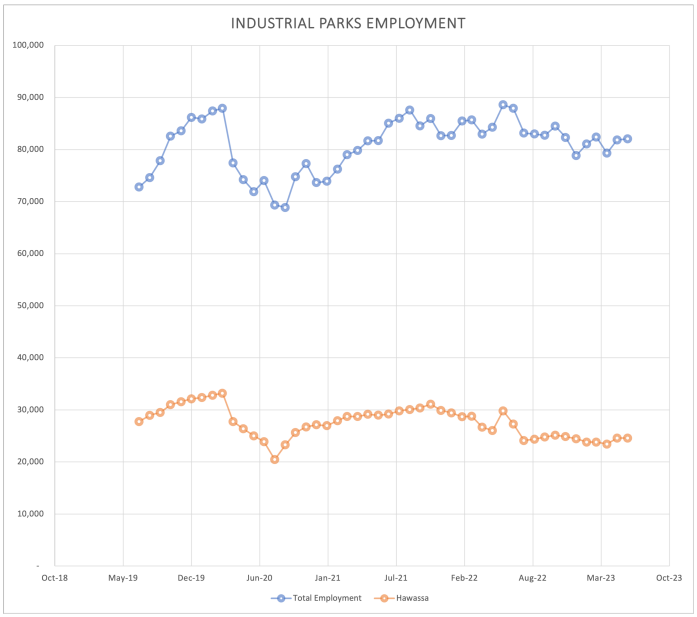
Table A16: Impacts of job-loss payments on willingness-to-pay for displacement insurance

	Monthly WTP (% of lump sum payment) for		
	(1) Pref. scheme	(2) Lump sum	(3) Monthly
Lump sum	0.001	0.002	0.001
	(0.001)	(0.001)	(0.001)
Monthly	-0.001	-0.001	0.000
	(0.001)	(0.001)	(0.001)
Δ Control - Non-displaced	-0.000	-0.002	-0.001
Control mean	0.027	0.024	0.024
Lump sum = monthly (p)	0.184	0.016	0.452
Observations	1312	1312	1312

Notes: **Table A16 shows that treatment did not significantly affect respondents' willingness-to-pay (WTP) for displacement insurance.** WTP is measured as fraction of the one-off lump sum payment (3850 Birr) willing to pay each month. Column 1 show treatment estimates for the preferred displacement insurance scheme. Column 2 shows treatment effects for the lump sum insurance scheme. Column 3 shows treatment effects for the monthly payment insurance scheme. WTP elicitation is described in detail in Appendix C. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

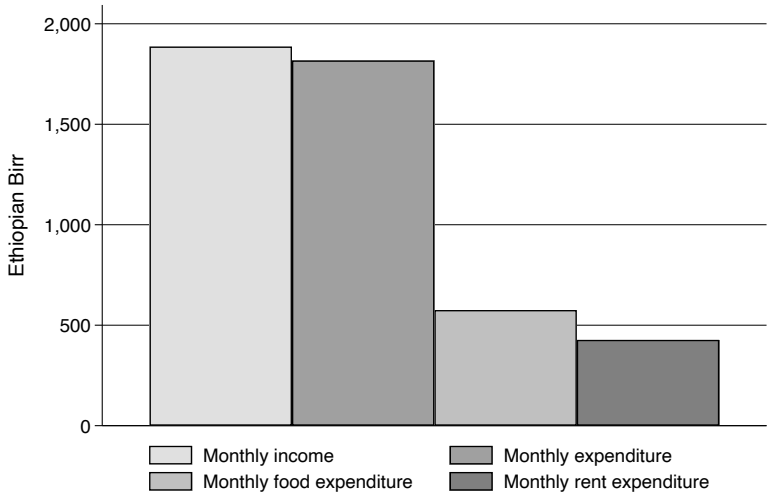
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Figure A1: Employment in Industrial Parks over time



Notes: **Figure A1 shows how total employment in Ethiopian industrial parks developed over time.** Authors' calculations based on data from the Ethiopian Investment Commission.

Figure A2: Income and expenditure of non-displaced sample

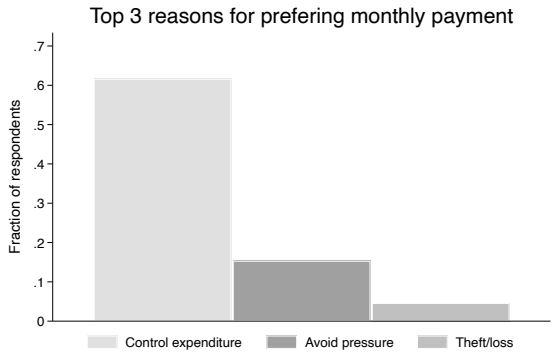


Notes: **Figure A2 displays baseline income and expenditure for the non-displaced sample.**

A.2 Appendix Figures

Figure A3: Self-reported reasons for policy choice

(a) Top 3 reasons for preferring monthly



(b) Top 3 reasons for preferring lump sum

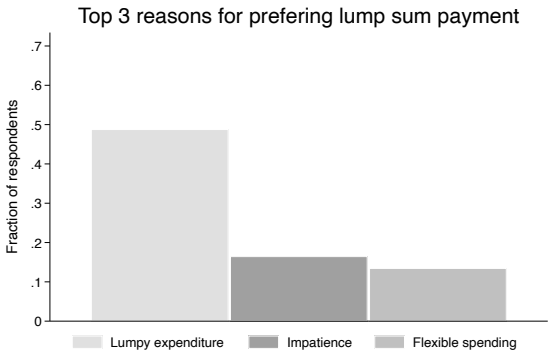
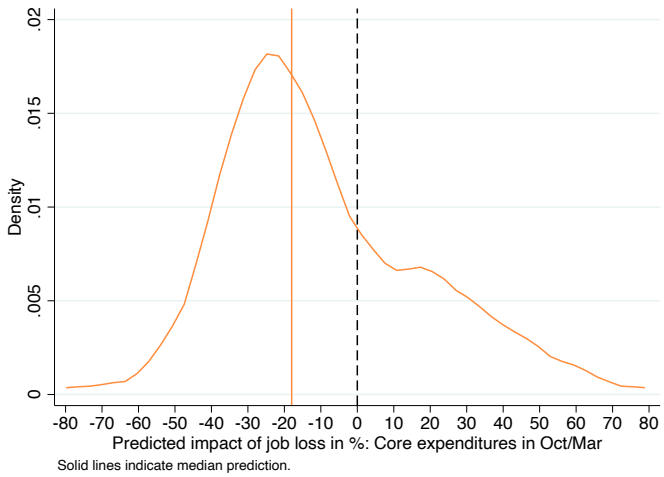


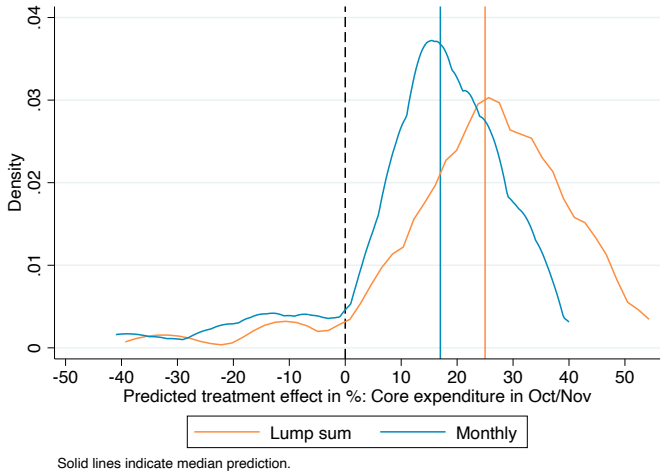
Figure A4: Expert predictions of impacts of job loss on core consumption expenditure



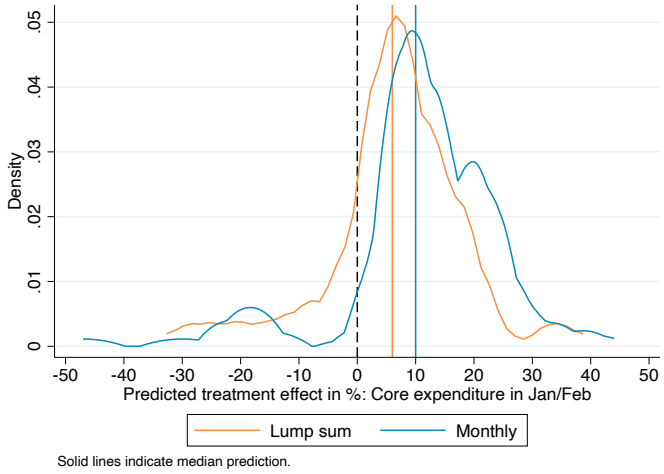
Notes: **Figure A4 displays experts' predictions of the impact of job loss on core consumption expenditures from October 2022 until March 2023.**

Figure A5: Expert predictions of impacts of job-loss payments on core consumption expenditure

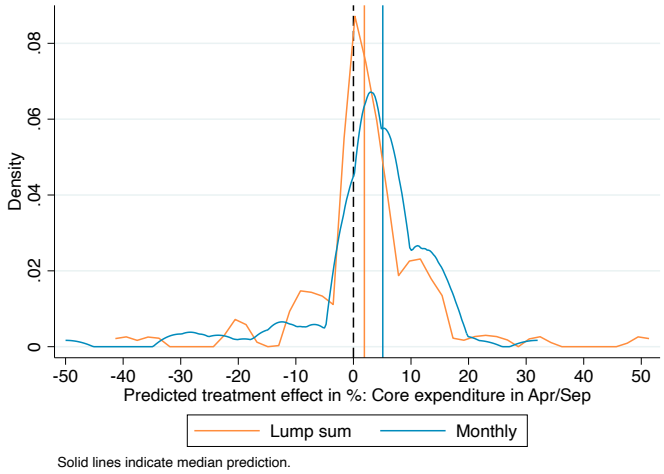
(a) Post lump sum impact



(b) End of monthly payments impact



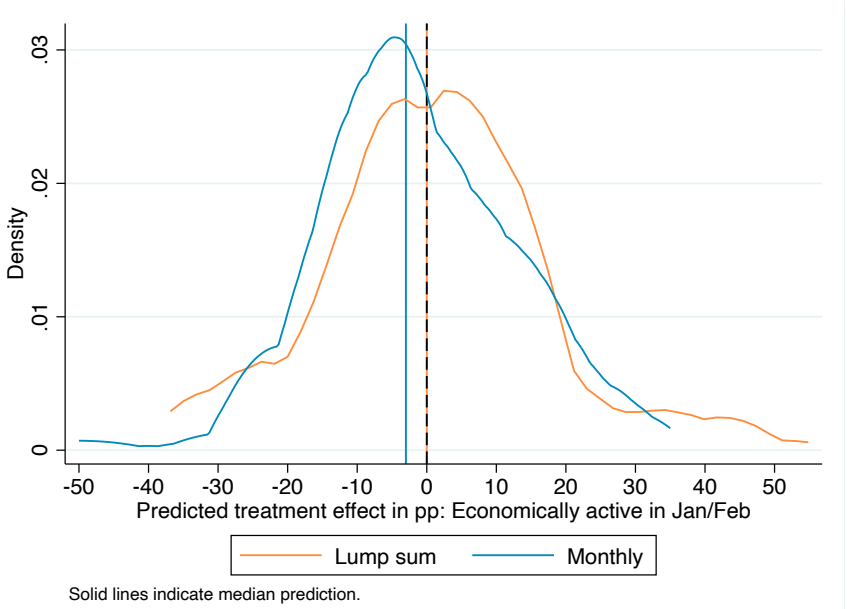
(c) Post intervention impacts



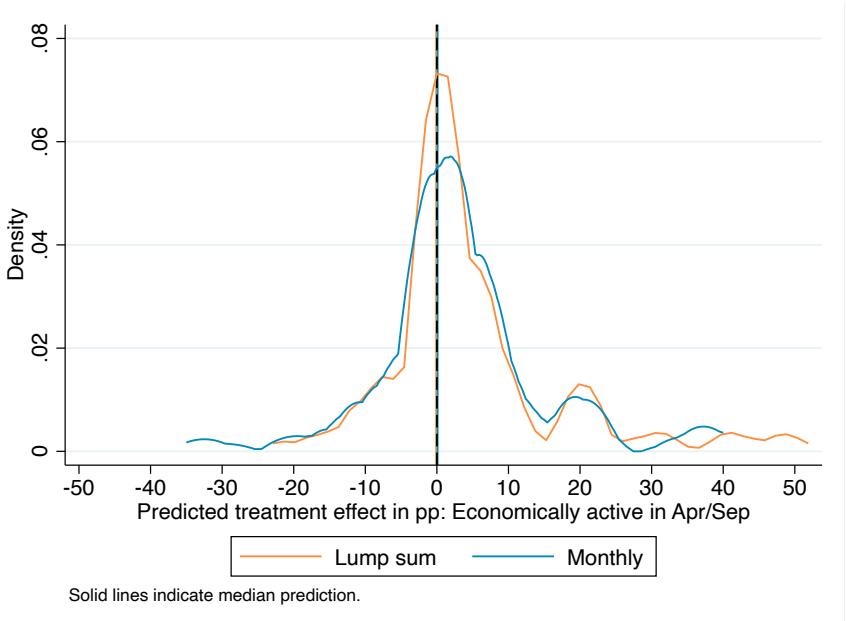
Notes: **Figure A5 displays experts' predictions of the effects of job-loss payments on core consumption expenditures.** Figure A5a displays predictions of short-term impacts for the two months just after the lump sum payments. Figure A5c displays predictions of impacts for the last two months of the end of the monthly payment period. Figure A5c displays predictions of impacts for the six month following the end of the monthly payment period.

Figure A6: Expert predictions of impacts of job-loss payments on employment

(a) Post lump sum impact



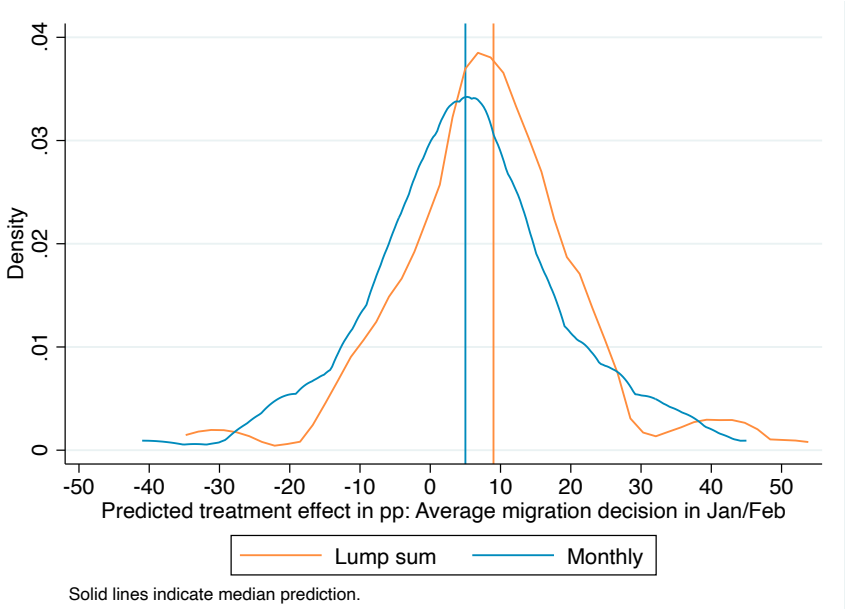
(b) End of monthly payments impact



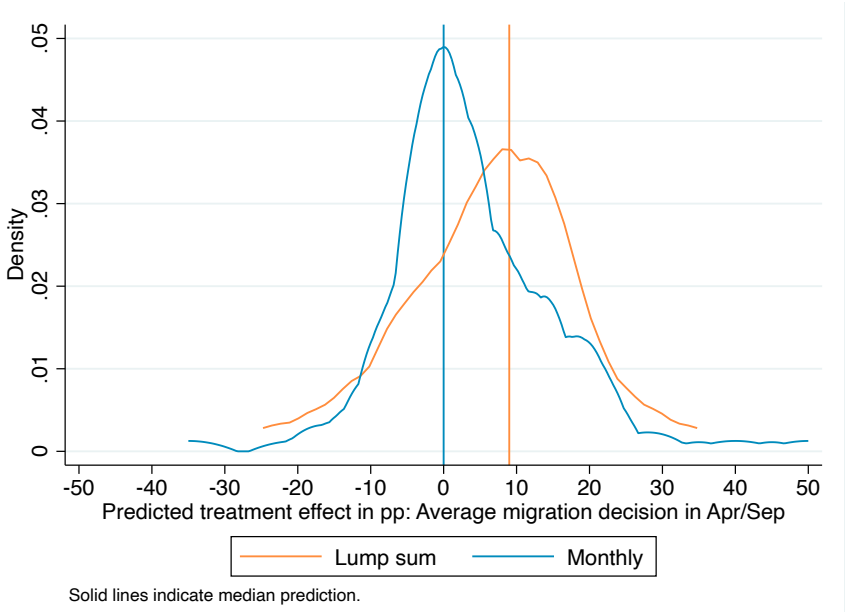
Notes: **Figure A6 displays experts' predictions of the effects of job-loss payments on rates of economic activity.** Figure A6a displays predictions of short-term impacts for the two months just after the lump sum payments. Figure A6b displays predictions of impacts for the last two months of the end of the monthly payment period.

Figure A7: Expert predictions of impacts of job-loss payments on migration

(a) Post lump sum impact

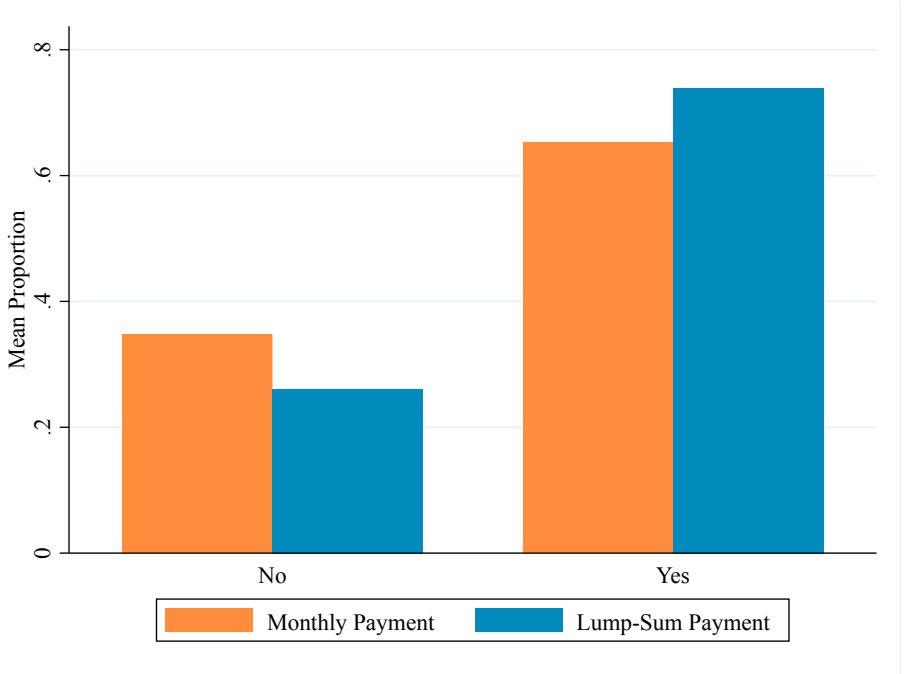


(b) End of monthly payments impact



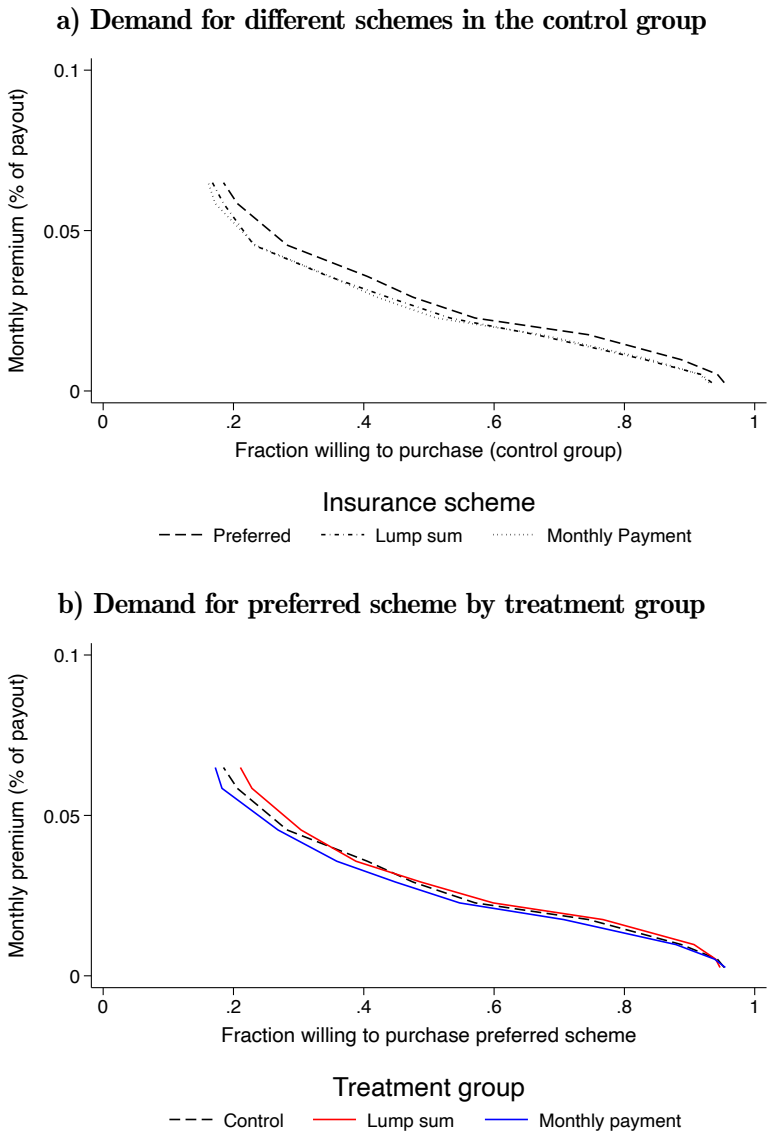
Notes: **Figure A7 displays experts' predictions of the effects of job-loss payments on rates of economic activity.** Figure A7a displays predictions of short-term impacts for the two months just after the lump sum payments. Figure A7b displays predictions of impacts for the last two months of the end of the monthly payment period.

Figure A8: Expert prediction of the fiscal sustainability of job loss payments



Notes: **Figure A8 displays experts' predictions of whether respondents' willingness-to-pay would be sufficient to pay for the job loss insurance.**

Figure A9: Demand for displacement insurance



Notes: **Figure 3 displays demand for displacement insurance in our sample.** .

B General measurement appendix

This appendix section describes how we deal with potential measurement inconsistencies arising mostly due to logistical constraints during the fieldwork. Specifically, we deal with three issues around recall data for specific months.

- Month 6 (March 2023) was only partially covered in round three of the high-frequency survey (the first interview happened on March 10th). We therefore do not plot month 6 values in any of the timeseries plots and exclude it from the main regression results tables (e.g., Table 1 columns 2 to 5. To calculate the mean in column 1, we take the month 5 value for month 6.
- Month 10 (July 2023) was only partially covered in round five of the high-frequency

survey (the first interview happened on July 8th). We therefore do not plot month 10 values in any of the timeseries plots and exclude it from the main regression results tables (e.g., Table 1) columns 2 to 5. To calculate the mean in column 1, we take the month 9 value for month 10.

- We changed the structure of the expenditure recall module in the endline survey to save survey time and accommodate additional questions. Specifically, we did not ask respondents to recall expenditures by sub-category for month 11 and 12 (though we did for months 13). While we asked for total expenditure, this question did not follow more detailed expenditure questions as it did for all other total expenditure data points. Hence, we do not use data for month 11 and 12 expenditure. To calculate means in columns 1 of the main regression result tables (e.g., Table 1), we assign months 11 and 12 the value of months 13.

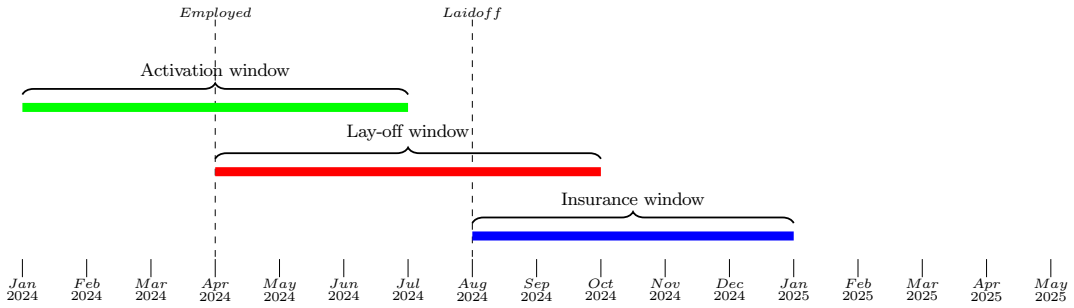
C Willingness-to-pay for insurance: measurement details

This section describes how we measured respondents' willingness to pay (WTP) for displacement insurance. We measure individuals' WTP for three insurance schemes. The first and second schemes have the same payout structure and the experimental payments. The third scheme is a lump sum scheme scaled by a randomly chosen factor α to assess how demand varies with the generosity of the insurance payment (α was randomly selected from the set $\{0.25,0.5,0.75,1.25\}$ with equal probability.

WTP elicitation To elicit individuals' WTP we ask a series of binary choices between "Keep the full wage subsidy of 250 Birr per month for up to six months." and "Give up x Birr of the monthly wage subsidy and be eligible for [*scheme*]", x takes the values of 5, 15, 25, 50, 75, 100, 125, 150, 200, and 250 Birr in ascending order. For the scaled insurance scheme we scale all prices by the same factor α to keep the price relative to payouts constant. We ask these questions until a respondent chooses to keep the full wage subsidy for the first time. We offer individuals a wage subsidy as part of the scheme to ensure that individuals pay for the insurance should they be selected.

We construct our main monetary measure of WTP for displacement insurance by taking the midpoint between the switch-point and the previous choice. For example, if an individual is willing to purchase the displacement insurance for 50 Birr not for 75 Birr, we code their willingness to pay as 67.5 Birr. For individuals not willing to purchase the insurance for 5 Birr, we code their WTP as 2.5 Birr (the midpoint between 0 Birr and 5 Birr). For individuals willing to pay 250 Birr, we use a value of 250 Birr, which is a conservative bound on their WTP. (Our results are insensitive to choosing other reasonable values for the top category).

Figure C10: Displacement insurance timeline



Notes: **Figure C10 displays the temporal structure of the displacement insurance schemes we elicit demand for.**

Incentivization The WTP elicitation is incentivized. We first truthfully tell individuals that "we will randomly select participants who will receive this [wage] subsidy." When introducing the binary choices we also tell individuals that "we will select one of these scenarios and we will implement your choice in that scenario for real." We further remind individuals that "you should think hard about all your choices as they can have real consequences" throughout the elicitation. In practice, we select 5 individuals to be eligible for each scheme.

Insurance schemes All insurance schemes have the same eligibility conditions but vary in their pay-out structure. Their eligibility structure is described in Figure C10. The insurance would become active if individuals started a job with a written contract at any point during the six-month *activation window*. This window lasts from January to July 2024. If individuals were already employed with a written contract in January 2024, the insurance would start in January 2024. The insurance was only valid for the first formal job they start during the activation window. We tell individuals that we check their employment status with their employer. The insurance was valid for six months, the *lay-off window*. During this period, they are eligible for displacement insurance payments if they are laid off from their employment. During the lay-off window, they are also eligible for the wage subsidy we offer as part of the WTP elicitation minus the price they paid for the insurance. If they are laid off, they receive their payments during the *insurance window* (again, we confirm the layoff with the employer). If they are eligible for a lump sum payment, they receive the lump sum payment in the month after lay-off. The lump sum amount is either 3850 Birr, the same amount as in our experiment, or a scaled version of the amount. If they are eligible for monthly payments, they receive monthly payments of 810 Birr in each of the following five months, again mimicking the main experiment.

C.1 Actuarially Fair Insurance Schemes

To assess whether respondents' WTP for insurance is sufficient to pay for the expected payouts to workers, we derive the actuarially fair insurance schemes. Define p_l and p_q as

the monthly probability of layoffs and quits respectively and δ as the monthly discount rate of the insurance provider.

Given some premium π, the expected total premium payment (discounted from the point of view of the first month of the eligible 6-month period, $t=1$) is then:

$$E[TotalPremiumPaid]=\pi\times\sum_{t=1}^{t=6}\left[\delta^{t-1}\times(1-p_l)^{t-1}\times(1-p_q)^{t-1}\right] \tag{C1}$$

where $\sum_{t=1}^{t=6}\left[(1-p_l)^{t-1}\times(1-p_q)^{t-1}\right]$ is the expected number of periods spent paying the premium (number of months employed up to 6 months).

Given some payout S, the expected payout (discounted from the point of view of the first month of the eligible 6-month period, $t=1$) is then:

$$E[Payout]=p_l\times S\times\sum_{t=1}^{t=6}\left[\delta^{t}\times(1-p_q)^{t}\times(1-p_l)^{t-1}\right] \tag{C2}$$

where we assume that the first payout can only take place at $t=2$ while the first premium is paid at $t=1$ (quit and layoff events are realized between each monthly period), and the quit event is realized just before the layoff event for simplicity. That means that the likelihood of receiving S in period $t=2$ is $p_l\times(1-p_q)$, the likelihood of receiving S in period $t=3$ is $p_l\times(1-p_q)^2\times(1-p_l)$, etc.

Actuarially fair premiums Given the simple framework above, we can calculate the cutoff value $\overline{\pi}$ with the actuarially fair premium value $\widehat{\pi}$ such that $E[TotalPremiumPaid]=E[Payout]$:

$$\widehat{\pi}=p_l\times S\times\frac{\sum_{t=1}^{t=6}\left[\delta^{t}\times(1-p_q)^{t}\times(1-p_l)^{t-1}\right]}{\sum_{t=1}^{t=6}\left[\delta^{t-1}\times(1-p_l)^{t-1}\times(1-p_q)^{t-1}\right]}=p_l\times S\times(1-p_q)\times\delta \tag{C3}$$

C.2 Calibrating Lay-Off and Quit Rates

We compare demand for displacement insurance to three different benchmarks.

Benchmark First, we calculate the average lay-off and quit probabilities for Ethiopia based on existing evidence. Shiferaw and Söderbom (2023) document 6-month separation rate of 17.95%. We assume that 20% of these separations are due to layoffs based on data on the nature of separations in a representative sample of large firms in Addis Ababa (Abebe and Caria, 2018). Using the simple framework above, we can recover the monthly lay-off and

quit hazards as p_l and p_q:

$$p_l^{bench} = .2 \times \left[1-(1-.1795)^{1/6}\right] = 0.0065 \tag{C4}$$

$$p_q^{bench} = .8 \times \left[1-(1-.1795)^{1/6}\right] = 0.0259 \tag{C5}$$

Subjective probabilities Second, we elicit the subjective probabilities of layoff and quits during the insurance window (assuming that the other event does not take place within the 6-month period) before we elicit respondents' WTP. Specifically, we ask the following two questions:

> *Imagine that you are working in a job with a written contract in January 2024. And imagine that you will not quit the job until August 2024. What is the chance that your employer will* ***lay you off*** *before the end of July 2024?*
>
> *Imagine that you are working in a job with a written contract in January 2024. And imagine that you will not be laid off the job until August 2024. What is the chance that you will have* ***quit*** *before the end of July 2024?*

We denote these subjective probabilities by η_l and η_q. We take the median subjective probabilities of $\eta_l = 0.25$ and $\eta_q = 0.15$ to assess the fiscal viability of the insurance schemes. We can recover p_l and p_q similar to above:

$$E[p_l] = 1-(1-\eta_l)^{1/6} = 1-(1-0.25)^{1/6} = 0.0468 \tag{C6}$$

$$E[p_q] = 1-(1-\eta_q)^{1/6} = 1-(1-0.15)^{1/6} = 0.0267 \tag{C7}$$

$$\tag{C8}$$

This implies expected lay-off rates that are 7.2 times larger than the benchmark based on pre-existing data. Expected quit rates are similar to what we extrapolate from existing data.

Realized separations Finally, we also measure respondents' realized separation rates four to nine months after the endline survey (the mean duration is 6.95 months). We sample all participants who have formal work at endline> We also include a subsample stratified by WTP quartile of those who did not have work at endline. We include all individuals in the second ($N = 53$) and fourth quartile ($N = 46$) and 50 randomly selected individuals in the first and third WTP quartile. In total, we attempted to contact 939 individuals and reached 880 individuals for a response rate of 93.7 percent. We use 764 individuals who are formally employed during or at some point after the endline survey for our analysis. For this sample, we observe potential separations during all or a subset of the period we elicit subjective expectations over. Because individuals have employment spells of different lengths, we estimate monthly quit and lay-off hazards using maximum likelihood estimation. We assume that, in each period,

Table C17: Maximum likelihood estimation of quitting and leaving hazards

Panel A: Pooled sample		
	Monthly	Six monthly
$\widehat{p_q}$	2.07%	11.8%
$\widehat{p_l}$	0.39%	2.35%
Panel B: Sample with jobs at endline		
	Monthly	Six monthly
$\widehat{p_q}$	2.06%	11.75%
$\widehat{p_l}$	0.35%	2.21%
Panel C: Sample with jobs found after endline		
	Monthly	Six monthly
$\widehat{p_q}$	1.65%	9.49%
$\widehat{p_l}$	0.75%	4.39%

Notes: **Table C17 displays maximum likelihood estimates of quit and lay-off hazards after the endline survey. Panel A** shows results for the full sample. **Panel B** shows results for the sample of respondents who were employed at endline in late 2023. **Panel C** shows results for the sample of respondents who were not employed at endline in late 2023 but found a formal job before the follow-up survey.

lay-offs happen after potential quits have been realized (which yields a relatively conservative lay-off rate estimate). Initially, we assume constant probabilities of lay-off and quitting.

$$\phi_i = \begin{cases} \phi_{i,q}(t) = (1-p_q)^{t-1}(1-p_l)^{t-1}p_q, & \text{if } i \text{ quit} \\ \phi_{i,l}(t) = (1-p_q)^{t}(1-p_l)^{t-1}p_l, & \text{if } i \text{ was laid off} \\ \phi_{i,0}(t) = (1-p_q)^{t}(1-p_l)^{t}, & \text{if } i \text{ neither quit nor left} \end{cases} \tag{C9}$$

The aggregate likelihood of observing the data for N individuals is:

$$\Phi = \Pi_{i=1}^{N} \phi_i \tag{C10}$$

We estimate p_q and p_l by maximizing this likelihood. We use monthly (30-day) periods for t. That is we define being laid-off in period t, as being laid off in the t^{th} 30-day period after the endline survey (or after starting the job if that was after the endline survey; constant hazards mean that the estimation is independent of the start point).

The resulting estimates are in Table C17. The difference in quit rates between the samples is not statistically significant ($p=0.611$), but the lay-off rates are substantially higher in the sample without jobs at the endline ($p=0.001$). This suggests that lay-off rates decrease in employee's tenure.

Table C18: Predictive validity of willingness-to-pay

	WTP lump sum			WTP monthly		
	(1)	(2)	(3)	(4)	(5)	(6)
Perceived lay-off hazard	222.218***		228.727***	173.024***		180.106***
	(54.583)		(49.705)	(56.985)		(52.516)
Perceived quit hazard		-64.958***	-68.217***		-71.662***	-74.228***
		(8.795)	(10.209)		(7.387)	(8.526)
Outcome mean	93.78	93.78	93.78	94.40	94.40	94.40
Observations	1694	1694	1694	1694	1694	1694

Notes: **Table C18 shows that perceived lay-off probabilities are positively correlated with WTP for displacement insurance while perceived quit probabilities are negatively correlated with WTP.** Columns 1 to 3 show correlations with WTP for the lump sum scheme. Columns 4 to 6 show correlations with the monthly payment scheme. WTP is measured in Ethiopian Birr per month of formal employment. Hazard refers to implied monthly hazards ranging between 0 and 1. The mean monthly lay-off hazard is 0.062 and the mean monthly quit hazard is 0.087. Robust standard errors are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Discount factor We calibrate the monthly payment scheme to have the same net present value as the lump sum payment assuming an expected annual inflation rate of 34%. We use the same inflation rate to impute the monthly discount factor as:

$$\delta = \frac{1}{1.34^{1/12}} = 0.976 \tag{C11}$$

C.3 Data Quality

Understanding checks We conducted a range of understanding checks to ensure that respondents understood how the wage subsidy and displacement insurance schemes worked. If respondents answered wrongly on one of the checks, enumerators were instructed to reexplain the scheme and give them the opportunity to ask further questions. On average, respondents answer 95% of understanding check questions correctly, indicating that the enumerators convey the WTP elicitation reliably.

Predictive validity To assess the predictive validity of our measures, we also correlate perceived lay-off and quit probabilities with elicited WTP for displacement insurance. We elicit these probabilities before eliciting WTP. Table C18 shows correlations are in line with theoretical predictions. We observe a strong positive correlation between WTP and perceived lay-off probability and a strong negative correlation between WTP and perceived quit probability. This suggests that respondents understand the nature of the displacement insurance schemes.