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

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Abstract

Job loss is an understudied risk for formal workers in lower-income countries. In these settings, lump-sum severance pay is often the only source of job-loss insurance. We quasiexperimentally show that female factory workers in Ethiopia displaced by a tariff hike experience lasting declines in employment and consumption spending, and rising poverty. Experimentally, we find that additional lump-sum support induces early spending and reduces overall and manufacturing employment persistently. Disbursing an equivalent amount in tranches improves consumption smoothing and avoids adverse employment effects. Further, we document a high willingness to pay for additional insurance, alongside heterogeneous preferences over disbursement modality that shape responses to our interventions. These findings imply that increasing job-loss insurance raises welfare, although moving away from the lump-sum default can generate substantial additional gains.

JEL classification

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Keywords

job-loss, job-loss insurance, trade shock

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Job loss is an understudied risk for formal workers in low- and lower-middle-income countries (Donovan et al., 2023; Gerard et al., 2025). A large and expanding literature has studied the barriers that prevent workers in these settings from finding formal employment (Bandiera et al., 2022; Caria et al., 2024; Carranza and McKenzie, 2024; Ulyssea, 2020). However, much less is known about what happens to these workers when they lose a job and how to best protect them against this shock — a key evidence gap as trade disruptions, automation, and climate change increasingly threaten formal jobs in lower-income countries.[1]

To what extent are formal workers in lower-income countries able to cope with job loss, and how should additional support be provided? Evidence on the effects of job loss on workers' consumption and subsequent employment in these settings remains scant. For instance, while *formal* financial support after job loss is more limited, informal work and transfers from others may partially substitute for formal assistance. Further, in lower-income countries, the main source of formal support after job loss comes from severance pay (SP) policies, which mandate employers to provide an unconditional lump-sum payment to displaced formal workers (Gerard et al., 2025). Unemployment insurance (UI) programs — which provide regular payments conditional on remaining without a formal job — are very rare.[2] Concentrating support in a single lump-sum transfer may not be optimal as workers with self-control problems may find it hard to regulate their spending (Gerard and Naritomi, 2021). However, the welfare gains from boosting job-loss insurance payments and spreading them over time remain undocumented.

These questions are especially relevant for workers in the export-oriented manufacturing sector. First, these workers are highly exposed to external shocks, such as those arising from recent US tariff hikes or the planned EU carbon-adjustment mechanism (Goldberg and Ruta, 2025). Second, increasing employment in this sector remains a central objective of industrial policy in many lower-income countries because of its development externalities (Juhász et al.,

[1] We use the term 'lower-income countries' to jointly refer to low- and lower-middle-income countries. See Dix-Carneiro and Kovak (2025) for a recent review of the employment effects of tariff shocks. Kala et al. (2023) summarize the key evidence on the impacts of climate shocks in developing countries. Demombynes et al. (2025) estimate that about 12 percent of jobs in low-income countries and 15 percent of jobs in lower-middle-income countries are at high risk of automation from AI.

[2] UI programs are found only in a few lower-middle-income countries and are non-existent in low-income countries. 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 (Parsons, 2016).

2023; Oqubay, 2015), including the fact that it is often a source of employment for women (Heath and Mobarak, 2015). Third, many of the investments necessary to develop this sector are worker- and sector-specific — relocating, acquiring new skills, adjusting to new work and life arrangements (Bick et al., 2018; Blattman and Dercon, 2018; Lagakos et al., 2023; Thompson, 2017) — and are lost if a worker leaves the sector and migrates away. These features make any potential tradeoff between consumption smoothing and employment outcomes – a core concern in the optimal insurance literature – particularly salient for these workers.

Our central contribution is to show that raising financial support after job loss yields large welfare gains for female factory workers in Ethiopia displaced by a tariff shock, and that these workers would be willing to pay for the additional job-loss insurance. However, larger transfers would generate adverse employment effects and undermine industrial policy objectives unless the structure of these payments is shifted from lump-sum (the status quo) to monthly disbursements. Moreover, there is sizable demand for such tranche payments, particularly from workers seeking better control over their spending. Conceptually, the contrasting results for lump-sum and monthly payments indicate that the negative employment effect of lump-sum transfers at layoff – documented in various settings (e.g., Britto, 2022; Card et al., 2007; LaLumia, 2013) – may not be driven by the canonical liquidity effect of Chetty (2008).[3] From a policy perspective, our results have implications for both the *level* and *structure* of job-loss insurance in lower-income countries, highlighting a promising direction for expanding the limited social insurance provision in these settings (Banerjee et al., 2024).

The study setting is ideal for three reasons. First, Ethiopia has invested heavily in export-based industrialization through the creation of industrial parks such as the one where our study takes place, the Hawassa Industrial Park (HIP). HIP focuses on garment manufacturing, hosts 20 international investors, and employs over 25,000 mostly female internal migrants in a city of 400,000 people (*Hawassa*). Thus, the workers in our study are part of one of the most important government initiatives to promote structural transformation and growth in the country. Second, in early 2022, Ethiopian exporters to the US suffered a sudden hike in

[3] In his seminal paper decomposing the employment effects of UI into a substitution effect and a liquidity effect, Chetty (2008) argues that one can use variation from lump-sum severance payments to inform the impact of unconditional tranche payments of the sort that we study in this paper – which are instrumental to such a decomposition – because their employment effects are similar in his framework. We discuss this point in part B of Section 2.2.1.

tariffs caused by the termination of Ethiopia's AGOA membership. This shock caused a mass layoff in one firm in the park (the "study firm"), but did not affect employment in other firms (which had more geographically diversified buyer networks) at that time. This provides a suitable empirical setting for our analysis. Third, while our results refer to a specific population, large numbers of similarly low-skilled female manufacturing workers are found across Africa and South Asia today, and, historically, played a key role in the industrialization of East Asia, Europe, and North America (Chang, 2009; Smil, 2013; Stiglitz, 1996; Tsurumi, 1990; Zamagni, 1990). Our results may thus generalize beyond our context, as we discuss in Section 4.

In this paper, we combine several research designs and multiple rounds of data collection to quantify the impacts of additional job-loss payments, the consequences of job loss, and workers' willingness to pay (WTP) for job-loss insurance — all topics on which there is minimal evidence for lower-income countries. In our main piece of analysis, we *experimentally* estimate the impacts of providing additional financial support for a sample of 1,410 workers laid off by the study firm. All workers in this *displaced* sample receive the statutory severance pay that employers are required to pay in Ethiopia, worth about two to three monthly wages in this sample. We offer additional support to randomly chosen workers either in a single lump-sum or in five monthly tranches. We calibrate the transfer value to approximate the difference in support available to workers in upper-middle-income countries such as South Africa and that available to Ethiopian workers. The lump-sum treatment pays the entire amount just after job loss. 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 status and can thus be implemented in a low-capacity environment. Second, we *quasi-experimentally* estimate the impacts of job loss by comparing the outcomes of the displaced controls to those of 403 comparable workers employed in a nearby unaffected firm (the *non-displaced* sample), who remained employed with very high rates over the entire period. Our methods are similar to those employed in the recent literature on job loss in higher-income countries (Gerard and Naritomi, 2021; Kolsrud et al., 2018). Third, we carry out *controlled choice tasks* to measure workers' willingness to pay and private surplus for different forms of additional job-loss insurance (Landais and Spinnewijn, 2021). We then use a standard welfare framework to highlight the implications of our findings for welfare and policy design.

In the next paragraphs, we describe the results of these three exercises in detail. First, we document large negative and lasting impacts of job loss on employment and consumption spending. Most displaced control workers are unable to secure a new job quickly, and thus search for work (mostly blue-collar jobs in the industrial park) or migrate out of the city (mostly back home in small towns and rural areas). Employment rates grow for about eight months and then stabilize, while migration rates increase gradually. Fourteen months after job loss, the displaced controls are 32 percentage points (pp) less likely to be employed than workers in the non-displaced sample, 35 pp less likely to be employed in manufacturing, and 21 pp less likely to live in Hawassa. Over this period, labor income is halved. These losses are only partly mitigated by formal insurance — statutory SP — and informal insurance — transfers from family and friends. Total expenditure drops by 10 percent on average, and by 14 percent fourteen months after layoff. Further, extreme (expenditure) poverty rates more than double. Overall, these results indicate that job loss generates a large fall in welfare in our sample, and that, due to the out-migration and fall in manufacturing employment, the investments incurred to generate many of the original jobs (relocation, training, work and life adjustments) are lost.

Second, we turn to the experimental results. Additional job-loss payments disbursed in monthly tranches raise total expenditure over the fourteen months after job loss, mitigating on average 40 percent of the expenditure drop induced by job loss. Lump-sum payments, on the other hand, raise expenditure strongly just after the shock — an effect that is *not* driven by expenditure on durables — but impacts shrink quickly. The average impact on expenditure over the study period is about 75 percent lower than that of monthly payments. A key driver of these diverging effects is the employment response to the two interventions. Lump-sum payments induce a substantial and persistent reduction in wage and manufacturing employment, which is not compensated by a short-lived increase in self-employment. Fourteen months after job loss, manufacturing employment drops by 11 pp and out-migration (mostly back home) increases by 8 pp. Monthly payments, by contrast, have no persistent effect on employment or migration (and only modest impacts on these variables throughout the study period). Thus, changing the structure of job-loss payments from lump-sum to monthly disbursements can both improve consumption smoothing and avoid adverse employment effects.

These results are hard to reconcile with canonical models of consumption smoothing and job search in the UI literature (Chetty, 2008). We shed light on the mechanisms underlying them by leveraging an incentivized policy preference measure collected at baseline. First, we show that the short-run expenditure hike induced by lump-sum payments compared to the monthly treatment is driven by workers who expressed a strong preference to be allocated to the monthly treatment, and justified it as a way to better control their spending. This finding is consistent with the models of sophisticated present bias and temptation spending that motivated our monthly treatment in the first place (Gerard and Naritomi, 2021). Second, we show that the reduction in employment and the increase in out-migration induced by lump-sum payments, relative to monthly payments, is driven by workers who *did not* express a strong demand for the monthly treatment at baseline. The lump-sum enabled these workers to transition away from manufacturing work, with multi-faceted consequences.[4] This suggests that, while lump-sum transfers generate a commitment problem for some workers, they are valued by other workers who need cash-on-hand to pursue their plans.

Third, we show that additional job-loss insurance leads to large welfare gains, but also clarify a key tradeoff in the design of this policy between workers' private surplus and the government industrialization objective. In particular, we use a Becker-DeGroot-Marschak mechanism to quantify workers' WTP for additional job-loss insurance, and recover their private surplus from the policy. We then compare the private surplus to two sources of efficiency losses arising from the policy: (i) a standard 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 manufacturing employment). We perform two exercises. First, we evaluate different modalities of disbursing additional support. We find that monthly payments dominate lump-sum payments, since they generate similar private surplus and lower externalities. We also show that allowing workers to choose their preferred scheme (i) substantially raises surplus because of the heterogeneity in workers' preferences, but (ii) generates larger fiscal and industrialization externalities, compared to mandating monthly payments. If the industrial policy externality does not exceed 16% of workers' wages, then allowing workers to choose

[4] By our endline survey, they were less likely to aspire to have a factory job and to engage in any training than workers with similar preferences who received monthly payments, and they were more likely to be married and have children. We did not anticipate these latter results, but they echo recent evidence highlighting that demands from the marriage market may arise quickly for unemployed young women in lower-income countries (Bandiera et al., 2025).

their preferred payment modality is optimal.[5] Second, we show that expanding job-loss insurance would raise welfare in our context. Regardless of the payment policy, we document a substantial willingness to pay for additional insurance, well above the actuarially fair price, implying a private surplus in the upper range of recent estimates (Landais and Spinnewijn, 2021) that vastly exceeds the likely externalities. Importantly, we document a similarly high private surplus for a representative sample of formal workers in the capital city, Addis Ababa, indicating that workers in other contexts may also value additional job-loss insurance highly.

Our results make three key contributions to the literature. First, we provide novel evidence on the persistent impacts of job loss for formal workers in a lower-income country. Job loss in higher-income countries has received remarkable attention over the last thirty years. More recently, several papers study the impacts of job loss in upper-middle-income countries (e.g., Britto et al., 2022b; Gerard and Gonzaga, 2021; Gerard and Naritomi, 2021; Liepmann and Pignatti, 2024). Whether job loss has more or less pronounced consequences in lower-income countries is an open question. Our results show that, about one year after job loss, the workers in our sample experience employment losses that are about twice as large as those of workers in the U.S. (Fallick et al., 2025) or Sweden (Athey et al., 2026). Consumption expenditure losses are similar to those experienced by Brazilian workers (Gerard and Naritomi, 2021), but they are mitigated by a form of informal insurance — kinship transfers — that does not appear as relevant in upper-middle-income countries (Liepmann and Pignatti, 2024). Related to this first contribution, the closest paper to ours is Hardy et al. (2024), which provides correlational evidence from the Hawassa Industrial Park on the food security and depression effects of non-employment during the early months of the COVID-19 pandemic.

Second, we present unique experimental evidence on the impacts of providing more generous support after job loss. No study to date has been able to use experimental variation in the size or modality of financial support after job loss.[6] Our results thus contribute to a growing literature that studies social insurance programs in lower-income countries and in particular

[5] While this figure may appear high, it is important to note that it would include any excess of workers' marginal product over and above their wage (and the taxes levied on it), as well as the broader development externalities that motivate governments to invest substantial resources in creating such factory jobs. So, empirically, we expect that some governments may judge that the externality exceeds this threshold in their context.

[6] See Hanna et al. (forthcoming) for the results of an innovative policy targeted at informal workers in Indonesia.

job-loss insurance (Gerard et al., 2025). Most importantly, we show promising results on the impacts 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. We document two key advantages of this policy over the default of lump-sum transfers in lower-income countries. First, it enables beneficiaries to achieve better consumption smoothing. Through a detailed mechanism analysis, we can attribute this effect to the existence of a group of sophisticated individuals with imperfect self-control, as hypothesized by Gerard and Naritomi (2021) for the Brazilian setting. Second, it mitigates adverse employment effects. This is a surprising effect, which is not contemplated in the canonical literature on job-loss insurance (Chetty, 2008), and contrasts the recent finding that, among the rural poor, lumpy transfers encourage employment (Kansikas et al., 2025).[7] Our results point to the relevance of fixed costs for urban workers' coping strategies after job loss, and document that some of these strategies, such as moving back home, may actually decrease long-term employment.

Third, we provide the first estimates for a lower-income country of the welfare gains from additional job-loss insurance. Two lessons emerge from this analysis. First, formal workers appear to value additional insurance highly. Second, preferences for disbursement modalities are highly heterogeneous, so that, in line with recent evidence on the benefits of policy personalization (Dizon-Ross and Zucker, 2023), allowing workers to choose their preferred modality would increase worker welfare. These findings showcase, in a policy-relevant context, the welfare gains from expanding and personalizing social insurance in lower-income countries.

1 The RCT and the Natural Experiment

We begin by describing the context of our study, the quasi-experimental variation that we use to study the effect of job loss, and the randomized control trial that we design to evaluate the impact of additional job loss support payments.

[7] An additional dimension of impact explored in the literature is that of informality. For example, Liepmann and Pignatti (2024) shows that, in Mauritius, UI encourages informal employment, while Ndiaye et al. (2024) discuss the implication of informality and fraudulent claims for the creation of a UI system in Senegal. Informality effects are less relevant for our monthly treatment, since payments are not conditional on formal employment.

1.1 Context

Over the past decade, the Ethiopian government has actively promoted industrialization by establishing 18 Special Economic Zones, commonly known as “Industrial Parks," focusing on export-oriented light manufacturing. Our study takes place in the Hawassa Industrial Park (HIP), which is located in Hawassa, a city of 400,000 people in the southern Sidama Region. It is one of the largest industrial parks in Ethiopia: as of 2022, it housed over 20 firms in the ready-made garment (RMG) industry, employing more than 25,000 workers (see Figure A1 in the Appendix), with approximately 90% being female.

The mass layoff examined in this study was triggered by a tariff shock. The RMG company involved – our partner in a previous project, henceforth the “study firm" – relied heavily on exports to the U.S. market. Prior to 2021, under the Africa Growth and Opportunity Act (AGOA), investors were able to export goods produced in Ethiopia to the United States with preferential tariffs. However, Ethiopia lost its AGOA beneficiary status on December 31, 2021, due to concerns related to the civil war in the Tigray region. Orders from the U.S. declined dramatically and, 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 followed a “last-in-first-out” approach and included all workers with less than 24 months of tenure at the firm.[8]

We note that, to our knowledge, no other mass layoff occurred around the same time in the Hawassa Industrial Park, likely because other investors had stronger ties to markets in Europe and the Middle East. In fact, as shown in Figure A1, the level of employment in the park was fairly stable in the lead-up and 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, labor market conditions were otherwise not unusual. Importantly, the civil war did not have a direct effect on this labor market, since Tigray is located at the opposite end of the country.

[8] We were informed of the layoffs before these were announced to workers since we were running a field experiment on pay bonuses with the study firm. Some workers in the laid-off sample were part of this earlier experiment. Table A1 shows that participation and treatment status in the earlier experiment does not affect our key treatment effect estimates.

1.2 The Displaced Sample and the Non-Displaced Sample

We collect data from two separate samples of workers. First, we draw a random sample of 1,410 factory-floor workers from a list of 2,000 female workers employed by the study firm in June 2022 and made redundant at the end of August 2022. We conduct our experiment evaluating the impact of job-loss payments with this *displaced sample*. Second, we draw a random sample of 403 women from a list of workers with fewer than 24 months of tenure employed by a neighboring RMG factory in the Hawassa Industrial Park. We refer to this sample as the *non-displaced sample*, as there was no mass layoff in this factory at that time. We study the effects of job loss via a difference-in-differences design, comparing outcomes over time for control-group workers in the displaced sample and for all workers in the non-displaced sample.

Column (1) of Table A2 summarizes the demographics of these 1,813 female workers (using our baseline survey). They are young – 22 years old on average – and most of them are unmarried (87 percent) with no children (they have 0.15 children on average), have at least a high school education (95 percent; 25 percent have university education), and migrated to Hawassa (3 percent were born in the city) from surrounding areas (85 percent speak the local Sidamegna as mother tongue; 91 percent are Protestant, the main religion in Sidama), including many from rural areas (60 percent). Moreover, they are still investing in skill acquisition (42 percent are enrolled in some form of education while working). In June 2022, when the mass layoff was announced, they had on average 12.8 months of tenure and specialized in one of four job types: packer (9 percent), cutter, help, or layer (10 percent), sewer (62 percent), and quality controller and printer (14 percent). They derived most of their monthly income (1772 Ethiopian Birr – ETB – on average) from their main job (1442 ETB on average), but were still the net recipient of kinship transfers.[9] Accordingly, they report spending an amount close to their monthly income, and had limited liquid savings, amounting to less than half of one monthly wage on average. This vulnerability to negative income shocks is exacerbated by the fact that their average reported total expenditures is only about 39% above the international extreme poverty line. A moderate shock could thus push many of these workers into extreme poverty.

How representative is this sample? And what are the plausible outside options of these

[9] The exchange rate implies that 100 ETB equaled 5.09 USD PPP at the time of our experiment.

workers after layoff? We provide some answers using data from the 2021 National Labor Force Survey. Table A3 compares the demographic characteristics of the women in our sample to those of a representative sample of working-age women employed in the broader garment and textile sector in Ethiopia.[10] We find that women in our sample are fairly representative of women formally employed in the sector, though they are somewhat younger, less likely to be married, and less likely to live in their place of birth. The key difference is education: virtually all women in our sample have secondary education, since factories in HIP restrict employment to those who have secondary education, whereas, nationally, only 29 percent of the women formally employed in the sector have secondary education. Overall, this suggests that the women in our sample are likely to have a higher propensity to work (due to lower marriage rates and migrant status) and to have better outside options (due to their education) compared to the typical women formally employed in the sector. We explore what these outside options may be in Table A4. Focusing on women aged 18-27 — the age range in our sample — we look at the types of employment of those who live in an urban area (column 1), those with secondary education (column 2), those who are migrants (column 3), and finally, those who share all three characteristics, which best match our sample. One potential outside option is to leave the labor force. Indeed, only about 50% of young women with secondary education are employed, including among urban migrants.[11] Conditional on staying in the labor force, the most likely outside option is wage employment. For instance, among urban migrants with a secondary education who work, the vast majority are wage employees.[12] Only 13.9 percent are self-employed and 4.4 percent unpaid workers. These two categories also capture the main informality margin in our context, as there is little informality among private-sector wage employees (more than 75 percent are formally employed).

1.3 Experimental Variation in Job-Loss Payments

We study the impacts of two kinds of job-loss payments experimentally. First, we consider a lump-sum payment — the payment modality of severance pay (SP), which is the main source

[10] Women make up 56 percent of the workforce in the garment and textile sector and they are 54 percent of the formal workforce in the sector.

[11] About 24% are out-of-labor-force students, 36% are married and 43% are staying home with their parents.

[12] About 15% of wage employees work in manufacturing. Work in education, health or retail is also common.

of job-displacement insurance in lower-income countries and the only one in Ethiopia. This treatment arm aims to assess the impact of increasing payment amounts above their statutory level on workers' outcomes. Gerard and Naritomi (2021) highlight that lump-sum payments may only yield limited insurance value if consumption is over-sensitive to cash-on-hand after job loss. We thus consider a second treatment that provides a comparable amount in monthly tranches, a payment modality that — like in UI programs — is better aligned with the consumption-smoothing goal of job-loss insurance. However, this treatment does not condition payments on reemployment outcomes, so that we assess the impact of a form of payment that — like SP — does not generate moral hazard and can be implemented in a lower-capacity environment. This type of job-loss insurance scheme — unconditional support paid in tranches — has been proposed in Gerard and Naritomi (2021) but has not been adopted in any country.

We randomize workers in the displaced sample into three groups stratifying over tenure and job type.[13] Workers in the **monthly** payment group (488 workers) receive a monthly payment from our data-collection partner, EconInsight, for five consecutive months following layoff, beginning in October 2022. Each payment is equal to 810 ETB per month, or about 60% of the median worker's salary in June 2022.[14] Although the payments are disbursed in tranches, as in many UI programs in richer countries, we emphasize again that they are made regardless of workers' reemployment outcomes, thereby preserving the unconditional nature of SP policies. 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 ETB, which is equivalent to the value of the monthly payments discounted for expected inflation. Workers learn about the payments that they will receive from EconInsight at the end of our baseline interview, in late July or early August 2022. These are presented as having the objective of supporting workers after job loss and are transferred directly to workers' bank accounts, minimizing transaction

[13] Using administrative data provided by the two firms prior to the baseline, we created four tenure groups (less than 6 months, between 6 and 12 months, between 12 and 18 months, more than 18 months) and five job types (cutters, helpers and layers; packers; quality controllers or printers; sewing machine operators; other occupations).

[14] These payments are in line with those available to workers in other African countries that have a UI system in place and who claim full unemployment benefits. For example, in South Africa, the maximum replacement rate for a minimum wage worker is 50 percent and can last up to about 8 months (Bhorat et al., 2011; Horn, 2021). In Egypt, there is a sliding replacement rate from 75% to 45%, lasting up to 28 weeks (ISSA, 2022). In general, ILO convention 168 recommends a minimum replacement rate of 50 percent and a minimum duration of 26 weeks (ILO, 1988).

costs and enabling workers to collect their payments from any ATM in the country.[15]

Workers in the control group and in the non-displaced sample do not receive any payment from EconInsight. However, 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 under Ethiopian labor law. This consists of a one-off severance pay from the study firm equal to about two to three months of salary, typically received in September 2022.[16]

1.4 Data Collection

Figure 1 shows the timeline for the project. Data was collected at seven points in time. The baseline survey was conducted face-to-face in late July and early August 2022 for the displaced sample and in late August and early September 2022 for the non-displaced sample.[17] It asked questions about each of the preceding three months separately, so that we have three monthly observations for each individual prior to the layoff of workers in the displaced sample. Five rounds of "high-frequency" surveys were conducted over the phone between November 2022 and August 2023, with each survey having a two- or three-month recall period to cover monthly outcomes between September 2022 and July 2023. Finally, we asked for outcomes in October 2023 – and for a subset of outcomes in August and September 2023 – during the face-to-face endline survey, which we started to collect in December 2023.[18] We thus have monthly panel data for a series of key variables (e.g., employment) with up to 14 monthly observations per individual following the mass layoff at the study firm. We also have additional information at the time of each survey (e.g., characteristics of the more recent job, mental health).

A. The baseline survey collected detailed information about socio-demographic characteristics and relevant outcome variables of study participants, such as expenditures, job-search,

[15] All workers have a bank account since monthly salaries at HIP are generally paid through bank transfers. ATMs are easily accessible throughout the country, including in rural areas.

[16] Workers who had been at the firm for over a year received three monthly salaries (around 4500 ETB); those with less than a year of tenure received two monthly salaries (around 3000 ETB). In addition, workers were paid for unpaid leave days, typically between 300 and 500 ETB.

[17] The slight difference in survey timing is due to the fact that we prioritized surveying the displaced sample first, to ensure that experimental payments could be administered as close as possible to the statutory severance pay.

[18] All recall questions in our surveys refer to the Ethiopian calendar, which does not perfectly overlap with the Gregorian calendar. The Ethiopian calendar has twelve months of thirty days and one short five-day month in mid September (Pagumē). We have no recall data for Pagumē directly following the baseline data collection. Month zero in our study is the Ethiopian month of Meskerem in 2022 (the Ethiopian year 2015) which started on September 11th. We ask for combined Pagumē and September recall in the endline survey. For simplicity, we use Gregorian dates throughout the paper and abstract from the difference between the two calendar systems.

employment characteristics, finances (income, saving and debt), life satisfaction, physical and mental health, and autonomy from partner and parents.

Importantly, prior to treatment announcement, we collected an incentivized measure of preferred payment modality among workers in the lump-sum and monthly groups.[19] These workers were asked to express their preference for either the lump-sum or the monthly payments. The task is incentivized because workers were told that their preference may determine the treatment that they will eventually be offered (five workers had their preference implemented for real). Furthermore, the question is repeated for different values of the lump-sum transfer, to identify the strength of the preference for either payment modality.

Panel (a) of Figure A2 displays the distribution of the value of the lump-sum payment at which workers switch from preferring the monthly scheme to preferring the lump-sum payment. At the values offered in the experiment, only 42% of respondents would prefer the lump-sum payment to the monthly payments, although there is substantial heterogeneity in preferences. About half of the sample is approximately indifferent between the two treatment arms: their switch point falls within 15% of 3850 ETB, the value of the lump-sum in our experiment (and within 15% of 4050 ETB, the nominal value of the sum of monthly payments). However, a quarter of the sample expresses a clear preference for the monthly payment: they require a lump-sum above 4500 ETB to switch to that payment modality. In other words, these workers have a sizable willingness-to-pay (WTP) for tranche payments. Moreover, panel (b) of Figure A2 shows that the main reason for these workers to prefer the monthly scheme is to better "control their spending",[20] consistent with survey evidence in Gerard and Naritomi (2021). In the analysis below, we thus study heterogeneity with respect to a dummy for having a strong preference for monthly payments, as captured by the quarter of the sample that most strongly prefers monthly payments. The type of model used in Gerard and Naritomi (2021) – with sophisticated present-biased agents – would predict that these workers' consumption

[19] 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 were aware that they are being randomized in different experimental groups, but were not given precise information about the nature of the different treatments).

[20] Pressure to spend seems to be mostly internal, since only 10.5 percent of workers report that monthly payments would allow them to resist spending pressure from others.

expenditures after layoff are particularly sensitive to the modality of experimental payments.[21]

B. The high-frequency surveys collected information on a subset of outcomes that we wanted to trace over time and that we expected to change frequently between the baseline and endline surveys: expenditures, job-search, employment, finances (income, saving and debt), life satisfaction and mental health, and autonomy from partner and parents.

C. The endline survey covers all outcomes included in the high-frequency surveys, as well as more comprehensive modules related to aspirations, friendship networks, savings stock, and psychological well-being. We also collected additional information on informal transfers: the providers of kinship transfers, the expectations embedded in them (e.g., reciprocity, repayment), and whether any money was transferred to co-workers. Additionally, we conducted another elicitation task at endline to recover workers' demand for higher job displacement insurance coverage. This exercise complements the elicitation task at baseline, which only recovers the relative demand between two payment modalities. Specifically, we elicit workers' WTP for an insurance product that would provide an additional payment at layoff equivalent to the payment we offered in our experiment (see Section 3 for more detail).

In Appendix B, we include a detailed discussion of how we aggregated data across surveys to create a time series of outcomes, as well as a number of checks showing the robustness of our expenditure measure to potential confounders.

Finally, we conducted two separate data collection exercises to shed further light on our findings. We surveyed a **representative sample of formal workers in Addis Ababa**, Ethiopia's capital city, between August and September 2025. The main goal is to assess the external validity of our endline estimates of the demand for higher job displacement insurance coverage in a different labor market and time period. Moreover, before we started presenting our results (in the summer of 2023), we elicited **economists' priors** on the impacts of job loss for our sample, on the extent to which job-loss payments can mitigate these impacts, on the relative effect of the two payment modalities, and on workers' demand for higher coverage.[22]

[21] We do not study the quarter of the sample that express a strong preference for the lump-sum payment separately because we do not have a clear prediction for these workers; this group could, for instance, include both naive myopic agents and individuals with a strong demand for lumpy expenses.

[22] We posted a belief-elicitation survey on the Social Science Prediction Platform. To publicize the survey, we sent out personal email invitations to 365 economists selected from the list of individuals who would attend the NBER 2023 summer meetings in development and public economics, as well as to a list of researchers affiliated with the

1.5 Balance and sample composition

Table A2 compares our samples in two ways. First, it shows that the three experimental groups that make up the displaced sample have good balance in terms of demographic, employment, financial, and well-being variables. For each variable, columns (2)-(4) report its mean in each group and column (5) reports a p-value for a test of the null hypothesis that the variable is balanced across all three groups. Out of 31 balance tests, we find two imbalances significant at the five percent level and two significant at the ten percent level, in line with a successful randomization. We account for these imbalances in our empirical strategy (see Section 1.7), but we note that their magnitude is quantitatively small. For example, the controls have about one week of additional tenure in the firm compared to the two treatment groups; and the lump-sum group has about 0.05 children in school more than the control. The other variables we study, capturing age, marital status, religion, geographical origin, education, the nature of the work in the study firm, earnings, transfers, expenditure, savings and well-being, are all balanced.

Second, columns (6)-(8) study sample composition on the same set of variables between the experimental control group and the non-displaced sample, i.e., the two samples used in our difference-in-differences analysis of the effects of job loss. Overall, these two samples are very similar in their core characteristics: young women with a rural origin and secondary education, who work on the factory floor of a large RMG manufacturing firm and have about one year of tenure. It is thus plausible that the non-displaced sample offers a reasonable counterfactual of what would have happened to the displaced controls had they not been laid off. However, we also note that there are small, but precisely estimated differences along some dimensions, which we also account for in our empirical strategy (see Section 1.7). Regarding demographics, the two samples are balanced in terms of marriage, fertility, rural origin, and secondary school education. The non-displaced sample is, however, about six months older, and more likely to be protestant, to speak Sidamegna and to have some University education. Regarding workplace characteristics, the non-displaced sample has similar tenure levels as the displaced controls, and is equally likely to work as sewing machine operator or in storage or quality assurance. They are, however, less likely to be packers and more likely to be cutters or helpers.

Institute for Fiscal Studies in London. We obtained 84 responses. See Appendix C for more detail.

Earnings and savings are also lower among the non-displaced, while total and core expenditure are similar across the two groups. Finally, while job satisfaction is similar, the displaced controls are more satisfied with their lives but have a higher depression index.

1.6 Attrition

Table A5 shows the relationship between attrition, treatment assignment and baseline characteristics. We have low attrition: 92.4% of baseline respondents were interviewed at endline and 99.4 percent of individuals were interviewed at least once after layoff. We see no differential attrition by treatment group, with coefficients generally 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 covariates predict attrition: having above median baseline savings and speaking the regional language reduce attrition. The interactions of treatment dummies with covariates also do not significantly predict either attrition outcomes (two out of 36 coefficients are significant). Overall, we conclude that attrition is low, non-differential and unlikely to affect our conclusions.

1.7 Analysis

In the empirical analysis, we first estimate the impacts of job loss by comparing the evolution of outcomes of the displaced controls to that of workers in the non-displaced sample. We then estimate the impacts of job-loss payments by comparing the outcomes of displaced individuals treated with either lump-sum or monthly payments to those of the displaced controls.[23]

We study **the impacts of job loss** using a standard difference-in-differences specification:

$$Y_{it} = \alpha + \beta_1 \text{Job loss}_i \times \text{Post}_t + w_i + t_t + \epsilon_{it}, \tag{1}$$

where Y_{it} is an outcome of interest for worker i in period t. Job loss$_i$ is a dummy variable taking the value of 0 for workers in the non-displaced sample and the value of 1 for displaced controls, and we estimate impacts across different post-layoff periods, Post$_t$. Because the tim-

[23] Our experimental analysis was pre-registered here: https://www.socialscienceregistry.org/trials/10551. We pre-specified our core hypotheses before the beginning of the experiment. Before the endline survey, we pre-specified a more focused set of core hypotheses, which guide our presentation in the draft. We discuss how our analysis deviates from these plans and show the primary pre-specified analysis in Appendix E.

ing of baseline surveys differs slightly across samples, June 2022 is the only pre-layoff month for which we observe all workers; therefore, it serves as the single pre-period in our specification. The worker fixed effects w_i control for time-invariant differences between workers in the two samples and the time fixed effects t_t for shocks common to workers in both samples. The coefficient β_1 is thus the difference-in-differences estimator of the impact of job loss under a common trend assumption. Standard errors are clustered at the level of the worker.

We present two sets of estimates in the paper, which capture two different counterfactuals used in the literature on the impact of job loss in higher-income countries. In the main text, we compare the evolution of outcomes for the displaced controls to those of the entire non-displaced sample. As these workers can separate from their job after the baseline survey, this captures the impact of a single job-loss event. In the Appendix, we present estimates of the impact of job loss against the counterfactual of continuous employment (as in, e.g., Gerard and Naritomi, 2021; Kolsrud et al., 2018). To do so, we restrict attention to the subset of workers in the non-displaced sample who remained employed in all months of the study.

As discussed in Section 1.5, there are some (quantitatively small) differences in outcomes of interest between the two groups at baseline. Therefore, we estimate equation (1) using weights that ensure full balance on each outcome Y in June 2022 prior to layoff. This addresses the potential identification concern that outcome trends could be correlated with their baseline values (or common shocks could have heterogeneous effects by baseline value). This concern is relevant in our setting because we cannot provide standard pre-trend tests to support the common trend assumption underlying our identification strategy.

Finally, one challenge in our setting is that the baseline survey was conducted only after workers in the displaced sample had been informed of the upcoming layoff, as this information was disclosed to us roughly at the same time as to the workers. We must thus consider the possibility that the baseline survey is affected by workers' responses in anticipation of their layoff. Such responses may not be immediate, so that using June 2022 – when the mass layoff was announced – as pre-period likely mitigates the magnitude of any bias. Quantitatively, this concern is likely small, given that many variables offer limited scope for anticipatory responses. Moreover, they will bias our difference-in-differences estimates towards zero whenever the response goes in the same direction as the main treatment effect, which is likely to be the case for most of the outcomes that we analyze.[24] Our estimates of the impacts of job loss are thus likely to be conservative. Nevertheless, as a robustness check, we also show results using May 2022 – before the layoff was announced – as pre-period for the displaced controls.[25]

We estimate **the impacts of job-loss payments** using the following specification:

$$Y_i = \alpha + \beta_1 \text{Lump sum}_i + \beta_2 \text{Monthly}_i + \mathbf{d}_i + \Gamma \mathbf{X}_i + \epsilon_i \tag{2}$$

where Lump sum_i indicates that a worker belongs to the lump-sum group, Monthly_i indicates that a worker belongs to the monthly group, $\mathbf{d}_i$ is a vector of strata fixed effects, and $\mathbf{X}_i$ is a vector of controls chosen with a double-LASSO regression, including – when available – the baseline level of the outcome.[26] Outcomes Y_i are averaged over one of four pre-specified time periods post-layoff: months 0-1 when both the statutory severance pay and our experimental lump-sum payment were disbursed; month 2-5 when workers in the monthly group still received experimental payments; month 13, the last month covered in our monthly panel; and the whole period between months 0 and 13. Robust standard errors are used throughout.[27]

[24] For example, the displaced controls may reduce expenditure both in anticipation of their layoff and after job loss.

[25] We note that the lack of pre-trend tests and the possibility of anticipatory responses are unfortunate consequences of the data environment in lower-income countries. The literature on job loss in higher-income countries has largely overcome these limitations thanks to extensive administrative datasets or large longitudinal surveys. In the absence of such data sources, studying the effects of job loss on a rich set of outcomes requires data collected specifically for this purpose. In turn, such data collection requires advanced knowledge that a layoff is about to take place, information that is difficult for firms to share with researchers while withholding it from the affected workers themselves.

[26] The double-LASSO selects our controls from a long list of variables measured at baseline, which capture (i) the baseline level of the outcome, (ii) 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), (iii) education outcomes (tertiary and secondary education dummies, current enrollment dummies (any, university, diploma), dummies indicating future education plans (any, university, diploma), (iv) preferences (life and job satisfaction, risk preferences, wage expectations), (v) labor income, and (vi) expenditure in the three months before layoff (core expenditure, total expenditure, net transfers, food expenditure). For completeness, we also report estimates of the impact of job loss comparing outcomes between displaced controls and those of workers in the non-displaced sample using the same double-LASSO estimator in Table A8. However, our preferred estimator is the one that re-weights the sample to ensure balance on pre-layoff outcomes, for the reasons specified in the text.

[27] One potential concern with the experimental analysis is that treated individuals may share some of their job-loss payments with their old co-workers in the control group, confounding our estimates of counterfactual outcomes. In practice, however, transfers among workers were limited and of similar magnitude across groups. At endline, only three percent of control workers report having received a transfer from their old co-workers since job loss. Average transfers from co-workers amount to 1.3 percent of overall transfers received by the controls. Similarly, among workers in the monthly and lump-sum groups, between three and four percent of individuals received transfers from co-workers, averaging between 1.3 and 1.7 percent of total transfers. This suggests that the controls did not receive an unusual amount of resources from co-workers, ruling out concerns about direct treatment-control spillovers.

2 Empirical Results

We present our empirical results in this section, first on the effects of job loss and then on the impacts of additional job loss payments. We examine welfare implications in the next section.

2.1 The Impacts of Job Loss

We study the impacts of job loss over a period of fourteen months after the event, from September 2022 (month 0) to October 2023 (month 13). Figure 2 displays the evolution of key outcomes over time and Table 1 presents our difference-in-differences estimates using the specification in equation (1). We include estimates that study the impacts of job loss against a counterfactual of continuous employment in Table A6. The robustness checks using May 2022 instead of June 2022 as pre-period for the displaced controls are reported in Table A7. Results are highly consistent across these estimators, so we only discuss a few meaningful differences in the text.

We have eight main findings. First, job loss has a large and persistent negative impact on employment. Right after job loss, most individuals stop working altogether, opening a large employment gap between the displaced controls and the non-displaced. Wage employment among the displaced controls then grows steadily for about eight months, peaking at close to 60 percent, with no further growth after that point. On average, over the fourteen months of the study, job loss reduces wage employment by 46 percentage points. By month 13, this effect remains large, a drop of 32 percentage points. Self-employment remains limited over the whole period, and thus does not meaningfully mitigate the shock. Moreover, formal work declines in the same proportion as overall wage employment, by about 43 percentage points on average, so informal wage employment is not a relevant coping mechanism in our setting. At endline, we further explore what our displaced controls might be doing when not back in wage employment: ten percent directly report working on their family farm in the previous seven days, and a list experiment allows us to infer that ten percent also engaged in sex work in the previous 12 months (see Table A19). Finally, we note that the reduction in the magnitude of our wage employment estimates over time is partly driven by job separations in the non-displaced sample: compared to continuously employed individuals, the average effect and the month-13 effect are not only larger, they are also closer to each other in magnitude.

Second, non-employed displaced workers actively search for work. Using our main specification, job loss increases the probability of searching for work during the study period by about 50 percentage points. This effect increases by about 4 percentage points when using May 2022 as pre-period for the displaced controls, suggesting that displaced workers increased job search in the month of the layoff announcement. Further, the effect is much higher when we use the counterfactual of continuous employment, indicating that separations in the non-displaced sample raise job-search, and thus attenuate differences in job-search rates between the two groups. Importantly, we also observe substantial job search among those that remain out of work for the entire period: 78 percent search for work in at least one of the fourteen months after job loss, 58 percent search for longer than two months, and, when active, these jobseekers spend substantial time on job search.[28] Altogether, these patterns suggest that at least some of the non-employment that we observe is involuntary.

Third, job search and employment are largely focused on industrial work. Just after job loss, 86 percent of displaced control jobseekers focus their search on manufacturing jobs, and factory work declines in the same proportion as overall wage employment, by about 47 percentage points on average. At endline, 89 percent of those who found employment work in a manufacturing job, and 73 percent in HIP (only 1 percent work again for the study firm). Also, among those that still search for work at endline, 77 percent focus their search on manufacturing jobs. This is striking in light of the fact that, at endline, the majority of workers aspire to be either white-collar employees (38 percent) or service-sector employees (46 percent). When asked why they do not search for jobs outside of the industrial park, workers most frequently answer that 'HIP jobs offer 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).

Fourth, job loss leads to a substantial drop in labor income, which is not fully mitigated by the income workers receive from formal and informal insurance. On average, job loss reduces

[28] For example, in the week prior to the first follow-up survey, workers who would remain unemployed for the entire period spent on average 16 hours on activities related to job search (conditional on doing any job search).

the earnings of displaced controls by about 60 percent (841 ETB). By month 13, job loss still depresses earnings among these workers by about 46 percent. These losses are only partially mitigated by severance pay: averaged over the time frame of our study, statutory severance pay amounts to 236 to 357 ETB per month. Interestingly, these losses are also partially mitigated by an increase in net kinship transfers – what they receive from family and friends minus what they give — of about 159 ETB per month, a form of informal insurance found to be less relevant in upper-middle-income countries. Workers report that 90 percent of these transfers come from their own family, and these transfers are strongly negatively correlated with employment and earnings after layoff (Table A9), suggesting that they indeed serve an insurance purpose. Moreover, the estimated effect size increases by 50% (to 221 ETB) using May 2022 as pre-period for the displaced controls (Table A7), suggesting that informal transfers also responded in anticipation of the layoff. Yet, even using this higher estimate, formal and informal insurance combined provide resources that mitigate between 54% and 68% of the lost labor income.

While informal transfers play a meaningful role in supporting workers after job loss, they are also likely to impose a number of costs on recipients. At endline, 70 percent of workers report that if they had received more transfers in the past, they would receive fewer transfers today. Moreover, about 20 percent report that they will have to repay the money they received. Accordingly, about half of the displaced controls report that they would be willing to forgo all kinship assistance in order to receive an unconditional payment worth only 70 percent of the value of their informal transfers, suggesting that the costs associated with informal insurance mechanisms amount to at least 30% of the value of kinship transfers for these workers.

Fifth, consumption expenditure drops persistently, and consumption poverty increases.[29] On average, core expenditure — a pre-specified measure that includes food and basic hygiene products — drops by 8 percent and total expenditure by 10 percent. These effects increase over time. By month 13, these figures reach 11 percent and 14 percent, respectively.[30] If we use as counterfactual the non-displaced workers who remain employed over the period, the month-13 gap in core expenditure grows to 12.4 percent and the gap in total expenditure to

[29] In Appendix B, we show that our findings on expenditure are unlikely to be driven by strategic misreporting, recall bias or a changing household composition.

[30] Estimates are similar using May 2022 as pre-period for the displaced controls, smaller for total expenditure but almost identical for core expenditure, suggesting anticipation effects are limited to less essential spending categories.

18.4 percent.[31] These changes are comparable to those reported in studies of the impacts of job loss on consumption expenditure in middle-income countries. For example, using a similar counterfactual, Gerard and Naritomi (2021) find that job loss in Brazil is associated with a drop in non-durable expenditure of about 13 percent, 12 months after displacement. However, the workers in our sample are on average much poorer. As a result, job loss implies a 16-percentage-point increase in extreme poverty by the end of the study period, more than doubling the counterfactual rate of 11 percent.[32] Finally, by the end of the period, the savings stock remains equally limited for both displaced and non-displaced workers (Table A14). Overall, these results indicate that job loss leads to a substantial loss in welfare in our setting.

Sixth, job loss causes a large increase in out-migration. Within just one month after job loss, 20 percent of the displaced control sample has left Hawassa, rising to 30 percent by the end of the study period. This translates into an estimated effect of 20 percentage points on average and 21 percentage points by month 13, although the latter estimate is reduced by out-migration following job separation in the non-displaced sample. The predominant destination is "home", the location where workers lived before coming to Hawassa, although part of the out-migration effect (5 percentage points) is also directed to other urban areas. Of those who moved "home", 52 percent report that they wanted to live with their parents or other family members, and 26 percent that housing costs in Hawassa were unaffordable. Only 5 percent moved home to find work and 7 percent to live with a partner. Of those who moved to another location, 49 percent moved to find work, 11 percent to live with a partner, and 20 percent mention unaffordable housing costs. Only 1 percent moved for educational opportunities, regardless of destination.

Seventh, job loss leads to an increase in marriage, but no changes in fertility. At the time of the endline survey, 12 percent of the non-displaced sample is married and the average woman has 0.15 children. Job loss increases marriage rates by four percentage points for the displaced controls (significant at the 10 percent level). Fertility rates remain unchanged, though it is plau-

[31] In our expert survey, the median prediction was a drop in core expenditure of 18 percentage points. However, we only asked about the first 6 months after job loss, a period over which we estimate a drop of only 6 percentage points. Thus, surveyed economists either were too pessimistic or they did not anticipate that workers would use their statutory severance pay quickly after layoff (the expenditure gap only appears in month 2 after job loss in Figure 2).

[32] We define extreme poverty using the World Bank 2.15 USD per day threshold and the average of the PPP conversion factors for years 2022 and 2023 (weighted by the relative number of months that we observe in each year). Given that inflation in Ethiopia was higher than inflation in the US during this period, this procedure somewhat overstates the poverty rate at the start of the period and understates the poverty rate at the end of the period.

sible that an increase in fertility will eventually follow the increase in marriage. Additionally, an index of autonomy, which captures the ability of women to make independent decisions from their parents, does not show any meaningful changes (either on average or at endline).[33]

What role do migration and marriage play in explaining the persistently lower employment rate after job loss? While out-migration is associated with a lower propensity to work, the rise in out-migration cannot fully explain the persistent employment gap. Among displaced controls who stay in Hawassa, employment rates are about 63 percent by month 13. This rules out a scenario where everybody who stays in the city finds a new job quickly. However, among displaced controls who leave Hawassa, employment rates are only 21 percent by month 13, indicating that out-migration may exacerbate the employment effect. Marriage is also associated with a lower propensity to work, but the estimated increase of 4 percentage points suggests that it is too small to be a major driver of the employment outcomes of the displaced controls. Overall, a mediation analysis suggests that, taken together, migration and marriage explain 34 percent of the month 13 wage-employment effect.[34] In other words, individuals who neither migrate nor get married still experience the majority of the effect documented in Table 1.

Finally, mental health does not deteriorate significantly, based on a pre-specified index of psychological well being (both an average and at endline).[35]

2.2 The Impacts of Additional Job-Loss Payments

We next document the impacts of additional job-loss payments using the specification in equation (2). We report short-run results for our main outcomes in Table 2, and average and end-of-study results in Table 4. We present results for additional outcomes and heterogeneity analyses shedding light on the mechanisms behind these main effects in Tables 3 and 5.

[33] Following Anderson (2008), we construct an index based on six variables that measure whether respondents have to consult their parents and whether they follow their parents' wishes in case of disagreement on three decisions domains: marriage, employment, and expenditures. The index also includes a variable measuring their general willingness to express disagreement with parents.

[34] See Table A20 and footnote 47 for an explanation of the mediation technique used.

[35] This index is based on the CESD-10 depression scale and the HADS-A anxiety scale. In the first three rounds of high-frequency surveys, we only measure anxiety, whereas we only measure depression in the last two rounds. At endline, we measure both anxiety and depression and combine them into a single mental health index by summing the indices and re-standardizing the result.

2.2.1 Short-Run Effects

A. Consumption expenditures. The first takeaway from the results in Table 2 is that lump-sum payments generate only a very short-lived boost in consumption expenditure, while monthly payments provide better consumption-smoothing benefits. This pattern is in line with economists' expectations in our expert survey (Table C1). Our experimental design allows us to provide direct evidence that the differential effect between the two treatments captures an *excess* sensitivity to cash-on-hand, and thus provides a policy rationale for payments in tranches, as it is driven by workers who expressed a strong preference for the monthly payments at baseline.

In the first two months after job loss (months 0-1), lump-sum payments raise total expenditure not only compared to the displaced controls but also compared to the non-displaced sample, as the effect of job loss is small in those months (reported in the bottom panel of Table 2). Total expenditure for the lump-sum group then drops back to the level of the control group already by the next four months (months 2-5), when the effect of job loss becomes more negative. By contrast, monthly payments do not generate a similar spike in months 0-1 and mitigate the negative effect of job loss on total expenditure in months 2-5, when workers in the monthly payment group are the only ones receiving experimental transfers. These impacts are significantly different between the lump-sum group and the monthly group in both periods (the relevant p-values are reported in the bottom panel of Table 2). This is also the case if we focus on core expenditures (see Table A11). Moreover, because the impact of the monthly treatment is higher when the effect of job loss becomes more negative, extreme poverty decreases significantly more (by five percentage points) in the monthly group than in the lump-sum group in months 2-5, but the difference is not significant in months 0-1 (see Table A12).[36]

We interpret these findings as consumption responses stemming from limited self-control for two reasons. First, Table 3 shows that the magnitude of treatment effects in months 0 and 1 is strongly correlated with the incentivized policy preference measure we collected at baseline

[36] Table A12 also provides another measure of the consumption-smoothing benefits of tranche payments: the lump-sum treatment – but not the monthly treatment – increases the within-worker standard deviation of total expenditure across months 0-5.

(see Panel A, column 1).[37] Among workers who strongly prefer monthly payments,[38] those that are assigned to the lump-sum treatment consume 432 ETB more per month right after job loss compared to those that are assigned to their preferred monthly treatment. Among the other workers, the differential increase is a significant 75 percent smaller (106 ETB). Thus, when workers who report a desire to control their expenditure are not offered the spending commitment built into the monthly intervention, they display high sensitivity to cash-on-hand. Second, Table 3 shows that about 35 percent of the higher spike with the lump-sum treatment among these workers is driven by core expenditure, which is in line with its share in total expenditure (see Panel A, column 2). These results are consistent with models of imperfect self-control, such as models of present bias or temptation (Gul and Pesendorfer, 2001; Laibson, 1997), and suggest that individuals are sophisticated about their self-control problems.

The standard *rational* explanation for a high sensitivity of expenditure to cash-on-hand — that individuals are liquidity constrained and have investment opportunities (in durable goods or social capital) that exceed their existing liquidity — would not predict a higher spike among workers with a strong demand for tranche payments. Moreover, Table 3 shows that the effect of the lump-sum treatment is higher among those workers even if we consider lumpy spending categories (investment, business, and durable expenditure; see Panel A, column 3). Table A13 provides a more detailed breakdown of the effects in months 0 and 1 by expenditure category. The only categories for which the effect of the lump-sum treatment significantly exceeds that of the monthly treatment *among workers who do not express a strong demand for tranche payments* are luxury goods, durables, and transfers to workers' kin network, which is consistent with a

[37] We show that treatment assignment is balanced within preference group, and show the key demographic differences between these two groups in Table A10.

[38] As explained in Section 1.4, 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 1.4 for a precise definition of this dummy variable. Further, note that we do not observe policy preferences for individuals in the control condition. This means that we cannot estimate the impact of a given treatment for individuals with a specific policy preference. We can however estimate the difference in treatment effects for individuals with a specific policy preference. To see this, let T_L be a dummy for people assigned to the lump-sum treatment, T_M a dummy for people assigned to the monthly treatment, T_C a dummy for the control group, and P a dummy capturing individuals with a given policy preference. The difference in the impacts of the two treatments for the group with preference P is given by

$$(E[Y|T_L, P] - E[Y|T_C, P]) - (E[Y|T_M, P] - E[Y|T_C, P]).$$

The two terms capturing outcomes in the control group ($E[Y|T_C, P]$) cancel out. So, the difference in treatment effects boils down to $E[Y|T_L, P] - E[Y|T_M, P]$, which we observe in our design.

liquidity-constraint story. In terms of magnitude, for each of these categories, the differential effect of the lump-sum treatment among workers who do not strongly prefer monthly is not larger than the differential effect among workers who strongly prefer monthly payments.[39]

B. Employment. The second takeaway from the results in Table 2 is that the employment effects of lump-sum transfers differ markedly from those of the monthly intervention. This contrasts with economists' expectations in our expert survey, which anticipated no employment effects in either treatment arm (Table C1).

The lump-sum treatment induces a large drop in wage employment in the short run. The negative effect of 4 percentage points in the first two months is compensated by an equal-size increase in self-employment. In the next four months, the self-employment effect shrinks to 2 percentage points, while the negative effect on wage employment rises to 11 percentage points, or a 25 percent reduction in wage employment compared to the control group (we show estimated effects on overall employment in Table A15). Consistent with this negative effect on overall employment, we estimate a negative effect of the lump-sum treatment on labor income and a positive effect on net transfers from workers' kin network.[40]

The employment effect of the lump-sum treatment could be driven by a standard negative income effect on labor supply, as in the typical model of job-search and consumption in the UI literature (Chetty, 2008). Risk-averse workers would like to borrow against their future income to smooth consumption after layoff, but they cannot, so they "self-insure" by increasing job-search effort over their privately optimal level with no such liquidity constraint; an increase in cash-on-hand then reduces job-search effort closer to the unconstrained optimum. That model, however, would predict a comparable effect with a treatment paying a similar amount in monthly tranches (see Chetty, 2008). Instead, Table 2 shows that the monthly payments do not lead to any large or significant changes in employment (nor to changes in labor income or net transfers). As a result, wage employment remains 7 percentage points higher in the monthly group compared to the lump-sum group in months 2-5, when self-employment is

[39] Table A24 shows that we do not observe treatment effect heterogeneity by the second primary heterogeneity dimension we pre-registered, baseline savings stock, which was relatively low to begin with (Table A2).

[40] We provide evidence for these kinship transfers' insurance motive in Table A9. First, net transfers received in months 2-5 are strongly associated with employment status; by contrast, there is no correlation with the transfers given to others in previous months, which would have been consistent with a reciprocity motive. Second, once we control for employment status, net transfers received in months 2-5 are no longer correlated with treatment status.

neither economically significant nor statistically different between the two groups. This result provides novel evidence that the negative employment effect of lump-sum transfers at layoff may not be driven by risk aversion and a standard income effect (as in Card et al., 2007, and many subsequent papers), and that the effect of job loss insurance payments on employment can differ substantially based on their payment modality. Moreover, it cannot be simply explained by out-migration, as the lump-sum did not increase out-migration significantly in the short run (and the point estimate is too low compared to the drop in overall employment).

By comparing the effect of the two treatments by baseline policy preferences, Table 3 sheds again some light on mechanisms. Results for workers who express a strong preference for payments in tranches are consistent with a model à la Chetty (2008) and sophisticated present-bias agents as in Gerard and Naritomi (2021). Indeed, the lump-sum treatment does not decrease employment – compared to the monthly treatment – among these workers. Moreover, it leads to a differential increase on job-search activity in months 0-1 (see Panel A, column 4), as well as on job-search expenditures (see Table A13): these workers should expect that they will experience a larger consumption drop in subsequent months relative to the monthly treatment. Yet, the increased job search does not lead to any sizable increase in wage employment. We also find no differential effect on self-employment, which is not considered in these models.

By contrast, Table 3 shows that workers who do not express a strong preference for the monthly treatment — or at least a subset of them — make very different employment and migration decisions depending on the intervention they receive. Relative to the monthly treatment, the lump-sum decreases job search among these workers, decreases wage employment, increases self-employment, and increases overall out-migration, this latter effect being entirely driven by workers migrating back home.[41] These results are in line with some of the few significant differences in workers' characteristics between our two policy preference groups at baseline (see Table A10). Indeed, prior to layoff, workers who do not express a strong preference for the monthly treatment were more likely to be married and have children (suggesting a weaker attachment to the labor force), were less likely to report that they expect to stay in Hawassa, and had higher self-employment earnings. We return to these results in the next section, where

[41] The lack of a significant differential effect on overall out-migration in Table 2 comes from a response in the opposite direction by workers with a strong preference for the monthly treatment.

we further explore the different employment responses elicited by the two interventions.

Before moving to the longer-run results, a brief comment on the magnitude of the financial impacts of the interventions documented in Table 2 is warranted. The impacts on total expenditure are smaller than the size of the transfers, and this difference cannot be reconciled by the limited short-run changes in income. Moreover, in Table A14, we show that increases in workers' savings stock – as measured at the time of interview in the first two rounds of high-frequency surveys – do not appear large enough to account for this discrepancy (although savings are noisily measured in the high-frequency surveys as discussed in footnote 49). Therefore, as in Gerard and Naritomi (2021), the overall short-run increase in cash outflows resulting from job-loss payments may be larger than what we measure, and in particular expenditure data may underestimate the size of the expenditure spike induced by the lump-sum treatment. At the end of Section 2.2.2, we will present estimates of the marginal propensity to spend out of the transfers that circumvent this problem.[42] Here, it is worth noting that we investigate the causes of the underestimate in Appendix B, and we find no evidence that the duration of the recall period — which is short by the standards of the field experimental literature (Crosta et al., 2024)) — or strategic misreporting play a role. Additionally, we note that, in the baseline period, total expenditure was equal to income plus informal transfers, suggesting that, in a typical month, our survey instruments do not underestimate expenditure. A potential explanation may then be that cognitive load leads to 'retrieval failures' when individuals have to report an unusually long list of expenditures after a transfer (Augenblick et al., 2024). Crucially, this phenomenon would be limited to expenditure in the very *short-run*, just after transfers are received, and does not cause particular concern for the longer-run analysis we present next.

2.2.2 Long-Run Effects

A. Main results. The main takeaway from Table 4 is that the key patterns that had already emerged by months 2 to 5 after job loss persist over the fourteen-month period of our study.

First, monthly payments raise consumption expenditure, while lump-sum payments do not. The significant increase in average total expenditure for the monthly payment group, which reaches 5 percent of the control mean, offsets about 40 percent of the loss caused by

[42] We also note that we do not rely on estimates of expenditure effects in our welfare analysis in Section 3.

job displacement. By month 13, the boost in monthly expenditure reaches 6.5 percent of the control mean (a noisily estimated effect) or about one-third of the expenditure loss from job displacement. By contrast, the effect of the lump-sum treatment on average total expenditure is insignificant and only a quarter of the effect size we estimate for the monthly group ($p = 0.06$). By month 13, lump-sum payments are associated with a small and insignificant decrease in expenditure ($p = 0.08$ for the difference with the monthly payment group). We find a similar differential effect between the two treatment arms on core expenditures (Table A11).

In addition, the monthly intervention reduces expenditure poverty and enables better consumption smoothing. We show in Table A12 that monthly payments significantly reduce expenditure poverty by 3 percentage points on average or 12.5 percent of the control mean, while lump-sum payments do not — an effect that is statistically different from that of the monthly treatment ($p = .01$). The difference between the two treatments reaches 6 percentage points by month 13 ($p = .06$), although the effect of the monthly intervention itself becomes smaller and insignificant. Table A12 also shows that the lump-sum treatment – but not the monthly treatment – significantly increases the within-worker standard deviation of total expenditures across the 14 months of our study ($p = .06$ for the difference between the two interventions).

How do these results come about? Most importantly, we find that the negative employment effects of the lump-sum payments persist beyond the short run, in contrast again with economists' expectations in our expert survey (Table C1).

Table 4 shows that the lump-sum treatment significantly reduces wage employment by 9 percentage points on average and 10 percentage points by month 13. It significantly reduces formal work, factory work, work in the Hawassa Industrial Park, and work that uses the skills picked up at the park, with effects ranging between 6 and 11 percentage points at endline (Table A16). Moreover, increases in self-employment are limited, so that overall employment falls by 7 percentage points on average and 9 percentage points by month 13 (Table A15), and labor income declines by 11 percent on average and 20 percent by the end of our study period. Monthly payments, on the other hand, have modest and largely insignificant effects. As a result, the effects of the monthly intervention are significantly different from those of the lump-sum intervention for all employment outcomes that are negatively affected by lump-sum payments. Finally, job quality among employed lump-sum beneficiaries is statistically indistinguishable from job quality among employed controls; the same is true among employed monthly beneficiaries (Table A17).[43] Thus, the resources made available to workers in our experiment do not generate better employment outcomes.[44] Making payments in tranches, however, can avoid the negative employment effects caused by the typical payment modality of severance pay, the main source of job displacement insurance in lower-income countries.

That employment effects are the main mechanism behind the persistent difference in consumption expenditures between treatment arms is further supported by the lack of any persistent effect on net kinship transfers received (Table 4) or workers' savings stock (Table A14).

Table 4 also shows that lump-sum payments increase out-migration by the end of our study period, while monthly payments do not. Migration rates among lump-sum beneficiaries rise above those of the controls shortly after layoff, and the magnitude of the effect more than doubles over time: from an insignificant 3 percentage points in months 2-5 (Table 2) to a significant 8 percentage point (28 percent of the control mean) by month 13. In contrast, monthly payments do not raise migration in either the short or the long run, and we can reject the null that the two treatments have the same impact by the end of our study period. Interestingly, while they did not expect the negative employment effects, economists in our expert survey did anticipate the out-migration effect of lump-sum payments (and a smaller effect for the monthly payments). This is, perhaps, because they anticipated migration in search for employment opportunities; in fact, as we discuss below, the effect is driven by migration back home.

B. Mechanisms. These impacts on employment and migration reflect a clear shift away from wage employment in the lump-sum group, and are — as in the short run — primarily driven by workers *who do not express a strong preference for monthly payments*. Indeed, among these workers, the lump-sum persistently reduces wage employment compared to the monthly payments: the differential effect is significant both on average and by the end of our study period, at 6 percentage points and 7 percentage points, respectively (Table A23).

Why do workers without a strong preference for monthly payments reduce wage employ-

[43] Results on job quality can be confounded by selection out of employment generated by the lump-sum treatment. If the jobs that are missing from the lump-sum sample had below-average quality, then the null impact in Table A17 could mask two offsetting effects: a decrease in job quality caused by the intervention and a positive selection effect.

[44] This is also true when considering the dynamics of job quality. Table A18 shows that the monthly payments group does not see significant job quality effects in any survey round.

ment when they are paid in a lump-sum as opposed to monthly tranches? We present a detailed mechanism analysis in Table 5.[45] First, we show that, at endline, they are significantly less likely to aspire to be factory workers in the future — a drop of 6 percentage points or almost 50 percent. Second, consistent with a reduced interest in factory work, we document that they disconnect from the labor market for factory jobs on various margins: they are more likely to experiment with self-employment,[46] more likely to migrate back to their original homes and less likely to invest in human capital. They are also more likely to work in a family farm, but not to engage in sex work (see Table A19). Third, we show that lump-sum beneficiaries without a strong preference for monthly payments make different family formation decisions compared to monthly beneficiaries with the same policy preferences: they are more likely to be married and have 0.09 more children (a marginally significant effect).

These occupation, migration and family-formation responses account for the vast majority of the surprising difference in wage employment between lump-sum and monthly beneficiaries in this preference group. We show this through a mediation analysis, which identifies the natural indirect effect (NIE) of different surrogate indices of potential mediators (Chetty and Imai, 2025), in Table A20.[47] In particular, the early drop in job search (Table 3), combined with the additional self-employment over the year following job loss (Table 5) and the long-term out-migration (Table A23) mediate 69 percent of the month-13 difference in the wage-employment effect between the lump-sum treatment and the monthly condition. If we include the mediating effect of higher endline marriage rates and number of children (Table 5), we explain 92 percent of the differential effect. Workers without a strong preference for monthly payments

[45] This analysis looks at several outcomes that were not pre-specified and should be thus considered as exploratory.

[46] Most businesses shut down quickly. Overall, while 24 percent of lump-sum beneficiaries were self-employed at some point after job loss, only 7 percent of them are in self-employment by the end of our study period, a proportion similar to the controls (Table 4).

[47] The NIE is the impact of the intervention that is attributable to the change in the mediator: $NIE = E[Y_i(1, M_i(1)] - E[Y_i(1, M_i(0))]$, where $Y_i(t, M(t))$ is an outcome of interest for individual i, exposed to a binary treatment with value t and a mediator of value $M(t)$. Further, we can define the natural direct effect (NDE) as $NDE = E[Y_i(1, M_i(0)] - E[Y_i(0, M_i(0))]$. Under these definitions, the treatment effect can be decomposed as $TE = NDE + NIE$ (Chetty and Imai, 2025). We identify the NIE using a parsimonious linear model that does not allow for treatment-mediator interactions, and then report NIE/TE — the share of the treatment effect that is explained by the mediators. Since we are interested in multiple possible mediators, in our setting M is an index of different variable. We construct this index as follows. First, we estimate the relationship between month 13 wage employment and the different potential mediators included in the index by running a linear regression using control group observations. Second, we use the coefficients of this regression to generate a predicted employment score for every individual in the sample. This predicted employment score corresponds to our index M.

— or at least a subset of them — thus appear to have a desire to shift towards different career and life trajectories, and embark on such transitions if offered the lump-sum intervention.

On the other hand, among workers who strongly prefer monthly payments, the lump-sum treatment does not reduce wage employment compared to the monthly treatment in the short run (Table 3), or on average (Table A23), although it leads to a sharp reduction in month 13. Only 18 percent of this latter effect is explained by the occupation, migration and family-formation variables that mediate most of it for the other preference group. Instead, for workers with a strong preference for monthly payments, two pieces of evidence point to an alternative explanation. First, in this preference group, employed lump-sum beneficiaries have lower job quality relative to employed monthly beneficiaries (Table A18 — a significant effect for 3 out 5 periods). Second, about half of the change in the treatment effect between month 5 and month 13 can be attributed to terminations of existing jobs.[48] It is possible that, after overspending in months 0 and 1, this group bargains less aggressively at job entry or accepts jobs with lower match quality. As a result, these jobs are terminated at higher rates in the following months.

C. Propensities to spend, earn, and save. We summarize the main findings of this section by highlighting their implications for the relative propensities to spend, earn, and save out of the increased cash-on-hand provided by our two treatment arms. To do so, we combine the income flows reported over the course of our study with the detailed information on workers' savings stock reported at endline, to recover an alternative measure of total expenditure over the period using the standard accounting identity (as in, e.g., Kolsrud et al., 2018): $SavingStock_k - SavingStock_0 = \sum_{t=0}^{t=k} TotalIncome_t - \sum_{t=0}^{t=k} TotalExpenditure_t$.[49]

[48] At month 5, lump-sum payments increase wage employment relative to monthly payments by 1.4 percentage points in this preference group. At month 13, they reduce wage employment by 7.9 percentage points. The difference between these two effects — 9.3 percentage points – can be decomposed as the sum of the treatment effect on the probability of transitioning from not having a wage job in month 5 to having one in month 13 (-4 percentage points), and the treatment effect on the probability of having a wage job in month 5 and not having one in month 13 (+5.3 percentage points). These two forces have roughly similar magnitude.

[49] This is to address the limitation of our expenditure data raised at the end of Section 2.2.1. In theory, we could also construct an alternative measure of expenditure in months 0-1 and months 2-5 using the same identity. However, for this exercise to be informative, savings stock would need to be measured precisely at the end of each of these two time windows, given the sharp changes in total expenditures over time documented in Table 2. In practice, due to the timing of survey interviews, savings stock are recorded with a delay for all respondents, and this delay varies across individuals (as interviews are carried out over several weeks within each survey round). Moreover, we have evidence that our measure of savings stock collected in the high-frequency surveys underestimates total savings. At endline, we randomized whether we ask about saving stocks in a single question as during the high frequency surveys or whether we elicit stocks separately for four stocks (banks, cash on hand, at home, with others). We find

The results are presented in Table A21. Panel A shows that the implied marginal propensity to spend is high for both interventions, at 0.86 in the lump-sum group and 1.08 in the monthly group. It is below 1 in the first case and above 1 in the second case because the lump-sum treatment reduces earned income over our study period (accounting for a shortfall of 0.12), whereas the monthly treatment increases earned income over the same period (accounting for the extra 0.08). In lump-sum and monthly treatments alike, the marginal propensity to save is low (0.02 and 0.01, respectively). Finally, Panel B shows the difference in these propensities between the two treatment arms by policy preference. The smaller propensity to spend with lump-sum payments is primarily driven by workers who do not express a strong preference for tranche payments. This is because these workers use the one-off increase in cash-on-hand to reduce their labor supply — and thus their earned income — over the course of our study.[50]

3 Welfare implications and policy design

We now examine the implications of our results for welfare and policy design. First, we compare different modalities of disbursing additional support. As in the status quo, one possibility is to provide it in a single lump-sum payment. Alternatively, (i) it could be paid in five monthly tranches or (ii) workers could be offered the choice between the two payment modalities. Second, we assess the welfare effect of increasing support for a given payment modality, to shed light on whether expanding job-loss insurance is desirable in our setting.

Throughout this section, we assess the welfare effect of increasing job-loss insurance through an insurance-efficiency framework à la Baily (1978) and Chetty (2006), using social efficiency as a normative criterion:[51]

$$dW(x) = E[\frac{\pi_i(x)}{\overline{\pi}} - 1] + FE(x) + IndExt(x) \tag{3}$$

that asking four separate questions increases reported savings stock by 41% (256 ETB). We thus carry out this exercise only in the longer-run analysis, which is less sensitive to the timing of interview – expenditure levels are likely more stable across months at endline – and for which we observe a more comprehensive measure of savings stock.

[50] They also saved relatively more than those workers with a strong preference for tranche payments, which is in line with the different policy preferences reflecting differences in present bias or temptation.

[51] Social efficiency is the natural normative criterion in our context: while the insurance motive for job displacement insurance policies is strong in lower-income countries, their redistributive motive is weak, i.e., potential beneficiaries — formal workers exposed to job loss — are not at the bottom of the income distribution (Gerard et al., 2025).

where the welfare effect $dW(x)$ is expressed per ETB of additional support provided at job loss, and $x = \{lsum, month, choice\}$ indicates the payment policy.

The first component in equation (3) is the average gain in private surplus, i.e., the insurance value. It depends on formal workers' willingness-to-pay for the additional insurance $\pi_i(x)$ — how much workers are willing to pay when formally employed per additional ETB of support at layoff — relative to the actuarially fair price for such insurance $\overline{\pi}$ (Landais et al., 2021).[52]

The second component is the fiscal externality $FE(x)$, i.e., the impact on the government budget due to behavioral responses. This is a standard measure of efficiency cost (Finkelstein and Hendren, 2020). In contrast to UI programs, behavioral responses do not affect the amount of benefits paid in our setting, as payments are not conditional on remaining without a formal job. However, behavioral responses can affect the government budget through their impact on formal wage employment and the associated income taxes paid.

The third component — $IndExt(x)$ — captures another relevant consideration for efficiency in our application, which comes from the industrial policy objective of retaining workers in the manufacturing sector. We model it as a separate externality arising from changes in manufacturing employment multiplied by an external marginal value of manufacturing employment (κ). This external value — which stands for any gain from manufacturing employment over the wage received by workers and the income tax received by the government — is likely positive given the industrial policy of the Ethiopian government. However, it is conceptually difficult to quantify, so we present results for a range of values in our analysis.

Table 6 presents our computation of each of these components. We use our estimated treatment effects directly to quantify the fiscal and industrial policy externalities for additional support disbursed in a lump-sum or in monthly tranches. For the third payment policy, we use the elicited willingness-to-pay for different payment modalities at baseline. This exercise provides us with the distribution $F(S_i)$ in our sample, where S_i is the maximum lump-sum value at which a worker would still prefer the monthly treatment, which (as a reminder) provided the equivalent of a lump-sum of $S_0 = 3850$ ETB. We then recover the externalities of providing additional support with a choice of payment modality by combining the effect of the monthly

[52] A ratio of $E[\frac{\pi_i(x)}{\overline{\pi}}] = 1$ implies that, on average, there is no private surplus gain for additional job-loss insurance.

treatment among the 58% who prefer monthly payments (those with $S_i > S_0$) and the effect of the lump-sum treatment among the other 42%.[53] Next, we rely on a revealed preference assumption to evaluate workers' private surplus gains.[54] Under that assumption, the elicited willingness-to-pay at baseline allows us to measure the *relative* private surplus gain from an ETB of additional support under payment policy x and under the default lump-sum disbursement: $E[s_i(x) - 1]$, where $s_i(month) = S_i/S_0$, $s_i(choice) = max\{S_i/S_0, 1\}$, and $s_i(lsum) = 1$ (by definition). Using only baseline data and estimated treatment effects, we can thus highlight the potential benefits of moving away from using lump-sum payments as the typical modality of disbursement for job-loss insurance policies in lower-income countries:

$$dW(x) - dW(lsum) = E[s_i(x) - 1] + FE(x) - FE(lsum) + IndExt(x) - IndExt(lsum) \quad (4)$$

Yet, it does not inform the value of increasing job displacement insurance itself. To shed light on this, we designed an additional data collection effort at endline that we describe below.

A. Fiscal externality. In column (1) of Table 6, we measure the fiscal externality from increasing job displacement insurance. Considering a two-year horizon after job loss,[55] treatment effects imply that providing additional support would decrease average formal wage employment by 2.10 months for the lump-sum treatment, by 0.57 month for the monthly treatment, and by 1.41 months when offering the choice of payment modality. Assuming a wage of 1500 ETB (the median in our sample), and thus an average income tax rate of 6%, the fiscal external-

[53] We can compute this combined treatment effect without having elicited S_i in the control group by using:

$$\begin{aligned}&(1 - F(S_0)) \times (E[Y|T_M, S_i > S_0] - E[Y|T_C, S_i > S_0]) + F(S_0) \times (E[Y|T_L, S_i \leq S_0] - E[Y|T_C, S_i \leq S_0])\\ &= (1 - F(S_0)) \times E[Y|T_M, S_i > S_0] + F(S_0) \times E[Y|T_L, S_i \leq S_0] - E[Y|T_C].\end{aligned}$$

where the second line uses $E[Y|T_C] = (1 - F(S_0)) \times E[Y|T_C, S_i > S_0]) + F(S_0) \times E[Y|T_C, S_i \leq S_0])$.

[54] We use workers' revealed preference because we have no evidence that workers are making any mistake in preferring a lump-sum payment, despite the negative employment effect. Table A22 shows that the monthly treatment is associated with a small decline in an index of psychological welfare at endline, driven by an increase in anxiety, while the lump-sum treatment is associated with a small increase in that index, driven by a decrease in depression ($p = .05$ for the difference between the two interventions). On the other hand, in the first five months after job loss, only the monthly intervention significantly increases welfare, driven by a gain in life satisfaction. Overall, these results are mixed and it is unclear whether either treatment has a consistent effect on psychological well-being. We also find no systematic impact of the interventions on an index of gender empowerment (see Table A22).

[55] 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 month-13 treatment effects persist until the end of the 24-month period.

ity with these three payment policies would be $FE(lsum) = -0.049$ ETB, $FE(month) = -0.013$ ETB, and $FE(choice) = -0.031$ ETB per ETB received after job loss.

In sum, moving away from the typical payment modality for job-loss insurance policies in lower-income countries, and mandating the increased support to be disbursed in tranches, would reduce the fiscal externality from 4.9% to 1.3% of the value of the increased support. Offering a choice of payment modality would have a smaller impact because workers who prefer the lump-sum are those who reduce formal employment the most with this payment modality. Yet, these fiscal externalities remain overall limited — compared to those of UI programs (Gerard et al., 2025) — since payments are not conditional on remaining without a formal job.

B. Industrial policy externality. In column (2) of Table 6, we turn to the industrial policy externality from retaining workers in manufacturing employment. Treatment effects imply that providing additional support would lead to a decrease in average manufacturing employment in the two years after job loss by 2.30 months for the lump-sum treatment, by 0.36 month for the monthly treatment, and by 1.37 months when offering the choice of payment modality. Table 6 uses these estimates and reports the level of the industrial policy externality for three values of the external marginal value of manufacturing employment. For instance, if we use a value of $\kappa = 6\%$ of the median wage (equivalent to workers' income tax), the industrial policy externality would be $IndExt(lsum) = -0.054$ ETB, $IndExt(month) = -0.008$ ETB, and $IndExt(choice) = -0.032$ ETB per ETB received, for the three payment policies, respectively. We also present estimates for values of 25% and 50% of the median wage (estimates scale linearly in κ), i.e., assuming that the workers' wage only captures 80% and 66% of the total social value of their employment in the manufacturing sector, respectively. Table 6 shows that the industrial policy gains from moving away from a lump-sum disbursement and mandating payment in tranches — and to a lesser extent of offering a choice of payment modality — become much larger than the gains from a reduced fiscal externality with such values of κ.

C. Private surplus of changing payment policy. Column (3) of Table 6 shows that mandating additional support to be disbursed in tranches rather than in a lump-sum would increase the average worker's surplus by $E[s_i(month) - 1] = 0.029$ ETB per ETB received. However, the 42% of our sample that prefers lump-sum payments would be worse off. By contrast, allowing workers to choose their preferred payment modality would not decrease the insurance value for any worker and would strictly increase it for the other 58%, with an average surplus gain in that sample of 0.194 ETB per ETB received. Overall, allowing workers to choose their preferred payment modality would lead to an average surplus gain of $0.58 \times 0.194 = 0.112$ ETB per ETB received. In other words, offering a choice of payment modality would be valued by workers as equivalent to an increase of 11.2% in the value of the additional support provided.

D. Welfare effect of changing payment policy. Column (5) of Table 6 combines the estimates in columns (1)-(3) to report the overall social efficiency gains from moving away from a lump-sum disbursement of the additional support, following equation (4).

For the lower value of the industrial policy externality, the welfare gain from providing the additional support in monthly tranches rather than in a lump-sum would be 0.110 ETB per ETB received. The welfare gain from allowing workers to choose their preferred payment modality would be 0.152 ETB per ETB received. These welfare gains would increase for higher values of κ, but more so for the policy that mandates monthly payments, given the smaller negative impact of the monthly treatment on manufacturing employment. Our estimates imply that offering workers the choice of payment modality generates a larger (resp. lower) welfare gain than mandating monthly payments as long as κ is below (resp. above) about 16% of the median wage. The external value of manufacturing employment would thus need to be sizable to justify a policy that mandates tranche payments rather than allowing workers to choose their preferred payment modality, which would also avoid making any worker privately worse off.

In sum, simple changes in disbursement modality away from the default of lump-sum payments for job displacement insurance policies in lower-income countries can have sizable welfare effects. Even using the low external value of manufacturing employment, we find that offering the possibility to receive financial support in tranches increases the social value of the support by 15%, and that mandating monthly payments would raise its social value by 11%.

E. Private surplus of additional insurance. The analysis so far leaves an important question unanswered: would an increase in financial support at job loss raise welfare? The challenge lies in assessing workers' average surplus from additional insurance $E[\frac{\pi_i(x)}{\overline{\pi}} - 1]$. To shed light on this quantity, we conducted a bespoke elicitation task at endline.

We elicited workers' WTP for an insurance product that would provide a payment at layoff equivalent to the payment we offered during our experiment (3850 ETB). For practical reasons, this insurance product (i) covered only one formal employment spell; (ii) was activated as soon as the worker became formally employed within a pre-specified six-month "activation window" following the endline survey; and (iii) lasted for a six-month "insurance window," starting from the moment the policy was activated.[56] Using a multiple price list, we elicit how much of a 250 ETB wage subsidy offered by the researchers during the insurance window workers would be willing to give up to buy this insurance product.[57] We repeat the elicitation twice, for a product that would make a lump-sum payment at layoff and for one that would disburse the payment in five monthly tranches, randomizing the order across workers. To incentivize truthful responses, we implement one randomly selected choice for 15 randomly selected workers. Appendix D provides more details on this elicitation exercise.

Figure 3 displays the implied (inverse) demand curves for additional job displacement insurance in the control group. They are very similar for the lump-sum and monthly schemes: in both cases, workers are willing to pay a monthly insurance premium of $\mathrm{E}[\pi_i(lsum)] = E[\pi_i(month)] = 0.024$ ETB while formally employed per ETB of additional support after layoff. However, as in our baseline elicitation, workers are not indifferent between the two schemes. Figure 3 shows a marked rightward shift in the demand curve if workers were to choose their preferred payment modality; the average WTP would increase by 12.5 percent. We find no meaningful differences in the elicited WTP if we compare treatment and control groups or if we compare workers in the non-displaced sample to those in the control group (see Figure A3 and Table A26). This suggests that neither the experience of receiving additional support nor the experience of job loss influences demand for job displacement insurance in our setting.

To recover workers' average surplus gain from additional insurance, we must also compute the actuarially fair price for this insurance product $\overline{\pi}$, which equates expected contributions and payouts. Formally, it depends on the monthly hazard rates of layoff (p_l) and quit (p_q), and the monthly discount factor δ: $\overline{\pi} = p_l \times (1 - p_q) \times \delta$ (see Appendix D.4 for details). We calibrate $\delta = 0.976$ based on the annual inflation rate of 34% used to make the lump-sum and monthly payments equivalent in our experiment. It is difficult to estimate relevant hazard rates of layoff and quit for our sample. To make progress, we use existing evidence from Ethiopia

[56] Since the policy only covered a single employment spell, coverage stopped if workers quit their job.
[57] The 250 ETB amount to 16.7 percent of the median income of formally employed individuals at endline.

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 the nature of separations in a representative sample of large firms in Addis Ababa (Abebe et al., 2018). We then recover the monthly hazard rates of layoff and quit as 0.0065 and 0.0259, respectively. These are broadly in line with realized separations in our sample following the endline survey.[58]

We obtain an actuarially fair monthly premium of $\overline{\pi} = 0.0061$ ETB while employed per ETB of additional support after job loss (see Table D3), which is substantially smaller than the average WTP elicited at endline.[59] This implies an average surplus gain of $E[\frac{\pi_i(lsum)}{\overline{\pi}} - 1] = 2.935$ in the control group, as reported in the top panel of column (4) in Table 6.[60]

Our estimate lies at the upper end of existing estimates in the literature, although these are largely based on indirect approaches (e.g., using observational data on consumption drops after layoff). A rare study based on Swedish data that uses actual insurance choices obtains a lower-bound for $E[\frac{\pi_i(lsum)}{\overline{\pi}} - 1]$ around 2 (Landais and Spinnewijn, 2021).[61] This is the estimate that is most comparable to ours. Chetty and Looney (2007) argue that the value of job loss insurance could be particularly high among poorer populations. Moreover, a high surplus for additional *formal* insurance in our context is consistent with the suggestive evidence in Section 2.1 that prevalent informal transfers may impose a number of costs on recipients.

[58] We conducted a short follow-up survey six to nine months after the endline survey to measure 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 of them, implying 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 hazard rates of 0.0040 and 0.0206 for layoff and quit, respectively, using maximum likelihood estimation and reported separation outcomes. We note that these are substantially smaller than average subjective layoff and quit rates reported by workers in 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: the correlations between WTP and subjective layoff rate and between WTP and subjective quit rate are of the expected sign in our data, i.e., positive in the first case and negative in the second one (see Table D1). But the absolute magnitude of the reported beliefs may be a fairly noisy indicator of workers' true perceptions of separation risk.

[59] Table D3 shows that most workers would be willing to pay the actuarially fair price — 82.6% with the lump-sum scheme, 83% with the monthly scheme, 88.4% with the choice of payment modality — and that this price would be even smaller if we inferred hazards of layoff and quit from realized separations. In our expert survey, about 70% of economists expected workers' WTP to be sufficient to pay for the additional job displacement insurance (see Figure C2), which they also report as the most important criteria for policymaking in this context (Figure C1).

[60] We evaluate the difference in this surplus gain between payment policies by using the estimates in column (3), rather than by computing the average surplus gain based on the endline elicitation separately for each payment policy. The latter approach is sensitive to the assumptions underlying our computation of the actuarially fair price; the former approach uses our data directly. We note that all our conclusions would be robust to using a different sample in Table 6, e.g., we would have $E[\frac{\pi_i(lsum)}{\overline{\pi}} - 1] = 2.854$ using the non-displaced sample instead.

[61] This is equivalent to their estimate of the average marginal rate of substitution ($E[MRS_i]$) minus one.

F. Welfare effect. Finally, column (6) of Table 6 combines the estimates in columns (1)-(4) to report the overall welfare effect of increasing job displacement insurance $dW(x)$. Given the size of our estimate of the average private surplus gain from additional insurance,[62] the overall gain in social efficiency is always positive and large, irrespective of the payment policy. For low values of κ, the welfare effect is larger if we offer workers a choice of payment modality, because of the heterogeneity in workers' preference over payment modalities. For higher external values of manufacturing employment, however, mandating monthly payments generates a larger welfare effect because of the smaller negative impact on manufacturing employment.

4 External validity

In this final section, we briefly consider how (i) the characteristics of the sample, (ii) the nature of the shock, (iii) the design of the experiment and (iv) the potential for scale-up may affect the external validity of our results — with a focus on our two core conclusions that expanding job-loss insurance and changing the disbursement modality away from the default of lump-sum payments in lower-income countries have the potential to generate large welfare gains.

A. The characteristics of the sample. Our sample is entirely composed of young women with secondary education. So, a key concern is that workers with different socio-demographic characteristics may (i) value insurance differently and (ii) have different behavioral responses to job-loss payments and hence generate different externalities.

To address this point, we collect data on a quasi-representative sample of formal workers in Addis Ababa.[63] Using insurance choices similar to those in our endline survey, we also elicit a willingness-to-pay for additional insurance well above the actuarially fair rate; in fact, we obtain an even higher average surplus gain for this sample: $E[\frac{\pi_i(lsum)}{\overline{\pi}} - 1] = 3.750$. Moreover, we find again that demand curves are very similar for the lump-sum and monthly schemes, with a clear rightward shift in the demand curve if workers were to choose their preferred payment modality (see Figure D3). Our conclusion that there are large private surplus gains from additional job-loss insurance and significant heterogeneity in workers' preference over payment modalities thus generalizes to the most important labor market in the country.

[62] See footnote 60 regarding our computation of the average private surplus gain for $x' = \{month, choice\}$.
[63] The sample is described in Appendix D.5 and Table D4.

While we cannot quantify the fiscal and industrial policy externalities for the Addis Ababa sample, they would need to be implausibly large to overturn our conclusion regarding the desirability of additional insurance. For example, suppose that the Addis Ababa sample had the same behavioral responses as the Hawassa sample, and hence larger externalities since wages in Addis Ababa are much higher — a conservative assumption since a smaller payout relative to the wage would likely result in a lower behavioral response, and since a large share of the Addis Ababa sample does not work in manufacturing. Yet, even with this assumption, the value of insurance still exceeds the combined fiscal and industrial policy externalities for all three values of κ (see Table D5). Moreover, with higher wages and associated income taxes, fiscal externalities become more important, making the case for the tranche disbursement of financial support even stronger.

B. The nature of the shock. Our study takes place in the context of (i) a mass layoff and (ii) otherwise strong overall growth at the time of the layoff.[64] Are these features likely to change how much workers would value and respond to additional job loss insurance? First, mass layoff events have been used extensively in the literature on job displacement and the impacts of job loss insurance (e.g., Britto et al., 2022a; Jacobson et al., 1993), as they are believed to be less subject to selection concerns compared to individual layoff designs (e.g., Gibbons and Katz, 1991). Second, the literature has documented that the negative impacts of UI on reemployment — and associated externalities — are smaller in contexts of aggregate labor market shocks, but that the value of insurance is, if anything, larger (e.g., Kroft and Notowidigdo, 2016; Schmieder et al., 2012). It is thus likely that our conclusions on the desirability of additional job-loss insurance would be reinforced in alternative settings where job losses are more widespread, e.g., in settings where tariff hikes cause an aggregate economic contraction.

C. The design of the experiment. A key feature of our experiment is that the additional support is unexpected. This has two implications. First, by design, we are unable to capture anticipation effects, which may emerge if expanded job-loss insurance became a policy. For example, if job loss had less severe consequences, individuals might exert less effort at work, increasing their likelihood of being laid off and exacerbating fiscal and industrial policy externalities. We provide suggestive evidence on this channel through a survey experiment admin-

[64] Ethiopia experienced real GDP growth of 5.3 percent in 2022 and 6.6 percent in 2023.

istered at endline. We ask workers in the non-displaced sample to predict their willingness to work overtime for higher pay. We randomize whether workers make this prediction (i) for a scenario with the status quo provision of job-loss insurance or (ii) for a scenario in which the total job-loss-insurance package includes the additional transfers studied in our experiment. We find no impact, suggesting that reductions in effort on the job would likely be limited.

Second, the families of workers in our study may not be aware of the additional payments. As a result, they may provide greater informal transfers than they would if the policy were widely known. In our sample, however, beneficiaries who report that their family is aware of the experimental payments receive kinship transfers in month 0-5 that are only 3.5 percent smaller than those of beneficiaries with uninformed families. This suggests that common knowledge of job-loss payments is unlikely to have a first-order effect on the size of kinship transfers. Yet, it may increase workers' preference for receiving job-loss support in tranche payments. Indeed, among workers with a strong preference for monthly payments at baseline, about 15% report "pressure to share" and about 10% report "risk of theft" as reasons for their preference. Such concerns would only become more severe if the policy were widely known.

D. Potential for scale-up. Finally, we consider the potential for scaling up the intervention. Most importantly, because monthly payments are not conditioned on subsequent employment status, they can be implemented with limited administrative capacity. Moreover, the increased banking penetration and widespread use of mobile money imply that formal workers could receive monthly job-loss payments with minimal transaction costs in many lower-income countries. The policy could be scaled up either by requiring firms to make the monthly payments (or offer that option) as part of a mandated severance pay package, or by having the state collect the funds from firms before disbursing them to workers. Our paper does not inform on the relative merits of the two strategies, but in ongoing work we explore whether firms in Ethiopia are able to absorb the risk that severance pay policies impose on them (Abebe et al., 2026).

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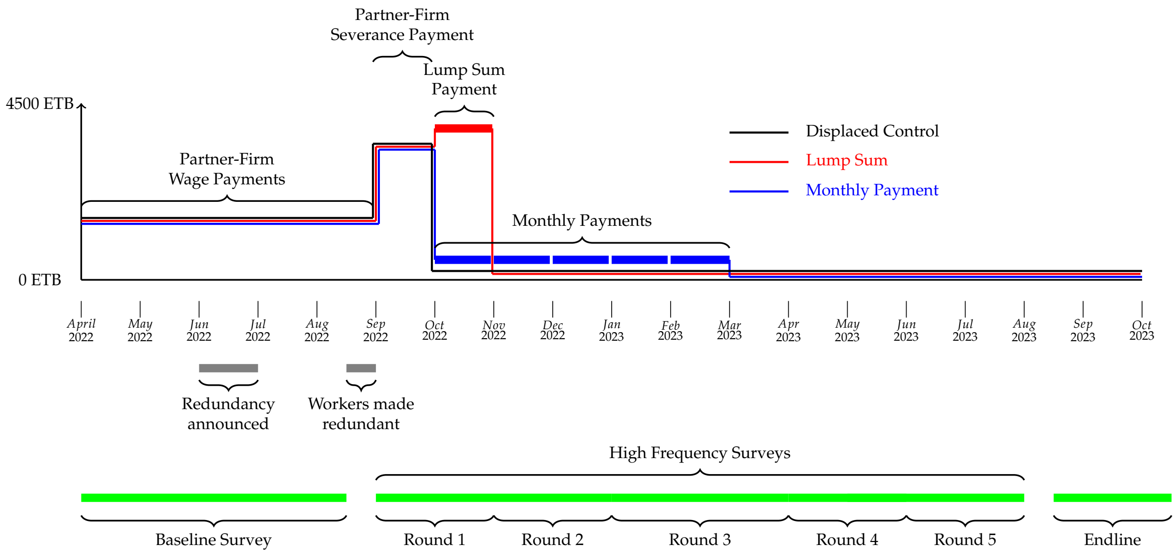
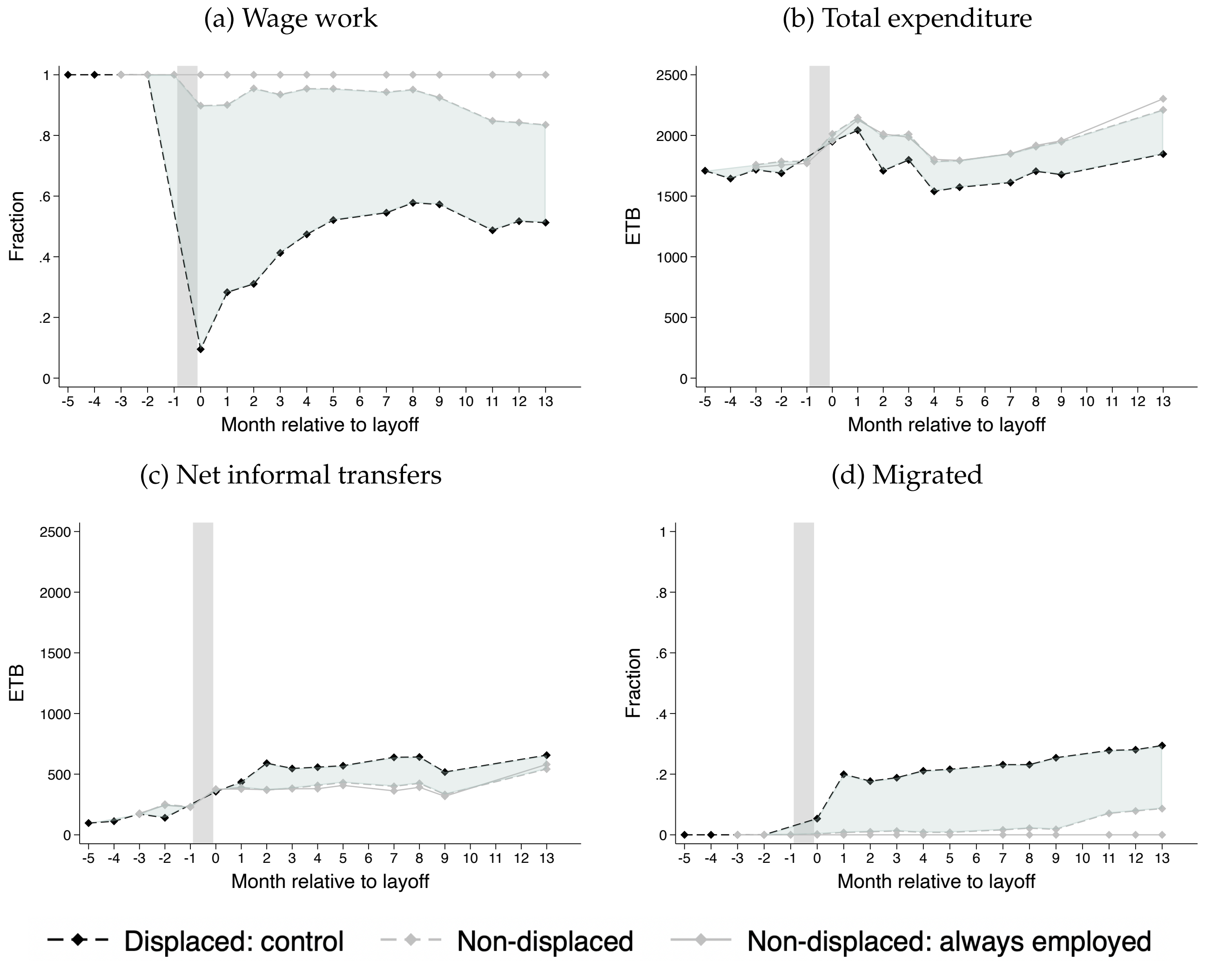
Figure 1: Project Timeline



Notes: **Figure 1 displays the project timeline and payment structures.** Red, blue, and black lines indicate payments in the different treatment groups. Payment amounts are indicated on the y-axis in Ethiopian Birr (ETB). The statutory severance payment from the study firm is displayed for workers with less than one year of tenure. 100 ETB equaled 5.09 USD PPP at the time of the experiment. Green bars indicate the time-period covered by each survey.

Figure 2: The evolution of key outcomes over time



Notes: **Figure 2 shows that job loss has persistent effects on key outcomes.** Observations in the non-displaced group are weighted to match the average pre-treatment outcomes of the displaced control group. The shaded areas indicate the difference between the non-displaced sample (gray dashed line) and the displaced control group (black dashed line) and can be interpreted as difference-in-differences estimate of the layoff effect. Solid gray lines indicate the outcomes of the subsample of non-displaced workers who worked in the Hawassa Industrial Park throughout the study period. For each group, panels (a)-(d) show the evolution of wage employment rates, average total expenditures, average net informal transfers (transfers received minus transfers sent out), and out-migration rates (the share not living in Hawassa), respectively. Unbounded variables are winsorized at the 99^{th} percentile. Month 6 and 10 are omitted because of incomplete coverage through the high frequency surveys. Months 11 and 12 are omitted for total expenditure and net informal transfers because we did not measure them for those months. The layoff in the displaced sample happened between period -1 and 0. 100 ETB equaled 5.09 USD PPP at the time of the experiment.

Table 1: The impacts of job loss

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Employment and job search variables										
	Wage work		Self-employed		Formal work		Factory work		Job search active	
	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13	any mon. 0-13	mon. 13
Displacement effect	-0.46***	-0.32***	0.03	0.04	-0.43***	-0.27***	-0.47***	-0.35***	0.50***	-0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Non-displaced mean	0.92	0.84	0.01	0.01	0.81	0.69	0.88	0.81	0.17	0.07
Observations	12150	1634	12150	1634	12150	1634	12150	1634	1748	1634
Panel B: Expenditure and financial variables										
	Labor income		Net transfers received		Total expenditure		Core expenditure		Extreme poverty	
	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13
Displacement effect	-841.13***	-673.66***	159.45***	122.57*	-202.35***	-312.98***	-71.36***	-106.76***	0.14***	0.16***
	(43.51)	(72.71)	(40.10)	(65.94)	(45.14)	(82.55)	(21.21)	(36.18)	(0.05)	(0.05)
Non-displaced mean	1414.63	1476.33	400.23	541.81	1989.49	2210.79	892.33	937.98	0.09	0.11
Observations	12150	1634	10516	1634	12150	1634	12150	1634	12150	1634
Panel C: Migration and welfare variables										
	Migrated		Urban not home	At home	Married	# children	Psych. welfare index		Autonomy index	
	mean	mon. 13	mon. 13	mon. 13	endline	endline	mean	endline	mean	endline
Displacement effect	0.20***	0.21***	0.05***	0.17***	0.04*	0.01	-0.04	-0.04	0.01	0.01
	(0.02)	(0.03)	(0.01)	(0.03)	(0.02)	(0.04)	(0.06)	(0.09)	(0.08)	(0.10)
Non-displaced mean	0.03	0.09	0.02	0.09	0.12	0.15	0.05	0.03	-0.01	-0.03
Observations	12150	1634	1634	1634	1634	1634	5715	1634	5715	1634

Notes: **Table 1 reports difference-in-differences estimates of the effects of job loss using the specification in equation (1).** Panels A-C consider employment and job-search outcomes, expenditure and financial outcomes, and migration and welfare outcomes, respectively. Mean effects are estimated using all available month-individual observations. Month-13 effects and endline effects are estimated using a single post-treatment observation, using recall data collected during the endline survey and outcomes measured at the time of the endline survey, respectively. Non-displaced observations are weighted using weights estimated to balance average pre-layoff values of each outcome between the displaced control and the non-displaced group. Weights and unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. Standard errors (in parentheses) are clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: The short-run impacts of job-loss payments

	Total expenditure		Labor income		Net transfers received		Wage work		Self-employed		Migrated	
	(1) mon. 0-1	(2) mon. 2-5	(3) mon. 0-1	(4) mon. 2-5	(5) mon. 0-1	(6) mon. 2-5	(7) mon. 0-1	(8) mon. 2-5	(9) mon. 0-1	(10) mon. 2-5	(11) mon. 0-1	(12) mon. 2-5
Lump sum	231.78***	-24.33	53.43	-83.61*	-43.01	100.44**	-0.04**	-0.11***	0.04**	0.02*	0.03	0.04
	(63.26)	(36.90)	(43.32)	(44.11)	(49.12)	(40.49)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)
Monthly	28.58	67.97*	32.08	-14.87	0.59	-53.16	-0.01	-0.04	0.01	0.01	0.00	0.02
	(60.05)	(35.28)	(41.07)	(44.54)	(46.19)	(38.21)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)
Control mean	1995.85	1654.60	274.42	661.82	395.19	562.97	0.19	0.42	0.03	0.04	0.13	0.20
Displacement effect	-46.95	-191.33***	-1144.17***	-899.31***	10.80	171.78***	-0.71***	-0.53***	0.04	0.04	0.12***	0.19***
Lump sum = monthly (p)	0.00	0.01	0.65	0.14	0.37	0.00	0.15	0.01	0.17	0.32	0.13	0.50
Observations	1314	1350	1314	1350	1314	1350	1314	1350	1314	1350	1314	1350

Notes: **Table 2 reports experimental estimates of the short-run effects of the lump-sum and monthly treatments using the specification in equation** (2)**.** The table focuses on key outcomes: total monthly expenditure (columns 1 and 2), labor income (columns 3 and 4), net informal transfers (columns 5 and 6), a dummy indicating wage employment (columns 7 and 8), a dummy indicating self-employment (columns 9 and 10), and a dummy indicating migration out of Hawassa (columns 11 and 12). Odd and even columns consider impacts during the first two months post-layoff and the following four months (when individuals in the monthly group were still receiving monthly transfers), respectively. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Short-run impacts and baseline preference for monthly payments

	(1)	(2)	(3)	(4)
Δ_i: lump sum - monthly				
Panel A: Expenditure and job search (months 0 and 1)				
	Total expenditure	Core expenditure	Lumpy expenditure	Any job search
Δ_1 | Str. preferred monthly	431.84***	141.20***	55.21*	0.08*
	(115.42)	(41.67)	(33.15)	(0.04)
Δ_2 | Not str. preferred monthly	106.04	27.15	11.88	-0.06**
	(74.58)	(28.15)	(18.99)	(0.03)
$\Delta_1 = \Delta_2$ (p)	0.02	0.02	0.26	0.01
Monthly payment mean	2027.03	815.67	81.66	0.31
Number of observations	883	883	883	939
Panel B: Work and migration (months 0 to 5)				
	Any wage work	Any self-employment	Any migration	Any migration home
Δ_1 | Str. preferred monthly	0.01	0.00	-0.10*	-0.05
	(0.06)	(0.04)	(0.06)	(0.06)
Δ_2 | Not str. preferred monthly	-0.07*	0.05**	0.11***	0.12***
	(0.04)	(0.03)	(0.04)	(0.04)
$\Delta_1 = \Delta_2$ (p)	0.26	0.30	0.00	0.01
Monthly payment mean	0.56	0.11	0.32	0.31
Number of observations	918	918	918	918

Notes: **Table 3 reports estimates of the differential effects of the lump-sum and monthly treatments by preference for the monthly payments.** Strongly preferred monthly is defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. Δ_i indicates the difference between the lump-sum and monthly treatment effects conditional on preferences. The analysis is restricted to the two treatment groups because we do not elicit preferences in the control group. Columns 1-3 in Panel A reports impacts in months 0 to 1 on different expenditure categories: total monthly expenditure, core expenditure (groceries, hygiene products, and phone costs), and lumpy expenditure (investment, business, and durable expenditure), respectively. Column 4 considers a dummy indicating any job search in months 0 to 1. Columns 1-4 in Panel B report impacts on a series of dummies indicating any wage employment in months 0 to 5, any self-employment in months 0 to 5, having left Hawassa at any time in months 0 to 5, and any moving in with their parents in months 0 to 5, respectively. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: The longer-run impacts of job-loss payments

	Total expenditure		Labor income		Net transfers received		Wage work		Self-employed		Migrated	
	(1) mean	(2) mon. 13	(3) mean	(4) mon. 13	(5) mean	(6) mon. 13	(7) mean	(8) mon. 13	(9) mean	(10) mon. 13	(11) mean	(12) mon. 13
Lump sum	21.27	-27.62	-84.37**	-201.11***	37.74	60.86	-0.09***	-0.10***	0.03***	0.02	0.03	0.08**
	(35.13)	(79.95)	(37.96)	(71.38)	(33.14)	(60.68)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)
Monthly	86.22***	117.66	-37.30	-109.37	-6.40	61.41	-0.04*	-0.02	0.01	0.01	-0.01	-0.00
	(33.46)	(79.98)	(37.55)	(71.31)	(32.55)	(64.04)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)
Control mean	1738.11	1846.83	750.91	985.57	561.35	656.30	0.44	0.51	0.04	0.05	0.23	0.29
Displacement effect	-202.35***	-312.98***	-841.13***	-673.66***	159.45***	122.57*	-0.46***	-0.32***	0.03	0.04	0.20***	0.21***
Lump sum = monthly (p)	0.06	0.08	0.22	0.18	0.19	0.99	0.01	0.02	0.13	0.31	0.09	0.01
Observations	1400	1312	1400	1312	1400	1312	1400	1312	1400	1312	1400	1312

Notes: **Table 4 reports experimental estimates of the longer-run effects of the lump-sum and monthly treatments using the specification in equation (2).** The table focuses on the same key outcomes as in Table 2: total monthly expenditure (columns 1 and 2), labor income (columns 3 and 4), net informal transfers (columns 5 and 6), a dummy indicating wage employment (columns 7 and 8), a dummy indicating self-employment (columns 9 and 10), and a dummy indicating migration out of Hawassa (columns 11 and 12). Odd columns consider mean effects over the whole period from months 0 to 13 after layoff (we extrapolate missing value as described in Appendix B); even columns consider treatment effects in month 13 after layoff. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Mechanisms behind the persistent treatment effects on employment

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Work and migration						
	Any self-emp	Asp. factory work	# emp. friends	Urban not home	At home	Job search active
	mon. 0-13	endline	endline	mon. 13	mon. 13	mon. 13
Main results						
Lump sum	0.09***	-0.04*	-0.39***	0.01	0.06*	0.02
	(0.03)	(0.02)	(0.11)	(0.02)	(0.03)	(0.03)
Monthly	0.04	0.00	-0.06	-0.02	-0.00	0.07**
	(0.02)	(0.02)	(0.12)	(0.02)	(0.03)	(0.03)
Control mean	0.15	0.12	2.53	0.07	0.25	0.20
Lump sum = monthly (p)	0.06	0.04	0.00	0.10	0.06	0.08
Observations	1400	1312	1307	1312	1312	1312
Preference het. (Δ_i: lump sum - monthly)						
Δ_1 | Str. preferred monthly	0.00	0.01	-0.14	0.02	-0.03	-0.06
	(0.05)	(0.04)	(0.22)	(0.03)	(0.06)	(0.05)
Δ_2 | Not str. preferred monthly	0.06*	-0.06***	-0.39***	0.03	0.09***	-0.04
	(0.03)	(0.02)	(0.13)	(0.02)	(0.04)	(0.03)
$\Delta_1 = \Delta_2$ (p)	0.33	0.10	0.33	0.80	0.08	0.75
Monthly payment mean	0.19	0.13	2.47	0.06	0.25	0.26
Number of observations	932	877	875	877	877	877
Panel B: Personal life						
	Has partner	Married	Pregnant	# children	Autonomy	In education
	endline	endline	endline	endline	endline	last 12 mon.
Main results						
Lump sum	0.01	0.01	0.00	0.02	0.06	-0.03
	(0.03)	(0.02)	(0.01)	(0.03)	(0.04)	(0.03)
Monthly	0.00	-0.02	-0.01	-0.05*	0.04	0.08**
	(0.03)	(0.02)	(0.01)	(0.03)	(0.04)	(0.03)
Control mean	0.51	0.16	0.03	0.17	-0.12	0.43
Lump sum = monthly (p)	0.77	0.12	0.34	0.03	0.78	0.00
Observations	1312	1312	1312	1312	1312	1312
Preference het. (Δ_i: lump sum - monthly)						
Δ_1 | Str. preferred monthly	-0.10*	-0.04	-0.01	-0.01	0.03	-0.03
	(0.06)	(0.03)	(0.01)	(0.04)	(0.07)	(0.06)
Δ_2 | Not str. preferred monthly	0.05	0.05**	0.02	0.09**	-0.01	-0.14***
	(0.04)	(0.02)	(0.01)	(0.04)	(0.05)	(0.04)
$\Delta_1 = \Delta_2$ (p)	0.03	0.03	0.13	0.09	0.63	0.13
Monthly payment mean	0.51	0.15	0.02	0.14	-0.07	0.49
Number of observations	877	877	877	877	877	877

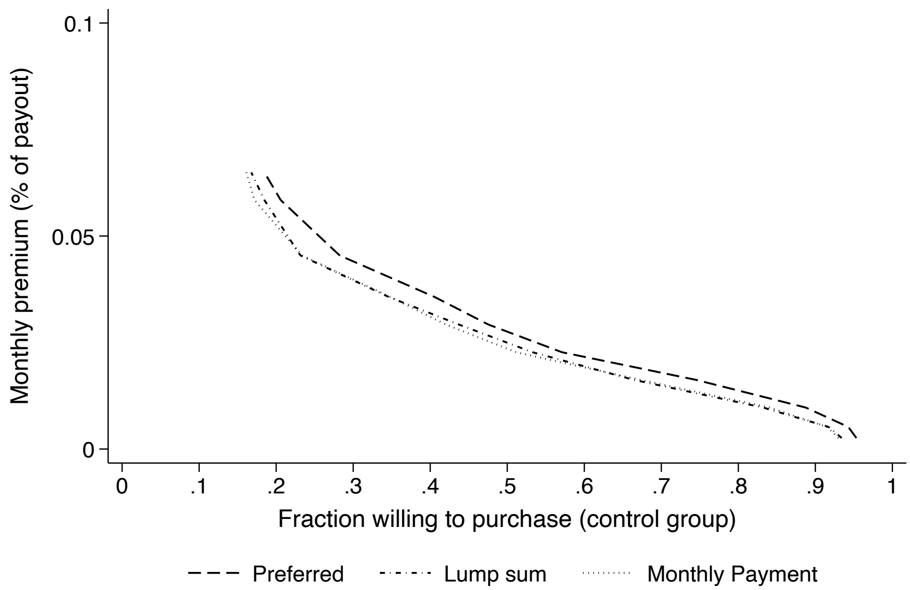
Notes: **Table 5 sheds light on the mechanisms behind the persistent treatment effects on employment.** It reports estimates of the effects of the lump-sum and monthly treatments, and of their differential effects by preference for the monthly payments. Panel A considers work-, job-search- and migration-related outcomes: a dummy indicating any self-employment during the study period (column 1), a dummy indicating an aspiration to work in factories in five years at endline (column 2), the number of employed friends at endline (column 3), a dummy indicating having left Hawassa and living in another urban area that is not the location where they lived before coming to Hawassa (column 4), a dummy indicating having left Hawassa and living where they lived before coming to Hawassa (column 5), a dummy indicating any job search in month 13. Panel B considers personal life outcomes: a dummy indicating having a romantic partner at endline (column 1), a dummy indicating being married at endline (column 2), a dummy indicating being pregnant at endline (column 3), the number of children at endline (column 4), an index of autonomy from parents and partners at endline (column 5), a dummy indicating having been enrolled in further education at any point during the 12 months before endline (column 6). Strongly preferred monthly is defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. Δ_i indicates the difference between the lump-sum and monthly treatment effects conditional on preferences. The preference heterogeneity analysis is restricted to the two treatment groups because we do not elicit preferences in the control group. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: The welfare effect of increasing job displacement insurance

		Externalities		Private surplus		Welfare effects	
		$FE(x)$	$IndExt(x)$	$E[s_i(x)-1]$	$E[\frac{\pi_i(x)}{\overline{\pi}}-1]$	$dW(x)-dW(lsum)$	$dW(x)$
		(1)	(2)	(3)	(4)	(5)	(6)
Lump sum payment	κ=0.06*w	-0.049	-0.054	0	2.935	0	2.833
	κ=0.25*w	-0.049	-0.224	0	2.935	0	2.662
	κ=0.5*w	-0.049	-0.448	0	2.935	0	2.439
Monthly payment	κ=0.06*w	-0.013	-0.008	0.029	2.964	0.110	2.943
	κ=0.25*w	-0.013	-0.035	0.029	2.964	0.254	2.916
	κ=0.5*w	-0.013	-0.070	0.029	2.964	0.442	2.881
Choice of payment modality	κ=0.06*w	-0.031	-0.032	0.112	3.047	0.152	2.985
	κ=0.25*w	-0.031	-0.133	0.112	3.047	0.221	2.883
	κ=0.5*w	-0.031	-0.267	0.112	3.047	0.311	2.750

Notes: **Table 6 displays estimates of the welfare effect of increasing job displacement insurance using the formula in equation (3):** $dW(x) = E[\frac{\pi_i(x)}{\overline{\pi}} - 1] + FE(x) + IndExt(x)$. All estimates are in ETB of additional support at job loss. The three panels consider different payment policies x for the additional support: as a lump sum ($x = lsum$), in five monthly tranches ($x = month$), or with the choice of payment modality ($x = choice$). Columns 1 and 2 report estimates of the fiscal externality and the industrial policy externality, respectively. The latter depends on the unknown external marginal value of manufacturing 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 income tax paid) to 50% of the median wage. Columns 3 and 4 consider the private surplus from additional insurance in two steps. First, column 3 uses the elicitation exercise at baseline to measure workers' average surplus gain *from changing the payment policy* for the additional support at job loss from lump-sum disbursement to policy x ($E[s_i(lsum) - 1] = 0$ by definition). Column 5 then reports the associated welfare effects combining the estimates in columns 1-3 as in equation (4): $dW(x) - dW(lsum) = E[s_i(x) - 1] + FE(x) - FE(lsum) + IndExt(x) - IndExt(lsum)$. Second, column 4 measures the average surplus gain *from additional insurance*. Its value for the case of lump-sum disbursement is based on the elicitation exercise at endline (within the displaced control group). Its value for the other two payment policies is then the sum of the estimates in columns 3 and 4: $E[\frac{\pi_i(lsum)}{\overline{\pi}} - 1] + E[s_i(x) - 1]$. We evaluate the difference in this surplus gain between payment policies by using the estimates in column (3) because they use our data directly, while differences based on the endline elicitation exercise are more sensitive to the assumptions underlying our computation of the actuarially fair price $\overline{\pi}$ (see text for details). Finally, column 6 reports the welfare effects from additional insurance for each payment policy combining the estimates in columns 1, 2, and 4.

Figure 3: Demand for additional job-loss insurance in the control group



Notes: **Figure 3 displays the incentivized (inverse) demand curve for additional job-loss insurance in the control group at endline.** The x-axis displays the fraction of individuals willing to purchase the insurance product at a given price, and the y-axis the maximum premium they are willing-to-pay while formally employed as a fraction of the additional financial support at job loss (3850 ETB; 100 ETB equaled 5.09 USD PPP at the time of the experiment). The three lines describe demand for the insurance product under three payment modalities for the additional support at job loss: lump-sum disbursement (dark-gray short-dashed line), five monthly tranches (light-gray dotted line), and workers' preferred payment modality (black long-dashed line). The elicitation procedure for this insurance product is described in detail in Appendix D.

Online Appendix

This appendix presents additional exhibits, explanations, and context for the main paper. Appendix A contains additional Tables and Figures mentioned in the paper. Appendix B provides details on the measurement and aggregation of key variables. Appendix C provides details on the expert survey. Appendix D provides details on the measurement of workers' willingness-to-pay for additional job displacement insurance and on the calibration of the associated actuarially fair premium, which are both used in the welfare analysis. Appendix E describes how our analysis deviates from the pre-analysis plan and shows additional pre-specified results.

A Figures and Tables

This Appendix Section contains additional exhibits referenced in the main text.

- Table A1 shows that participation and treatment status in a previous experiment do not affect our key results.
- Table A2 describes the composition of our samples and shows that the experimental groups are mostly balanced on observables.
- Table A3 provides descriptive statistics on a representative sample of working-age women in Ethiopia.
- Table A4 provides descriptive statistics on the work status and sector of activity on a representative sample of women aged 18 to 27 in Ethiopia.
- Table A5 shows that attrition levels are low and balanced across treatment groups.
- Table A6 reports estimates of the effects of job loss using as counterfactual workers in the non-displaced sample who remained employed at HIP throughout the study period.
- Table A7 reports estimates of the effects of job loss using May 2022 — before the layoff was announced — rather than June 2022 as pre-period for the displaced controls.
- Table A8 reports estimates of the effects of job loss comparing outcomes between the displaced controls and the non-displaced sample using post double selection LASSO (as for the experimental results).

- Table A9 shows that net transfers received from others are strongly correlated with employment status and labor earnings.
- Table A10 describes our experimental groups — and shows that they are mostly balanced on observables — by baseline preference for monthly payments.
- Table A11 reports estimates of the effects of the lump-sum and monthly treatments on core expenditure.
- Table A12 reports estimates of treatment effects on expenditure poverty and expenditure smoothing.
- Table A13 reports estimates of treatment effects on expenditure categories in months 0 to 1 after layoff.
- Table A14 reports estimates of treatment effects on workers' savings stock.
- Table A15 reports estimates of treatment effects on total employment.
- Table A16 reports estimates of treatment effects on different types of wage employment: formal wage employment, manufacturing wage employment, wage employment at HIP, and wage employment that requires workers to use the skills acquired at HIP.
- Table A17 reports estimates of treatment effects on job quality at endline.
- Table A18 reports estimates of dynamic treatment effects on job quality over the period of our study.
- Table A19 reports estimates of treatment effects on family farm work and sex work.
- Table A20 presents a mediation analysis of the month-13 wage-employment effects of (i) job loss and (ii) of the lump-sum relative to the monthly treatment.
- Table A21 reports estimates of the marginal propensities to spend, earn, and save from our interventions, implied by their treatment effects on income flows and savings stock over the period of our study.
- Table A22 reports estimates of treatment effects on psychological welfare and empowerment.
- Table A23 reports estimates of the differential effects of the two treatments on our main longer-run outcomes by baseline preference for monthly payments.

- Table A24 reports estimates of treatment effects on our main short-run and longer-run outcomes by baseline savings stock.
- Table A25 reports estimates of the effects of receiving one's 'preferred' treatment on different employment outcomes.
- Table A26 reports estimates of treatment effects on workers' willingness-to-pay for additional job loss insurance schemes.
- Figure A1 shows that the mass layoff at the study firm is not accompanied by other mass layoffs in industrial parks in Hawassa and Ethiopia.
- Figure A2 displays the distribution of workers' baseline preference for monthly payments, and reports the main reasons invoked by workers to justify their preference for either payment modalities.
- Figure A3 displays workers' incentivized (inverse) demand curve for their preferred additional job-loss insurance scheme in the three experimental groups.

A.1 Appendix Tables

Table A1: The impact of participation in the previous experiment

	Retention bonus experiment			Treatment effect controlling for retention experiment status			
	(1) included	(2) late bonus	(3) early bonus	(4) mon. 0-1 expend.	(5) mon. 2-5 wage emp.	(6) mon. 13 wage emp.	(7) mon. 13 migrated
Lump sum	0.03	0.05*	-0.02	232.32***	-0.11***	-0.10***	0.08**
	(0.02)	(0.03)	(0.03)	(63.51)	(0.03)	(0.03)	(0.03)
Monthly	0.03	0.02	0.01	27.80	-0.04	-0.02	-0.00
	(0.02)	(0.03)	(0.03)	(60.02)	(0.03)	(0.03)	(0.03)
Control mean	0.51	0.25	0.26	1995.85	0.42	0.51	0.29
Lump sum = monthly (p)	0.73	0.26	0.39	0.00	0.01	0.03	0.01
Observations	1410	1410	1410	1314	1350	1312	1312

Notes: **Table A1 shows that participation and treatment status in the previous experiment do not affect our key results.** Part of our experimental sample participated in a different field experiment prior to layoff. This experiment randomized individuals into receiving either a) a 3-month retention bonus of 300 ETB or b) an 8-month retention bonus of 2,000 ETB. Columns 1-3 show how participation and treatment status in the previous experiment is related to treatment status in this paper's experiment. Columns 4-7 show that key treatment effects do not change when controlling for participation and treatment status in the previous experiment. All specifications include strata fixed effects. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Balance and sample composition

	Pooled	Displaced sample				Non-displaced sample		
		Control	Lump sum	Monthly	p(balanced)	Mean	Δ	p(=control)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Demographics								
Age	22.23	22.16	22.06	22.09	0.79	22.66	-0.50	0.00
Married	0.13	0.13	0.17	0.13	0.19	0.10	0.02	0.33
# children	0.15	0.14	0.22	0.15	0.06	0.10	0.04	0.13
# children in school	0.09	0.09	0.14	0.06	0.02	0.05	0.04	0.12
Secondary education	0.95	0.96	0.95	0.93	0.28	0.96	0.00	0.87
University education	0.25	0.23	0.23	0.24	0.79	0.33	-0.10	0.00
Enrolled in education	0.42	0.40	0.41	0.38	0.66	0.48	-0.08	0.02
Living at place of birth	0.03	0.04	0.05	0.03	0.45	0.02	0.02	0.08
Protestant	0.91	0.90	0.90	0.92	0.62	0.95	-0.05	0.01
First language = Sidamegna	0.85	0.86	0.81	0.84	0.14	0.90	-0.04	0.05
Rural origin	0.60	0.60	0.57	0.60	0.69	0.63	-0.04	0.27
Panel B: Workplace variables								
Tenure (months)	12.79	12.87	12.42	12.50	0.33	13.47	-0.60	0.09
Work as cutter, helper, or layer	0.10	0.09	0.09	0.10	0.72	0.12	-0.04	0.08
Work as packer	0.09	0.11	0.10	0.09	0.70	0.07	0.04	0.06
Work as quality assurance or printer	0.14	0.15	0.15	0.15	0.97	0.12	0.03	0.25
Work as sewer	0.62	0.61	0.61	0.61	0.96	0.64	-0.02	0.46
Work as storage, cleaner, or trimmer	0.04	0.04	0.04	0.05	0.55	0.04	-0.00	0.89
Panel C: Financial variables								
Wage earnings	1441.81	1495.83	1454.19	1441.94	0.13	1364.65	131.18	0.00
Self-employment earnings	92.20	105.33	119.08	121.45	0.80	11.34	93.99	0.00
Net transfers received	192.41	171.97	170.08	181.13	0.92	254.96	-82.99	0.01
Total expenditure	1762.76	1760.26	1754.69	1746.84	0.97	1794.01	-33.75	0.49
Core expenditure	869.49	850.70	878.91	873.09	0.36	876.55	-25.85	0.21
Rent expenditure	406.62	401.45	398.04	403.45	0.91	426.10	-24.64	0.05
Savings stock	658.52	752.74	708.35	795.70	0.59	326.54	426.20	0.00
Panel D: Wellbeing variables								
Life satisfaction (1 to 5)	3.24	3.29	3.25	3.30	0.68	3.09	0.20	0.00
Job satisfaction (0 to 10)	6.81	6.79	6.82	6.85	0.89	6.79	-0.00	0.99
Depression index (standardized CESD-10)	-0.00	0.02	0.05	0.10	0.09	-0.21	0.24	0.00
Observations	1813	471	451	488		403		

Notes: **Table A2 presents the mean of a series of variables in our samples prior to job loss, and shows that the experimental groups are mostly balanced on observables.** Column 1 pools the displaced and non-displaced samples, columns 2-4 consider the three experimental groups composing the displaced sample separately, and column 6 considers the non-displaced sample. Column 5 uses the whole displaced sample and reports the p-value of an F-test of a regression of each variable on the two treatment dummies and strata fixed effects. Columns 7 and 8 report the difference in means for each variable and the p-value of a test of equality of means, respectively, between the experimental control group and the non-displaced sample. Variables in Panels A and D are measured at the time of the baseline survey; variables in Panel B are based on administrative data before the layoff announcement. Panel C presents means in June 2022, the only pre-layoff month for which we observe all workers.

Table A3: Demographics of working-age women in Ethiopia

		Formal and informal			Formal only		
	Ethiopia	Wage employee	Manufacturing	Textile and garment	Wage employee	Manufacturing	Textile and garment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	32.596	29.847	29.527	26.419	30.785	28.612	24.901
Married	0.659	0.477	0.423	0.334	0.558	0.382	0.218
Secondary education	0.078	0.515	0.258	0.226	0.678	0.312	0.292
University education	0.023	0.235	0.074	0.040	0.315	0.090	0.055
With parents	0.185	0.120	0.150	0.242	0.143	0.178	0.329
Rural	0.737	0.187	0.163	0.261	0.180	0.082	0.089
Migration	0.709	0.368	0.334	0.403	0.399	0.349	0.412
Observations	51093	7803	459	150	5826	394	135

Notes: **Table A3 presents the mean of demographic variables in a representative sample of women aged 16 to 65 in Ethiopia based on the 2021 National Labor Force Survey.** Column 1 considers all of Ethiopia. Columns 2-4 focus on women working as wage employee, in manufacturing, and in the garment, textile and leather sector, respectively. Columns 5-7 consider similar sample restrictions, but further focus on the subset who are formally employed. Observations are weighted using sampling weights provided by the Ethiopian Statistical Service.

Table A4: Work status and sector of activity for women aged 18 to 27 in Ethiopia

	Urban (1)	Secondary edu (2)	Migrant (3)	Urban & sec & migrant (4)
Panel A: Work status				
Engaged in productive activity	0.414	0.490	0.496	0.496
Wage employed	0.234	0.376	0.203	0.436
Wage employed (public sector)	0.089	0.288	0.085	0.314
Wage employed (formal public sector)	0.089	0.287	0.085	0.314
Wage employed (private sector)	0.145	0.089	0.118	0.122
Wage employed (formal private sector)	0.066	0.071	0.047	0.093
Unpaid work	0.059	0.076	0.158	0.022
Self-employed, non-agriculture	0.127	0.056	0.116	0.069
Self-employed, agriculture	0.017	0.008	0.065	0.000
Panel B: Sector | productive activity				
Manufacturing	0.082	0.056	0.067	0.067
Textile/apparel/leather manufacturing	0.025	0.015	0.020	0.025
Agriculture	0.073	0.093	0.256	0.006
Retail	0.159	0.063	0.114	0.090
Food and beverage service	0.066	0.021	0.049	0.039
Education	0.062	0.256	0.074	0.205
Health	0.049	0.118	0.034	0.146
Public administration	0.025	0.038	0.013	0.066
Employer of domestic personnel	0.211	0.057	0.217	0.040
Other	0.272	0.297	0.176	0.341
Panel C: Other outcomes				
Out of labor force student	0.203	0.241	0.099	0.184
Married	0.479	0.361	0.618	0.472
Total observations	14423	4742	9211	2467

Notes: **Table A4 presents the mean of a series of variables describing the work status and sector of activity of a representative sample of women aged 18 to 27 in Ethiopia based on the 2021 National Labor Force Survey.** Columns 1-3 consider women living in an urban area, women with at least secondary education, and women who do not live in their place of birth, respectively. Column 4 considers those who share all three characteristics. Panel B conditions on engaging in any productive activity as defined by the National Labor Force Survey. Observations are weighted using sampling weights provided by the Ethiopian Statistical Service.

Table A5: 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	0.007	(0.005)			0.012	(0.064)	0.012	(0.015)			0.017	(0.170)
Monthly	0.000	(0.003)			0.022	(0.038)	-0.009	(0.013)			-0.029	(0.154)
Age			0.000	(0.001)	-0.000	(0.001)			0.003	(0.003)	0.003	(0.004)
Married			-0.001	(0.006)	-0.002	(0.003)			0.009	(0.019)	0.027	(0.032)
Above med. retention			-0.002	(0.002)	-0.003	(0.004)			-0.023	(0.017)	-0.014	(0.022)
Sidemegna = first language			-0.017*	(0.009)	-0.007	(0.009)			-0.059***	(0.021)	-0.100***	(0.037)
From rural area			-0.003	(0.004)	-0.004	(0.005)			-0.012	(0.012)	-0.027	(0.018)
Has university education			-0.000	(0.004)	-0.002	(0.006)			0.000	(0.014)	-0.001	(0.020)
Has secondary education			-0.008	(0.012)	0.001	(0.003)			-0.025	(0.032)	-0.004	(0.045)
Baseline depression index			0.005	(0.004)	0.001	(0.006)			-0.009	(0.011)	0.007	(0.016)
Above med. savings			-0.008**	(0.004)	-0.003	(0.003)			0.009	(0.012)	0.013	(0.017)
Lump sum× Age					0.001	(0.004)					-0.004	(0.007)
Monthly× Age					0.000	(0.001)					0.001	(0.006)
Lump sum× Married					-0.006	(0.007)					-0.022	(0.045)
Monthly× Married					-0.006	(0.007)					-0.022	(0.045)
Lump sum × Above med. retention					0.009	(0.012)					-0.007	(0.030)
Monthly× Above med. retention					-0.004	(0.008)					-0.029	(0.028)
Lump sum× Sidemegna = first language					-0.038	(0.025)					0.054	(0.053)
Monthly× Sidemegna = first language					0.011	(0.009)					0.096**	(0.046)
Lump sum× From rural area					-0.002	(0.014)					0.053*	(0.031)
Monthly× From rural area					0.002	(0.009)					-0.003	(0.028)
Lump sum× Has university education					0.010	(0.013)					0.029	(0.038)
Monthly× Has university education					-0.002	(0.007)					-0.024	(0.030)
Lump sum× Has secondary education					0.003	(0.006)					-0.010	(0.071)
Monthly × Has secondary education					-0.031	(0.032)					-0.061	(0.077)
Lump sum × Baseline depression index					0.011	(0.013)					-0.023	(0.030)
Monthly × Baseline depression index					-0.001	(0.007)					-0.037	(0.025)
Lump sum× Above med. savings					-0.013	(0.012)					0.006	(0.031)
Monthly× Above med. savings					-0.005	(0.007)					-0.021	(0.028)
Lump sum = monthly (p)	0.401						0.413					
Lump sum = monthly = 0 (p)	0.241						0.184					
Δ Control - Non-displaced							0.024	(0.017)				
Control mean	0.006						0.076					
Observations	1813						1813					

Notes: **Table A5 shows that attrition levels are low and balanced across treatment groups.** Columns 1-6 show effects on not having been contacted for any follow-up data collection. Columns 7-12 show effects on not having been contacted at endline. All specifications include strata fixed effects. Robust standard errors are given in even columns in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Job loss effects relative to the always employed counterfactual

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Employment and job search variables										
	Wage work		Self-employed		Formal work		Factory work		Job search active	
	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13	any mon. 0-13	mon. 13
Displacement effect	-0.55***	-0.49***	-0.03*	-0.02	-0.51***	-0.39***	-0.58***	-0.54***	0.60***	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)
Non-displaced mean	1.00	1.00	0.00	0.01	0.88	0.80	0.98	1.00	0.07	0.04
Observations	9547	1288	9547	1288	9547	1288	9547	1288	1372	1288
Panel B: Expenditure and financial variables										
	Labor income		Net transfers received		Total expenditure		Core expenditure		Extreme poverty	
	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13
Displacement effect	-966.67***	-872.06***	166.76***	85.36	-235.91***	-409.91***	-86.76***	-125.64***	0.17***	0.21***
	(43.07)	(65.70)	(44.04)	(73.13)	(49.83)	(88.88)	(23.50)	(38.42)	(0.05)	(0.06)
Non-displaced mean	1523.82	1662.65	387.88	579.79	2007.24	2303.75	903.46	955.22	0.07	0.08
Observations	9547	1288	8259	1288	9547	1288	9547	1288	9547	1288
Panel C: Migration and welfare variables										
	Migrated		Urban not home	At home	Married	# children	Autonomy index		Psych. welfare index	
	mean	mon. 13	mon. 13	mon. 13	endline	endline	mean	endline	mean	endline
Displacement effect	0.23***	0.29***	0.07***	0.21***	0.04*	0.02	0.07	0.04	-0.08	-0.19*
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)	(0.09)	(0.12)	(0.07)	(0.11)
Non-displaced mean	0.00	0.00	0.00	0.00	0.11	0.09	-0.08	-0.06	0.08	0.17
Observations	9547	1288	1288	1288	1288	1288	4490	1288	4490	1288

Notes: **Table A6 reports difference-in-differences estimates of the effects of job loss using as counterfactual workers in the non-displaced sample who remained employed at HIP throughout the study period.** Panels A-C consider employment and job-search outcomes, expenditure and financial outcomes, and migration and welfare outcomes, respectively. Mean effects are estimated using all available month-individual observations. Month-13 effects and endline effects are estimated using a single post-treatment observation, using recall data collected during the endline survey and outcomes measured at the time of the endline survey, respectively. Non-displaced observations are weighted using weights estimated to balance average pre-layoff values of each outcome between the displaced controls and the subset of the non-displaced sample used in this analysis. Weights and unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. Standard errors (in parentheses) are clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Robustness of job loss effects to anticipatory responses

	Self-employed		Job search active		Labor income		Net transfers received		Total expenditure		Core expenditure		Extreme poverty	
	(1) mean	(2) mon. 13	(3) any mon. 0-13	(4) mon. 13	(5) mean	(6) mon. 13	(7) mean	(8) mon. 13	(9) mean	(10) mon. 13	(11) mean	(12) mon. 13	(13) mean	(14) mon. 13
Displacement effect	0.03	0.04	0.54***	0.03	-894.09***	-728.63***	220.73***	126.34*	-152.55***	-340.35***	-69.04***	-105.40***	0.13***	0.16***
	(0.03)	(0.03)	(0.03)	(0.03)	(37.85)	(69.30)	(34.80)	(64.52)	(46.53)	(84.05)	(21.89)	(36.62)	(0.05)	(0.05)
Non-displaced mean	0.01	0.01	0.17	0.07	1415.04	1478.55	390.66	537.90	1972.22	2196.43	892.15	937.61	0.09	0.12
Observations	12150	1634	1748	1634	12150	1634	10516	1634	12150	1634	12150	1634	12150	1634

Notes: **Table A7 reports difference-in-differences estimates of the effects of job loss, using May 2022 — before the layoff was announced — rather than June 2022 as pre-period for the displaced controls.** We present estimates for all the outcomes in Table 1, which have non-constant pre-layoff values. Mean effects are estimated using all available month-individual observations. Month-13 effects and endline effects are estimated using a single post-treatment observation, using recall data collected during the endline survey and outcomes measured at the time of the endline survey, respectively. Non-displaced observations are weighted using weights estimated to balance average June 2022 values of each outcome for the non-displaced sample with average May 2022 values for the displaced controls. Weights and unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Job loss effects estimated using LASSO

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Employment and job search variables										
	Wage work		Self-employed		Formal work		Factory work		Job search active	
	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13
Displacement effect	-0.46***	-0.33***	0.02**	0.03	-0.42***	-0.24***	-0.42***	-0.41***	0.54***	0.12***
	(0.03)	(0.05)	(0.01)	(0.02)	(0.04)	(0.06)	(0.04)	(0.06)	(0.05)	(0.04)
Non-displaced mean	0.91	0.84	0.01	0.02	0.80	0.69	0.86	0.81	0.17	0.07
Observations	870	816	870	816	870	816	870	816	873	816
Panel B: Expenditure and financial variables										
	Labor income		Net transfers received		Total expenditure		Core expenditure		Extreme poverty	
	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13
Displacement effect	-681.70***	-556.40***	280.21***	362.29***	-234.87***	-373.96***	-66.68***	-132.40**	0.12***	0.12**
	(50.34)	(133.49)	(49.24)	(123.66)	(59.53)	(144.29)	(23.99)	(59.48)	(0.03)	(0.05)
Non-displaced mean	1399.87	1448.94	410.82	554.14	1985.32	2215.25	892.22	944.16	0.08	0.09
Observations	870	816	870	816	870	816	870	816	870	816
Panel C: Migration and welfare variables										
	Migrated		Urban not home	At home	Married	# children	Psych. welfare index		Autonomy index	
	mean	mon. 13	mon. 13	mon. 13	endline	endline	mean	endline	mean	endline
Displacement effect	0.18***	0.20***	0.02	0.13***	0.10***	0.11**	-0.03	-0.15	-0.15**	0.07
	(0.03)	(0.05)	(0.01)	(0.03)	(0.04)	(0.05)	(0.07)	(0.13)	(0.07)	(0.13)
Non-displaced mean	0.03	0.09	0.00	0.04	0.11	0.12	0.09	0.10	-0.02	-0.03
Observations	870	816	870	870	816	816	870	816	870	816

Notes: **Table A8 reports estimates of the impact of job loss comparing outcomes between the displaced controls and those of workers in the non-displaced sample using post double selection LASSO (as for the experimental results).** Panels A-C consider employment and job-search outcomes, expenditure and financial outcomes, and migration and welfare outcomes, respectively. Mean effects are estimated using one observation per individual, averaging outcomes over months 0 to 13. Month-13 effects and endline effects are estimated using a single post-treatment observation, using recall data collected during the endline survey and outcomes measured at the time of the endline survey, respectively. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Correlation of net transfers received with employment and labor income

	Net transfers received					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Net transfers mon. 0 to 13						
Wage work	-447.42***	-491.11***			-209.47***	-242.80***
	(37.74)	(22.35)			(50.13)	(25.87)
Labor earnings			-0.28***	-0.29***	-0.19***	-0.19***
			(0.03)	(0.01)	(0.03)	(0.02)
Control mean	458.46	458.46	458.46	458.46	458.46	458.46
Observations	5050	15376	5050	15376	5050	15376
Panel B: Net transfers mon. 2 to 5						
Outgoing transfers (mon. 0 and 1)	-0.16	-0.16	-0.12	-0.12	-0.15	-0.15
	(0.17)	(0.17)	(0.18)	(0.18)	(0.17)	(0.17)
Outgoing transfers (mon. 0 and 1) × Lump sum		-0.09		-0.13		-0.10
		(0.20)		(0.21)		(0.20)
Outgoing transfers (mon. 0 and 1) × Monthly		-0.13		-0.14		-0.13
		(0.20)		(0.22)		(0.21)
Fraction mon. worked (mon. 2 and 5)	-490.38***	-490.38***			-271.39***	-271.39***
	(64.17)	(64.17)			(90.32)	(90.31)
Fraction mon. worked (mon. 2 to 5) × Lump sum		109.66				132.19
		(94.92)				(123.81)
Fraction mon. worked (mon. 2 to 5) × Monthly		-35.09				-69.79
		(83.85)				(116.08)
Average labor income (mon. 2 to 5)			-0.31***	-0.31***	-0.17***	-0.17***
			(0.04)	(0.04)	(0.06)	(0.06)
Average labor income (mon. 2 to 5) × Lump sum				0.06		-0.02
				(0.05)		(0.07)
Average labor income (mon. 2 to 5) × Monthly				0.03		0.04
				(0.05)		(0.07)
Lump sum				40.52		27.34
				(63.81)		(66.96)
Monthly				-67.71		-59.21
				(62.41)		(65.36)
Control mean	567	576	567	576	576	576
Observations	422	1291	422	1291	422	1291
Sample	Control	Displaced	Control	Displaced	Control	Displaced

Notes: **Table A9 shows that net transfers received from others are strongly correlated with employment status and labor earnings.** Panel A shows contemporaneous correlations between net transfers received with wage work and labor earnings, using all available month-individual observations post-layoff. Panel B uses one observation per individual and shows correlations between average monthly net transfers received in month 2 to 5 with the share of months in wage work and average labor earnings over the same period, and with average monthly transfers sent to others in months 0 and 1. Average outgoing transfer in months 0 and 1 is 44 ETB in the control group and 103 ETB in the lump-sum group. Columns 1, 3, and 5 show correlations in the displaced control group. Columns 2, 4, and 6 show correlations in the full displaced sample. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Balance by baseline preference for monthly payments

	Strongly prefer monthly			Not strongly prefer monthly			Δ preference group	
	Lump sum (1)	Monthly (2)	p(equal) (3)	Lump sum (4)	Monthly (5)	p(equal) (6)	Δ (7)	p(Δ=0) (8)
Panel A: Demographics								
Age	22.18	22.37	0.52	22.00	21.98	0.90	0.29	0.10
Married	0.12	0.09	0.34	0.18	0.15	0.26	-0.06	0.01
# children	0.15	0.10	0.31	0.25	0.16	0.06	-0.08	0.02
# children in school	0.10	0.07	0.56	0.16	0.06	0.00	-0.03	0.40
Secondary education	0.95	0.94	0.66	0.94	0.93	0.50	0.01	0.53
University education	0.18	0.31	0.01	0.25	0.22	0.38	0.02	0.62
Enrolled in education	0.47	0.41	0.36	0.39	0.37	0.63	0.06	0.09
Living at place of birth	0.06	0.02	0.11	0.04	0.04	0.67	0.00	0.96
Protestant	0.95	0.91	0.29	0.88	0.92	0.13	0.03	0.14
First language = Sidamegna	0.81	0.85	0.39	0.81	0.83	0.54	0.01	0.83
Rural origin	0.60	0.70	0.08	0.56	0.56	0.92	0.09	0.01
Expects to stay in Hawassa	0.53	0.49	0.46	0.40	0.45	0.22	0.08	0.02
Panel B: Workplace variables								
Tenure (months)	12.41	12.43	0.97	12.43	12.53	0.77	-0.06	0.86
Work as cutter, helper, or layer	0.08	0.10	0.64	0.10	0.10	0.79	-0.01	0.76
Work as packer	0.13	0.12	0.71	0.09	0.08	0.73	0.04	0.11
Work as quality assurance or printer	0.15	0.14	0.84	0.15	0.15	0.88	-0.01	0.73
Work as sewer	0.60	0.59	0.92	0.62	0.61	0.82	-0.02	0.60
Work as storage, cleaner, or trimmer	0.04	0.05	0.63	0.04	0.05	0.40	-0.00	0.85
Panel C: Financial variables								
Wage earnings	1436.28	1420.86	0.64	1461.44	1450.25	0.55	-27.26	0.15
Self-employment earnings	76.54	44.57	0.39	136.31	151.76	0.65	-84.29	0.00
Net transfers received	149.77	180.07	0.56	178.30	181.54	0.93	-14.62	0.64
Total expenditure	1739.81	1650.46	0.22	1760.71	1784.84	0.67	-79.50	0.08
Core expenditure	852.56	864.49	0.73	889.58	876.49	0.63	-24.04	0.28
Rent expenditure	401.47	385.47	0.44	396.64	410.54	0.37	-10.66	0.41
Savings stock	760.12	876.45	0.48	687.38	763.87	0.44	92.75	0.33
Panel D: Wellbeing								
Life satisfaction (1 to 5)	3.32	3.30	0.83	3.21	3.29	0.28	0.05	0.44
Job satisfaction (0 to 10)	6.69	6.95	0.25	6.87	6.81	0.68	-0.01	0.94
Depression index (standardized CESD-10)	0.15	0.02	0.06	0.02	0.13	0.01	0.00	0.91
Observations	130	138		321	350			

Notes: **Table A10 presents the mean of a series of variables in our samples, and shows that the two treatment groups are mostly balanced on observables by baseline preference for monthly payments.** The sample is restricted to the two treatment groups because we do not elicit preferences in the control group. Columns 1 and 2 report the mean of each variable for the two treatment groups, and column 3 the p-value of a test of equality of means between them, among workers who *strongly prefer monthly* payments, defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. Columns 4-6 provide the same information for the other 75% of the sample. Columns 7 and 8 report the difference in means for each variable and the p-value of a test of equality of means, respectively, between the two preference groups overall. Variables in Panels A and D are measured at the time of the baseline survey; variables in Panel B are based on administrative data before the layoff announcement. Panel C presents means in June 2022, the only pre-layoff month for which we observe all workers.

Table A11: Impacts on core expenditure

	Core expenditure			
	(1) mon. 0-1	(2) mon. 2-5	(3) mon. 13	(4) mean
Lump sum	66.17***	6.19	-33.16	0.31
	(23.66)	(16.76)	(33.88)	(15.61)
Monthly	6.23	36.51**	27.47	28.28*
	(22.46)	(15.98)	(33.74)	(15.02)
Control mean	809.76	802.64	825.86	818.63
Lump sum = monthly (p)	0.01	0.07	0.08	0.07
Observations	1314	1350	1312	1400

Notes: **Table A11 reports experimental estimates of the effects of the lump-sum and monthly treatments on core expenditure**, which includes groceries, hygiene products, and mobile phone expenditure. Columns 1-4 consider mean effects during the first two months post-layoff, the following four months, in month 13, and over the whole period from months 0 to 13 after layoff, respectively. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Impacts on expenditure poverty and expenditure smoothing

	Extreme poverty				Total expenditure	
	(1) mon. 0-1	(2) mon. 2-5	(3) mon. 13	(4) mean	(5) sd (0-5)	(6) sd (0-13)
Lump sum	-0.03	0.02	0.05	0.01	117.71***	66.96***
	(0.02)	(0.02)	(0.03)	(0.02)	(28.41)	(23.93)
Monthly	-0.01	-0.03	-0.01	-0.03**	11.46	20.35
	(0.02)	(0.02)	(0.03)	(0.02)	(26.79)	(23.37)
Control mean	0.17	0.24	0.27	0.24	529.72	589.07
Lump sum = monthly (p)	0.30	0.02	0.06	0.01	0.00	0.06
Observations	1314	1350	1312	1400	1373	1400

Notes: **Table A12 reports experimental estimates of the effects of the lump-sum and monthly treatments on expenditure poverty and expenditure smoothing.** Columns 1-4 consider mean effects on being in absolute poverty during the first two months post-layoff, the following four months, in month 13, and over the whole period from months 0 to 13 after layoff, respectively. Being in absolute poverty is defined as spending less than 42.23 ETB (2.15 USD PPP) per day for a given month. Columns 5 and 6 consider the within-worker standard deviation of total expenditure in months 0 to 5 and over the whole period from months 0 to 13 after layoff, respectively. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Short-run impacts on expenditure categories

	Expenditure components (average months 0 to 1)											
	(1) Core	(2) Rent	(3) School	(4) Health	(5) Transport	(6) Job search	(7) Restaurant	(8) Luxury	(9) Durables	(10) Investments	(11) Transfer	(12) Debt service
<u>Main results</u>												
Lump sum	66.17***	2.49	22.05	0.42	2.35	-4.51	18.65**	23.73***	20.79*	3.86	59.41***	5.64
	(23.66)	(15.92)	(15.01)	(8.67)	(6.81)	(5.86)	(8.67)	(8.71)	(11.70)	(10.50)	(13.46)	(7.00)
Monthly	6.23	3.80	0.44	-6.24	-3.94	-4.69	4.35	-4.93	-9.76	9.64	20.68*	9.74
	(22.46)	(14.25)	(14.69)	(8.74)	(6.44)	(5.88)	(8.43)	(7.40)	(10.06)	(11.20)	(11.02)	(7.17)
Control mean	809.76	318.20	242.81	55.67	50.01	60.87	57.49	90.74	59.46	21.11	44.37	32.23
Displacement effect	-8.37	-98.15***	-32.30*	-4.16	-106.58***	46.04***	-7.95	17.46*	11.35	20.35***	7.58	-5.92
Lump sum = monthly (p)	0.01	0.93	0.14	0.42	0.35	0.97	0.13	0.00	0.00	0.63	0.00	0.57
Observations	1314	1314	1314	1314	1314	1314	1314	1314	1314	1314	1314	1314
<u>Δ: lump sum - monthly</u>												
Δ_1 ∣ Str. preferred monthly	141.20***	35.68	15.01	26.34*	9.41	20.81**	24.36*	28.58*	42.66*	12.55	35.33	5.00
	(41.67)	(28.44)	(28.79)	(15.40)	(11.72)	(9.94)	(14.57)	(15.55)	(21.82)	(24.51)	(23.12)	(11.87)
Δ_2 ∣ Not str. preferred monthly	27.15	-16.71	17.35	-1.13	5.69	-8.55	9.64	28.87***	25.40**	-13.52	40.13**	-9.54
	(28.15)	(18.18)	(17.71)	(9.75)	(8.08)	(6.62)	(11.91)	(10.07)	(12.37)	(13.74)	(16.87)	(9.08)
$\Delta_1 = \Delta_2$ (p)	0.02	0.12	0.95	0.14	0.79	0.01	0.44	0.99	0.49	0.35	0.87	0.33
Monthly payment mean	815.67	319.18	237.99	50.28	45.73	56.70	61.49	86.10	50.75	30.91	65.67	40.93
Number of observations	883	883	883	883	883	883	883	883	883	883	883	883

Notes: **Table A13 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by preference for the monthly payments, on expenditure categories in months 0 to 1 after layoff.** Core expenditure includes groceries, hygiene products, and mobile phone expenditure. Strongly preferred monthly is defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. Δ_i indicates the difference between the lump-sum and monthly treatment effects conditional on preferences (that analysis is restricted to the two treatment groups because we do not elicit preferences in the control group). Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Impacts on savings stock

	Savings Stock					
	(1) HF 1	(2) HF2	(3) HF 3	(4) HF4	(5) HF5	(6) endline
Main results						
Lump sum	540.22***	238.19***	16.07	14.57	23.44	-39.29
	(113.11)	(75.35)	(65.01)	(55.96)	(59.85)	(149.80)
Monthly	225.38**	287.58***	122.94*	111.05*	14.89	-149.58
	(95.07)	(73.35)	(67.56)	(61.37)	(59.76)	(144.25)
Control mean	924.162	562.088	544.325	444.776	412.497	883.607
Lump sum = monthly (p)	0.01	0.55	0.12	0.10	0.89	0.30
Displacement effect	149.91	-16.65	8.17	-169.18*	-115.14	-17.12
Observations	1314	1332	1246	1200	1317	1312
Preference het. (Δ_i: lump sum - monthly)						
Δ_1 ∣ Str. preferred monthly	120.10	-205.66	-369.53***	-199.82*	-303.36**	71.33
	(207.05)	(160.82)	(141.30)	(110.93)	(120.05)	(233.02)
Δ_2 ∣ Not str. preferred monthly	398.52***	5.94	5.16	-46.61	137.92**	128.20
	(142.94)	(94.91)	(77.21)	(72.39)	(67.70)	(114.40)
$\Delta_1 = \Delta_2$ (p)	0.272	0.253	0.019	0.248	0.001	0.827
Monthly payment mean	1155.32	847.03	667.30	566.42	432.35	733.16
Number of observations	883	891	843	802	884	877

Notes: **Table A14 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by preference for the monthly payments, on workers' saving stock**. Columns 1 to 5 consider the savings stock measured in each of our five high-frequency survey rounds. Column 6 considers the savings stock measured in the endline survey, for which we randomized whether we ask workers about their savings stock in a single question — as in the high frequency surveys — or whether we elicit separate values for four stocks (banks, cash on hand, at home, with others). Strongly preferred monthly is defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. Δ_i indicates the difference between the lump-sum and monthly treatment effects conditional on preferences (that analysis is restricted to the two treatment groups because we do not elicit preferences in the control group). Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Impacts on total employment

	Total employment			
	(1) mon. 0-1	(2) mon. 2-5	(3) mon. 13	(4) mean
Lump sum	-0.00	-0.09***	-0.09**	-0.07***
	(0.02)	(0.03)	(0.03)	(0.02)
Monthly	-0.00	-0.04	-0.01	-0.03
	(0.02)	(0.03)	(0.03)	(0.02)
Control mean	0.22	0.46	0.55	0.48
Lump sum = monthly (p)	0.93	0.04	0.02	0.03
Observations	1314	1350	1312	1400

Notes: **Table A15 reports experimental estimates of the effects of the lump-sum and monthly treatments on total employment**, which includes both wage employees and self-employed workers. Columns 1-4 consider mean effects during the first two months post-layoff, the following four months, in month 13, and over the whole period from months 0 to 13 after layoff, respectively. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Impacts on types of wage employment

	Formal work		Factory work		HIP work		Uses HIP skills	
	(1) mean	(2) mon. 13	(3) mean	(4) mon. 13	(5) mean	(6) mon. 13	(7) mean	(8) mon. 13
Lump sum	-0.08***	-0.09***	-0.08***	-0.11***	-0.08***	-0.06**	-0.08***	-0.10***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Monthly	-0.02	-0.03	-0.01	-0.02	-0.02	-0.00	-0.03	-0.01
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Control mean	0.33	0.42	0.40	0.46	0.33	0.35	0.38	0.40
Lump sum = monthly (p)	0.00	0.06	0.00	0.01	0.00	0.06	0.01	0.01
Observations	1400	1312	1400	1312	1400	1312	1400	1312

Notes: **Table A16 reports experimental estimates of the effects of the lump-sum and monthly treatments on different types of wage employment.** Columns 1 and 2 consider formal wage employment (having a written contract), columns 3 and 4 manufacturing wage employment, columns 5 and 6 wage employment at HIP, and columns 7 and 8 wage employment that requires workers to use the skills acquired in prior work at HIP. Odd columns consider mean effects over the whole period from months 0 to 13 after layoff, and even columns mean effects in month 13. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A17: Impacts on job quality at endline

	Wage work		Job quality					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	any	hours	index	written contr.	perm. contr.	earnings	job satis. (sd)	exp. tenure
Lump sum	-0.11***	2.11	0.01	-0.04	-0.00	-58.87	0.07	0.82
	(0.03)	(1.57)	(0.10)	(0.04)	(0.04)	(46.45)	(0.10)	(1.29)
Monthly	-0.03	2.49*	-0.09	-0.06*	-0.03	-64.20	0.05	-0.17
	(0.03)	(1.41)	(0.10)	(0.04)	(0.04)	(40.23)	(0.10)	(1.08)
Control mean	0.52	42.56	-0.00	0.84	0.79	1680.46	0.00	14.90
Lump sum = monthly (p)	0.03	0.79	0.36	0.60	0.52	0.90	0.82	0.44
Observations	1312	618	618	618	618	618	618	618

Notes: **Table A17 reports experimental estimates of the effects of the lump-sum and monthly treatments on job quality at endline.** Column 1 considers a dummy for doing any wage work in the previous seven days. Outcomes in columns 2-8 are then conditional on doing any wage work at endline. Column 2 considers the number of hours worked in the previous seven days. Column 3 considers a pre-specified Anderson (2008) job quality index with its components displayed in columns 4-8. Column 4 considers a dummy for having a written contract; column 5 considers a dummy for having a permanent contract; column 6 considers earnings in the last 30 days; column 7 considers a standardized measure of job satisfaction; and column 8 considers workers' expected tenure in the job (in months). Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A18: Impacts on job quality before endline

	Job quality index				
	(1) HF1	(2) HF2	(3) HF3	(4) HF4	(5) HF5
Main results					
Lump sum	-0.09	-0.09	-0.03	-0.07	0.05
	(0.12)	(0.10)	(0.11)	(0.11)	(0.10)
Monthly	0.06	0.06	0.09	0.15	0.11
	(0.12)	(0.10)	(0.09)	(0.11)	(0.09)
Control mean	-0.00	0.00	0.00	0.00	0.00
Lump sum = monthly (p)	0.25	0.17	0.29	0.07	0.54
Observations	479	616	641	629	710
Preference het. (Δ_i: lump sum - monthly)					
Δ_1 | Str. preferred monthly	-0.55**	-0.32	-0.24	-0.67***	-0.38**
	(0.27)	(0.20)	(0.18)	(0.22)	(0.17)
Δ_2 | Str. preferred lump sum	-0.02	-0.09	-0.10	-0.07	0.04
	(0.15)	(0.13)	(0.13)	(0.14)	(0.12)
$\Delta_1 = \Delta_2$ (p)	0.08	0.33	0.52	0.02	0.05
Monthly payment mean	0.10	0.07	0.12	0.16	0.13
Observations	315	376	404	398	454

Notes: **Table A18 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by preference for the monthly payments, on job quality before endline.** Columns 1 to 5 consider the same job quality index as in column 3 in Table A17, constructed for each of our five high-frequency survey rounds, respectively. The index consists of all non-missing job quality variables. Strongly preferred monthly is defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. Δ_i indicates the difference between the lump-sum and monthly treatment effects conditional on preferences (that analysis is restricted to the two treatment groups because we do not elicit preferences in the control group). All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A19: Impacts on family farm and sex work

	Family farm work			Sex work	
	(1) ever	(2) last week	(3) hours last week	(4) list experiment	(5) % colleagues
Main results					
Lump sum	0.01	0.02	-0.03	-0.02	0.00
	(0.03)	(0.02)	(0.48)	(0.06)	(0.02)
Monthly	-0.00	-0.02	-0.98**	-0.05	-0.02
	(0.03)	(0.02)	(0.41)	(0.06)	(0.02)
Control mean	0.28	0.10	2.12	0.10	0.11
Lump sum = monthly (p)	0.64	0.06	0.02	0.68	0.22
Observations	1312	1311	1311	966	683
Preference het. (Δ_i: lump sum - monthly)					
Δ_1 ∣ Str. preferred monthly	-0.03	0.03	0.65	0.18	0.03
	(0.06)	(0.04)	(0.86)	(0.12)	(0.03)
Δ_2 ∣ Not str. preferred monthly	0.03	0.05**	1.17**	-0.02	0.01
	(0.04)	(0.02)	(0.46)	(0.07)	(0.02)
$\Delta_1 = \Delta_2$ (p)	0.40	0.73	0.59	0.15	0.63
Monthly payment mean	0.27	0.08	1.21	0.05	0.10
Number of observations	877	877	877	639	452

Notes: **Table A19 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by preference for the monthly payments, on family farm work and sex work.** Columns 1-3 consider different measures of family farm work: a dummy indicating any family farm work since layoff, a dummy indicating any family farm work in the 7 days before the endline survey, and the number of hours of family farm work in the 7 days before the endline survey, respectively. Columns 4 and 5 consider impacts on two indirect measures of sex work. Column 4 considers a dummy for engaging in any sex work in the previous 12 months, measured using a randomized list experiment at endline (when the survey was conducted in person). Column 5 considers the percentage of colleagues perceived to have received money for sex in the 12 months before the endline survey. Strongly preferred monthly is defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. Δ_i indicates the difference between the lump-sum and monthly treatment effects conditional on preferences (that analysis is restricted to the two treatment groups because we do not elicit preferences in the control group). All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A20: Mediation analysis of the month-13 wage employment effects

	Job loss effect	Treatment effect difference (lump sum - monthly)			
	Pooled	Str. pref. monthly	Not str. pref. monthly	Str. pref. monthly	Not str. pref. monthly
	(1)	(2)	(3)	(4)	(5)
Natural Direct Effect	-0.214	-0.074	-0.022	-0.069	-0.006
Natural Indirect Effect	-0.108	-0.011	-0.048	-0.015	-0.065
Total Effect	-0.322	-0.085	-0.070	-0.085	-0.070
Percent mediated	0.336	0.128	0.690	0.181	0.921
Mediators	Migration & marriage	Migration, job search & self-emp. exp.		Migration, marriage, job search & self-emp. exp., # children	

Notes: **Table A20 presents a mediation analysis of the month-13 wage-employment effects of (i) job loss and (ii) of the lump-sum relative to the monthly treatment.** The Natural Indirect Effect is the effect that is attributable to the change in the mediator, the Natural Direct Effect the change that cannot be attributed to the change in the mediator, and the Total Effect is simply the sum of its two components (see footnote 47 for details). We identify the NIE using a parsimonious linear model that does not allow for treatment-mediator interactions, and we consider five key mediators. Job search is a dummy indicating any job search in months 0-1 (as in Table 3). Self-employment is a dummy for engaging in any self-employment activities in months 0-13 (as in Table 5). Migration is a dummy indicating that the respondent does not live in Hawassa in month 13 (as in Table A23). Being married and number of children are measured at endline (as in Table 5). Note that these variables thus refer to a point in time shortly after month 13 — we view this as uncontroversial since these variables can only change slowly over time. Column 1 considers the effect of job loss and columns 2-5 consider the differential effect of the lump-sum and monthly treatments by preference for the monthly payments. Strongly preferred monthly is defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. All regressions underlying our estimates in this table include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO.

Table A21: Implied marginal propensity to spend, earn, and save

	Spend	Earn	Save
	(1)	(2)	(3)
Main results			
Lump sum	0.86	-0.12	0.02
Monthly	1.08	0.08	0.01
Preference heterogeneity			
Δ_1 | Str. preferred monthly	-0.01	-0.10	-0.09
Δ_2 | Not str. preferred monthly	-0.31	-0.25	0.06

Notes: **Table A21 presents estimates of the propensities to spend, earn, and save — over the whole period of our study — out of the increased cash-on-hand provided by our two treatment arms over the period of our study.** All propensities are computed as the difference between values in a treatment arm and in the control group, normalized by the size of the experimental payments in that treatment arm (i.e., they are expressed in "per ETB received" term). The propensity to earn in column 2 captures the difference in average total income — excluding only the experimental payments — between layoff and the time of the endline survey (we assign month-13 values to monthly earnings between month 13 and the month of the endline survey). To improve measurement, the sample is restricted to individuals with complete income recall data over our study period (see Section B for details on how we construct a times series of outcomes). The propensity to save in column 3 captures the change between workers' average savings stock at layoff and as measured at the time of the endline survey. The former is extrapolated linearly (and assumed to be weakly positive) from the savings stock measured in the baseline survey as: 'baseline savings + pre-layoff income − pre-layoff expenditure,' where we assign the latest monthly income value in the baseline survey to the baseline-to-layoff period and assume that pre-layoff expenditure is balanced across treatment groups. The average propensity to spend in column 1 is then recovered using the accounting identity (as in, e.g., Kolsrud et al., 2018): $SavingStock_k - SavingStock_0 = \sum_{t=0}^{t=k} TotalIncome_t - \sum_{t=0}^{t=k} TotalExpenditure_t$, where the total income here includes the experimental payments. When we compute propensities by policy preference we use the unconditional control group mean, as we do not observe preferences for payment modalities in the control group.

Table A22: Impacts on psychological welfare and empowerment

	Welfare index		Anxiety index		Depression index	Life satis. (sd)		Empowerement index	
	(1) HF 1-2	(2) endline	(3) HF 1-2	(4) endline	(5) endline	(6) HF 1-2	(7) endline	(8) HF 1-2	(9) endline
Main results									
Lump sum	0.07	0.08	-0.04	-0.04	-0.13*	0.07	0.03	-0.03	0.08
	(0.05)	(0.07)	(0.06)	(0.07)	(0.07)	(0.05)	(0.07)	(0.05)	(0.07)
Monthly	0.12**	-0.05	-0.06	0.11*	-0.05	0.11**	-0.03	-0.02	0.04
	(0.05)	(0.07)	(0.05)	(0.07)	(0.07)	(0.05)	(0.07)	(0.05)	(0.07)
Control mean	0.017	-0.000	-0.020	-0.000	0.000	0.007	0.000	-0.017	0.000
Lump sum = monthly (p)	0.39	0.05	0.65	0.02	0.25	0.36	0.35	0.96	0.56
Observations	1364	1312	1364	1312	1312	1364	1312	1364	1312
Preference het. (Δ_i: lump sum - monthly)									
Δ_1 | Str. preferred monthly	0.05	0.22*	0.02	-0.27**	-0.11	0.09	0.13	-0.09	0.06
	(0.10)	(0.13)	(0.11)	(0.12)	(0.12)	(0.09)	(0.14)	(0.10)	(0.12)
Δ_2 | Not str. preferred monthly	-0.07	0.09	0.02	-0.11	-0.09	-0.09	0.04	0.03	0.03
	(0.06)	(0.08)	(0.07)	(0.08)	(0.09)	(0.06)	(0.08)	(0.06)	(0.08)
$\Delta_1 = \Delta_2$ (p)	0.308	0.410	0.976	0.270	0.875	0.090	0.557	0.298	0.865
Monthly payment mean	0.13	-0.05	-0.08	0.11	-0.03	0.12	-0.03	-0.04	0.04
Number of observations	913	877	913	877	877	913	877	913	877

Notes: **Table A22 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by preference for the monthly payments, on measures of psychological welfare and empowerment.** Columns 1 and 2 consider a pre-specified psychological welfare index composed of mental health and life satisfaction. The mental health component differs across survey rounds. In high-frequency survey rounds 1-3, it includes an anxiety index (HADS-A). In high-frequency survey rounds 4 and 5 and at baseline, it includes a depression index (CESD-10). At endline, it includes both the depression and the anxiety index. Columns 3 and 4 consider the anxiety index in the first two high-frequency survey rounds and at endline, respectively. Column 5 considers the depression index at endline. Higher values indicate worse mental health. Columns 6 and 7 consider a standardized measure of life satisfaction in the first two high-frequency survey rounds and at endline, respectively. Columns 8 and 9 consider an empowerment index combining measures of autonomy from parents and partners (if applicable) and respondents' network size and strengths, in the first two high-frequency survey rounds and at endline, respectively. All indices are constructed using variance-covariance weights following Anderson (2008). Strongly preferred monthly is defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. Δ_i indicates the difference between the lump-sum and monthly treatment effects conditional on preferences (that analysis is restricted to the two treatment groups because we do not elicit preferences in the control group). All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A23: The long-run impacts of job-loss payments by baseline preference for monthly payments

	Total expenditure		Labor income		Net transfers received		Wage work		Self-employed		Migrated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13	mean	mon. 13
Δ_i: lump sum - monthly												
Δ_1 | Str. preferred monthly	-38.68	-113.14	-93.48	-96.91	52.26	-29.29	-0.03	-0.09	-0.01	0.02	-0.04	0.01
	(64.71)	(154.00)	(71.99)	(125.60)	(64.24)	(124.53)	(0.04)	(0.06)	(0.02)	(0.03)	(0.04)	(0.06)
Δ_2 | Not str. preferred monthly	-69.06*	-117.32	-34.72	-69.93	43.30	-9.92	-0.06**	-0.07*	0.02*	0.02	0.07***	0.11***
	(41.69)	(99.85)	(45.88)	(84.92)	(39.66)	(72.70)	(0.03)	(0.04)	(0.01)	(0.02)	(0.03)	(0.04)
$\Delta_1 = \Delta_2$ (p)	0.69	0.98	0.49	0.86	0.91	0.89	0.54	0.86	0.18	0.95	0.02	0.14
Monthly payment mean	1820.25	1970.40	714.72	873.25	551.30	723.77	0.41	0.49	0.05	0.06	0.23	0.30
Number of observations	932	877	932	877	932	877	932	877	932	877	932	877

Notes: **Table A23 reports estimates of the differential longer-run effects of the lump-sum and monthly treatments by preference for the monthly payments.** The table focuses on the same outcomes as in Tables 2 and 4: total monthly expenditure (columns 1 and 2), labor income (columns 3 and 4), net informal transfers (columns 5 and 6), a dummy indicating wage employment (columns 7 and 8), a dummy indicating self-employment (columns 9 and 10), and a dummy indicating migration out of Hawassa (columns 11 and 12). Odd columns consider mean effects over the whole period from months 0 to 13 after layoff; even columns consider treatment effects in month 13 after layoff. Strongly preferred monthly is defined as being in the 25% of the sample with the strongest preference for the monthly payment scheme. Δ_i indicates the difference between the lump-sum and monthly treatment effects conditional on preferences (that analysis is restricted to the two treatment groups because we do not elicit preferences in the control group). Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A24: Main results by savings stock at baseline

	Total expenditure				Labor income				Net transfers received			
	(1) mon. 0-1	(2) mon. 2-5	(3) mon. 13	(4) mean	(5) mon. 0-1	(6) mon. 2-5	(7) mon. 13	(8) mean	(9) mon. 0-1	(10) mon. 2-5	(11) mon. 13	(12) mean
Panel A: Expenditure												
Lump sum	294.62***	63.81	-159.64	32.03	18.18	-44.57	-273.14**	-103.95*	-40.57	141.35**	42.03	56.58
	(82.63)	(52.32)	(114.43)	(49.17)	(64.70)	(65.34)	(106.09)	(54.84)	(71.95)	(64.09)	(98.03)	(50.37)
Lump sum × Above med. savings	-118.17	-157.87**	247.08	-6.37	66.30	-70.18	147.31	24.48	9.53	-71.60	28.49	-30.06
	(123.16)	(73.02)	(157.81)	(69.53)	(88.07)	(88.20)	(144.81)	(75.46)	(98.97)	(82.48)	(123.68)	(67.17)
Monthly	136.88*	78.32	97.74	88.50*	-43.38	-49.05	-213.62**	-114.85**	40.98	15.71	50.35	40.66
	(82.65)	(51.35)	(116.10)	(48.48)	(56.59)	(61.59)	(105.36)	(54.35)	(64.02)	(59.00)	(99.97)	(48.56)
Monthly × Above med. savings	-189.54	-12.69	45.61	5.31	133.13	55.49	193.37	128.86*	-68.33	-119.07	20.31	-81.32
	(118.56)	(70.17)	(159.75)	(66.59)	(81.09)	(88.65)	(140.63)	(74.66)	(91.61)	(76.78)	(128.50)	(65.26)
Lump sum | Above med. savings	176.45*	-94.06*	87.44	25.66	84.48	-114.76*	-125.83	-79.47	-31.04	69.75	70.52	26.52
	(92.08)	(51.36)	(109.80)	(49.74)	(60.25)	(60.04)	(97.95)	(52.18)	(67.94)	(51.68)	(76.28)	(44.19)
Monthly | Above med. savings	-52.66	65.62	143.35	93.82**	89.76	6.44	-20.24	14.00	-27.35	-103.36**	70.66	-40.66
	(84.59)	(48.33)	(110.12)	(45.68)	(58.05)	(62.94)	(94.86)	(50.75)	(65.47)	(49.66)	(83.81)	(43.77)
Control mean	1995.85	1654.60	1846.83	1738.11	274.42	661.82	985.57	750.91	395.19	562.97	656.30	561.35
Observations	1314	1350	1312	1400	1314	1350	1312	1400	1314	1350	1312	1400
Panel B: Employment and migration												
	Wage work				Self-employed				Migrated			
Lump sum	-0.05*	-0.08**	-0.11**	-0.08**	0.00	0.01	0.03	0.02	0.00	0.02	0.06	0.00
	(0.03)	(0.04)	(0.05)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.05)	(0.03)
Lump sum × Above med. savings	0.02	-0.05	0.02	-0.02	0.06**	0.02	-0.01	0.02	0.07	0.02	0.03	0.05
	(0.04)	(0.05)	(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.05)	(0.04)	(0.06)	(0.04)
Monthly	-0.02	-0.05	-0.05	-0.06*	0.00	0.01	-0.00	0.00	0.02	0.01	0.00	0.00
	(0.03)	(0.04)	(0.05)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.04)	(0.03)
Monthly × Above med. savings	0.01	0.01	0.06	0.04	0.02	-0.00	0.02	0.01	0.00	-0.01	-0.01	-0.01
	(0.04)	(0.05)	(0.07)	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)	(0.05)	(0.04)	(0.06)	(0.04)
Lump sum | Above med. savings	-0.03	-0.13***	-0.09*	-0.10***	0.06***	0.03*	0.02	0.03**	0.07**	0.04	0.09**	0.05*
	(0.03)	(0.04)	(0.05)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.04)	(0.03)
Monthly | Above med. savings	-0.01	-0.04	0.01	-0.02	0.03*	0.01	0.01	0.02	0.02	-0.00	-0.01	-0.01
	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.02)	(0.04)	(0.03)
Control mean	0.19	0.42	0.51	0.44	0.03	0.04	0.05	0.04	0.20	0.13	0.29	0.23
Observations	1314	1350	1312	1400	1314	1350	1312	1400	1350	1314	1312	1400

Notes: **Table A24 reports estimates of the differential effects of the lump-sum and monthly treatments by baseline savings stock,** i.e., a dummy indicating a value above the median (300 ETB). The table focuses on the same outcomes as in Tables 2 and 4: total monthly expenditure (panel A, columns 1-4), labor income (panel A, columns 5-8), net informal transfers (panel A, columns 9-12), a dummy indicating wage employment (panel B, columns 1-4), a dummy indicating self-employment (panel B, columns 5-8), and a dummy indicating migration out of Hawassa (panel B, columns 9-12). For each outcome, we report mean effects in the first two months post-layoff, in the following four months, in month 13, and over the whole period of our study. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A25: Impacts of preferred treatment on types of employment

	Wage work		Self employment		Formal work		Factory work		HIP work	
	(1) mean	(2) mon. 13	(3) mean	(4) mon. 13	(5) mean	(6) mon. 13	(7) mean	(8) mon. 13	(9) mean	(10) mon. 13
Preferred treatment	-0.06***	-0.06*	0.02**	0.01	-0.05**	-0.06*	-0.05**	-0.06*	-0.05**	-0.05
	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Control mean	0.44	0.51	0.04	0.05	0.33	0.42	0.40	0.46	0.33	0.35
Observations	943	880	943	880	943	880	943	880	943	880

Notes: **Table A25 reports experimental estimates of the effects of receiving the preferred treatment on different types of employment.** The treatment group for the results in this table combines all workers who received the lump-sum and monthly payments, and excludes those who did not received their preferred treatment, as elicited in the baseline survey. Columns 1 and 2 consider wage employment, columns 3 and 4 self-employment, columns 5 and 6 formal wage employment (having a written contract), columns 7 and 8 manufacturing wage employment, and columns 9 and 10 wage employment at HIP. Odd columns consider mean effects over the whole period from months 0 to 13 after layoff; even columns consider treatment effects in month 13 after layoff. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

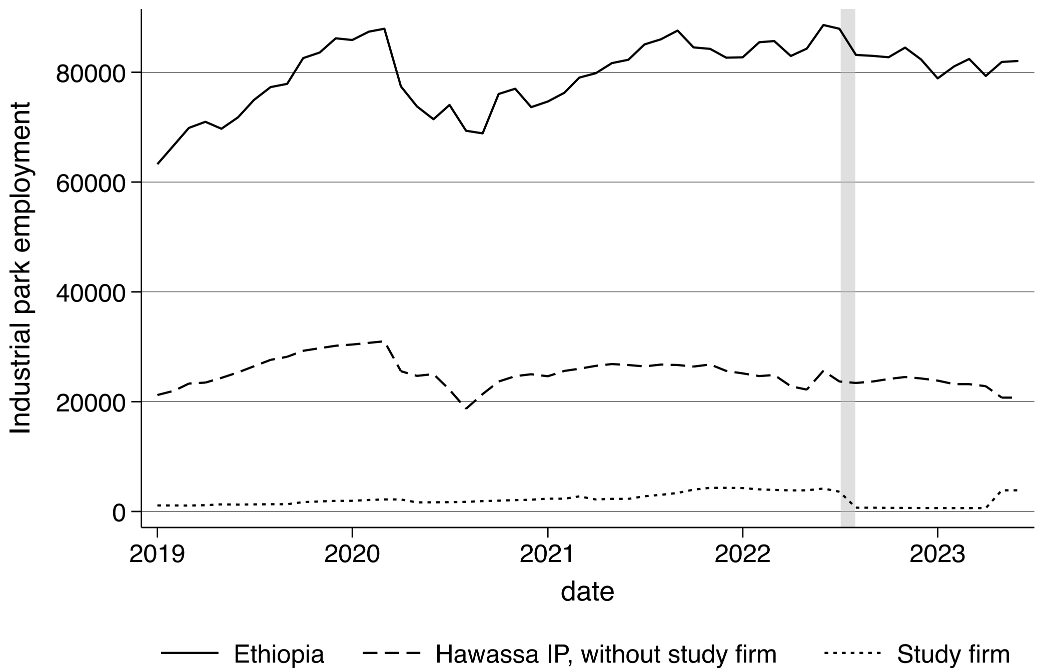
Table A26: Impacts on willingness-to-pay for additional job-loss insurance

	WTP per month (% of payout)		
	(1) pref. scheme	(2) lump sum	(3) monthly
Lump sum	0.002	0.002**	0.002
	(0.001)	(0.001)	(0.001)
Monthly	-0.000	-0.001	0.001
	(0.001)	(0.001)	(0.001)
Control mean	0.03	0.02	0.02
Lump sum = monthly (p)	0.19	0.02	0.46
Observations	1694	1694	1694

Notes: **Table A26 reports experimental estimates of the effects of the lump-sum and monthly treatments on respondents' willingness-to-pay (WTP) for additional job displacement insurance at endline.** The WTP is measured as the maximum premium that they are willing-to-pay while formally employed as a fraction of the additional financial support at job loss (3850 ETB; 100 ETB equaled 5.09 USD PPP at the time of the experiment). Columns 1-3 consider the WTP for the insurance product under three payment modalities for the additional support at job loss: workers' preferred payment modality, lump-sum disbursement, and five monthly tranches, respectively. The elicitation procedure for this insurance product is described in detail in Appendix D. All specifications include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2 Appendix Figures

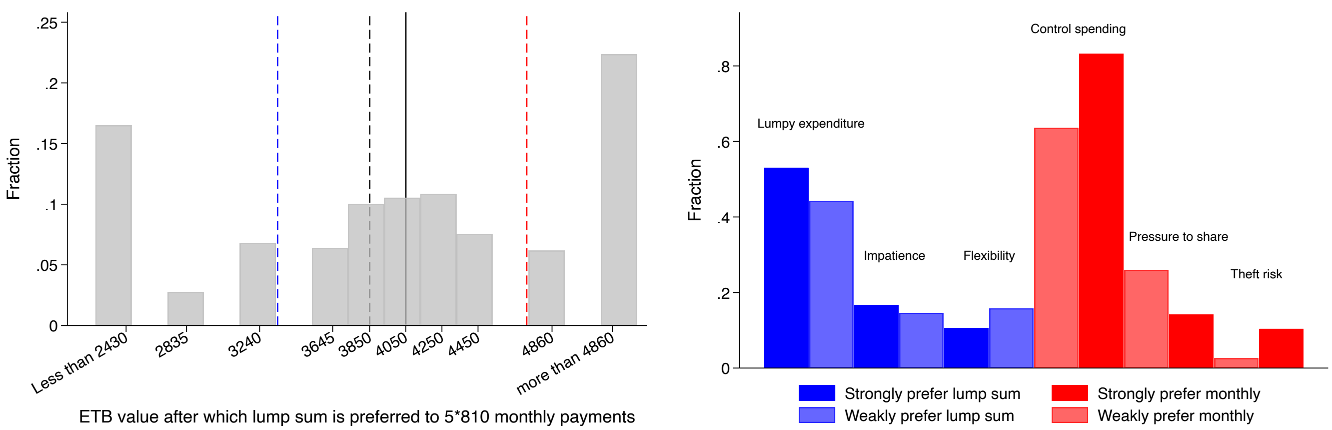
Figure A1: Employment in Ethiopian industrial parks over time



Notes: **Figure A1 shows that the mass layoff at the study firm is not accompanied by other mass layoffs in industrial parks in Hawassa and Ethiopia.** It shows how total employment in Ethiopian industrial parks, Hawassa Industrial Park, and the study firm in this paper evolve over time (based on our calculations using data from the Ethiopian Investment Commission). The vertical area indicates the time of the mass layoff at the study firm.

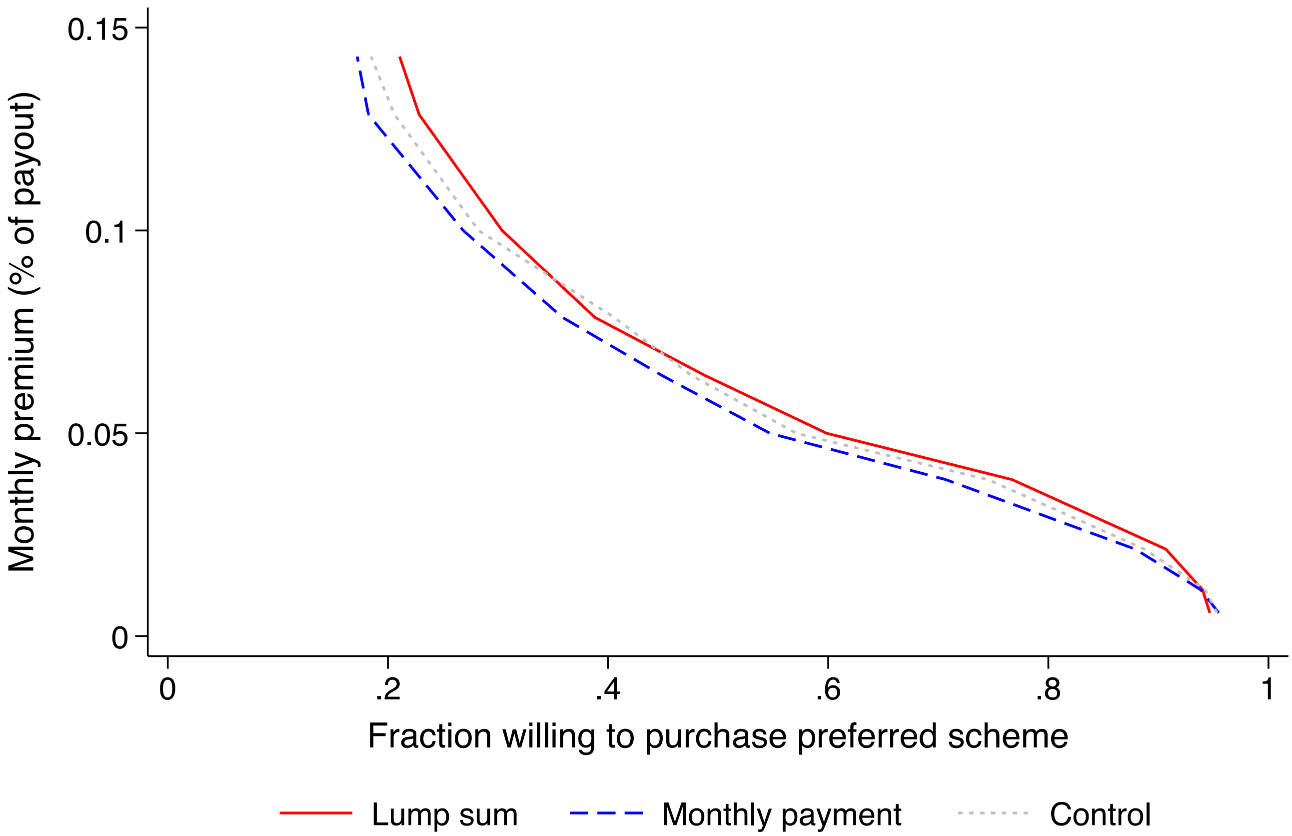
Figure A2: Baseline preferences for payment modalities

(a) Preferences (b) Self-reported reasons



Notes: **Figure A2 displays the distribution of workers' baseline preference for payment modalities, and reports the main reasons invoked by workers to justify their preference for either payment modality.** Panel (a) displays the distribution of the value of the lump-sum payment at which workers switch from preferring the monthly scheme — 5 monthly payments of 810 ETB — to preferring the lump-sum payment. The red vertical line indicates the threshold that we use to define a strong preference for the monthly payments in our results by baseline preference for monthly payments in the paper, i.e., identifying the top 25% of the distribution. The black vertical lines indicate the value of the lump-sum in the experiment (3850 ETB; dashed line) and the nominal value of the sum of monthly payments (4050 ETB, solid line). The blue vertical line indicates the threshold that we use to define a strong preference for the lump-sum payment in panel (b), i.e., identifying the bottom 25% of the distribution. At baseline, we also ask workers to report the reasons why they prefer the monthly payments — if they choose a value above 4050 ETB in panel (a) — or the reasons why they prefer the lump-sum payment — if they choose a value of at most 4050 ETB in panel (a). Panel (b) displays the main self-reported reasons provided by workers in response to this question, and the share of workers who mention a response that fits in each of these main categories. We separately consider workers who strongly prefer monthly payments (switch point above the red line in panel a), who weakly prefer monthly payments (switch point above 4050 ETB and below the red line in panel a), who weakly prefer the lump-sum payment (switch point above the blue line and at most 4050 ETB in panel a), and who strongly prefer the lump-sum payment (switch point below the blue line), respectively. 100 ETB equaled 5.09 USD PPP at the time of the experiment. The sample is restricted to the two treatment groups because we do not elicit preferences in the control group.

Figure A3: Demand for additional job-loss insurance by experimental group



Notes: **Figure A3 displays the incentivized (inverse) demand curve for additional job-loss insurance in the control group and in the two treatment groups at endline, for workers' preferred payment modality.** The x-axis displays the fraction of individuals willing to purchase the insurance product at a given price, and the y-axis the maximum premium they are willing-to-pay while formally employed as a fraction of the additional financial support at job loss (3850 ETB; 100 ETB equaled 5.09 USD PPP at the time of the experiment). The elicitation procedure for this insurance product is described in detail in Appendix D.

B Data aggregation and measurement

B.1 The aggregation of data across surveys

This section describes how we construct a time series of outcomes. Specifically, when we aggregate data across different survey rounds, we must address three issues:

- 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. To calculate mean outcomes we assign the month-5 value to 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. To calculate mean outcomes we assign the month-9 value to 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 months 11 and 12 (though we did for month 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 month-12 expenditure. To calculate mean outcomes we assign the month-13 value to months 11 and 12.

We winsorize all unbounded monthly variables at the 99^{th} percentile. All averages are taken using winsorized measures but are then not winsorized again.

B.2 Consumption expenditure measurement

This section provides evidence against three possible confounders of our expenditure results.

Strategic misreporting. First, we consider the possibility that workers may intentionally misreport their expenditure. For example, they may want to appear poorer, as they may think that this will increase their chances of obtaining financial support in the future. To test for this, in the endline survey, we included a question capturing expenditure on Teff — an expensive local grain, and a luxury good since its income elasticity is above one (Tafere et al., 2011). We randomized whether the question on Teff expenditure was asked before or after the module where we elicited demand for a future real insurance product that we would offer to selected respondents. The logic of this test is that strategic misreporting should be highest after respondents have been told that we are considering offering additional financial support to some of them, as this would give them additional reasons to alter their report to manipulate our behavior. Table B1 reports the results of this analysis. We do not find any evidence that respondents who were asked about their Teff expenditure after the module on future financial support changed their answers in any systematic way, compared to those who were asked about their Teff expenditure prior to the same module. Thus, we do not find evidence of strategic misreporting.

Table B1: Test for strategic misreporting of expenditure

	Teff expenditure	
	(1) Any	(2) ETB
Induced strategic concerns	0.013	-0.109
	(0.018)	(15.477)
Control mean	0.15	99.45
Observations	1690	1690

Notes: **Table B1 shows that reported expenditure on a luxury good is not affected by induced strategic concerns.** Teff is an expensive grain that is perceived as luxury food consumption for our sample population in Ethiopia. *Induced strategic concerns* is a dummy indicating that the question about Teff expenditure is asked after the elicitation of WTP for additional job displacement insurance. This may induce expectations that the research team would offer additional payments based on perceived need. Teff expenditure is winsorized at the 99th percentile. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Recall bias. Second, we consider recall bias. Individuals may struggle to appropriately recall past expenditure. While it is unclear ex-ante in which direction this would affect our estimates of treatment effects, it is important to establish whether this is a likely source of measurement error. First, we note that one of the primary reasons for running the high frequency surveys was to limit recall bias. Second, to study recall bias directly, we perform two different exercises, using data from the high-frequency surveys. First, in each survey, we asked about consumption expenditure for up to three different completed months. We can thus test whether individuals report a different level of expenditure for recall months that are closer in time, compared to recall months that are farther away. Second, while recall months are fixed for each survey wave, we have natural variation in the time of the interviews. We can thus study whether reported expenditure for the same recall month changes as the time between recall month and interview date increases. We report our results in Table B2. We do not find evidence of recall bias with either of these two tests, nor do we find evidence that the impact of recall delay changes by treatment status. Thus, overall, we do not believe that overall recall bias confounds our expenditure results in a particular direction.

Table B2: Recall effects on expenditure

	Monthly total expenditure					
	Survey design-based			Survey timing based		
	(1)	(2)	(3)	(4)	(5)	(6)
Recall (months)	-1.28	-4.52	-10.99	-35.58	42.11	46.58
	(4.97)	(4.66)	(11.69)	(23.60)	(26.75)	(37.02)
Recall (months)			0.85			12.00
× Lump sum			(18.05)			(41.81)
Recall (months)			9.94			-38.62
× Monthly			(17.56)			(41.52)
Recall (months)			15.49			10.90
× Non-displaced			(15.65)			(38.38)
Total expenditure sample mean	1831.78	1831.78	1831.78	1831.78	1831.78	1831.78
Recall sample mean	0.62	0.62	0.62	0.67	0.67	0.67
Recall sample standard deviation	0.64	0.64	0.64	0.63	0.63	0.63
Observations	18718	18705	18705	15448	15448	15448
HF round and Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	Yes	Yes	No	Yes	Yes

Notes: **Table B2 shows that reported monthly total expenditure is not related to recall length.** Data is at the month-individual level. Columns 1-3 use variation in recall length induced by the survey structure (we ask for up to three months of past expenditure data). The first month in each high-frequency survey is coded as 0. We restrict the sample to recall months that are completed before the interview (see sub-section B.1 for a discussion of this issue. Columns 4-6 use variation in recall length induced by variation in the timing of phone interviews for the same recall period. The sample is smaller because we only include observations with interviews completed after the end of the overall recall period for that specific survey round. All specifications include strata and survey round fixed effects. Standard errors (in parentheses) are clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Household composition. Third, we consider changes in household composition driven by the fact that some workers move in with their parents after job loss. Parents may pay for part of these workers' food costs and bills, enabling them to consume at a higher level than is implied by their expenditure. This mechanical drop in expenditure would be a particular concern for our job loss analysis, since it would imply that the fall in consumption expenditure that we observe after job loss would be an upper bound of the actual fall in consumption experienced by displaced workers. To study this hypothesis, we perform a mediation analysis, following the approach described in footnote 47. This exercise is complicated by the fact that living with one's parents is strongly associated with a lower propensity to be employed. Thus, we cannot simply compute the share of the treatment effect on expenditure that is mediated by living with one's parents, since this would reflect both (i) the mechanical impact of household composition on expenditure we are concerned with and (ii) the fact that individuals who live with their parents are less likely be employed and hence tend to earn and spend less. To circumvent this problem, we proceed as follows. First, we study the mediating role of employment, by computing the Natural Indirect Effect (NIE) of employment on endline expenditure, and the ratio of this NIE to the total treatment effect. Then, we introduce living with one's parents as a second mediator, and we recompute the NIE-to-total-effect ratio. This exercise identifies the additional mediation role played by living with one's parents once the role of employment has been accounted for. Hence, if no additional confounders are present, it captures the effect we are concerned with: the mechanical change in expenditure driven by household composition.

We report our results in Table B3. For the impact of job loss, we document that including living with one's parents increases the ratio of the NIE to the total treatment effect by a modest 10 percentage points (from 73 to 83 percent). In other words, once the role of employment is accounted for, a potential mechanical drop in expenditure when people move in with their parents does not appear to be a major driver of the fall in expenditure individuals experience after job displacement. For completeness, we also perform the same analysis for our two experimental treatments. Living with one's parents virtually plays no role in determining the impacts of the monthly treatment. For the lump-sum treatment, it reduces impacts on expenditure by about 14 ETB — a small effect that does not change any of our core conclusions.

Table B3: Does living with one's parents mediate the month-13 expenditure effects?

	Total expenditure in month 13					
	Job loss effect		Treatment effects			
			Lump sum	Monthly	Lump sum	Monthly
	(1)	(2)	(3)	(4)	(5)	(6)
Natural Direct Effect	-83.376	-51.465	44.379	124.330	57.925	122.056
Natural Indirect Effect	-229.602	-261.513	-71.998	-6.672	-85.544	-4.398
Total Effect	-312.978	-312.978	-27.619	117.658	-27.619	117.658
NIE/Total Effect	0.734	0.836	2.607	-0.057	3.097	-0.037
Mediators	ec. active	ec. active & at home	ec. active		ec. active & at home	

Notes: **Table B3 shows that living with one's parents cannot explain much of the month-13 expenditure effects of job loss and additional job-loss payments.** It presents a similar mediation analysis as in Table A20. The Natural Indirect Effect is the effect that is attributable to the change in the mediator, the Natural Direct Effect the change that cannot be attributed to the change in the mediator, and the Total Effect is simply the sum of its two components (see footnote 47 for details). We identify the NIE using a parsimonious linear model that does not allow for treatment-mediator interactions, and we consider two key mediators. Economically active is a dummy indicating that an individual works as wage employee or self-employed worker in month 13. At home is a dummy indicating that an individuals is living with one's parents in month 13. Columns 1 and 2 consider the effect of job loss and columns 3-6 consider the effect of the lump-sum and monthly treatments. All regressions underlying our estimates in columns 1 and 2 weight non-displaced observations using weights estimated to balance average pre-layoff values of each outcome between the displaced control and the non-displaced group. All regressions underlying our estimates in columns 3-4 include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO.

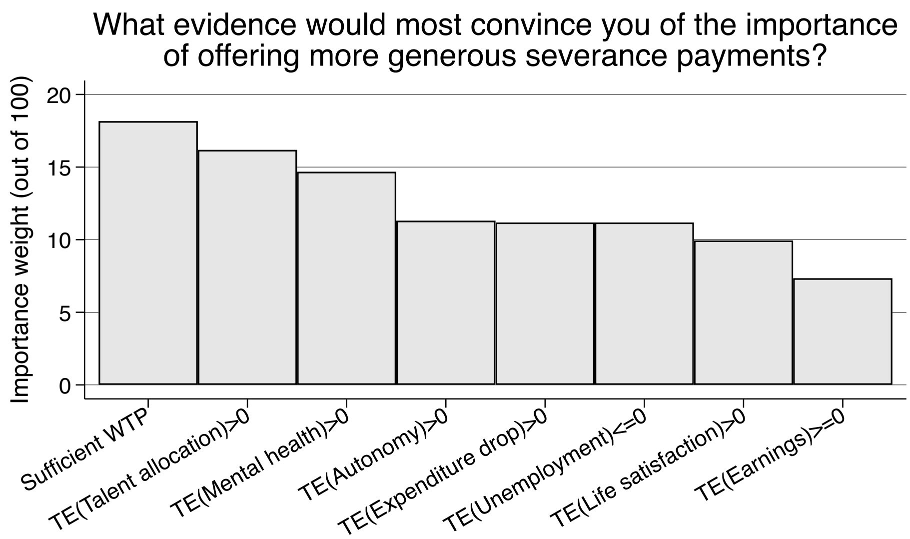
C Expert survey

This section describes the data on expert predictions mentioned in the paper.

We create a survey to elicit experts' predictions of the treatment effects of our experiment for a range of outcomes and time periods. We also elicit predictions about the effect of job loss on core expenditure. To anchor expectations, we provide baseline means of the outcomes where possible. Data collection lasted from June to October 2023 before we had presented the results of this study.

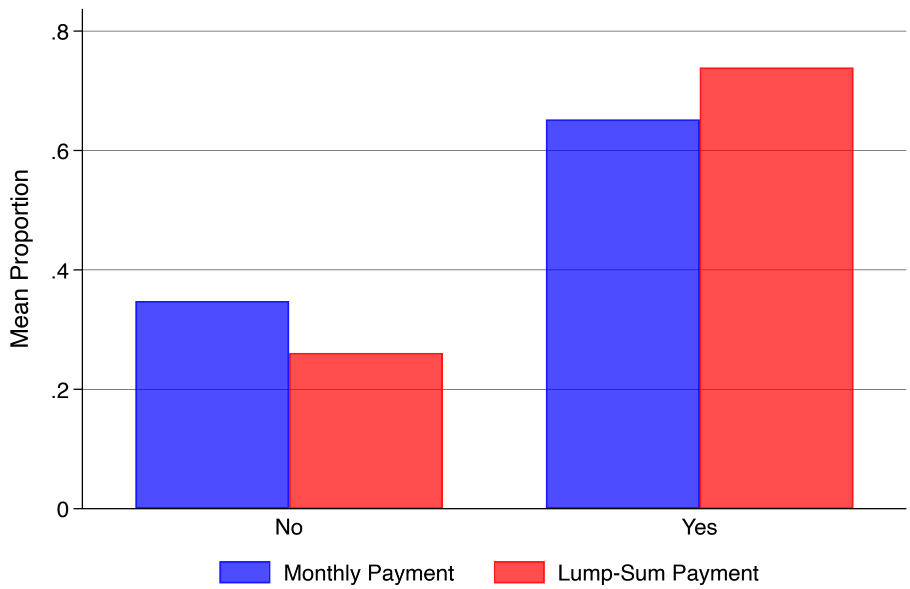
To publicize the survey, we sent out personal email invitations to 365 economists selected from the list of individuals who would attend the NBER 2023 summer meetings in Development Economics and in Public Economics. We also sent out an invite to complete our survey to a list of researchers affiliated to the Institute for Fiscal Studies (IFS) in London. The survey was hosted on the Social Science Prediction Platform through which a panel of expert social scientists are incentivized to predict treatment effects. IRB for this survey was obtained at Guanghua School of Management, Peking University (#2023-20). In total, 102 respondents predict at least one treatment effect and all treatment effect predictions have at least 75 responses. 69 respondents give a prediction about whether workers' willingness-to-pay would be sufficient to cover the actuarially-fair cost of the additional job loss insurance.

Figure C1: Experts' views on the most relevant evidence to justify increasing job-loss insurance



Notes: **Figure C1 reports the relative importance that experts assign to different pieces of evidence to justify increasing job-loss insurance.** Respondents were asked to distribute 100 points between different pieces of evidence to indicate the ones they considered most important to decide whether to increase job-loss insurance. 84 experts responded to this question. The table shows that "sufficient WTP" — i.e., that workers' willingness-to-pay for the additional job-loss insurance exceeds the actuarially-fair cost of providing it — is the most important piece of evidence according to them. *TE* in the title for the other pieces of evidence refers to "Treatment Effect."

Figure C2: Experts' prediction of the fiscal sustainability of increasing job-loss insurance



Notes: **Figure C2 displays experts' predictions about whether workers' willingness-to-pay would be sufficient to cover the actuarially-fair cost of the additional job loss insurance.** 69 experts responded to this question, separately for additional support provided as a lump-sum and for additional support provided in five monthly payments. In both cases, a large majority of experts expect workers' willingness-to-pay for the additional job-loss insurance to exceed the actuarially-fair cost of providing it.

Table C1: Comparing median expert predictions and related empirical results

	Core consumption								Economically active				Migrated			
	(% of non-displaced mean)		(% of control mean)						(percentage points)				(percentage points)			
	mon. 1 to 6		mon. 1 to 2		mon. 4 to 5		mon. 7 to 12		mon. 4 to 5		mon. 7 to 12		mon. 4 to 5		mon. 7 to 12	
	expert	data	expert	data	expert	data	expert	data	expert	data	expert	data	expert	data	expert	data
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Displacement effect	-0.18***	-0.06**														
	(0.04)	(0.02)														
Lump sum			0.25***	0.04**	0.06***	0.02	0.02***	-0.02	0.00	-0.11***	0.00	-0.07***	0.09***	0.02	0.08**	0.03
			(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.01)	(0.03)	(0.01)	(0.03)	(0.04)	(0.03)
Monthly			0.17***	-0.00	0.10***	0.08***	0.05***	0.01	-0.03	-0.07**	0.03**	-0.03	0.05***	0.01	0.05***	-0.03
			(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)	(0.02)

Notes: **Table C1 compare median expert predictions to related estimates of the effects of job loss and experimental payments.** Columns 1 and 2 consider the effects of job loss on core expenditure (in percent of the non-displaced mean) in months 1 to 6 after job loss. Columns 3-8 consider the effects of our experimental payments on core expenditure (in percent of the control mean) in months 1 to 2, in months 4 to 5, and in months 7 to 12, separately. Columns 9 to 12 consider the effects of our experimental payments on a dummy indicating that an individual works as wage employee or self-employed worker (in percentage points) in months 4 to 5 and in months 7 to 12, separately. Columns 13 to 16 consider the effects of our experimental payments on a dummy indicating migration out of Hawassa (in percentage points) in months 4 to 5 and in months 7 to 12, separately. Odd columns display the median predictions of experts surveys before we started presenting our results (standard errors are based on quantile regressions). In total, 102 respondents predict at least one treatment effect and all treatment effect predictions have at least 75 responses. Even columns display the empirical counterparts to the experts' predictions. The regression underlying our estimate in column 2 weight non-displaced observations using weights estimated to balance average pre-layoff values between the displaced control and the non-displaced group. The regressions underlying the estimates in the other even columns include strata fixed effects and controls selected from a large set of baseline variables (see footnote 26) using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Willingness-to-pay for additional job-loss insurance

This section describes how we elicit preferences for payment modalities at baseline and willingness-to-pay for additional job-loss insurance at endline and in a sample of workers in Addis Ababa. It also details how we compute an actuarially fair premium for this additional insurance.

D.1 Baseline elicitation

As part of our baseline survey, we measure individual preferences across payment modalities for workers in the two treatment groups. Specifically, after introducing the experimental payments to these workers, we ask them to choose between the monthly payment treatment and lump-sum payments of varying amounts. The control group does not receive any information about the experimental payments and is not asked to choose between payment modalities.

We start by asking respondents to choose between the 5 monthly payments of 810 ETB and a lump-sum payment of $5 \times 810 = 4050$ ETB. If respondents choose the monthly treatment, we *increase* the lump-sum amount until they choose the lump-sum option or until they hit a maximum of 4860 ETB (the intermediate amounts we ask about are 4250 and 4455 ETB). If respondents choose the lump-sum treatment, we *decrease* the lump-sum amount until they choose the monthly payments or until they hit a minimum of 2430 ETB (the intermediate amounts we ask about are 3850, 3465, 3240, and 2835 ETB).

We construct our measure of willingness-to-pay (WTP) for monthly payments as the lump-sum value at which workers switch from preferring the monthly scheme to preferring the lump-sum payment (i.e., the switch point). We assign the value of 2430 ETB to respondents who never prefer the monthly payments and the value of 5000 ETB to those who never prefer the lump-sum payment. Figure A2 shows the resulting distribution of preferences.

We incentivize this elicitation exercise by randomly selecting five workers for whom we implement one of their choices at random. Workers are informed beforehand that "*We will randomly select some workers in our study and we will implement their choice.*"

D.2 Endline elicitation

At endline, we measure workers' willingness to pay (WTP) for three insurance schemes. The first and second schemes have the same payout amount and structure as the two experimental interventions. The third scheme offers a lump-sum payment scaled by a randomly chosen factor α, which was randomly selected from the set $\{0.25, 0.5, 0.75, 1.25\}$ with equal probability. This enables us to assess how demand varies with the generosity of the insurance payout.

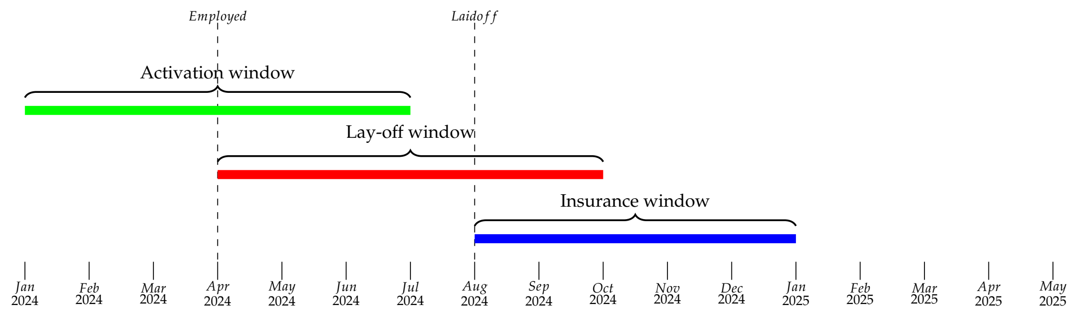
WTP elicitation First, we offer workers a wage subsidy as part of the scheme to ensure that individuals pay for the insurance scheme should they be selected. To elicit WTP for additional job-loss insurance, we then ask a series of binary choices between "Keep the full wage subsidy of 250 ETB per month for up to six months" and "Give up x ETB of the monthly wage subsidy and be eligible for [*scheme*]", where the insurance premium x takes the values of 5, 15, 25, 50, 75, 100, 125, 150, 200, and 250 ETB in ascending order. For the scaled insurance scheme, we scale the values of the insurance premium by the same factor α to keep the payout-to-premium ratios fixed. We ask these questions until a respondent switches to keeping the full wage subsidy.

We construct our measure of WTP for additional job-loss insurance by taking the midpoint between the premium value at which workers switch to keeping the full wage subsidy point and the premium value in the previous question. For example, we assign a WTP of 62.5 ETB to an individual who is willing to purchase the insurance product for 50 ETB but not for 75 ETB. We assign a WTP of 2.5 ETB to workers who are not willing to purchase the insurance product for 5 ETB (the midpoint between 0 ETB and 5 ETB). We assign a WTP of 250 ETB to workers who are willing to pay 250 ETB, which provides a conservative bound on their WTP (our results are insensitive to choosing other reasonable top-coding values).

We incentivize this elicitation exercise by randomly selecting 5 workers to be eligible for each scheme. At the start, when we introduce the wage subsidy, workers are informed that "We will randomly select participants who will receive this wage subsidy." When we introduce the binary choices, we then tell individuals that [if they have been selected for the wage subsidy] "We will select one of these scenarios and we will implement your choice in that scenario for real." Finally, throughout the elicitation, we further remind individuals that "You should think hard about all your choices as they can have real consequences."

Insurance schemes All schemes have the same eligibility conditions but vary in their payout structure. Their common eligibility condition is described in Figure D1. The insurance would become active if individuals started a job with a written contract at any point during a

Figure D1: Timeline for the additional job-loss insurance schemes



Notes: **Figure D1 displays the temporal structure of the job-loss insurance schemes we elicit demand for.**

six-month *activation window*, from January to July 2024. If individuals were already employed with a written contract in January 2024, the insurance would become active in January 2024. The insurance would only cover the first formal job workers started during this activation window. Individuals were told that we would check their employment status with their employer before activating their insurance coverage. The coverage would then be valid for a six-month *layoff window*. During this period, workers would be eligible for additional job-loss payments if they were laid off from their job. While employed, they would also receive the monthly wage subsidy offered as part of the WTP elicitation minus the monthly insurance premium. If they were laid off, they would receive the additional job-loss insurance payments during the *insurance window*. If they were eligible for a lump-sum payment (3850 ETB — the same amount as in our experiment — or a scaled amount), they would receive it in the month after layoff. If they were eligible for monthly payments, they would receive a payment of 810 ETB in each of the five months after layoff, again mimicking our main experiment. Individuals were told that we would check their layoff with their employer before making any job-loss insurance payment.

D.3 Data Quality

Understanding checks We conducted a range of understanding checks to ensure that respondents understood how the wage subsidy and additional job-loss insurance schemes worked. If respondents answered wrongly on one of the checks, enumerators were instructed to re-explain the scheme and give them the opportunity to ask further questions. On average, respondents answer 95% of understanding check questions correctly, indicating that the enumer-

Table D1: Predictive validity of our willingness-to-pay estimates

	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 D1 shows that workers' perceived layoff probabilities are positively correlated with their WTP for additional job-loss insurance while workers' perceived quit probabilities are negatively correlated with their WTP.** Columns 1-3 consider correlations with workers' WTP for the lump-sum scheme. Columns 4-6 consider correlations with the monthly payments scheme. The WTP is measured in ETB per month of formal employment. "Perceived hazard" refers to the monthly hazard rate implied by workers' responses to our subjective probability questions (see text below). The average monthly hazard rate of layoff is 0.062 and the average monthly hazard rate of quit is 0.087. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

ators conveyed the WTP elicitation reliably.

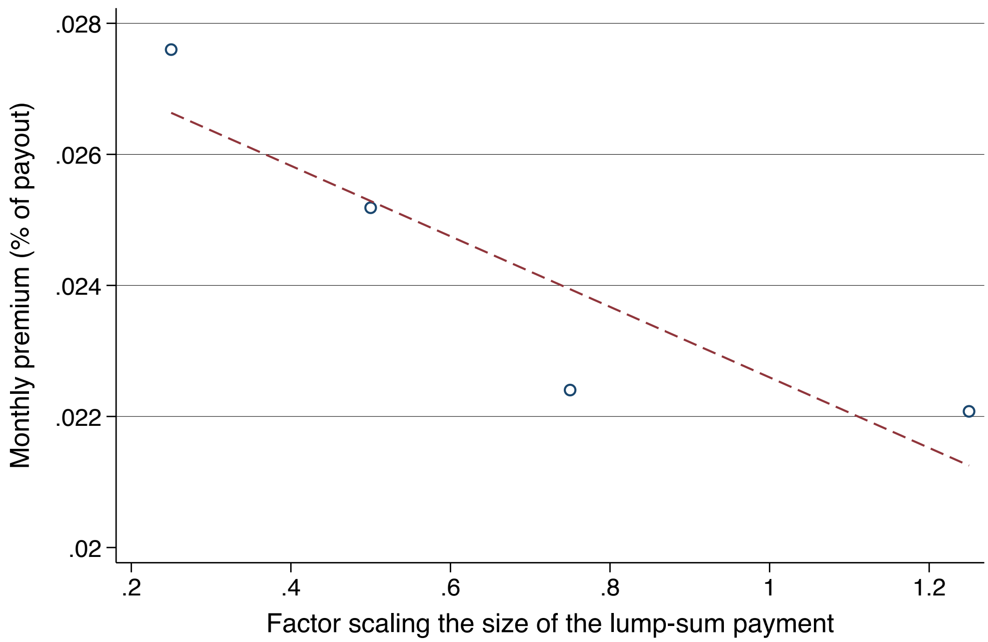
Predictive validity To assess the predictive validity of our measures, we also correlate workers' perceived layoff and quit probabilities with their elicited WTP for the additional job-loss insurance. Specifically, we ask the following two questions before eliciting the WTP:

> *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 from the job until August 2024. What is the chance that you will have* ***quit*** *before the end of July 2024?*

It is notoriously difficult to measure beliefs about such probabilities accurately, and their absolute magnitude may be a fairly noisy indicator of workers' true perceptions of separation risk. However, Table D1 shows that the sign of the correlations between workers' perceived layoff and quit probabilities and their elicited WTP are in line with theoretical predictions. In

Figure D2: Willingness-to-pay for additional job-loss insurance and payout amount at layoff



Notes: **Figure D2 shows that the marginal value of job-loss insurance is decreasing in the size of the payout amount at layoff.** The x-axis indicates the factor α scaling the size of the lump-sum payment for the third scheme we elicited demand for at endline, and the y-axis the maximum premium that workers are willing-to-pay while formally employed as a fraction of the additional financial support at job loss ($\alpha \times$ 3850 ETB; 100 ETB equaled 5.09 USD PPP at the time of the experiment). The slope of the regression line is -0.0054 with standard error 0.0014.

particular, we observe a strong positive correlation between WTP and perceived layoff probability and a strong negative correlation between WTP and perceived quit probability. This suggests that respondents understand the nature of the additional job loss insurance schemes.

Decreasing marginal value of insurance Finally, we also use the third scheme — offering a lump-sum payment scaled by a randomly chose factor $\alpha \in \{0.25, 0.5, 0.75, 1.25\}$ — to assess the internal validity of our WTP estimates. In particular, one would expect the marginal value of insurance — how much workers are willing-to-pay per additional ETB of support at layoff — to be decreasing in the size of the payout amount at layoff. This is what we find empirically: the maximum premium that workers are willing-to-pay, as a fraction of the additional financial support at job loss, is decreasing in the value of α. This is shown in Figure D2.

D.4 Actuarially fair insurance premium

We compute an actuarially fair premium for the additional job-loss insurance schemes we elicit demand for at endline, $\overline{\pi}$, as follows.

Framework. Define p_l and p_q as the monthly hazard rates of layoff and quit for formally employed workers, respectively, and δ as the monthly discount rate of the insurer provider. Given some monthly premium π paid while formally employed, the expected revenue payment (discounted from the point of view of the first month of the eligible 6-month period, $t = 1$) is:

$$E[Revenue] = \pi \times \sum_{t=1}^{t=6} \left[\delta^{t-1} \times (1 - p_l)^{t-1} \times \left(1 - p_q\right)^{t-1} \right] \tag{D1}$$

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

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

$$E[Payout] = p_l \times S \times \sum_{t=1}^{t=6} \left[\delta^{t} \times \left(1 - p_q\right)^{t} \times (1 - p_l)^{t-1} \right] \tag{D2}$$

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 quit events are realized just before layoff events for simplicity. As a result, the likelihood of receiving S in period $t = 2$ is $p_l \times \left(1 - p_q\right)$, in period $t = 3$ is $p_l \times \left(1 - p_q\right)^2 \times (1 - p_l)$, etc.

We can then obtain the actuarially fair premium $\overline{\pi}$ such that $E[Revenue] = E[Payout]$:

$$\overline{\pi} = p_l \times S \times \frac{\sum_{t=1}^{t=6} \left[\delta^{t} \times \left(1 - p_q\right)^{t} \times (1 - p_l)^{t-1} \right]}{\sum_{t=1}^{t=6} \left[\delta^{t-1} \times (1 - p_l)^{t-1} \times \left(1 - p_q\right)^{t-1} \right]} = p_l \times S \times (1 - p_q) \times \delta \tag{D3}$$

Calibrating the hazard rates of layoff and quit. To compute this actuarially fair premium in our setting, we calibrate the monthly hazard rates of layoff and quit, p_l and p_q, in two ways.

First, we rely on existing evidence for formal workers in Ethiopia. Shiferaw and Söderbom (2023) document a 6-month separation rate of 17.95% using matched employee-employer data.

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 et al., 2018). Using the simple framework above, we can recover the monthly hazard of layoff and quit as:

$$p_l^{bench} = .2 \times \left[1 - (1 - .1795)^{1/6}\right] = 0.0065 \tag{D4}$$

$$p_q^{bench} = .8 \times \left[1 - (1 - .1795)^{1/6}\right] = 0.0259 \tag{D5}$$

Second, we conduct a follow-up survey to measure respondents' realized separation rates six to nine months after the endline survey. We sample all individuals who have a formal job at endline. We also include a subsample of those without a formal job at endline, stratified by quartile of the elicited WTP for additional job-loss insurance. We include all individuals in the second ($N = 53$) and fourth quartiles ($N = 46$) and 50 randomly selected individuals in the first and third quartiles. In total, we attempted to contact 939 individuals and reached 880 individuals for a response rate of 93.7 percent. For our analysis, we use 764 individuals who were formally employed at the time of the endline survey or between the endline survey and the follow-up survey. Because individuals have employment spells of different lengths, we estimate monthly hazard rates of layoff and quit by maximum likelihood:

$$\phi_i = \begin{cases} \phi_{i,q}(t) = (1 - p_q)^{t-1}(1 - p_l)^{t-1} p_q, & \text{if } i \text{ quit} \\ \phi_{i,l}(t) = (1 - p_q)^{t}(1 - p_l)^{t-1} p_l, & \text{if } i \text{ was laid off} \\ \phi_{i,0}(t) = (1 - p_q)^{t}(1 - p_l)^{t}, & \text{if } i \text{ neither quit nor left} \end{cases} \tag{D6}$$

where we assume again that, in each period, layoffs happen after potential quits have been realized. The aggregate likelihood of observing the data for N individuals is:

$$\Phi = \Pi_{i=1}^{N} \phi_i \tag{D7}$$

In the maximum likelihood estimation, 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 formal job if it started after the endline survey (as we assume constant

Table D2: Maximum likelihood estimation of monthly hazard rates of layoff and quit

Panel A: Pooled sample		
	Monthly	Six-month
$\widehat{p}_q$	2.06%	11.74%
$\widehat{p}_l$	0.40%	2.36%
Panel B: Sample with jobs at endline		
	Monthly	Six-month
$\widehat{p}_q$	2.05%	11.70%
$\widehat{p}_l$	0.37%	2.20%
Panel C: Sample with jobs found after endline		
	Monthly	Six-month
$\widehat{p}_q$	1.65%	9.49%
$\widehat{p}_l$	0.75%	4.39%

Notes: **Table D2 displays maximum likelihood estimates of the monthly hazard rates of layoff and quit after the endline survey.** It shows results for the full analysis sample (panel A), for the subsample of respondents who were formally employed at endline (panel B), and for the subsample of respondents who were not formally employed at endline but who became formally employed between the endline survey and the follow-up survey.

hazard rates, the estimation is independent of the starting point).

Table D2 presents our estimates for the full analysis sample, for workers who were formally employed at the time of the endline survey, and for workers who later became formally employed, respectively. They are of a similar order of magnitude — although slightly lower — as the above estimates based on existing evidence for formal workers in Ethiopia. Differences in quit rates between the samples are not statistically significant ($p = 0.611$), but the layoff rate is substantially higher in the sample without a formal job at the endline ($p = 0.001$). This suggests that layoff rates decrease with job tenure.

Discount rate In our experiment, we calibrate the monthly payment scheme to have the same net present value as the additional lump-sum payment assuming an expected annual inflation rate of 34%. We use the same inflation rate to impute the monthly discount rate:

$$\delta = \frac{1}{1.34^{1/12}} = 0.976 \tag{D8}$$

Table D3: Actuarially fair premium and demand for additional job-loss insurance

	Calibration			Fraction willing to purchase		
	(1)	(2)	(3)	(4)	(5)	(6)
Data source	p_l	p_q	π^{fair}	Preferred	Lump sum	Monthly
Existing evidence	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 D3 reports the actuarially fair premium for the additional job-loss insurance that we elicit demand for at endline, as implied by each of the two approaches to calibrate hazard rates of layoff and quit (and the discount factor).** Columns 1 and 2 displays the monthly hazard rates of layoff and quit. In the first row, they are calibrated based on *existing evidence* for formal workers in Ethiopia from Shiferaw and Söderbom (2023) and Abebe et al. (2018); in the second row, they are estimated based on *realized separations* recorded six to nine months after the endline survey in our sample (see text above for details). Column 3 displays the resulting actuarially fair premium (given a calibrated monthly discount rate of $\delta = 0.976$) in ETB while employed per ETB of additional support after job loss. Columns 4-6 display the fraction of workers willing to pay this actuarially fair premium based on their WTP at endline, if they can choose the payment modality, if support is provided as a lump-sum payment, or if support is provided in five monthly tranches, respectively.

Results Table D3 reports the actuarially fair premium for the additional job-loss insurance that we elicit demand for at endline, as implied by each of the two approaches to calibrate hazard rates of layoff and quit (and the discount rate). Given the lower hazard rates, the actuarially fair premium is lower if we use realized separation rates ($\overline{\pi} = 0.0037$ ETB while employed per ETB of additional support after job loss) rather than existing evidence for formal workers in Ethiopia ($\overline{\pi} = 0.0061$ ETB). Using the latter value, as we do in the paper, is thus conservative. Because these estimates are substantially lower than the average WTP elicited at endline (see text), Table D3 also shows that most workers would be willing to pay the actuarially fair premium: 82.6% with the lump-sum scheme, 83% with the monthly scheme, and 88.4% with the choice of payment modality (with the premium calibrated using existing evidence).

D.5 WTP among workers in Addis Ababa

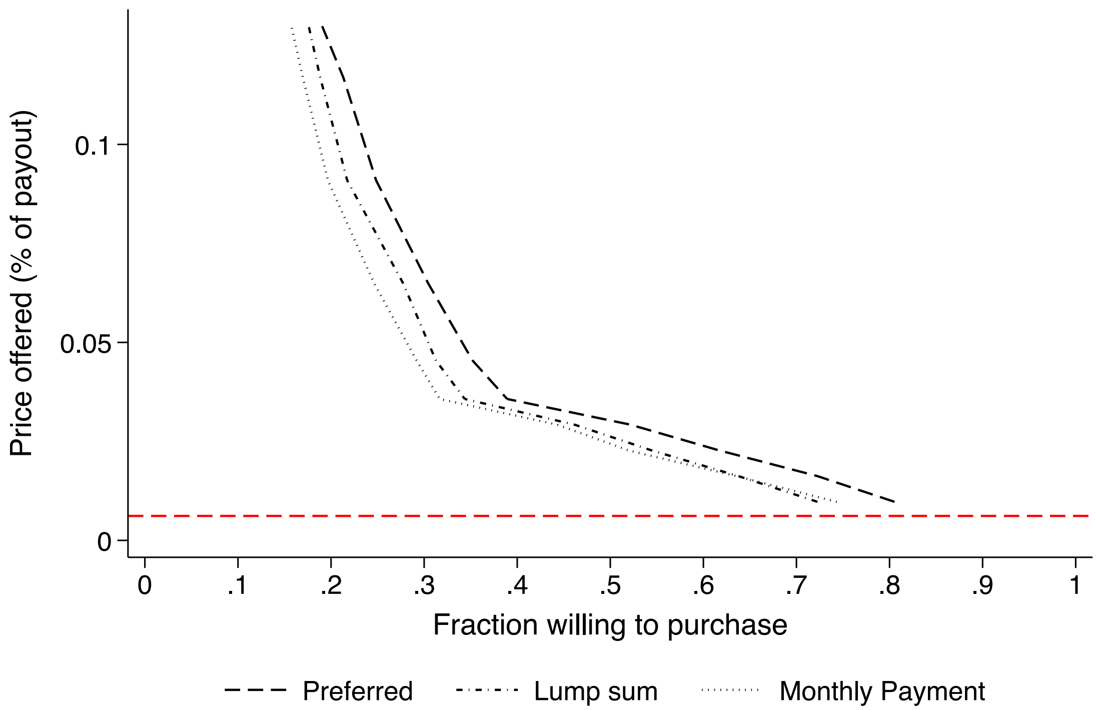
To assess the external validity of the high demand for additional job-loss insurance in our specific subsample, we replicate a similar elicitation exercise in the most important labor market in Ethiopia. Specifically, we recruit a sample of 81 formal employers randomly selected from a registry of formal firms located in Addis Ababa, provided by the Ministry of Trade and Regional Integration. We obtained IRB for this survey from University of Warwick (HSSREC 120/24-25). During the initial outreach, we screen out employers with less than five employees to maximize sample size and focus on formal workers. Within firm, the number of workers we sample depends on firm size: for firms with more than 100 workers, we target 25 worker interviews and require a minimum of 20; for firms with between 30 and 100 workers, we target 15 and require a minimum of 10 worker interviews; for firms with fewer than 30 workers, we target 10 and require a minimum of 5 worker interviews. We randomly sample workers present at the firm on the day of the interview, either through employee lists (stratified by white and blue collar) or, if a list is not available, we randomly approach every n^{th} worker present (where n is chosen to meet the target number of interviews).

Table D4 displays summary statistics for firms and workers in this sample. Sampled firms have, on average, 32 employees (median 10) and are in manufacturing, retail and wholesale, and the hospitality business (Panel A). Workers are 47% female, 34% are married, they are 31 years old and have 0.73 children on average, and 25% were not born in Addis Ababa. 62% have secondary education and 24% have university education. Average monthly earnings are 7591 ETB (median 5500) and 79% have a written contract. On average, they have been with their firm for 50 months (median 24 months).

Figure D3 mirrors Figure 3 in the paper by displaying the incentivized (inverse) demand curve in this sample for the same additional job-loss insurance products we elicited demand for in our endline survey. We find an even higher demand for additional job loss insurance among formal workers in Addis Ababa. Panel D in Table D4 reports average and median willingness-to-pay for each of the three schemes, and column 3 in Table D5 workers' average private surplus using the same actuarially fair premium $\overline{\pi}$ as in the paper (based on existing evidence).[65] Moreover, we find again that demand curves are very similar for the lump-sum and monthly payments schemes, with a clear rightward shift in the demand curve if workers were to choose their preferred payment modality. Our conclusions that workers' average willingness-to-pay and private surplus for additional job-loss insurance is high and that there is significant heterogeneity in workers' preference over payment modalities thus generalize to the most important labor market in the country.

[65] Columns 1 and 2 in Table D5 also extrapolate the findings of our experiment regarding the fiscal and industrial policy externalities for the Addis Ababa sample — as described in Section 4 — to discuss the magnitude of the potential welfare effects of additional job-loss insurance in this sample (in column 4).

Figure D3: Demand for additional job-loss insurance in Addis Ababa



Notes: **Figure D3 displays the incentivized (inverse) demand curve in the sample workers in Addis Ababa for the same additional job-loss insurance products we elicited demand for in our endline survey.** The x-axis displays the fraction of individuals willing to purchase the insurance product at a given price, and the y-axis the maximum premium they are willing-to-pay while formally employed as a fraction of the additional financial support at job loss (3850 ETB; 100 ETB equaled 5.09 USD PPP at the time of the experiment). The three lines describe demand for the insurance product under three payment modalities for the additional support at job loss: lump-sum disbursement (dark-gray short-dashed line), five monthly tranches (light-gray dotted line), and workers' preferred payment modality (black long-dashed line).

Table D4: Summary statistics for the sample of workers in Addis Ababa

	Sample of workers at firms in Addis Ababa		
	mean (1)	median (2)	N (3)
Panel A: Firm characteristics			
Number of employees	32.16	10.00	81
Exporting firm	0.10	0.00	81
Manufacturing	0.48	0.00	81
Retail and wholesale	0.15	0.00	81
Restaurants and hospitality	0.15	0.00	81
Last month turnover (1000 ETB)	3160.37	300.00	78
Last month profit (1000 ETB)	450.52	50.00	77
Value of assets (1000 ETB)	114567.66	5300.00	81
Panel B: Demographics			
Female	0.47	0.00	483
Age	31.12	28.00	483
Married	0.34	0.00	483
# children	0.73	0.00	483
# children in school	0.48	0.00	483
Migrant	0.25	0.00	483
University education	0.24	0.00	483
Secondary education	0.62	1.00	483
Panel C: Workplace variables			
Monthly earnings (ETB)	7591.40	5500.00	483
Written contract	0.79	1.00	483
Factory worker	0.29	0.00	483
Office worker	0.13	0.00	483
Tenure in months	50.06	24.87	483
Job satisfaction (0 to 10)	6.13	6.00	483
Subjective six-months quit probability	22.29	5.00	482
Subjective six-months layoff probability	7.40	0.00	482
Panel D: Displacement insurance preferences			
Prefers monthly payments	0.55	1.00	483
WTP for monthly payment insurance (ETB)	152.90	87.50	483
WTP for lump sum insurance (ETB)	162.16	87.50	483
WTP for preferred insurance (ETB)	180.62	112.50	483

Notes: **Table D4 shows summary statistics for the sample of workers in Addis Ababa.** Panel A shows firm characteristics. Panel B and C show individual and work characteristics, respectively. Panel D shows preferences for the same displacement insurance products offered at endline in the experimental sample.

Table D5: Extrapolating the welfare effect of increasing job displacement insurance in the Addis Ababa sample

		Externalities		Private surplus	Welfare effects
		$FE(x)$ (1)	$IndExt(x)$ (2)	$E[\frac{\pi_i(x)}{\overline{\pi}} - 1]$ (3)	$dW(x)$ (4)
Lump sum payment	κ=0.06*w	-0.441	-0.197	3.750	3.112
	κ=0.25*w	-0.441	-0.821	3.750	2.488
	κ=0.5*w	-0.441	-1.642	3.750	1.668

Notes: **Table D5 extrapolates the welfare effect of increasing job displacement insurance in the sample of workers in Addis Ababa.** The columns capture the same welfare components as columns 1, 2, 4, and 6 in Table 6. Columns 1 and 2 extrapolate the fiscal externality and the industrial policy externality, respectively. To do so, we assume that the Addis Ababa sample would have the same behavioral responses as the Hawassa sample. This leads to larger externalities since wages in Addis Ababa are much higher (we use the median wage of 5500 ETB, implying an average income tax rate of 14.73%) — a conservative assumption since a smaller payout relative to the wage would likely result in a lower behavioral response, and since a large share of the Addis Ababa sample does not work in manufacturing. Column 3 measures the average surplus gain from additional insurance based on the elicitation exercise conducted in the Addis Ababa sample, using the same actuarially fair premium $\overline{\pi}$ as in the paper (based on existing evidence). Column 4 reports the welfare effect from additional insurance combining the estimates in columns 1, 2, and 3. Even with our conservative assumption regarding the fiscal and industrial policy externalities, the welfare effect is positive and large for all three values of κ.

E Pre-analysis plan appendix

This Appendix presents the main results corresponding to the pre-analysis plan registered at https://www.socialscienceregistry.org/trials/10551.

We pre-registered the experimental analysis in two stages. We registered the analysis based on the high-frequency survey data before the start of the experiment. This pre-specification includes ten high-level hypotheses that are all tested in this paper. It also includes 12 primary and 9 secondary outcome families. We then amended the registration before collecting the endline data to narrow the focus of our analysis to three primary outcome families: expenditure, employment status (wage and self-employment), and migration outcomes. We report these outcomes plus labor income and net informal transfers in our main Tables 2 and 4, including for the early periods pre-specified in the initial registration.

We pre-registered using ANCOVA specifications with LASSO controls. However, for our main results in the paper, we use LASSO regressions without forcing baseline values of the outcome to be included because the baseline outcome is not consistently available for all outcomes. This choice also aligns with the spirit of letting the LASSO algorithm choose the variables most predictive of treatment status and the outcome. Additionally, in the paper, we average outcomes over time periods rather than use month-individual level observations. To be transparent, we report pre-specified average treatment effects for all primary outcomes, time periods, and heterogeneity for our primary heterogeneity dimensions in Appendices E.1 and E.2. These are estimated following exactly the pre-specified specifications and outcome definitions.

E.1 Pre-specified treatment effects on primary outcomes

This section presents estimates of treatment effects for all pre-specified primary outcomes. Panel A in all tables reports average treatment effects. Panels B and C report effects by two primary dimensions of heterogeneity, by whether workers have an above median savings stock at baseline and by preference for the monthly payments at baseline, respectively. For this second dimension of heterogeneity, we only estimate differences between the two treatment groups because we do not elicit preferences in the control group. Below, we present heterogeneous results using a dummy for preferring the monthly payments at baseline, as written in our pre-analysis plan. The distribution of the value of the lump-sum payment at which workers switch from preferring the monthly scheme to preferring the lump-sum payment in Figure A2a shows that about half of the sample is approximately indifferent between the two payment modalities, but that a quarter of the sample expresses a clear preference for the monthly payments. Moreover, Figure A2b shows that the main reason for these workers to prefer the monthly scheme is to better "control their spending." In the paper, we thus study heterogeneity with respect to a dummy for having a strong preference for monthly payments, as captured by the quarter of the sample that most strongly prefers monthly payments.

Table E1 reports estimates of treatment effects on planned core expenditure, savings stock, and weekly job search hours, all conditional on not being reemployed. Table E2 reports estimates of treatment effects on core expenditure. Table E3 reports estimates of treatment effects on workers' savings stock. Table E4 reports estimates of treatment effects on the number of job applications sent per month. Table E5 reports estimates of treatment effects on wage employment. Table E6 reports estimates of treatment effects on self-employment. Table E7 reports estimates of treatment effects on a job quality index (conditional on employment). Tables E8-E13 report estimates of treatment effects on each of the components of the job quality index, respectively. Table E14 reports estimates of treatment effects on whether respondents live in an urban area. Table E15 reports estimates of treatment effects on monthly reservation wages, elicited at baseline after the announcement of the treatment. Table E16 reports estimates of treatment effects on a psychological welfare index. Tables E17 and E18 reports estimates of treatment effects on each of the components of the psychological welfare index, respectively. Table E19 reports estimates of treatment effects on an index of individual empowerment. Tables E20-E24 reports estimates of treatment effects on each of the components of the empowerment index, respectively. Table E25 reports estimates of treatment effects on individuals' willingness-to-pay for additional job-loss insurance at endline. All specifications control for strata fixed effects and, where available, baseline values of the outcome. Further controls are selected using post-double LASSO.

Table E1: Impacts on workers' plans conditional on remaining non-employed after layoff

	Planned core expenditure				Planned savings stock				Job search hours			
	(1) mon. 1-2	(2) mon. 3-6	(3) mon. 7-8	(4) mean	(5) mon. 1-2	(6) mon. 3-6	(7) mon. 7-8	(8) mean	(9) mon. 1-2	(10) mon. 3-6	(11) mon. 7-8	(12) mean
Panel A: Main effect												
Lump sum	93.09***	141.78***	109.68***	124.62***	616.06**	1152.17***	580.26***	862.48***	0.48	0.69	0.22	0.47
	(18.72)	(16.85)	(18.38)	(15.40)	(240.57)	(178.01)	(134.80)	(176.03)	(0.63)	(0.58)	(0.58)	(0.56)
Monthly	77.69***	157.65***	153.56***	136.02***	-184.18	760.84***	1001.00***	552.00***	0.60	1.28**	1.17**	1.00*
	(18.46)	(16.97)	(18.54)	(15.52)	(227.62)	(164.70)	(135.79)	(164.97)	(0.63)	(0.56)	(0.57)	(0.55)
Control mean	650.47	542.46	462.84	549.56	4367.42	2623.77	1394.69	2752.41	17.87	16.43	15.57	16.57
p(monthly=lump sum)	0.42	0.34	0.02	0.47	0.00	0.03	0.00	0.08	0.84	0.30	0.09	0.34
Observations	2820	5640	2820	11280	2820	5640	2820	11280	2820	5640	2820	11280
Panel B: Heterogeneity by savings												
Lump sum	18.78	104.43***	81.68***	76.53***	19.51	865.90***	501.03***	539.42**	0.03	0.30	-0.25	-0.06
	(26.87)	(23.84)	(25.97)	(20.42)	(331.83)	(247.26)	(180.17)	(242.00)	(0.84)	(0.81)	(0.85)	(0.78)
Lump sum × Above med. baseline savings	141.58***	76.57**	56.18	95.67***	1078.48**	544.90	173.65	621.85*	0.69	0.70	0.95	0.97
	(37.39)	(33.01)	(36.73)	(30.17)	(476.04)	(352.63)	(264.44)	(347.52)	(1.25)	(1.14)	(1.16)	(1.11)
Monthly	40.77	150.53***	138.93***	116.27***	-195.87	862.66***	1084.66***	608.92**	0.80	1.83**	1.85**	1.54**
	(27.68)	(24.60)	(26.42)	(21.42)	(340.49)	(243.87)	(193.73)	(243.03)	(0.84)	(0.81)	(0.83)	(0.77)
Monthly × Above med. baseline savings	65.21*	16.00	22.59	40.35	-17.60	-168.94	-167.54	-75.29	-0.47	-0.93	-1.18	-0.92
	(36.62)	(33.13)	(36.89)	(30.04)	(466.20)	(336.61)	(272.54)	(336.62)	(1.25)	(1.14)	(1.16)	(1.12)
Effect: Lump sum ∣ Above med. baseline savings	160.36***	181.00***	137.86***	172.19***	1097.98***	1410.80***	674.68***	1161.27***	0.73	1.00	0.69	0.90
Effect: Monthly ∣ Above med. baseline savings	105.99***	166.54***	161.51***	156.62***	-213.47	693.72***	917.12***	533.62**	0.33	0.90	0.66	0.63
Panel C: Preference het. (Δ_i: lump sum - monthly)												
Δ_1 ∣ Prefer monthly	19.48	-26.97	-41.01*	-18.79	750.44**	466.61*	-277.46	351.43	0.59	-0.10	-0.66	-0.10
	(25.71)	(21.82)	(24.56)	(20.30)	(313.69)	(246.59)	(194.36)	(242.48)	(0.83)	(0.75)	(0.74)	(0.73)
Δ_2 ∣ Not prefer monthly	6.08	-4.29	-34.51	-6.15	926.59**	352.11	-564.38**	241.67	-1.31	-1.37	-1.18	-1.10
	(30.03)	(25.51)	(26.88)	(23.41)	(365.71)	(280.69)	(228.30)	(275.75)	(0.99)	(0.88)	(0.89)	(0.87)
$\Delta_1 = \Delta_2$ (p)	0.74	0.50	0.86	0.68	0.72	0.76	0.34	0.77	0.15	0.28	0.65	0.38
Monthly payment mean	741.47	704.11	610.05	689.93	4093.62	3309.21	2307.45	3254.87	18.87	17.75	17.75	16.65
Number of observations	1878	3756	1878	7512	1878	3756	1878	7512	1878	3756	1878	7512

Notes: **Table E1 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on planned core expenditure, savings stock, and weekly job search hours, conditional on not being reemployed after layoff (primary outcomes 1 to 3 in our pre-analysis plan).** At baseline, we only asked about these outcomes for month 1 to 8 after layoff. Columns 1-4 consider planned monthly core expenditure. Columns 5-8 consider planned saving stocks at the end of each month. Columns 9-12 consider planned weekly job search hours in each month. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E2: Impacts on core expenditure

	Core expenditure				
	(1) mon. 1-2	(2) mon. 3-6	(3) mon. 7-10	(4) mon. 14	(5) mean
Panel A: Main effect					
Lump sum	65.79***	5.20	3.46	-33.51	5.51
	(23.54)	(16.53)	(18.51)	(33.85)	(14.94)
Monthly	5.73	33.65**	34.97**	27.04	26.70*
	(22.40)	(15.80)	(17.26)	(33.82)	(14.47)
Control mean	835.40	821.75	838.63	822.38	833.99
p(monthly=lump sum)	0.01	0.09	0.09	0.08	0.16
Observations	2628	5156	4963	1312	18000
Panel B: Heterogeneity by savings					
Lump sum	107.32***	41.88*	22.59	-57.94	23.91
	(33.63)	(24.36)	(26.07)	(49.70)	(21.02)
Lump sum × Above med. baseline savings	-80.97*	-65.00*	-31.98	47.51	-31.79
	(46.96)	(33.40)	(36.98)	(67.49)	(29.75)
Monthly	45.28	31.54	29.48	9.11	25.28
	(32.14)	(22.65)	(24.67)	(48.36)	(20.51)
Monthly × Above med. baseline savings	-73.17	5.45	11.41	36.67	5.07
	(44.57)	(31.56)	(34.45)	(67.74)	(29.08)
Effect: Lump sum | Above med. baseline savings	26.35	-23.13	-9.39	-10.43	-7.88
Effect: Monthly | Above med. baseline savings	-27.89	36.99*	40.89*	45.78	30.35
Panel C: Preference het. (Δ_i: lump sum - monthly)					
Δ_1 | Prefer monthly	97.58***	-19.06	-37.76	-22.84	-8.11
	(31.09)	(22.45)	(24.70)	(46.11)	(19.80)
Δ_2 | Not prefer monthly	12.69	-43.12*	-22.73	-106.03**	-43.60*
	(35.68)	(25.68)	(27.73)	(52.35)	(22.81)
$\Delta_1 = \Delta_2$ (p)	0.07	0.48	0.69	0.24	0.24
Monthly payment mean	815.67	841.06	857.68	860.68	847.75
Number of observations	1766	3468	3331	877	12080

Notes: **Table E2 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on core expenditure (primary outcome 4 in our pre-analysis plan).** Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E3: Impacts on workers' savings stock

	Savings stock						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	547.44***	238.19***	16.80	14.57	23.44	142.83***	158.58***
	(112.17)	(75.35)	(65.03)	(55.96)	(59.85)	(53.52)	(49.02)
Monthly	213.23**	287.58***	122.86*	111.05*	14.89	77.95	125.76***
	(94.41)	(73.35)	(67.57)	(61.37)	(59.76)	(51.01)	(48.20)
Control mean	924.16	562.09	544.33	444.78	412.50	638.93	590.60
p(monthly=lump sum)	0.00	0.55	0.12	0.10	0.89	0.20	0.52
Observations	1314	1332	1246	1200	1317	1400	7809
Panel B: Heterogeneity by savings							
Lump sum	419.48***	208.12**	-126.99	-51.86	-58.41	87.06	82.31
	(156.20)	(100.95)	(90.29)	(59.89)	(80.41)	(74.81)	(65.06)
Lump sum × Above med. baseline savings	181.95	67.07	259.50**	125.14	152.30	104.81	143.71
	(220.93)	(146.60)	(126.55)	(106.16)	(114.19)	(104.59)	(95.24)
Monthly	163.77	349.72***	51.46	136.70*	-0.15	87.80	132.07**
	(127.31)	(106.29)	(96.11)	(77.42)	(86.35)	(72.95)	(66.16)
Monthly × Above med. baseline savings	60.43	-121.77	104.51	-60.83	7.76	-17.26	-12.40
	(187.78)	(147.50)	(134.02)	(120.92)	(118.99)	(100.25)	(95.27)
Effect: Lump sum ∣ Above med. baseline savings	601.43***	275.19**	132.51	73.28	93.89	191.88**	226.02***
Effect: Monthly ∣ Above med. baseline savings	224.20*	227.96**	155.97*	75.87	7.61	70.54	119.67*
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 ∣ Prefer monthly	248.48*	-80.62	-199.94**	-146.96*	-119.98	-14.17	-51.67
	(149.45)	(113.90)	(92.31)	(77.86)	(78.21)	(66.32)	(68.09)
Δ_2 ∣ Not prefer monthly	466.21**	-18.24	35.69	-16.09	191.37**	183.70**	154.04*
	(182.02)	(122.09)	(103.03)	(93.74)	(95.53)	(80.80)	(78.74)
$\Delta_1 = \Delta_2$ (p)	0.36	0.71	0.09	0.29	0.01	0.06	0.05
Monthly payment mean	1155.32	847.03	667.30	566.42	432.35	731.42	735.22
Number of observations	883	891	843	802	884	932	5235

Notes: **Table E3 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on workers' savings stock at the time of data collection (primary outcome 5 in our pre-analysis plan).** Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E4: Impacts on the number of job applications

	Number of job applications				
	(1) mon. 1-2	(2) mon. 3-6	(3) mon. 7-10	(4) mon. 14	(5) mean
Panel A: Main effect					
Lump sum	-0.01	0.14	0.08	-0.01	0.08
	(0.07)	(0.11)	(0.17)	(0.08)	(0.10)
Monthly	-0.03	0.10	0.16	0.15*	0.10
	(0.07)	(0.10)	(0.18)	(0.09)	(0.10)
Control mean	0.50	0.72	0.90	0.54	0.73
p(monthly=lump sum)	0.88	0.76	0.66	0.06	0.88
Observations	2628	5156	4963	1312	15376
Panel B: Heterogeneity by savings					
Lump sum	0.09	0.20	0.18	0.01	0.16
	(0.10)	(0.15)	(0.23)	(0.11)	(0.14)
Lump sum × Above med. baseline savings	-0.22	-0.14	-0.20	-0.03	-0.15
	(0.13)	(0.21)	(0.34)	(0.17)	(0.20)
Monthly	0.06	0.17	0.30	0.19	0.17
	(0.10)	(0.14)	(0.25)	(0.13)	(0.14)
Monthly × Above med. baseline savings	-0.18	-0.15	-0.30	-0.08	-0.16
	(0.14)	(0.21)	(0.36)	(0.18)	(0.21)
Effect: Lump sum ∣ Above med. baseline savings	-0.13	0.07	-0.03	-0.02	0.01
Effect: Monthly ∣ Above med. baseline savings	-0.12	0.03	0.00	0.10	0.01
Panel C: Preference het. (Δ_i: lump sum - monthly)					
Δ_1 ∣ Prefer monthly	0.04	-0.08	-0.03	-0.19*	-0.03
	(0.11)	(0.14)	(0.26)	(0.11)	(0.14)
Δ_2 ∣ Not prefer monthly	0.02	0.19	-0.11	-0.12	0.02
	(0.09)	(0.17)	(0.24)	(0.13)	(0.15)
$\Delta_1 = \Delta_2$ (p)	0.86	0.22	0.83	0.70	0.80
Monthly payment mean	0.49	0.75	0.98	0.62	0.77
Number of observations	1766	3468	3331	877	10326

Notes: **Table E4 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on the number of job applications sent per month (primary outcome 6 in our pre-analysis plan).** Unbounded variables are winsorized at the 99^{th} percentile. All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E5: Impacts on wage employment

	Wage employment				
	(1)	(2)	(3)	(4)	(5)
	mon. 1-2	mon. 3-6	mon. 7-10	mon. 14	mean
Panel A: Main effect					
Lump sum	-0.04**	-0.10***	-0.10***	-0.10***	-0.09***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
Monthly	-0.01	-0.05*	-0.06*	-0.02	-0.04*
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
Control mean	0.17	0.38	0.50	0.56	0.41
p(monthly=lump sum)	0.16	0.03	0.16	0.02	0.02
Observations	2628	5156	4963	1312	18000
Panel B: Heterogeneity by savings					
Lump sum	-0.05*	-0.08**	-0.08*	-0.11**	-0.07**
	(0.03)	(0.04)	(0.04)	(0.05)	(0.03)
Lump sum × Above med. baseline savings	0.02	-0.04	-0.04	0.02	-0.03
	(0.04)	(0.05)	(0.06)	(0.07)	(0.04)
Monthly	-0.02	-0.06	-0.07	-0.05	-0.06*
	(0.03)	(0.04)	(0.04)	(0.05)	(0.03)
Monthly × Above med. baseline savings	0.01	0.02	0.02	0.07	0.03
	(0.04)	(0.05)	(0.06)	(0.07)	(0.04)
Effect: Lump sum | Above med. baseline savings	-0.03	-0.12***	-0.12***	-0.08*	-0.10***
Effect: Monthly | Above med. baseline savings	-0.01	-0.04	-0.04	0.01	-0.03
Panel C: Preference het. (Δ_i: lump sum - monthly)					
Δ_1 | Prefer monthly	-0.03	-0.06	-0.03	-0.06	-0.04
	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)
Δ_2 | Not prefer monthly	-0.03	-0.06	-0.05	-0.09*	-0.06*
	(0.03)	(0.04)	(0.05)	(0.05)	(0.03)
$\Delta_1 = \Delta_2$ (p)	0.91	0.98	0.66	0.71	0.80
Monthly payment mean	0.18	0.38	0.50	0.49	0.41
Number of observations	1766	3468	3331	877	12080

Notes: **Table E5 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on a dummy for being in wage employment (primary outcome 7.1 in our pre-analysis plan).** All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E6: Impacts on self-employment

	Self employment				
	(1) mon. 1-2	(2) mon. 3-6	(3) mon. 7-10	(4) mon. 14	(5) mean
Panel A: Main effect					
Lump sum	0.04**	0.02*	0.02*	0.02	0.02***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Monthly	0.01	0.01	0.02	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Control mean	0.05	0.05	0.05	0.05	0.05
p(monthly=lump sum)	0.17	0.57	0.57	0.32	0.31
Observations	2628	5156	4963	1312	18000
Panel B: Heterogeneity by savings					
Lump sum	0.00	0.01	0.02	0.03	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Lump sum $\times$ Above med. baseline savings	0.06**	0.03	0.01	-0.01	0.02
	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)
Monthly	0.00	0.01	0.01	-0.00	0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Monthly $\times$ Above med. baseline savings	0.03	0.01	0.01	0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Effect: Lump sum $\mid$ Above med. baseline savings	0.06***	0.03**	0.03*	0.02	0.03***
Effect: Monthly $\mid$ Above med. baseline savings	0.03*	0.02	0.02	0.02	0.02*
Panel C: Preference het. (Δ_i: lump sum - monthly)					
Δ_1 $\mid$ Prefer monthly	0.02	0.00	0.00	0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Δ_2 $\mid$ Not prefer monthly	0.02	0.01	0.01	0.00	0.01
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
$\Delta_1 = \Delta_2$ (p)	0.87	0.95	0.90	0.69	0.93
Monthly payment mean	0.05	0.05	0.06	0.06	0.05
Number of observations	1766	3468	3331	877	12080

Notes: **Table E6 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on self employment (primary outcome 7.2 in our pre-analysis plan).** All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E7: Impacts on job quality

	Work quality index						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	-0.09	-0.09	-0.04	-0.07	0.05	0.01	-0.03
	(0.12)	(0.10)	(0.11)	(0.11)	(0.10)	(0.10)	(0.06)
Monthly	0.06	0.04	0.08	0.15	0.11	-0.09	0.06
	(0.12)	(0.10)	(0.09)	(0.11)	(0.09)	(0.10)	(0.06)
Control mean	-0.00	0.00	0.00	0.00	0.00	-0.00	0.00
p(monthly=lump sum)	0.25	0.20	0.26	0.07	0.54	0.36	0.18
Observations	479	616	641	629	710	618	3693
Panel B: Heterogeneity by savings							
Lump sum	-0.16	-0.22	0.02	0.05	0.06	-0.02	-0.01
	(0.18)	(0.15)	(0.14)	(0.15)	(0.14)	(0.14)	(0.09)
Lump sum $\times$ Above med. baseline savings	0.14	0.30	-0.07	-0.24	-0.02	0.05	-0.02
	(0.25)	(0.21)	(0.22)	(0.22)	(0.20)	(0.20)	(0.13)
Monthly	-0.01	-0.05	0.10	0.06	0.23*	-0.09	0.05
	(0.17)	(0.14)	(0.14)	(0.15)	(0.13)	(0.14)	(0.09)
Monthly $\times$ Above med. baseline savings	0.16	0.22	0.01	0.16	-0.26	0.01	0.01
	(0.23)	(0.19)	(0.19)	(0.22)	(0.18)	(0.20)	(0.12)
Effect: Lump sum | Above med. baseline savings	-0.02	0.08	-0.05	-0.20	0.04	0.04	-0.03
Effect: Monthly | Above med. baseline savings	0.15	0.17	0.11	0.22	-0.03	-0.07	0.06
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 | Prefer monthly	-0.26	-0.19	-0.20	-0.33**	-0.22*	0.12	-0.18**
	(0.18)	(0.14)	(0.13)	(0.16)	(0.12)	(0.12)	(0.08)
Δ_2 | Not prefer monthly	-0.13	0.02	0.02	-0.09	0.16	0.08	0.03
	(0.18)	(0.17)	(0.20)	(0.20)	(0.18)	(0.17)	(0.11)
$\Delta_1 = \Delta_2$ (p)	0.61	0.36	0.36	0.33	0.09	0.85	0.12
Monthly payment mean	0.10	0.07	0.12	0.16	0.13	-0.08	0.08
Number of observations	315	376	404	398	454	392	2339

Notes: **Table E7 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on an index of job quality conditional on being in wage employment (primary outcome 8 in our pre-analysis plan).** For individuals without any paid work in the seven days prior to the time of data collection, the index only consists of dummies for having a written contract and having a permanent contract. For individuals with paid work in the seven days prior to the time of data collection, the index also contains monthly earnings, job satisfaction, expected tenure, and a measure of worker surplus. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E8: Impacts on having a written contract

	Has written contract						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	0.01	-0.05	-0.07	-0.01	0.02	-0.05	-0.02
	(0.05)	(0.04)	(0.05)	(0.02)	(0.02)	(0.04)	(0.02)
Monthly	-0.01	-0.01	-0.01	0.01	0.02	-0.07*	-0.01
	(0.05)	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.02)
Control mean	0.77	0.76	0.70	0.96	0.94	0.84	0.83
p(monthly=lump sum)	0.58	0.35	0.20	0.46	0.96	0.60	0.55
Observations	479	616	641	629	710	618	3693
Panel B: Heterogeneity by savings							
Lump sum	-0.03	-0.05	0.01	0.02	0.04	-0.05	0.00
	(0.07)	(0.06)	(0.06)	(0.02)	(0.03)	(0.06)	(0.03)
Lump sum × Above med. baseline savings	0.07	-0.00	-0.16	-0.06	-0.03	0.01	-0.04
	(0.10)	(0.09)	(0.10)	(0.04)	(0.04)	(0.08)	(0.04)
Monthly	-0.07	0.04	-0.02	0.02	0.05*	-0.08	0.01
	(0.06)	(0.06)	(0.06)	(0.03)	(0.03)	(0.06)	(0.03)
Monthly × Above med. baseline savings	0.10	-0.09	0.02	-0.02	-0.07	0.02	-0.02
	(0.09)	(0.08)	(0.09)	(0.04)	(0.04)	(0.08)	(0.04)
Effect: Lump sum ∣ Above med. baseline savings	0.05	-0.05	-0.14**	-0.04	0.01	-0.05	-0.04
Effect: Monthly ∣ Above med. baseline savings	0.04	-0.04	0.01	-0.00	-0.01	-0.06	-0.01
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 ∣ Prefer monthly	-0.01	-0.09	-0.10	-0.01	0.03	-0.00	-0.03
	(0.06)	(0.06)	(0.06)	(0.02)	(0.02)	(0.06)	(0.03)
Δ_2 ∣ Not prefer monthly	0.09	0.04	0.00	-0.03	-0.05	0.05	0.01
	(0.08)	(0.06)	(0.08)	(0.04)	(0.04)	(0.06)	(0.03)
$\Delta_1 = \Delta_2$ (p)	0.33	0.15	0.31	0.65	0.08	0.50	0.32
Monthly payment mean	0.78	0.75	0.70	0.97	0.96	0.77	0.83
Number of observations	315	376	404	398	454	392	2339

Notes: **Table E8 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on a dummy for having a written contract conditional on wage employment (the first index component of primary outcome 8 in our pre-analysis plan).** All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E9: Impacts on having a permanent contract

	Has permanent contract						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	-0.06	-0.11**	-0.08*	-0.01	0.02	-0.00	-0.04
	(0.06)	(0.05)	(0.05)	(0.04)	(0.03)	(0.04)	(0.03)
Monthly	0.01	-0.04	0.03	0.04	0.06*	-0.03	0.02
	(0.05)	(0.04)	(0.05)	(0.04)	(0.03)	(0.04)	(0.03)
Control mean	0.70	0.59	0.54	0.18	0.16	0.79	0.47
p(monthly=lump sum)	0.20	0.17	0.02	0.21	0.30	0.59	0.04
Observations	479	616	641	629	710	618	3693
Panel B: Heterogeneity by savings							
Lump sum	-0.01	-0.13*	-0.05	0.01	0.05	0.02	-0.02
	(0.08)	(0.07)	(0.07)	(0.05)	(0.05)	(0.06)	(0.04)
Lump sum $\times$ Above med. baseline savings	-0.12	0.06	-0.06	-0.03	-0.04	-0.04	-0.02
	(0.11)	(0.10)	(0.10)	(0.08)	(0.07)	(0.08)	(0.06)
Monthly	0.05	-0.10	0.01	0.06	0.11**	0.02	0.03
	(0.07)	(0.07)	(0.07)	(0.06)	(0.05)	(0.06)	(0.04)
Monthly $\times$ Above med. baseline savings	-0.10	0.11	0.05	-0.05	-0.09	-0.09	-0.02
	(0.10)	(0.09)	(0.09)	(0.08)	(0.07)	(0.08)	(0.05)
Effect: Lump sum | Above med. baseline savings	-0.13*	-0.07	-0.11	-0.02	0.01	-0.02	-0.05
Effect: Monthly | Above med. baseline savings	-0.05	0.02	0.06	0.01	0.02	-0.06	0.01
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 | Prefer monthly	-0.11	-0.04	-0.11*	-0.06	-0.05	0.06	-0.05
	(0.07)	(0.07)	(0.07)	(0.05)	(0.05)	(0.06)	(0.04)
Δ_2 | Not prefer monthly	-0.03	-0.09	-0.10	-0.02	-0.02	-0.01	-0.05
	(0.08)	(0.07)	(0.08)	(0.06)	(0.06)	(0.07)	(0.04)
$\Delta_1 = \Delta_2$ (p)	0.47	0.63	0.91	0.62	0.63	0.39	0.94
Monthly payment mean	0.71	0.54	0.57	0.22	0.22	0.75	0.49
Number of observations	315	376	404	398	454	392	2339

Notes: **Table E9 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on a dummy for having a permanent contract conditional on wage employment (the second index component of primary outcome 8 in our pre-analysis plan).** All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E10: Impacts on monthly earnings

	Wage earnings (Birr)						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	-5.78	41.89	-8.25	23.12	-10.10	-45.65	7.08
	(47.47)	(39.61)	(42.70)	(39.77)	(34.73)	(44.80)	(25.14)
Monthly	36.99	52.84	14.69	31.35	-12.60	-52.06	6.11
	(48.28)	(35.90)	(40.59)	(38.90)	(31.46)	(38.71)	(24.46)
Control mean	1432.11	1460.83	1542.70	1571.15	1583.53	1671.62	1548.89
p(monthly=lump sum)	0.39	0.82	0.61	0.85	0.94	0.89	0.97
Observations	479	616	641	629	710	618	3693
Panel B: Heterogeneity by savings							
Lump sum	-28.11	30.86	-37.84	-27.26	-83.67*	-95.04	-20.00
	(68.47)	(60.72)	(55.66)	(50.23)	(44.63)	(64.43)	(31.89)
Lump sum × Above med. baseline savings	49.06	39.52	59.73	101.58	140.97**	92.08	53.82
	(97.72)	(82.09)	(81.79)	(79.37)	(69.08)	(87.16)	(49.26)
Monthly	-5.60	-14.37	41.14	44.60	-17.32	-41.10	6.93
	(65.07)	(44.48)	(59.04)	(53.84)	(47.24)	(57.85)	(32.65)
Monthly × Above med. baseline savings	87.18	137.43**	-52.94	-26.58	1.00	-30.49	-9.42
	(95.07)	(69.10)	(80.46)	(78.99)	(64.29)	(77.13)	(48.22)
Effect: Lump sum | Above med. baseline savings	20.95	70.38	21.89	74.33	57.30	-2.96	33.83
Effect: Monthly | Above med. baseline savings	81.58	123.06**	-11.81	18.02	-16.32	-71.59	-2.49
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 | Prefer monthly	-67.04	-70.06	-126.30**	-132.91**	-110.01**	-34.57	-86.51**
	(63.64)	(67.21)	(60.89)	(58.99)	(46.55)	(61.07)	(36.05)
Δ_2 | Not prefer monthly	-53.87	102.87	118.78*	168.90**	162.44***	89.87	112.22***
	(75.14)	(72.47)	(71.28)	(72.67)	(59.19)	(65.98)	(41.16)
$\Delta_1 = \Delta_2$ (p)	0.89	0.09	0.01	0.00	0.00	0.16	0.00
Monthly payment mean	1457.68	1563.18	1568.14	1596.78	1572.75	1621.70	1567.08
Number of observations	315	376	404	398	454	392	2339

Notes: **Table E10reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on monthly earnings conditional on wage employment (the third index component of primary outcome 8 in our pre-analysis plan).** Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E11: Impacts on job satisfaction

	Job satisfaction (sd)						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	-0.11	-0.12	0.01	-0.25**	-0.14	0.07	-0.04
	(0.12)	(0.10)	(0.10)	(0.11)	(0.09)	(0.10)	(0.07)
Monthly	-0.05	0.01	0.03	-0.11	-0.04	0.05	-0.00
	(0.11)	(0.09)	(0.09)	(0.09)	(0.08)	(0.10)	(0.06)
Control mean	-0.00	0.00	0.00	0.00	-0.00	0.00	0.64
p(monthly=lump sum)	0.60	0.20	0.82	0.22	0.26	0.82	0.59
Observations	479	616	641	629	710	618	4312
Panel B: Heterogeneity by savings							
Lump sum	-0.18	-0.14	-0.01	-0.13	-0.04	-0.03	-0.03
	(0.17)	(0.15)	(0.14)	(0.15)	(0.14)	(0.14)	(0.10)
Lump sum × Above med. baseline savings	0.11	0.04	0.07	-0.19	-0.19	0.19	-0.00
	(0.24)	(0.22)	(0.20)	(0.22)	(0.19)	(0.20)	(0.14)
Monthly	0.03	0.03	-0.10	-0.29**	-0.02	0.07	-0.02
	(0.17)	(0.13)	(0.14)	(0.13)	(0.13)	(0.13)	(0.09)
Monthly × Above med. baseline savings	-0.11	-0.03	0.28	0.38**	-0.07	-0.05	0.04
	(0.22)	(0.18)	(0.18)	(0.19)	(0.17)	(0.19)	(0.12)
Effect: Lump sum | Above med. baseline savings	-0.07	-0.10	0.06	-0.32**	-0.22*	0.16	-0.03
Effect: Monthly | Above med. baseline savings	-0.08	-0.00	0.17	0.09	-0.08	0.02	0.03
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 | Prefer monthly	-0.08	-0.10	-0.08	-0.16	-0.25**	-0.01	-0.09
	(0.16)	(0.14)	(0.13)	(0.15)	(0.12)	(0.13)	(0.09)
Δ_2 | Not prefer monthly	-0.12	-0.11	0.08	0.03	0.16	0.06	0.07
	(0.19)	(0.17)	(0.15)	(0.17)	(0.14)	(0.18)	(0.11)
$\Delta_1 = \Delta_2$ (p)	0.86	0.95	0.40	0.41	0.03	0.74	0.26
Monthly payment mean	-0.05	-0.00	0.06	-0.12	-0.02	0.04	0.64
Number of observations	315	376	404	398	454	392	2735

Notes: **Table E11 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on job satisfaction conditional on wage employment (the fourth index component of primary outcome 8 in our pre-analysis plan).** All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E12: Impacts on expected tenure

	Expected tenure (months)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	HF 1	HF 2	HF 3	HF 4	HF 5	Endline	mean
Panel A: Main effect							
Lump sum	0.29	0.16	0.05	0.60	-0.08	0.82	0.31
	(1.31)	(1.00)	(0.70)	(0.68)	(0.70)	(1.29)	(0.49)
Monthly	2.22*	-0.39	0.54	1.21*	0.75	-0.17	0.68
	(1.26)	(0.85)	(0.67)	(0.66)	(0.69)	(1.08)	(0.47)
Control mean	13.29	15.02	12.95	12.71	13.39	14.90	13.73
p(monthly=lump sum)	0.15	0.57	0.50	0.42	0.23	0.44	0.47
Observations	479	616	641	629	710	618	3693
Panel B: Heterogeneity by savings							
Lump sum	0.49	-1.42	0.36	1.42	0.91	0.73	0.28
	(1.98)	(1.43)	(0.98)	(1.00)	(1.04)	(1.85)	(0.72)
Lump sum × Above med. baseline savings	0.23	2.86	-0.60	-1.82	-1.88	0.25	0.09
	(2.62)	(2.03)	(1.41)	(1.36)	(1.42)	(2.56)	(0.97)
Monthly	2.58	-2.02	0.98	0.18	0.84	-1.16	0.21
	(1.86)	(1.33)	(0.99)	(0.95)	(0.99)	(1.49)	(0.68)
Monthly × Above med. baseline savings	-0.34	2.81	-0.84	1.71	-0.28	1.97	0.85
	(2.53)	(1.77)	(1.38)	(1.35)	(1.44)	(2.19)	(0.97)
Effect: Lump sum | Above med. baseline savings	0.71	1.44	-0.24	-0.40	-0.97	0.98	0.36
Effect: Monthly | Above med. baseline savings	2.24	0.78	0.14	1.89**	0.56	0.81	1.06
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 | Prefer monthly	-1.90	0.58	0.36	-0.69	-0.55	1.55	-0.15
	(1.79)	(1.28)	(0.98)	(1.04)	(0.89)	(1.73)	(0.67)
Δ_2 | Not prefer monthly	-2.23	0.41	-1.53	0.01	-0.99	-0.81	-0.59
	(2.00)	(1.47)	(1.12)	(1.15)	(1.19)	(1.80)	(0.77)
$\Delta_1 = \Delta_2$ (p)	0.90	0.93	0.20	0.65	0.78	0.34	0.67
Monthly payment mean	15.28	14.72	13.74	14.00	14.11	14.99	14.44
Number of observations	315	376	404	398	454	392	2339

Notes: **Table E12 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on workers' expected tenure at their current job conditional on wage employment (the fifth index component of primary outcome 8 in our pre-analysis plan).** Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E13: Impacts on worker surplus

	Worker surplus (Birr)					
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) mean
Panel A: Main effect						
Lump sum	-185.56	630.75	78.91	275.84	-454.82	98.68
	(1031.70)	(1103.62)	(1147.67)	(1243.26)	(1318.57)	(669.74)
Monthly	654.47	1373.25	1860.43*	91.14	-2631.62**	74.19
	(998.00)	(1025.24)	(1122.58)	(1162.84)	(1235.98)	(647.28)
Control mean	14500.00	22417.52	23954.69	26583.33	30549.24	24185.40
p(monthly=lump sum)	0.44	0.52	0.13	0.88	0.08	0.97
Observations	521	668	666	645	730	3230
Panel B: Heterogeneity by savings						
Lump sum	-800.55	-542.47	821.58	-304.84	-1854.55	-415.66
	(1487.30)	(1625.23)	(1588.27)	(1773.49)	(1963.18)	(972.08)
Lump sum × Above med. baseline savings	1352.38	2198.88	-1156.46	1105.84	2826.62	923.61
	(2029.76)	(2250.87)	(2329.37)	(2553.21)	(2730.66)	(1354.40)
Monthly	748.40	11.24	2507.79	650.19	-3892.76**	-426.21
	(1573.27)	(1574.76)	(1767.98)	(1813.77)	(1887.55)	(974.85)
Monthly × Above med. baseline savings	43.26	2341.15	-978.75	-806.76	2450.94	762.73
	(1990.05)	(2100.95)	(2350.29)	(2386.20)	(2474.79)	(1289.10)
Effect: Lump sum ∣ Above med. baseline savings	551.83	1656.40	-334.88	801.00	972.08	507.96
Effect: Monthly ∣ Above med. baseline savings	791.66	2352.39*	1529.04	-156.58	-1441.82	336.52
Panel C: Preference het. (Δ_i: lump sum - monthly)						
Δ_1 ∣ Prefer monthly	-991.25	752.84	-2722.11*	-2308.85	2012.11	-583.55
	(1496.55)	(1588.31)	(1508.66)	(1577.54)	(1595.34)	(869.33)
Δ_2 ∣ Not prefer monthly	-522.62	-2274.94	-593.48	3048.64	1683.11	852.41
	(1829.94)	(1790.54)	(1888.90)	(2031.43)	(2067.76)	(1128.13)
$\Delta_1 = \Delta_2$ (p)	0.85	0.21	0.38	0.04	0.90	0.32
Monthly payment mean	15303.11	23735.42	25796.62	26789.47	27889.53	24279.67
Number of observations	341	418	424	411	466	2060

Notes: **Table E13 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on worker surplus conditional on wage employment (the sixth index component of primary outcome 8 in our pre-analysis plan).** Worker surplus is measured by asking workers about the lump-sum amount that they would accept to leave their current job. The maximum possible value is 50,000 ETB. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E14: Impacts on living in an urban area

	Living in Urban Area				
	(1) mon. 1-2	(2) mon. 3-6	(3) mon. 7-10	(4) mon. 14	(5) mean
Panel A: Main effect					
Lump sum	-0.03	-0.03	-0.02	-0.04	-0.02
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Monthly	0.00	-0.00	0.03	-0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Control mean	0.90	0.84	0.83	0.88	0.84
p(monthly=lump sum)	0.11	0.18	0.06	0.33	0.05
Observations	2628	5156	4963	1312	18000
Panel B: Heterogeneity by savings					
Lump sum	-0.02	0.01	0.04	-0.03	0.01
	(0.02)	(0.03)	(0.03)	(0.04)	(0.02)
Lump sum $\times$ Above med. baseline savings	-0.01	-0.08**	-0.10**	-0.02	-0.07**
	(0.03)	(0.04)	(0.05)	(0.05)	(0.03)
Monthly	-0.00	-0.01	0.03	-0.01	0.01
	(0.02)	(0.03)	(0.03)	(0.04)	(0.02)
Monthly $\times$ Above med. baseline savings	0.01	0.02	-0.01	-0.01	0.00
	(0.03)	(0.04)	(0.04)	(0.05)	(0.03)
Effect: Lump sum $\mid$ Above med. baseline savings	-0.03	-0.07**	-0.06*	-0.05	-0.06**
Effect: Monthly $\mid$ Above med. baseline savings	0.00	0.01	0.02	-0.02	0.01
Panel C: Preference het. (Δ_i: lump sum - monthly)					
Δ_1 $\mid$ Prefer monthly	-0.00	0.01	-0.02	0.03	0.00
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
Δ_2 $\mid$ Not prefer monthly	-0.05**	-0.08**	-0.07**	-0.09**	-0.08***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
$\Delta_1 = \Delta_2$ (p)	0.15	0.03	0.26	0.02	0.02
Monthly payment mean	0.90	0.85	0.85	0.84	0.86
Number of observations	1766	3468	3331	877	12080

Notes: **Table E14 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on a dummy for living in an urban area (primary outcome 9 in our pre-analysis plan).** All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E15: Impacts on reservation wage at baseline

	Reservation wage (Birr)			
	(1) Formal	(2) Formal	(3) Informal	(4) Informal
Panel A: Main effect				
Lump sum	-15.96		10.10	
	(47.37)		(54.72)	
Monthly	26.19		65.57	
	(48.32)		(55.78)	
Treatment pooled		5.96		39.15
		(41.55)		(47.81)
Control mean	2541.61	2541.61	2627.81	2627.81
p(monthly=lump sum)	0.38		0.32	
Observations	1410	1410	1410	1410
Panel B: Heterogeneity by savings				
Lump sum	-45.42		-0.78	
	(73.31)		(85.26)	
Lump sum × Above med. baseline savings	56.78		11.49	
	(95.81)		(110.31)	
Monthly	-32.19		38.98	
	(74.79)		(86.99)	
Monthly × Above med. baseline savings	105.42		41.14	
	(98.27)		(114.64)	
Treatment pooled		-38.62		19.58
		(63.63)		(73.41)
Treatment pooled × Above med. baseline savings		82.96		28.20
		(84.24)		(96.76)
Effect: Lump sum | Above med. baseline savings	11.36		10.71	
Effect: Monthly | Above med. baseline savings	73.23		80.11	
Effect: Pooled | Above med. baseline savings		44.35		47.78
Panel C: Preference het. (Δ_i: lump sum - monthly)				
Δ_1 | Prefer monthly	-26.98		-55.61	
	(66.76)		(78.06)	
Δ_2 | Not prefer monthly	-56.63		-52.50	
	(67.58)		(84.07)	
$\Delta_1 = \Delta_2$ (p)	0.76		0.98	
Monthly payment mean	2603		2739	
Number of observations	939		939	

Notes: **Table E15 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on reservation wages elicited at baseline after the announcement of the treatment (primary outcome 10 in our pre-analysis plan).** Columns 1 and 2 consider formal full-time jobs, and columns 3 and 4 informal full-time jobs. Columns 1 and 3 show estimates of pre-specified treatment effects for each treatment separately, and columns 2 and 4 estimates of pre-specified treatment effects pooling both treatments together. Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications use baseline–individual level observations, and include strata fixed effects and controls selected from a large set of baseline variables using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E16: Impacts on psychological welfare

	Psychological welfare index						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	0.18**	0.02	0.07	0.10	0.10	0.08	0.09**
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.04)
Monthly	0.16**	0.10	0.15**	0.10	0.08	-0.03	0.09**
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.04)
Control mean	-0.00	0.00	0.00	-0.00	0.00	-0.00	0.00
p(monthly=lump sum)	0.85	0.28	0.24	0.97	0.81	0.08	0.90
Observations	1314	1332	1246	1200	1317	1312	7721
Panel B: Heterogeneity by savings							
Lump sum	0.15	-0.13	0.10	0.10	0.01	-0.08	0.02
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.06)
Lump sum $\times$ Above med. baseline savings	0.05	0.29**	-0.05	0.00	0.16	0.32**	0.13*
	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.08)
Monthly	0.07	0.10	0.23**	0.11	0.05	-0.23**	0.05
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.06)
Monthly $\times$ Above med. baseline savings	0.14	-0.00	-0.15	-0.02	0.05	0.37***	0.07
	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.13)	(0.08)
Effect: Lump sum ∣ Above med. baseline savings	0.20**	0.16	0.05	0.11	0.17*	0.24**	0.15***
Effect: Monthly ∣ Above med. baseline savings	0.21**	0.10	0.08	0.09	0.10	0.14	0.12**
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 ∣ Prefer monthly	0.04	0.01	0.01	-0.10	0.04	0.16*	0.03
	(0.09)	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)	(0.05)
Δ_2 ∣ Not prefer monthly	0.01	-0.18*	-0.18*	0.16	-0.01	0.06	-0.02
	(0.10)	(0.11)	(0.11)	(0.11)	(0.10)	(0.10)	(0.06)
$\Delta_1 = \Delta_2$ (p)	0.83	0.17	0.17	0.07	0.71	0.47	0.52
Monthly payment mean	0.15	0.09	0.15	0.09	0.07	-0.05	0.08
Number of observations	883	891	843	802	884	877	5180

Notes: **Table E16 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on an index of psychological welfare (primary outcome 11 in our pre-analysis plan).** The index components are a mental health index and life satisfaction. The mental health index consists of a reversed measure of depression at baseline and in survey rounds 4 and 5. It consists of a reversed measure of anxiety in rounds 1 to 3. At endline, the measure is an index of both measures. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E17: Impacts on mental health

	Mental health index						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	0.12^{*}	0.01	0.03	0.04	0.08	0.10	0.06
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.04)
Monthly	0.10	0.04	0.05	0.01	0.01	-0.03	0.03
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.04)
Control mean	0.00	-0.00	-0.00	0.00	0.00	0.00	0.00
p(monthly=lump sum)	0.78	0.60	0.70	0.59	0.27	0.05	0.45
Observations	1314	1332	1246	1200	1317	1312	7721
Panel B: Heterogeneity by savings							
Lump sum	0.20^{*}	-0.13	-0.05	0.07	0.00	-0.07	0.00
	(0.10)	(0.10)	(0.11)	(0.11)	(0.10)	(0.10)	(0.06)
Lump sum $\times$ Above med. baseline savings	-0.15	0.24^{*}	0.14	-0.05	0.15	0.33^{**}	0.10
	(0.14)	(0.14)	(0.15)	(0.15)	(0.14)	(0.14)	(0.08)
Monthly	0.07	-0.00	0.12	0.01	-0.12	-0.26^{***}	-0.03
	(0.10)	(0.10)	(0.10)	(0.11)	(0.10)	(0.10)	(0.06)
Monthly $\times$ Above med. baseline savings	0.02	0.07	-0.11	0.00	0.23^{*}	0.42^{***}	0.10
	(0.14)	(0.14)	(0.14)	(0.14)	(0.13)	(0.13)	(0.08)
Effect: Lump sum $\mid$ Above med. baseline savings	0.05	0.12	0.09	0.02	0.15	0.26^{***}	0.11^{**}
Effect: Monthly $\mid$ Above med. baseline savings	0.09	0.07	0.01	0.01	0.12	0.16^{*}	0.07
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 $\mid$ Prefer monthly	0.01	0.01	0.04	-0.06	0.10	0.16^{*}	0.05
	(0.09)	(0.09)	(0.10)	(0.10)	(0.09)	(0.09)	(0.05)
Δ_2 $\mid$ Not prefer monthly	0.07	-0.08	-0.09	0.21^{*}	0.03	0.06	0.04
	(0.11)	(0.10)	(0.11)	(0.12)	(0.10)	(0.11)	(0.06)
$\Delta_1 = \Delta_2$ (p)	0.70	0.51	0.40	0.08	0.64	0.48	0.93
Monthly payment mean	0.08	0.05	0.06	-0.01	-0.00	-0.05	0.02
Number of observations	883	891	843	802	884	877	5180

Notes: **Table E17 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on the first index component of primary outcome 11 in our pre-analysis plan.** Positive values indicate better mental health. The mental health index consists of a reversed measure of depression at baseline and in survey rounds 4 and 5. It consists of a reversed measure of anxiety in rounds 1 to 3. At endline, the measure is an index of both measures. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E18: Impacts on life satisfaction

	Life satisfaction (sd)						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	0.15**	0.02	0.08	0.11	0.06	0.04	0.07**
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.04)
Monthly	0.15**	0.09	0.18***	0.14**	0.11*	-0.03	0.10***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.04)
Control mean	0.00	-0.00	0.00	-0.00	-0.00	0.00	-0.00
p(monthly=lump sum)	0.99	0.33	0.15	0.68	0.42	0.34	0.39
Observations	1314	1332	1246	1200	1317	1312	7721
Panel B: Heterogeneity by savings							
Lump sum	0.03	-0.08	0.19*	0.10	-0.02	-0.06	0.02
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.05)
Lump sum × Above med. baseline savings	0.22	0.19	-0.20	0.03	0.13	0.18	0.09
	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.07)
Monthly	0.04	0.14	0.24**	0.14	0.18*	-0.12	0.10*
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.06)
Monthly × Above med. baseline savings	0.19	-0.07	-0.12	-0.01	-0.14	0.18	0.00
	(0.14)	(0.13)	(0.13)	(0.14)	(0.14)	(0.14)	(0.08)
Effect: Lump sum | Above med. baseline savings	0.25***	0.11	-0.01	0.13	0.12	0.12	0.12**
Effect: Monthly | Above med. baseline savings	0.23**	0.07	0.11	0.13	0.05	0.06	0.11**
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 | Prefer monthly	0.05	0.02	-0.01	-0.08	-0.04	0.11	0.01
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.05)
Δ_2 | Not prefer monthly	-0.05	-0.19*	-0.19*	0.05	-0.07	0.01	-0.07
	(0.10)	(0.10)	(0.10)	(0.11)	(0.10)	(0.11)	(0.06)
$\Delta_1 = \Delta_2$ (p)	0.46	0.12	0.19	0.35	0.80	0.49	0.27
Monthly payment mean	0.14	0.09	0.17	0.14	0.10	-0.03	0.10
Number of observations	883	891	843	802	884	877	5180

Notes: **Table E18 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on life satisfaction (the second index component of primary outcome 11 in our pre-analysis plan).** All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E19: Impacts on empowerment

	Empowerment index						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	-0.00	-0.07	-0.08	-0.05	-0.05	0.04	-0.03
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.05)	(0.04)
Monthly	0.07	-0.05	-0.01	0.07	-0.01	0.04	0.02
	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)	(0.05)	(0.04)
Control mean	0.00	-0.00	0.00	0.00	-0.00	0.00	0.00
p(monthly=lump sum)	0.35	0.81	0.34	0.06	0.55	0.91	0.26
Observations	1314	1332	1246	1200	1317	1312	7721
Panel B: Heterogeneity by savings							
Lump sum	0.08	0.09	-0.01	0.09	-0.02	0.01	0.04
	(0.10)	(0.10)	(0.10)	(0.09)	(0.09)	(0.08)	(0.06)
Lump sum $\times$ Above med. baseline savings	-0.14	-0.28*	-0.11	-0.25*	-0.05	0.05	-0.11
	(0.14)	(0.14)	(0.14)	(0.13)	(0.13)	(0.11)	(0.08)
Monthly	0.00	0.03	-0.01	0.19**	0.06	-0.04	0.04
	(0.11)	(0.10)	(0.10)	(0.09)	(0.09)	(0.08)	(0.06)
Monthly $\times$ Above med. baseline savings	0.13	-0.15	-0.02	-0.24*	-0.14	0.13	-0.03
	(0.14)	(0.13)	(0.14)	(0.13)	(0.13)	(0.10)	(0.08)
Effect: Lump sum | Above med. baseline savings	-0.06	-0.19*	-0.12	-0.16*	-0.06	0.06	-0.06
Effect: Monthly | Above med. baseline savings	0.13	-0.12	-0.03	-0.05	-0.08	0.09	0.01
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 | Prefer monthly	-0.14	0.01	-0.12	-0.15*	-0.09	-0.01	-0.08
	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.07)	(0.05)
Δ_2 | Not prefer monthly	0.04	-0.05	0.03	-0.07	0.05	0.04	0.01
	(0.12)	(0.11)	(0.11)	(0.10)	(0.10)	(0.08)	(0.06)
$\Delta_1 = \Delta_2$ (p)	0.23	0.67	0.28	0.54	0.30	0.63	0.28
Monthly payment mean	0.08	-0.05	-0.00	0.07	-0.02	0.00	0.01
Number of observations	883	891	843	802	884	877	5180

Notes: **Table E19 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on an index of individual empowerment (primary outcome 12 in our pre-analysis plan).** The index component 'autonomy from partner' is only included when respondents self-report having a romantic partner. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E20: Impacts on autonomy from parents

	Autonomy from parents index						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	-0.02	-0.05	-0.12*	0.02	-0.01	0.08	-0.02
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.04)
Monthly	-0.01	-0.08	-0.05	0.10	-0.00	0.04	-0.00
	(0.07)	(0.06)	(0.07)	(0.07)	(0.06)	(0.07)	(0.04)
Control mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00
p(monthly=lump sum)	0.89	0.66	0.33	0.23	0.86	0.56	0.68
Observations	1314	1332	1246	1200	1317	1312	7721
Panel B: Heterogeneity by savings							
Lump sum	0.10	0.10	-0.05	0.19*	0.01	0.11	0.07
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.05)
Lump sum × Above med. baseline savings	-0.22	-0.27*	-0.11	-0.33**	-0.03	-0.05	-0.16**
	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.08)
Monthly	-0.02	0.04	-0.04	0.21**	0.01	0.01	0.03
	(0.09)	(0.10)	(0.10)	(0.09)	(0.10)	(0.10)	(0.05)
Monthly × Above med. baseline savings	0.01	-0.20	-0.03	-0.23*	-0.04	0.06	-0.07
	(0.13)	(0.13)	(0.14)	(0.13)	(0.13)	(0.13)	(0.07)
Effect: Lump sum | Above med. baseline savings	-0.12	-0.16*	-0.16*	-0.14	-0.02	0.06	-0.09*
Effect: Monthly | Above med. baseline savings	-0.01	-0.16*	-0.08	-0.02	-0.03	0.07	-0.04
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 | Prefer monthly	-0.13	0.00	-0.09	-0.09	-0.03	-0.03	-0.06
	(0.09)	(0.09)	(0.09)	(0.09)	(0.08)	(0.09)	(0.05)
Δ_2 | Not prefer monthly	0.16	0.07	-0.02	-0.03	0.04	0.15	0.06
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.11)	(0.06)
$\Delta_1 = \Delta_2$ (p)	0.03	0.60	0.58	0.68	0.62	0.20	0.10
Monthly payment mean	-0.02	-0.07	-0.05	0.10	-0.01	0.04	-0.00
Number of observations	883	891	843	802	884	877	5180

Notes: **Table E20 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on an index of respondents' autonomy from their parents (the first index component of primary outcome 12 in our pre-analysis plan).** All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E21: Impacts on autonomy from partner

	Autonomy from partner index						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	-0.15	-0.05	-0.09	-0.06	0.03	0.05	-0.03
	(0.12)	(0.12)	(0.12)	(0.13)	(0.13)	(0.09)	(0.06)
Monthly	-0.01	0.04	-0.00	0.14	0.16	0.11	0.06
	(0.11)	(0.11)	(0.11)	(0.13)	(0.12)	(0.08)	(0.06)
Control mean	-0.00	0.00	-0.00	0.00	0.00	-0.00	0.00
p(monthly=lump sum)	0.25	0.42	0.47	0.14	0.32	0.51	0.09
Observations	433	447	403	324	395	678	2680
Panel B: Heterogeneity by savings							
Lump sum	0.09	0.11	0.04	-0.15	0.01	0.03	0.02
	(0.21)	(0.19)	(0.18)	(0.21)	(0.18)	(0.13)	(0.09)
Lump sum × Above med. baseline savings	-0.45*	-0.33	-0.19	0.15	0.04	0.05	-0.10
	(0.25)	(0.24)	(0.24)	(0.27)	(0.24)	(0.19)	(0.11)
Monthly	0.06	0.18	-0.06	0.24	0.32*	0.15	0.14*
	(0.18)	(0.17)	(0.17)	(0.20)	(0.19)	(0.13)	(0.08)
Monthly × Above med. baseline savings	-0.17	-0.26	0.08	-0.16	-0.25	-0.09	-0.14
	(0.23)	(0.22)	(0.22)	(0.27)	(0.23)	(0.17)	(0.11)
Effect: Lump sum ∣ Above med. baseline savings	-0.36**	-0.21	-0.14	-0.00	0.06	0.08	-0.08
Effect: Monthly ∣ Above med. baseline savings	-0.11	-0.08	0.02	0.08	0.06	0.06	-0.00
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 ∣ Prefer monthly	-0.33**	-0.24	-0.21	-0.29	-0.30	-0.16	-0.24***
	(0.16)	(0.16)	(0.16)	(0.19)	(0.20)	(0.12)	(0.08)
Δ_2 ∣ Not prefer monthly	0.10	-0.01	0.12	-0.16	0.17	0.13	0.08
	(0.18)	(0.20)	(0.16)	(0.23)	(0.19)	(0.14)	(0.08)
$\Delta_1 = \Delta_2$ (p)	0.08	0.37	0.15	0.66	0.09	0.11	0.01
Monthly payment mean	-0.02	0.04	-0.05	0.08	0.17	0.09	0.05
Number of observations	298	294	265	212	255	457	1781

Notes: **Table E21 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on an index of respondents' autonomy from their partner (the second index component of primary outcome 12 in our pre-analysis plan).** The index is only measured when respondents self-report having a romantic partner. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E22: Impacts on number of friends

	Number of friends						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	0.15	-0.09	-0.11	-0.09	-0.08	-0.11	-0.06
	(0.16)	(0.12)	(0.11)	(0.11)	(0.11)	(0.12)	(0.08)
Monthly	0.04	-0.02	-0.03	-0.01	-0.03	-0.04	-0.02
	(0.15)	(0.11)	(0.10)	(0.11)	(0.10)	(0.12)	(0.07)
Control mean	3.97	3.76	3.69	3.50	3.33	3.71	3.66
p(monthly=lump sum)	0.48	0.51	0.47	0.45	0.62	0.60	0.55
Observations	1314	1331	1245	1200	1317	1312	7719
Panel B: Heterogeneity by savings							
Lump sum	0.15	-0.03	0.01	-0.02	-0.03	-0.18	-0.03
	(0.22)	(0.18)	(0.15)	(0.16)	(0.16)	(0.18)	(0.11)
Lump sum $\times$ Above med. baseline savings	0.03	-0.11	-0.19	-0.12	-0.07	0.13	-0.03
	(0.31)	(0.24)	(0.22)	(0.22)	(0.21)	(0.25)	(0.15)
Monthly	-0.02	-0.02	0.02	-0.02	0.10	0.08	0.01
	(0.22)	(0.18)	(0.15)	(0.17)	(0.15)	(0.19)	(0.11)
Monthly $\times$ Above med. baseline savings	0.13	-0.00	-0.10	-0.00	-0.24	-0.23	-0.06
	(0.30)	(0.23)	(0.21)	(0.22)	(0.20)	(0.25)	(0.15)
Effect: Lump sum $\mid$ Above med. baseline savings	0.18	-0.14	-0.19	-0.14	-0.10	-0.04	-0.06
Effect: Monthly $\mid$ Above med. baseline savings	0.10	-0.02	-0.08	-0.02	-0.15	-0.15	-0.04
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 $\mid$ Prefer monthly	0.15	0.11	-0.09	-0.07	-0.05	0.06	0.02
	(0.20)	(0.16)	(0.13)	(0.14)	(0.13)	(0.16)	(0.09)
Δ_2 $\mid$ Not prefer monthly	0.09	-0.25	-0.01	-0.08	-0.05	-0.23	-0.08
	(0.24)	(0.17)	(0.16)	(0.17)	(0.16)	(0.20)	(0.12)
$\Delta_1 = \Delta_2$ (p)	0.85	0.12	0.68	0.94	0.99	0.27	0.50
Monthly payment mean	4.02	3.74	3.66	3.49	3.29	3.66	3.65
Number of observations	883	890	842	802	884	877	5178

Notes: **Table E22 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on the number of friends an individual has (the third index component of primary outcome 12 in our pre-analysis plan).** All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E23: Impacts on number of friends who can be consulted on important decisions

	Number of friends talk to about personal choices						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	0.08	0.02	0.04	0.00	-0.02	0.00	0.03
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.05)	(0.04)
Monthly	0.03	0.07	0.02	0.07	-0.03	-0.01	0.03
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.04)
Control mean	1.81	1.79	1.90	1.89	2.02	1.67	1.85
p(monthly=lump sum)	0.46	0.40	0.74	0.29	0.82	0.72	0.98
Observations	1303	1316	1229	1184	1281	1306	7619
Panel B: Heterogeneity by savings							
Lump sum	0.12	0.10	0.08	0.08	0.02	-0.01	0.06
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.08)	(0.05)
Lump sum $\times$ Above med. baseline savings	-0.06	-0.14	-0.07	-0.13	-0.05	0.02	-0.07
	(0.13)	(0.12)	(0.12)	(0.12)	(0.13)	(0.11)	(0.07)
Monthly	-0.03	0.09	-0.02	0.17*	0.02	0.03	0.04
	(0.09)	(0.08)	(0.09)	(0.09)	(0.10)	(0.08)	(0.05)
Monthly $\times$ Above med. baseline savings	0.09	-0.04	0.06	-0.18	-0.09	-0.07	-0.04
	(0.12)	(0.12)	(0.11)	(0.12)	(0.13)	(0.11)	(0.07)
Effect: Lump sum | Above med. baseline savings	0.05	-0.04	0.01	-0.05	-0.03	0.01	-0.01
Effect: Monthly | Above med. baseline savings	0.06	0.05	0.04	-0.01	-0.08	-0.05	0.00
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 | Prefer monthly	0.02	-0.10	-0.05	-0.06	-0.08	0.06	-0.04
	(0.09)	(0.08)	(0.08)	(0.08)	(0.09)	(0.07)	(0.05)
Δ_2 | Not prefer monthly	0.09	0.00	0.12	-0.08	0.15	-0.04	0.04
	(0.10)	(0.10)	(0.09)	(0.10)	(0.10)	(0.08)	(0.06)
$\Delta_1 = \Delta_2$ (p)	0.56	0.39	0.16	0.92	0.10	0.39	0.31
Monthly payment mean	1.84	1.85	1.93	1.96	1.99	1.66	1.87
Number of observations	875	881	833	792	863	875	5119

Notes: **Table E23 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on the number of friends an individual can discuss important personal decisions with (the fourth index component of primary outcome 12 in our pre-analysis plan).** All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E24: Impacts on number of friends who can provide comfort

	Number of friends to seek comfort						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Lump sum	0.04	-0.00	0.03	-0.02	-0.00	-0.00	0.01
	(0.07)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.03)
Monthly	0.12*	-0.01	0.03	0.06	0.02	-0.05	0.03
	(0.07)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.03)
Control mean	1.56	1.39	1.44	1.55	1.63	1.57	1.52
p(monthly=lump sum)	0.27	0.85	0.94	0.09	0.68	0.34	0.48
Observations	1304	1317	1229	1184	1281	1306	7621
Panel B: Heterogeneity by savings							
Lump sum	-0.02	0.03	0.03	0.02	0.07	0.02	0.02
	(0.09)	(0.06)	(0.06)	(0.06)	(0.07)	(0.08)	(0.04)
Lump sum $\times$ Above med. baseline savings	0.12	-0.06	0.01	-0.06	-0.14	-0.03	-0.02
	(0.13)	(0.08)	(0.09)	(0.09)	(0.10)	(0.11)	(0.05)
Monthly	0.05	0.01	0.03	0.10	0.07	-0.11	0.02
	(0.10)	(0.06)	(0.06)	(0.07)	(0.07)	(0.08)	(0.04)
Monthly $\times$ Above med. baseline savings	0.14	-0.03	0.01	-0.08	-0.11	0.12	0.01
	(0.14)	(0.08)	(0.09)	(0.09)	(0.09)	(0.10)	(0.05)
Effect: Lump sum $\mid$ Above med. baseline savings	0.09	-0.03	0.04	-0.05	-0.06	-0.01	0.01
Effect: Monthly $\mid$ Above med. baseline savings	0.19*	-0.02	0.03	0.02	-0.04	0.00	0.04
Panel C: Preference het. (Δ_i: lump sum - monthly)							
Δ_1 $\mid$ Prefer monthly	-0.09	-0.02	-0.03	-0.08	-0.03	0.11	-0.02
	(0.10)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.04)
Δ_2 $\mid$ Not prefer monthly	-0.07	0.03	0.05	-0.08	0.00	-0.03	-0.02
	(0.11)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.04)
$\Delta_1 = \Delta_2$ (p)	0.91	0.61	0.37	0.97	0.73	0.18	0.88
Monthly payment mean	1.68	1.38	1.48	1.61	1.65	1.52	1.55
Number of observations	876	881	833	792	863	875	5120

Notes: **Table E24 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on the number of friends an individual can turn to for comfort in difficult times (the fifth index component of primary outcome 12 in our pre-analysis plan).** All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E25: Impacts on willingness-to-pay for additional job-loss insurance

	WTP per month (ETB)	
	(1) Lump sum	(2) Monthly
Panel A: Main effect		
Lump sum	7.30	4.96
	(5.13)	(4.97)
Monthly	-4.74	1.02
	(4.84)	(4.84)
Control mean	92.86	91.44
p(monthly=lump sum)	0.02	0.43
Observations	1312	1312
Panel B: Heterogeneity by savings		
Lump sum	4.79	2.46
	(7.46)	(7.17)
Lump sum $\times$ Above med. baseline savings	4.67	4.60
	(10.29)	(9.92)
Monthly	-7.27	-1.26
	(7.27)	(7.24)
Monthly $\times$ Above med. baseline savings	4.60	3.55
	(9.79)	(9.75)
Effect: Lump sum $\mid$ Above med. baseline savings	9.46	7.06
Effect: Monthly $\mid$ Above med. baseline savings	-2.67	2.29
Panel C: Preference het. (Δ_i: lump sum - monthly)		
Δ_1 $\mid$ Prefer monthly	13.52**	3.75
	(6.58)	(6.54)
Δ_2 $\mid$ Not prefer monthly	9.45	4.30
	(7.96)	(7.81)
$\Delta_1 = \Delta_2$ (p)	0.70	0.96
Monthly payment mean	88.07	93.30
Number of observations	877	877

Notes: **Table E25 reports experimental estimates of the effects of the lump-sum and monthly treatments, and estimates of the differential effects by baseline savings stock and preference for the monthly payments, on individuals' willingness-to-pay (WTP) for additional job-loss insurance at endline (primary outcome 13 in our pre-analysis plan).** All estimates are expressed in ETB per month while formally employed (100 ETB equaled 5.09 USD PPP at the time of the experiment). Columns 1 and 2 consider the WTP for additional job-loss insurance providing support in the form of a lump-sum payment and in the form of five monthly payments, respectively. All specifications use endline–individual level observations, and include strata fixed effects and controls selected from a large set of baseline variables using post-double LASSO. Robust standard errors are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.2 Pre-specified treatment effects pooling the two treatments together

This section presents estimates of pre-specified treatment effects on primary outcomes pooling the two treatments together. Panel A in all tables reports average treatment effects. Panel B reports effects by a primary dimension of heterogeneity, i.e., whether workers have an above median savings stock at baseline. As we do not measure preferences for monthly payments in the control group, we cannot estimate the effects of pooling the two treatments together along this other primary dimension of heterogeneity.

Table E26 reports estimates of pooled treatment effects on core expenditure. Table E27 reports estimates of pooled treatment effects on wage employment. Table E28 reports estimates of pooled treatment effects on self-employment. Table E29 reports estimates of pooled treatment effects on the number of job applications sent per month. Table E30 reports estimates of pooled treatment effects on psychological welfare. Pooled treatment effects for reservation wages at baseline are reported in Columns 2 and 4 of Table E15. All specifications control for strata fixed effects and, where available, baseline values of the outcome. Further controls are selected using post-double LASSO.

Table E26: Pooled treatment effects on core expenditure

	Core expenditure				
	(1) mon. 1-2	(2) mon. 3-6	(3) mon. 7-10	(4) mon. 14	(5) mean
Panel A: Main effect					
Pooled treatment	34.00*	20.18	20.23	-1.78	16.69
	(19.73)	(13.81)	(15.30)	(29.14)	(12.67)
Control mean	835.40	821.75	838.63	835.61	833.99
Observations	2628	5156	4963	1312	18000
Panel B: Heterogeneity by savings					
Pooled treatment	75.51***	36.63*	26.12	-24.11	24.63
	(28.27)	(20.24)	(21.67)	(41.76)	(17.80)
Pooled treatment × Above med. baseline savings	-78.79**	-27.69	-8.06	44.14	-11.78
	(39.31)	(27.83)	(30.59)	(57.98)	(25.34)
Effect: Pooled treatment ∣ Above med. baseline savings	-3.28	8.94	18.06	20.03	12.86

Notes: **Table E26 reports experimental estimates of the effects of job-loss payments at layoff pooling the two treatments together, and estimates of the differential effects by baseline savings stock, on core expenditure.** Unbounded variables are winsorized at the 99^{th} percentile. 100 ETB equaled 5.09 USD PPP at the time of the experiment. All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E27: Pooled treatment effects on wage employment

	Wage employment				
	(1) mon. 1-2	(2) mon. 3-6	(3) mon. 7-10	(4) mon. 14	(5) mean
Panel A: Main effect					
Pooled treatment	-0.03	-0.07***	-0.07***	-0.06**	-0.06***
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)
Control mean	0.17	0.38	0.50	0.47	0.41
Observations	2628	5156	4963	1312	18000
Panel B: Heterogeneity by savings					
Pooled treatment	-0.04	-0.07**	-0.07*	-0.08*	-0.06**
	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)
Pooled treatment × Above med. baseline savings	0.02	-0.01	-0.00	0.05	0.00
	(0.04)	(0.05)	(0.05)	(0.06)	(0.04)
Effect: Pooled treatment ∣ Above med. baseline savings	-0.02	-0.08**	-0.08**	-0.03	-0.06**

Notes: **Table E27 reports experimental estimates of the effects of job-loss payments at layoff pooling the two treatments together, and estimates of the differential effects by baseline savings stock, on a dummy for being in wage employment.** All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E28: Pooled treatment effects on self-employment

	Self employment				
	(1) mon. 1-2	(2) mon. 3-6	(3) mon. 7-10	(4) mon. 14	(5) mean
Panel A: Main effect					
Pooled treatment	0.02**	0.02*	0.02**	0.02	0.02**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Control mean	0.05	0.05	0.05	0.06	0.05
Observations	2628	5156	4963	1312	18000
Panel B: Heterogeneity by savings					
Pooled treatment	0.00	0.01	0.01	0.01	0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
Pooled treatment × Above med. baseline savings	0.04*	0.02	0.01	0.00	0.02
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Effect: Pooled treatment ∣ Above med. baseline savings	0.04***	0.02*	0.03*	0.02	0.03***

Notes: **Table E28 reports experimental estimates of the effects of job-loss payments at layoff pooling the two treatments together, and estimates of the differential effects by baseline savings stock, on a dummy for being self-employed.** All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E29: Pooled treatment effects on the number of job applications

	Number of job applications				
	(1) mon. 1-2	(2) mon. 3-6	(3) mon. 7-10	(4) mon. 14	(5) mean
Panel A: Main effect					
Pooled treatment	-0.02	0.12	0.12	0.08	0.09
	(0.06)	(0.09)	(0.15)	(0.07)	(0.09)
Control mean	0.50	0.72	0.90	0.52	0.73
Observations	2628	5156	4963	1312	15376
Panel B: Heterogeneity by savings					
Pooled treatment	0.08	0.18	0.24	0.11	0.16
	(0.08)	(0.12)	(0.21)	(0.10)	(0.12)
Pooled treatment × Above med. baseline savings	-0.20*	-0.14	-0.25	-0.06	-0.15
	(0.11)	(0.18)	(0.31)	(0.15)	(0.17)
Effect: Pooled treatment ∣ Above med. baseline savings	-0.12	0.05	-0.01	0.05	0.01

Notes: **Table E29 reports experimental estimates of the effects of job-loss payments at layoff pooling the two treatments together, and estimates of the differential effects by baseline savings stock, on the number of job applications sent per month.** Unbounded variables are winsorized at the 99^{th} percentile. All specifications use month-individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E30: Pooled treatment effects on psychological welfare

	Psychological welfare index						
	(1) HF 1	(2) HF 2	(3) HF 3	(4) HF 4	(5) HF 5	(6) Endline	(7) mean
Panel A: Main effect							
Pooled treatment	0.16***	0.06	0.11*	0.11*	0.09	0.02	0.09***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.03)
Control mean	0.10	0.04	0.08	0.06	0.05	0.01	0.06
Observations	1314	1332	1246	1200	1317	1312	7721
Panel B: Heterogeneity by savings							
Pooled treatment	0.11	-0.02	0.17*	0.11	0.03	-0.16*	0.03
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.05)
Pooled treatment × Above med. baseline savings	0.10	0.14	-0.10	0.00	0.10	0.34***	0.10
	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.07)
Effect: Pooled treatment ∣ Above med. baseline savings	0.21***	0.12	0.07	0.11	0.13	0.18**	0.13***

Notes: **Table E30 reports experimental estimates of the effects of job-loss payments at layoff pooling the two treatments together, and estimates of the differential effects by baseline savings stock, on an index of psychological welfare.** The index components are a mental health index and life satisfaction. The mental health index consists of a reversed measure of depression at baseline and in survey rounds 4 and 5. It consists of a reversed measure of anxiety in rounds 1 to 3. At endline, the measure is an index of both measures. All specifications use survey-round–individual level observations, include strata fixed effects and, where available, control for baseline values of the outcome. Further controls are selected from a large set of baseline variables using post-double LASSO. Standard errors clustered at the individual level are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.