In Search of a Better Life: Self-Control in the Ethiopian Labor Market*

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Abstract

This paper investigates whether present bias correlates with savings and job search behavior in a population of low-skill workers in Ethiopia. I conduct a field experiment with 460 women who begin employment in the ready-made garment industry. Most are rural-urban migrants without work experience for whom the job represents a stepping stone into the labor market. Almost all workers plan to use their jobs to save money and to look for higher-wage employment, but many fall short of their intentions. I propose self-control problems as a candidate explanation. I elicit a measure of present bias in a tightly-controlled experiment and match results to high-frequency survey data that I collect over a period of three months. Present bias is a significant predictor of job search effort, controlling for liquidity and a broad range of covariates. Present-biased workers spend 57 percent less time on job search per week. As a result of reduced search, present-biased workers generate fewer offers and stay in their jobs significantly longer. In contrast, I find no significant correlation between present bias and savings behavior. I discuss implications for the design of commitment devices in this context.

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1. Introduction

Many choices in our lives involve costs and benefits spread out over time. Such choices often suffer from an apparent inconsistency: when we plan for tomorrow, we may decide to save money and search for better jobs. But when tomorrow arrives, we may instead want to spend our money and slack off on our job search. This preference for immediate gratification – present bias – is one of the most robust "anomalies" of intertemporal choice (DellaVigna, 2009; Frederick, Loewenstein, & O'Donoghue, 2002; Loewenstein & Prelec, 1992). This anomaly can be costly. The poor in particular, with less scope to absorb errors, may suffer from not following through on their own plans.

Prominent models that rationalize such self-control problems (Laibson, 1997; O'Donoghue & Rabin, 1999) provide two key predictions.[1] First, individuals with self-control problems have characteristic consumption patterns. They consume too little of a good with immediate costs and future rewards (such as saving money or searching for a job) and too much of a good with immediate rewards and future costs (such as spending money on consumption or enjoying leisure time). Second, individuals who are aware of their self-control problems value commitment. They want to improve their welfare by tying their hands. While the literature has established this demand for commitment in many domains (Ashraf, Karlan, & Yin, 2006; DellaVigna & Malmendier, 2006; Duflo, Kremer, & Robinson, 2011; Dupas & Robinson, 2013; Giné, Karlan, & Zinman, 2010; Kaur, Kremer, & Mullainathan, 2015; Thaler & Benartzi, 2004), evidence on the hypothesized link between self-control problems and the consumption patterns described above is relatively scarce (Castillo, Ferraro, Jordan, & Petrie, 2011; Falk et al., 2018; Meier & Sprenger, 2010). This paper provides an empirical test of the link between self-control problems and behavior. I conduct this test in an environment where failure to follow through can have significant negative consequences.

I use an experiment and high-frequency survey data from 460 women in Ethiopia's ready-made garment (RMG) industry to investigate the correlation between present bias and subsequent choices over savings and job search. I work with an RMG firm in peri-urban Addis Ababa that hired a large number of all-female workers during the study period in the spring of 2018. Workers start homogeneous production jobs (such as sewing t-shirts) without appreciable skill requirements, but with steady hours and the same low wage approximately equal to the local poverty line. Consistent with a narrative of low-skill industrial jobs acting as a safety net (Blattman & Dercon, 2018), workers use the jobs as a stepping stone to a better future in two ways. First, by accumulating assets and then leaving the job, for example to start a small business or engage in off-the-job search. Second, by financing continued on-the-job search for

[1] Alternative models that can rationalize self-control problems include dual-self models by Thaler and Shefrin (1981) and Fudenberg and Levine (2006) or models that focus on temptation by Banerjee and Mullainathan (2010) and Gul and Pesendorfer (2001). While predictions generated for example by Fudenberg and Levine (2006) are similar to the ones presented, I focus on quasi-hyperbolic models for ease of exposition.

better opportunities. Accordingly, savings and job search are the two intertemporal decisions that I study. I collect data on these and a broad range of other covariates using in-person interviews and phone surveys over three months after workers join the firm. The survey is designed to track workers as they leave their jobs, which many do. I correlate this survey data with structural estimates of present bias, which I obtain from a tightly-controlled experiment that I conduct on the day that workers start their new job.

Three features make this an ideal setting to study the effects of self-control problems. First, intentions to save money and to look for work are pervasive. All but one worker in my sample want to save considerable amounts of money, 47 percent in order to build assets to start their own business. I elicit workers' predictions of their monthly savings on the day they start their job. Workers on average expect that they can realistically save one-third of their wage, but most fall significantly short of their goal. This finding is consistent with workers overestimating either their future self control or their future efficiency in saving money (Acland & Levy, 2015; DellaVigna & Malmendier, 2006). On the day they start their new job, 20 percent of workers are still actively looking for other jobs and another 31 percent would like to search but find it too difficult, costly, or time-consuming. The fractions of those who search and those who would have liked to search in any given week increase over the first three months of employment. While various factors can explain intention-behavior gaps in both domains, the evidence is consistent with self-control problems affecting the ability of workers to follow through on their intertemporal plans.

Second, self-control problems can be consequential. Saving money and searching for other jobs are the only ways workers can meaningfully increase future consumption opportunities while in their jobs. This is because wages at the firm are not only low in absolute terms, but also do not increase significantly with individual performance or tenure at the firm. In addition, workers in my sample are poor in absolute terms and relative to their peers in the same age group in Addis Ababa. This means they have little slack income to absorb the potential costs of self-control problems.

Third, the setting allows for a clean experimental design. Enrolling workers into the study as they start their new job provides a relatively homogeneous sample and a clear starting point to study choices over time in a natural environment.

The analysis in this paper relies on the measurement of potential self-control problems for each worker in my sample. I use a version of the convex time budget (CTB) task (Andreoni & Sprenger, 2012). Each worker makes 15 allocations of a large experimental budget (20 to 40 percent of the monthly wage) over two points in time. By experimentally varying the timing of the payments and the implied interest rate between both payments I can recover individual-level measures of present bias along with other parameters of each worker's utility function. To implement the task in my setting I closely follow Giné, Goldberg, Silverman, and Yang (2017). Workers make their decisions by dividing a number of beans between two empty dishes that represent the two payoffs. Each of the two dishes is positioned below a small whiteboard that indicates the exact payoff date and the exchange rate at which beans are converted into the local currency.

The CTB method aims to address methodological problems of multiple price list (MPL) approaches that have been widely used in the literature. MPL approaches often assume linear utility, which may lead to biased inference when utility is in fact concave (Andersen, Harrison, Lau, & Rutström, 2008).[2] Previous work that correlates experimental estimates of present bias with actual consumption patterns has relied on the MPL method (Castillo et al., 2011; Meier & Sprenger, 2010), possibly because it is easier to implement in the field. Irrespective of whether time preferences are elicited with MPL or CTB, a number of other confounds may undermine identification of present bias from time-dated payments. Importantly, subjects may exhibit a preference for earlier payments because it is more costly to obtain the later payment or because there is uncertainty over whether the experimenter will deliver the payment as promised. Several recent studies that carefully equalize transaction costs between time-dated payments find little evidence of aggregate present bias (Andersen et al., 2008; Andreoni & Sprenger, 2012; Augenblick, Niederle, & Sprenger, 2015; Giné et al., 2017). I take several steps to address this and other potential confounds commonly found in the literature. One such step is to use Ethiopia's mobile money system for costless and precisely-timed experimental payments (Balakrishnan, Haushofer, & Jakiela, 2017). Measures of how well workers understand the experiment, individual-level estimates of present bias, and aggregate-level estimates of present bias are in line with other recent implementations of the CTB method. I find that 38 percent of the workers in my sample can be categorized as present-biased when they start their new job.

To guide the empirical analysis, I present a simple model that interprets employment at the industrial firm as akin to the safety net of a welfare system. Involuntary transitions are ruled out and workers are employed at their reservation wage.[3] Search increases the probability of receiving a better wage offer. Workers who expect to leave the firm within a fixed amount of time have an additional precautionary savings motive to smooth consumption.[4] Both search and savings thus represent workers' self-insurance efforts. To formalize self-control problems

[2]Consider utility from consumption $u(c_t)$ at an initial time period t and after a delay of k periods. The implied discount factor between utility in both periods can be calculated as $\delta_u \approx \left[u\left(c_t\right) / u\left(c_{t+k}\right)\right]^{1/k}$. MPL approaches typically infer discount factors in terms of time-dated consumption, not time-dated utility, so that $\delta_c \approx \left[c_t / c_{t+k}\right]^{1/k}$ and it is explicitly or implicitly assumed that $u(c_t) = c_t$. If utility is concave, as it is in Holt and Laury (2002) and Andersen et al. (2008), we will have $\delta_c < \delta_u$ and the implied discount will be biased upward.

[3]In my data, 90 percent of transitions over the first four months of employment are voluntary. The wage at the firm, which is the same for all workers in my sample, is approximately equal to the poverty line. Given that workers can likely not fulfill minimum nutrition requirements below this wage level, it is improbable that the wage at the firm is significantly above the reservation wage of workers.

[4]On the day that they start their new job, 25 percent of workers in my sample report that they plan to leave within a fixed amount of time. The median expected tenure of these workers is 12 months. After three months at the firm, 45 percent of workers report that they plan to leave within a fixed amount of time.

I assume quasi-hyperbolic (β-δ) preferences developed by Laibson (1997) and O'Donoghue and Rabin (1999), who build on earlier work by Strotz (1955) and Phelps and Pollak (1968). In the standard exponential discounted utility framework (Samuelson, 1937), every future period is discounted by a constant discount factor δ. In the (β-δ) framework, an additional present bias parameter β allows for higher discounting between the current and the next period. In the model I assume that if workers are present-biased, they are not aware of it (*naïve*). Every period the worker thus assumes that her future self will not have self-control problems.

The model shows how present bias undermines self-insurance through job search and savings in intuitive ways. First, an increase in present bias reduces savings and thus the ability of workers to smooth consumption after leaving the firm. Second, an increase in present bias reduces the present value of search (DellaVigna & Paserman, 2005). As a corollary, present-biased workers stay longer at the firm. This illustrates how workers may experience a type of "behavioral job-lock," where voluntary turnover is reduced due to self-control problems.[5]

I provide reduced-form evidence on the predictions of the model. My first set of empirical findings considers the relationship of present bias and savings over the three months after joining the firm. I do not find that baseline present bias is a statistically significant predictor of subsequent savings. My preferred specification, which controls for a broad range of covariates, finds that present-biased workers do save marginally less than workers who are not categorized as present-biased. This difference is, however, not significant at any conventional level. This holds for both savings in absolute terms and savings relative to self-set goals set when joining the firm.

This finding is consistent with the literature to the extent that there is little existing evidence on the relationship between experimentally-elicited measures of present bias and consumption behavior in line with predictions of the quasi-hyperbolic model. An important exception is the work by Meier and Sprenger (2010), who use the MPL method to show that present-biased individuals are more likely to have credit card debt.[6]

My second set of results considers the relationship of present bias and job search over the three months after joining the firm. I find that baseline present bias is an economically and statistically significant predictor of subsequent job search effort. In my preferred specification that controls for a broad range of covariates, present-biased workers spend on average 57 percent less time on job search (37 minutes per week for present-biased workers compared to 85 minutes for those who are not present-biased). Present-biased workers also place fewer than half as many phone calls in search for a new job (0.3 per week for present-biased workers

[5] The concept of "job-lock" is typically associated with the finding that employer-provided health insurance plays an important role in job mobility decisions (Madrian, 1994).

[6] Falk et al. (2018) use a hypothetical survey measure to provide global evidence of a link between patience and savings. Ashraf et al. (2006) elicit present bias using hypothetical choices, but do not assess the link with borrowing or savings. Karlan, Ratan, and Zinman (2014) review the literature and conclude that there is a "striking lack of empirical evidence" on correlations between present-bias and under-saving (p. 59).

compared to 0.7 those not present-biased). While my analysis focuses on search intensity, the results also hold on the extensive margin.

As an immediate consequence of less search, present-biased workers stay at the firm significantly longer. Controlling for the same broad set of covariates as before, the hazard of leaving the firm is 52 percent as high for present-biased individuals as it is for individuals who are not present-biased. This effect appears to operate through search effort. Search effort significantly increases the hazard of leaving the firm. When I include both present bias and search effort as predictors in a hazard model, baseline present bias loses its predictive power. Results hold when I restrict my analysis to voluntary departures from the firm and when using data on tenure from firm personnel records.[7] To further confirm the mechanism, I consider data on job search outcomes. Baseline present bias is associated with significantly fewer job offers.

Because the evidence presented is correlational, I consider potential confounders and alternative explanations for my findings. First, I show that individual observable characteristics and environmental factors do not predict experimental responses. Second, I provide evidence that individual financial wealth and liquidity are unlikely to explain my results. Randomized cash drops at baseline do not significantly affect responses in an additional convex time budget experiment. In addition, I use randomized cash drops to show that – consistent with existing theoretical and empirical findings – more liquidity causes less search. If individual liquidity constraints had caused subjects to both appear present-biased and to search less, we would expect that alleviating these constraints should lead to more search, not less. Third, I use detailed survey data on work experience, cognitive control, and non-cognitive skills to show that human capital is unlikely to be an alternative explanation. Fourth, I demonstrate the limited role of reservation wages in my setting. Fifth, I argue that it is improbable that workers who are categorized as present-biased systematically under-report search effort. While it is still possible that my experimental results reflect variation in unobserved variables that affect job search effort, I argue that self-control problems due to present bias offer the most parsimonious explanation for my results.

Taken together, this set of results provides the first experimental evidence of the theorized link between present bias and job search effort. DellaVigna and Paserman (2005) formalize how time preferences can affect job search behavior. They test their model in two large panel datasets from the United States and find a negative correlation between proxies for impatience and search effort during unemployment. In absence of experimentally-elicited measures of present bias, they proxy impatience with behavior such as health habits, use of contraceptives, and financial decisions. As they note, these indirect measures may pick up unobserved individual traits and preferences. My data allows for a more direct test. With these results my

[7] While the analysis of this paper focuses on self-reported data, I also collect rich administrative data from firm personnel records. Workers truthfully report tenure, so the significant correlation between baseline present bias and the hazard of leaving the firm holds.

paper contributes to a growing literature that has introduced insights from behavioral economics into models of job search (DellaVigna, Lindner, Reizer, & Schmieder, 2017; Paserman, 2008; Spinnewijn, 2015).

An immediate implication of my findings is that individuals looking for work might benefit from policies or devices that commit their future selves to more search. Whether and under what conditions such a commitment device can be welfare-improving depends on the exact welfare criterion, which is not obvious to define when we observe two individual choices that are in conflict with each other.[8] I discuss implications for the design of commitment devices in a conclusion.

More broadly the paper relates to a literature that studies the effects of low-skill industrial jobs, particularly in the RMG industry, on its workers (Blattman & Dercon, 2018; Heath & Mobarak, 2014). Most workers in my sample are recent rural-urban migrants for whom employment at the study firm represents the first formal work experience. My results suggest that present bias may undermine the ability of workers to use these jobs as a stepping stone into the formal labor market of Addis Ababa. This finding complements work by Atkin (2016), who shows that workers in Mexican *maquiladoras* took present-biased decisions by choosing short-term gains at work over long-term gains through schooling. It appears that present bias not only makes workers take low-skill industrial jobs – it also keeps workers in these jobs.

The paper proceeds as follows. Section 2 reviews the empirical setting and provides descriptive evidence on the job search and savings behavior of workers at the study firm. Section 3 uses this data to motivate a simple theoretical job search model to guide the empirical analysis. Section 4 discusses the experimental design and the elicitation of present bias. Section 5 presents results, and Section 6 concludes.

2. Setting

Ethiopia is one of the poorest countries in the world. The median person among its population of 107.5 million lives on $2.75 per day (adjusted for purchasing power parity) and 68 percent of the population works in the agricultural sector.

The Ethiopian economy and labor market are however undergoing rapid change. The economy grew at an average rate of 10.7 percent annually from 2003 to 2011. In line with the

[8] This is particularly the case in the quasi-hyperbolic model that this paper builds on. In some dual-self models such as Benhabib and Bisin (2005) or Fudenberg and Levine (2006), the long-run self has the same short-run preferences as the short-run self, so a welfare criterion is more obvious to define. Bryan, Karlan, and Nelson (2010) provide a discussion. Bai, Handel, Miguel, and Rao (2017) provide a field test of theoretically-motivated commitment devices and illustrate how they can be welfare-reducing if individuals are over-optimistic about their effectiveness.

government's push for structural transformation and related public investment, employment has shifted from low-productivity agriculture to services, construction, and tradable goods. The share of the labor force working in the informal sector more than halved from 50.6 to 22.8 percent between 1999 and 2013 (Seid, Tafesse, & Ali, 2016).

Urbanization is a key part of this transformation, as migrants from rural areas seek employment and education in the cities. From 2000 to 2014, Ethiopia's urban population almost doubled from 9.8 million to 18.4 million (World Bank, 2016). The median age of people who migrated from rural to urban areas in the last five years is 21 years, 56.4 percent are female (Central Statistical Agency, 2014). Most migrants come to the capital Addis Ababa, where overall unemployment is high at 24 percent and unemployment among those aged 20 to 24 is even higher at 33.2 percent (Central Statistical Agency, 2015).

The Ready-Made Garment Industry in Ethiopia and the Study Firm

Ethiopia's government is pursuing an ambitious industrialization strategy that aims to make the country Sub-Saharan Africa's leader in light manufacturing. The cornerstone of this strategy is the construction of industrial parks, which aim to attract foreign direct investment from American, Asian, and European producers of ready-made garments, leather goods, pharmaceuticals, and agricultural products (Oqubay, 2016). These parks produce exclusively for export, not for the domestic market.

For the Ethiopian government, the industrial parks with their labor-intensive industries represent formal employment opportunities for the country's youth. Given that light manufacturing firms often prefer to hire women, the parks also represent an opportunity to increase female labor force participation and empower women by giving them their own stable income. For international investors, the parks represent one of the lowest-cost manufacturing destinations in the world (Gelb, Meyer, Ramachandran, & Wadhwa, 2017). In addition to an abundance of labor, Ethiopia has relatively weak labor laws and currently no minimum wage. Firms pay extremely low wages clustered around the local poverty line.[9] They also offer little to no upward mobility, so that the vast majority of workers will not advance past the level of machine operators. With their stable but extremely low wages and almost no skill requirements, the firms in Ethiopia's industrial parks represent what Blattman and Dercon (2018) call an "industrial safety net."

[9] In Bole Lemi Industrial Park, where this study is set, entry level wages are about 1,000 birr (US$ 36.30) per month. The local poverty line is about 958 birr per adult per month. In the Hawassa Industrial Park, the largest industrial park currently in operation, entry-level wages are set at 750 birr ($US 27.23) per month. The local poverty line in Hawassa is about 695 birr per adult per month. Poverty lines are based on the official 2015/16 absolute poverty line of 7,184 birr per adult per year, adjusted to current values using the GDP deflator and adjusted for local prices using the spatial price indices reported in National Planning Commission (2017). Following the methodology of the National Planning Commission, spatial prices for food and non-food are weighted using the food share of the poorest quartile of the population (0.525).

For this paper I work with one such firm, located in Bole Lemi (BL) Industrial Park in the outskirts Addis Ababa. Appendix Figure D.1 locates the industrial park on a map of Addis Ababa and its surroundings. Inside the park, ten foreign-owned firms produce garments and leather goods in 20 factory buildings with a total capacity of about 20,000 workers. The study firm produces garments for well-known European and North American brands. The firm has about 3,300 workers, about half of which are machine operators, who sew and pack the garments. 95 percent of machine operators are female. Hours and compensation are in line with other firms in this and other industrial parks in Ethiopia.[10] Machine operators earn 1,000 birr (US$ 36.30) in the first month, 1,075 birr (US$ 39.00) in the second month, and 1,150 birr (US$ 41.75) after that. Wage growth and opportunities for promotion are extremely limited and depend on performance evaluations every three months.[11] In addition to their base salary, workers receive limited team-based productivity bonuses, subsidized meals, and free transportation to nearby neighborhoods.

I selected this firm because it was planning to hire a large number of production workers during the study period in the spring of 2018. Job seekers come to the factory gate every day. About 250 to 500 people apply every month. Out of those, 200 to 400 are hired (an average of 15 workers per day). This hiring is partly to expand production and partly to make up for the circa 100 workers who leave every month. Production jobs have no appreciable skill requirements beyond basic motor skills. The most common reasons for not being hired are insufficient Amharic language skills, missing documentation, and failure to meet the minimum education requirements. Due to relatively strict enforcement by the international brands that have their garments manufactured in the industrial park, child labor is not common in this context. If applicants are hired, they return on the next day to begin work.

Workers at the Study Firm

The workers in my sample are exclusively female and tend to be young, low-skill, rural-urban migrants with little to no previous work experience. As such they represent one of the most disadvantaged groups in the urban labor market of Addis Ababa. The median worker has completed primary school while the 75th percentile has completed lower secondary schooling.[12] 73 percent of the workers in my sample were not born in Addis Ababa or its outskirts

[10] Workers have 52.5 hour, six-day workweeks. Hours are Monday through Saturday from 7.30am to 5.30pm with 75 minutes of break time.

[11] A positive evaluation can increase the salary by 100 birr (US$ 3.63) per month up to a maximum wage of 1,650 birr (US$ 58.05) per month. The firm's human resource department estimates that out of 100 production workers, a maximum of five could ever advance to become team leaders, the next higher level of hierarchy on the factory floor, and even fewer to line supervisors, the group of factory floor managers studied by Macchiavello, Menzel, Rabbani, and Woodruff (2015).

[12] Abebe et al. (2016) illustrate how disadvantaged job seekers with low levels of education are. In their study of urban job seekers in Addis Ababa they show that a worker who has completed secondary schooling is four time

but moved to the city from rural areas. Out of those, 73 percent report having moved for the purpose of finding work, another 10 percent in order to find education or training. The median worker moved 103 months ago. 41 percent have never been employed. Excluding work as a housemaid, a common job for young female migrants, 75 percent do not have any formal work experience before joining the firm. Most workers live in the outskirts of the city around the industrial park (Appendix Figure D.1).

With what goals do workers start their jobs? First, saving money is an important goal for workers joining the firm. While 58 percent of workers report that they did not manage to save any money in the month before starting employment, all but one respondent report that they are planning to save money during their time at the firm. 47 percent of those state that they are planning to save mainly to build assets, for example to start their own business. Another 36 percent say that they are planning to save money mainly for precautionary reasons. Most workers indeed build up savings over time, however not as much as they hope. I can compare actual savings over time with a savings goal that workers set when joining the firm. I elicit workers' predictions for savings in two ways: The monthly amount that workers would ideally like to save and the monthly amount that workers think they can realistically save.[13] Panel (a) of Figure 1 plots cumulative savings over time compared to the self-set goal and the amount that workers think they can realistically save. Workers clearly fall short of both. One month into the job, the median worker has not accumulated any savings. The mean worker has reached about 53 percent of her ideal monthly savings goal. After three months at the firm, 68 percent of workers feel that they did not save as much as they had hoped when they joined, mostly because they spent more than they were planning (51 percent of those that reported saving less than planned). Overall, the intention-behavior gap in savings is consistent with workers overestimating either their future self-control or their future efficiency in saving money. Using administrative and experimental data, DellaVigna and Malmendier (2006) and Acland and Levy (2015) show similar overconfidence over future self-control or efficiency for gym attendance.

Second, many workers in my sample see employment at the firm as temporary and use it to finance search for other opportunities. On the day that they join the firm, 25 percent of the workers in my sample reports that they are planning to leave within a fixed amount of time. The median expected tenure of these workers is 12 months. This is reflected in workers' job search efforts at the beginning of employment: 20 percent of the sample are still looking for other opportunities on the day that they join the firm. Of those, 87 percent report that they are looking for higher-wage jobs. Notably, another 31 percent of the sample report on the day

less likely to have formal sector job and seven times less likely to have a permanent job than workers with a vocational or university degree.

[13] Workers in my sample have ambitious savings goals. The median reported goal for saving money on the job is 500 birr (US$ 18.15) per month, or approximately half the monthly salary. The median worker thinks that she can realistically save 350 birr (US$ 12.71) per month, or approximately a third of the monthly salary. Appendix B.2 elaborates on the elicitation of subjective probabilities.

that they join that they would like to look for work, but they find it too costly (10 percent of all constrained), time-consuming (31 percent), difficult (20 percent), or face other binding constraints (39 percent). Panel (b) of Figure 1 plots the fraction of workers who are searching and the fraction of workers who would have liked to search at a weekly level over the first three month of employment.[14] High cost of search is consistent with the results of Abebe et al. (2016) and Franklin (2017) in a similar context.[15] On the job search while at the firm mostly works through social networks (61 percent of all workers engaged in search), vacancy boards (21 percent), and job brokers (9 percent). Overall, on the day they join just 49 percent of workers report that are satisfied and do not currently intent to look for other work. The other 51 percent appear to use their job at the firm to queue for better alternatives.

Saving money and searching for work appear to be at least partial substitutes for workers in my sample. As an indication, consider the mean savings goal on the day that workers join the study firm. Workers who are still looking for other employment report that they would like to save 499 birr (95% CI: 447 to 552) over the next month. Workers who are not looking for work report that they would like to save 628 birr (95% CI: 595 to 662) over the next month.[16]

In line with the notion that a significant share of workers sees the job as temporary, turnover from the study firm is high. In my sample, 117 out of 460 workers leave the firm within the first three months. The earliest departure from the firm comes after 12 days, the median exit occurs after 70 days. Panel (c) of Figure 1 plots Kaplan-Meier survival estimates. 90 percent of departures from the firm are voluntary. Of all the voluntary departures, 31 percent are due to low pay. The high turnover rate is consistent with previous results from the same context (Abebe, Buehren, & Goldstein, 2018; Blattman & Dercon, 2018).

Overall, the descriptive evidence paints a picture of workers who are disadvantaged in the local labor market and who seek temporary low-wage industrial employment to build assets or finance continued search for better opportunities. There are quasi no entry barriers to employment and few involuntary separations, which suggests that firms in this context represent a safety net, rather than a desirable form of employment.

[14]Note that this is on-the-job search only, so it excludes search that happens after leaving the firm. I also measure search intensity using the number of hours spent searching, the number of phone calls made in search for work, and a subjective assessment of search intensity. Appendix Figure D.2 plots all three measures as well as the extensive margin of search. The overall pattern similar, except for the subjective intensity measure which decreases towards the end of the panel.

[15]Abebe et al. (2016) and Franklin (2017) study search costs in the urban labor market of Addis Ababa and focus on large travel distances to centrally-located vacancy boards that make search costly. Data from my (different) sample suggests that cost in terms of time and effort are the most binding constraints and that social networks, not job boards, are the most common search methods. Appendix Table E.1 breaks down reported reasons for not searching while on the job, though the numbers in later survey waves are too small for meaningful analysis.

[16]A similar pattern holds for workers who expect their tenure at the firm to be limited. Workers who do not see the job as temporary report that they would like to save 620 birr (95% CI: 584 to 656) per month, while workers who expect their tenure to be limited report that they would like to save 554 birr (95% CI: 509 to 599) per month.

Importantly, the data above shows that workers are failing to follow through on their goals of saving money and searching for other opportunities. They fall short of their savings goals and they report not looking for work because they find it too time consuming or costly. This indicates that self-control problems may play a role in this context. Notably, both the savings decision and the search decision involve an intertemporal trade-off between large immediate costs and future benefits. A large literature has shown that behavior in each domain can be rationalized using models that allow for time-inconsistent intertemporal choice (DellaVigna, 2009; DellaVigna & Paserman, 2005; Laibson, Repetto, & Tobacman, 1998; Paserman, 2008). In the next section I present a theoretical framework to help build intuition for how such time-inconsistent intertemporal choice can affect both decisions.

3. Theoretical Framework

Workers in the study setting can increase their future consumption possibilities in two ways: They can continue to engage in job search and they can use their wage income to save money. Continued search allows workers to find higher-wage employment. Saving allows workers to smooth consumption when leaving the job at the firm, for example when they are fired or if want to become in self-employed.

It is useful to consider both decisions jointly because they can affect each other. If a worker expects to leave the safety net firm after a year (the median expected tenure reported by workers in my experiment) she will want to insure herself against expected income losses in the months leading up to that point. In that case, both search and savings represent self-insurance efforts. Everything else constant, the relative costs of and returns from search and savings will affect how much she searches and how much she saves.[17]

Both the savings decision and the search decision involve an intertemporal trade-off between immediate costs and future benefits. Given the suggestive evidence on self-control problems in the previous section, I allow for workers to be time-inconsistent in this trade-off by using (β-δ) preferences (Laibson, 1997; O'Donoghue & Rabin, 1999).

To guide the interpretation of my reduced-form analysis below, I provide a model that illustrates how both of these choice problems are affected by their relative costs and by time preferences, in particular by present bias. I simplify the environment significantly with the aim of building intuition. My approach rests on the re-interpretation of employment at the

[17]Note that I focus here on a precautionary savings motive of workers holding the amount of initial wealth constant, not the relationship of (initial) wealth on search. Initial wealth could increase for example due to a lump-sum severance pay as in Card, Chetty, and Weber (2007). Lentz and Tranæs (2005) show analytically and with simulations that job search effort is negatively related to initial wealth under the assumption of additively separable utility. This is in line with empirical results, e.g. in Algan, Chéron, Hairault, and Langot (2003) and Bloemen and Stancanelli (2001).

RMG firm as akin to the safety net of a welfare system. This follows directly from the anecdotal evidence presented above, where involuntary separations are rare and many workers continue to search for higher-wage opportunities from the day they start employment at the firm.

3.1. Job Search and Savings with Present-Biased Preferences

In this section I provide a discrete-time, partial-equilibrium job search model with endogenous savings that builds on the framework of Lentz and Tranæs (2005) and Card et al. (2007). DellaVigna et al. (2017) present a version of this framework that allows for hyperbolic time preferences (Laibson, 1997; O'Donoghue & Rabin, 1999) to affect both job search behavior (DellaVigna & Paserman, 2005) and endogenous savings.

For tractability reasons, I follow the previous literature in making several key simplifications: First, wages are exogenously fixed. The distribution of wages in the economy is exogenous to the worker and workers are currently employed at their reservation wage. This assumption reflects the fact that the study firm employs all production workers at the same wage near subsistence income levels. Given that workers can likely not fulfill minimum nutrition requirements below this wage level, it is improbable that their wages reflect a reservation wage. The assumption is also in line with empirical evidence, including in my data below, that reservation wages play a limited role in job search (Schmieder, von Wachter, & Bender, 2016) and are not significantly affected by time preferences (DellaVigna & Paserman, 2005; Krueger & Mueller, 2016). Second, once a worker finds a new job, she will stay in this job indefinitely. Third, utility is separable in consumption and search effort. If search costs were monetary and entered a concave utility function, the marginal costs of search would decrease with consumption. To ease interpretation I want to abstract from this case, which is discussed in detail by Lentz and Tranæs (2005). Fourth, if workers are present-biased with discount factor $\beta < 1$, I assume that they are naïve about it. Every period, each worker assumes that her future self will be an exponential discounter with $\beta = 1$. Workers overly optimistic predictions of savings over the course of employment, reported in the previous section, can be interpreted as evidence of such naïve. While there is substantial evidence for naïveté in the literature, including from the widespread lack of commitment (Laibson, 2015), it is more likely that most individuals are neither fully naïve nor fully sophisticated (O'Donoghue & Rabin, 2001). Importantly, unlike individuals who are (partly) sophisticated about their present-bias, naïve individuals will not demand any commitment devices to help them overcome their present-bias.

Setup

Consider a worker with finite planning horizon who in each period t chooses assets in the next period A_{t+1} as well as contemporaneous job search effort $s_t \in [0, 1]$, which represents the probability of receiving a job offer at the end of the current period and thus having a new job in $t+1$. Search costs $k(s_t)$ are twice continuously differentiable and convex with $k'(s_t) > 0$, $k'(s_t) < 0$, $k(0) = 0$, and $k'(0) = 0$. Flow utility in each period is $u(c_t) - k(s_t)$, where c_t is the period consumption and utility from consumption $u(c_t)$ is strictly concave. Income y_t comes from a wage w_t paid at the safety net firm or an outside option $\widetilde{w} > w_t$ in a different job. Once a worker has found and accepted another job at $\widetilde{w}$, the search choice becomes mute. The path of wages at the firm $\{w_t\}_{t=1,...,T}$ is exogenous and reflects the firm's fixed pay scale which depends on tenure. In each period workers can accumulate or run down assets A_t, which earn a return R and are constrained by $A_t \geqslant -L$. The per-period budget constraint is thus $y_t - c_t = \frac{A_{t+1}}{1+R} - A_t$.

Value Functions

The formulation of the dynamic programming problem follows DellaVigna et al. (2017), so my exposition here is brief. A worker without present bias who is on her job at the safety net firm chooses s_t and asset level A_{t+1}, which implicitly defines consumption c_t. Her value function is

$$\begin{aligned} V_t^F(A_t) = \max_{s_t \in [0,1];\, A_{t+1} \geq -L} & \; u(c_t) - k(s_t) \\ & + \delta \left[s_t V_{t+1}^O(A_{t+1}) + (1-s_t) V_{t+1}^F(A_{t+1}) \right], \end{aligned} \tag{1}$$

where δ is the regular per-period discount factor. V^O is the value of an outside job opportunity in period t given by

$$V_t^O(A_t) = \max_{A_{t+1} > 0} u(c_t) + \delta V_{t+1}^O(A_{t+1}). \tag{2}$$

The maximization of equations (1) and (2) is subject to the common budget constraint

$$c_t = A_t + y_t - \frac{A_{t+1}}{1+R} \tag{3}$$

and liquidity constraint $A_t \geqslant -L$ for all t. The maximization in V_t^O is a well-behaved sequence problem as the objective is concave, continuous, and the constraint is compact.[18] As noted by Lentz and Tranæs (2005), V_t^F could theoretically be convex, though they show in simulations that nonconcavity never arises. I will follow Card et al. (2007) in simply assuming concavity.

A worker on her job at the safety net firm chooses s_t to maximize expected utility. Substituting the budget constraint into (1), the first-order condition for optimal search intensity of a worker without present bias s^* is

$$c'\left(s_t^*\left(A_{t+1}\right)\right) = \delta \left[V_{t+1}^O(A_{t+1}) - V_{t+1}^F(A_{t+1}) \right]. \tag{4}$$

[18] As is shown, for example, in Adda and Cooper (2003), Chapter 2.3.

Intuitively, the optimal level of search effort equates the marginal costs of search effort with the marginal gain from search, given by the difference between the value of the outside option and the value of remaining at the safety net firm. The right-hand side of (4) is the net value of the outside option.

Compare this to a naïve present-biased worker who is on her job at the safety net firm. She faces the value function

$$\begin{aligned} V_t^{F,n}(A_t) = & \max_{s_t \in [0,1];\, A_{t+1} \geq -L} u\left(c_t\right) - k\left(s_t\right) \\ & + \beta\delta \left[s_t V_{t+1}^{O}(A_{t+1}) + \left(1 - s_t\right) V_{t+1}^{F}\left(A_{t+1}\right) \right] \end{aligned} \tag{5}$$

where the additional parameter $\beta \leq 1$ allows for the worker to be present-biased between the current period and the future. Recall that I assume naïveté, so that continuation values V_{t+1}^{F} and V_{t+1}^{O} above are equivalent to those of the exponential discounters in (1) and (2), respectively. Intuitively, the naïve present-biased worker assumes in every period that in the next period she will discount the future only by the factor δ. The naïve present-biased worker who found another opportunity faces the value function

$$V_t^{O,n}(A_t) = \max_{A_{t+1} > 0} u\left(c_t\right) + \beta\delta V_{t+1}^{O}(A_{t+1}). \tag{6}$$

The first-order condition for optimal search intensity of present-biased workers s^{n*} given budget constraint (3) and value function (5) is now

$$k'\left(s_t^{n*}\left(A_{t+1}\right)\right) = \beta\delta \left[V_{t+1}^{O}(A_{t+1}) - V_{t+1}^{F}(A_{t+1}) \right]. \tag{7}$$

Due to equivalent continuation values I can directly compare search effort with and without present bias by combining first order equations (4) and (7):

$$\beta\, k'\left(s_t^{*}\left(A_{t+1}\right)\right) = k'\left(s_t^{n*}\left(A_{t+1}\right)\right). \tag{8}$$

We can now see that search effort is strictly increasing in β due to assumed convexity of the search cost function $k(s_t)$.

It is difficult to fully characterize the model with search and savings analytically.[19] I thus obtain key predictions numerically.

[19] DellaVigna and Paserman (2005) characterize the relationship of impatience and search effort in a model without savings, where agents choose the reservation wage and search effort. Endogenizing savings leads to a significantly more complicated setup. One simplification that helps to characterize the model analytically is to change the timing so that successful job search in period t leads to a new job in the same period. This is the approach in Lentz and Tranæs (2005) and Card et al. (2007).

3.2. Simulations

To simulate the model from the previous section I make additional functional form assumptions described in Appendix A.1. Parameters are set based on survey data from my experiment, which I present in more detail below.

Before discussing the main results it is worth noting that in the framework presented above, workers at the safety net firm do not have a savings motive if they believe that they will never leave the firm. Recall that involuntary transitions are ruled out, so workers will simply continue to consume their wage indefinitely. While they will continue to invest in search as the value of being at the firm decreases relative to the value of being in another higher-wage job (compare first order condition 7), they will not accumulate any assets. Consider instead the case where I exogenously impose that workers leave after one year, which is equivalent to the median expected tenure that workers in my experiment report on the day that they join the firm. I assume that after leaving income falls to the median pre-employment income reported in my data. In that case, workers have precautionary savings motive that leads them to reduce consumption and accumulate savings towards the end of their expected tenure. Figure A.1 in Appendix A.2 plots the optimal paths of search effort and asset accumulation and the implied paths of consumption and turnover for both cases.

To see how workers use both search and savings as insurance mechanisms, it is instructive to look at substitution between search and savings. Following Paserman (2008) and DellaVigna et al. (2017) I simulate the model allowing for heterogeneity in search costs with six different worker types who have costs $\{\psi_j\}_{j=1,...,6}$ where $0 < \psi_1 < ... < \psi_6$. I focus on the assumption of finite tenure at the firm, so that the asset choice is not mute. We can see from Figure A.3 that types with low search costs search more (plotted on the right y-axis) and save less (plotted on the left y-axis). Intuitively, workers adjust the use of both insurance mechanisms in response to their relative costs.

I focus on comparative statics with respect to present bias β. In the framework presented above, both present bias β and the exponential discount parameter δ affect search effort and savings in qualitatively similar ways because they both reduce the present value of the benefits of search and the benefits of saving.[20] Unlike exponential discounting, present bias leads to suboptimal decisions because workers today will under-invest in search and savings because they erroneously expect to search and save more tomorrow. This undermines self-insurance efforts of workers.

First, I examine the relationship of asset accumulation and present bias (Figure 2, panel a). As explained above, workers who plan to stay at the firm indefinitely do not have a savings motive and thus do not accumulate any assets. Under the assumption of finite tenure, workers do accumulate assets. The simulations illustrate that assets are increasing in β. Lower asset

[20] Appendix Figure A.2 illustrates the relative magnitudes with simulations that also vary the discount factor.

accumulation due to present bias reduces the ability of workers to smooth consumption after leaving the firm. This suggests that there is scope for a welfare-improving intervention that helps present-biased workers save.

Prediction 1 (Present Bias and Savings): Asset accumulation is increasing in β.

Next, I consider the relationship of job search effort and present bias. Panel (b) of Figure 2 plots total job search effort by present bias β. As can be seen from the comparison of first order conditions of workers with and without present bias (equation 8), search effort is strictly increasing in β. An increase in present bias reduces the present value of search. This effect is identical to the finding of DellaVigna and Paserman (2005).

Prediction 2 (Present Bias and Search): Search effort is increasing in β.

Finally, I turn to survival rates that are implied by worker choices of optimal job search effort and savings (Figure 2, panel b). Prediction 3 follows as a direct corollary from Prediction 2 because the probability of leaving the safety net firm depends on the probability of receiving an alternative wage offer and thus directly on search effort s.

Prediction 3 (Present Bias and Turnover): The survival rate at the safety net firm is decreasing in β.

In the next section, I present data on these three predictions that I collected from a sample of workers who join a firm in Ethiopia's RMG industry. I then present results of an intervention designed to alleviate self-control problems in savings due to present bias.

4. Experimental Design

4.1. Description of Data Collection

For data collection I cooperate with a garment manufacturing firm described in Section 2 above. The study firm is typical of the low-skill, low-wage, export-oriented manufacturing industry that can be found in many parts of the developing world. It is located in an industrial park in a peri-urban area of Addis Ababa, the capital of Ethiopia.

Data on a random sample of 460 workers who start employment at the firm is collected in three steps: First, I conduct an in-person baseline survey and a lab-in-the-field experiment to elicit individual time preferences on the day that workers join the study firm. Second, I track individual behavior with a high-frequency phone survey. Third, I conduct an in-person endline survey after the conclusion of the panel. The survey is set up to track workers if they leave the firm. I combine my own data with firm administrative records on tenure and basic demographic data on 238 *pure control* workers who are randomized out of study participation.[21]

Women looking for employment as production workers come to the factory gate of the study firm every day. At the end of each day, I draw a random sample from all job seekers who are hired by the firm on that day. No other criteria are used for inclusion in the study. Appendix Section B.1.1 provides details on the randomization procedure. On the next day, the hired candidates return to the firm to begin employment. Informed consent, baseline interview, and time preference elicitation are administered through in-person interviews on the morning of the second day immediately before candidates are assigned to a production line.[22] As part of the baseline interview, I also collect detailed data on employment histories, subjective expectations over search and savings behavior, and a battery of tests for cognitive and non-cognitive skills. Appendix Sections B.2 and B.3 summarize. Baseline interviews are conducted every day from March to July 2018. On average the team of enumerators conducts 7 baseline interviews per day and 29 per week. Baseline interviews last 85 minutes on average.

In the three months after the baseline, enumerators call respondents every 14 days to collect data on consumption expenditures and savings, job search behavior, job search outcomes, transitions to other jobs (if any), and measures of psychological well-being. Phone surveys are practical in the local context because all workers have a mobile phone and because they allow me to track subjects even if they leave the firm. The same approach has been used successfully in a similar setting by Abebe et al. (2016) and Franklin (2017). Phone calls last 7.5 minutes on average. After three months, enumerators conduct another in-person endline interview. All interviews are conducted in Amharic and Oromiffa using computer-assisted personal interviewing (CAPI). Appendix Section B.1.2 gives a detailed description of survey protocols.

Attrition from the phone survey is relatively low. The second follow up call after one month reaches 437 out of 460 or 95 percent of all respondents. The fourth call after two months reaches 331 out of 460 or 72 percent of all respondents. Enumerators stay reasonably well on schedule: At the first follow-up, 93 percent of calls are within 4 days of the scheduled interview day. At the fourth call after two months, 85.5 percent of calls are within four days of the scheduled interview day. Appendix Figure D.3 illustrates.

4.2. Experimental Elicitation of Time Preferences

I estimate time preferences over money for each respondent using an adapted version of the convex time budget (CTB) task by Andreoni and Sprenger (2012). Each subject is asked to

[21] I also collect rich productivity data for all workers, which we analyze in a companion paper (Hardy, Kagy, & Meyer, 2018).

[22] Given that I work closely with the firm, I take a number of precautions to maximize privacy and confidentiality of the respondents. This includes selecting a random subset of workers to be interviewed at home instead of at the factory. Appendix Section B.1.3 elaborates.

allocate an experimental budget $m > 0$ between an amount c_t available at an earlier time t and another amount c_{t+k} available after a delay $k > 0$, i.e. paid out at point $t + k$. Let $(1 + r)$ be the simple gross interest rate to be paid over period k. This means that subjects maximize utility subject to the experimental budget constraint $(1 + r)c_t + c_{t+k} = m$. Let the unit of time be days since the experiment and all monetary amounts be measured in experimental tokens.

The CTB method aims to address methodological problems of multiple price list (MPL) approaches, which frequently rely on the assumption of linear utility and may lead to biased estimates of time preferences when utility is in fact concave. Importantly, the CTB method lends itself to structural estimation of aggregate and individual-level (β-δ) time preference parameters. Given functional form assumptions, the discounting parameter δ can be identified from variation in the interest rate r and delay k while present bias β can be identified from variation in the timing of the earlier payout t. Section C.5 provides details on the estimation technique.

Irrespective of whether time preferences are elicited using MPL or CTB approaches, a number of confounds may undermine identification of preference parameters from time-dated payments. One important concern relates to real or perceived transaction costs of receiving payments in the future versus receiving payments today. Subjects may exhibit a preference for earlier payments because it is more costly to obtain the later payment or because of uncertainty over whether the experimenter will deliver the payment as promised. Several recent studies that carefully equalize transaction costs between time-dated payments find little evidence of aggregate present bias (Andersen et al., 2008; Andreoni & Sprenger, 2012; Augenblick et al., 2015; Giné et al., 2017). I take several steps to carefully equalize transaction that I discuss in more detail below.

A second potential confound relates to the assumption of time-invariant utility of experimental subjects. Even if transaction costs are equalized between two time-dated payments, experimental subjects may simply have have preferences that change over time (Halevy, 2015; Janssens, Kramer, & Swart, 2017). One way to rationalize such time-varying preferences are economic conditions outside of the experiment, in particular liquidity constraints on the part of subjects (Dean & Sautmann, 2016). I argue that liquidity are unlikely to be a driver of my experimental results in 5.3. I show that a randomized cash drop between the baseline and the endline of the experiment does not affect estimated present bias at endline. In addition, all results below control for measures of baseline liquidity and access to finance.

A third and related concern is whether choices over time-dated monetary payments can identify time preferences over consumption (Augenblick et al., 2015; Cubitt & Read, 2007). If experimental subjects can borrow (save) at a market interest rate that is lower (higher) than the experimental interest rate between earlier and later payments, subjects could allocate their whole budget to the later (earlier) period and arbitrage between the experiment and the market. Such arbitrage likely requires highly sophisticated subjects.[23] Augenblick et al. (2015) overcome arbitrage concerns in the monetary domain by using the CTB method with a real-effort laboratory task and time-dated effort allocations. They find that aggregate present bias is limited for preferences over time-dated effort, but significant for preferences over time-dated monetary payments. While relatively limited access to capital markets in my context combined with the fact that most time-dated allocations in my data are interior solutions suggests that arbitrage concerns are not first order, I do acknowledge this as a limitation of my study.[24]

Implementation of CTB Task in the Field

I follow the procedures of Giné et al. (2017) to implement the CTB method in the field. Respondents allocate 20 tokens in the form of beans between two empty dishes that represent the two payoffs. Each of the two dishes is positioned below a small whiteboard that indicates the exact payoff date and an exchange rate at which beans can be converted into birr.[25]

I vary the initial payment date t and delay between earlier and later payment k, so that each subject faces three choice sets $(t, k) \in [(1, 14), (1, 28), (15, 14)]$. Within each choice set, a bean on the earlier dish is always worth 10 birr. A bean on the later dish is worth $10 \times (1 + r)$ birr, where $r \in [0.10, 0.25, 0.50, 0.75, 1.00]$. This implies that experimental stakes are large. Subjects can receive between 200 and 400 birr (US$7.26 to US$14.52), or 20 to 40 percent of their monthly starting salary.

Experimental Protocol

The CTB task is administered as part of the baseline survey and again as part of the endline survey. In this paper I focus on the baseline results. The CTB task is administered towards the middle of the survey questionnaire. This was done so that the enumerator can build trust with the respondent while at the same time minimizing respondent fatigue. The order in which

[23] In a carefully designed laboratory experiment, Coller and Williams (1999) assess the arbitrage argument by providing information about market interest rates. They show that some subjects attempt to exploit arbitrage opportunities between the laboratory experiment and the market, but that they either do not know outside opportunities or fail to determine the correct market rate.

[24] Similar to Augenblick et al. (2015) I also elicit time preferences over real effort by asking subjects to allocate an amount of work at the factory between an earlier and a later point in time. I use a design that builds on Carvalho, Meier, and Wang (2016), where I vary the length of the shorter work assignment and the time by which it needs to be accomplished while holding the delay between earlier and later assignment constant. Results are omitted here because the study firm did not let me implement an incentivized version of this task and all decisions are purely hypothetical. (No deception was involved because the wording of the question was adapted in due time to reflect the hypothetical nature of the task.)

[25] Respondents use the same beans and dishes for various other parts of the baseline survey before the CTB task (see Appendix Section B.2). Extensive piloting over several weeks before the study confirmed that respondents found this a natural and easily-comprehensible way to allocate a budget.

respondents are presented with the CTB choice sets is randomized while the exchange rate for beans on the later dish always increases in each set. As in Giné et al. (2017), enumerators administer a set of unrelated question from the baseline survey between each choice set to reduce the chance that respondents attempt to be consistent between sets.

At the beginning of the CTB task, the enumerator explains the task to the respondent. Respondents are required to pass three questions that test their understanding. The respondent then practices one allocation that will not be implemented. The enumerator also informs the respondent that there is a 50 percent chance that one allocation (with earlier and later payouts) is implemented, determined by a coin flip at the end of the experiment. At the beginning of each choice set, the enumerator uses one of the whiteboards to provide an overview of the value of beans in each of the five decisions in the set. At the beginning of each allocation decision, the enumerator wipes the whiteboards and writes down the payment date and the exchange value of one bean at the top of each board. The enumerator then asks the respondent to allocate all 20 beans to the two dishes. Care was taken to use neutral language and not lead respondents away from corner solutions. Once the respondent has made her allocation, the enumerator calculates the total monetary value on each dish and writes it on the whiteboard next to it. The respondents is asked if she is satisfied with the allocation and is given the opportunity to revise as many times as she likes. For each revised decision the enumerator re-calculates the total monetary amount and again writes it on the board. Once the respondent is satisfied, the enumerator records the decision in the survey software. The software confirms both the number of beans and the total monetary amounts. The coin flip and draw of the payoff-relevant decision are done at the end of the survey. Appendix Section B.1.1 gives details. Appendix Figure C.6 shows a picture of the allocation decision.

Experimental Payments

To obtain unbiased time preference estimates it is critical to equalize real and perceived transaction costs between the earlier and the later time frame. This is particularly true in the setting of this study because respondents are not subjects from a laboratory pool that have a relationship with the experimenter, but a highly mobile and disadvantaged population who may not have full confidence in experimental payments being delivered on time or delivered at all. I take several steps to equalize transaction costs and reduce uncertainty over whether and how payments are delivered.

First, I rely on mobile money payments to respondent cell phones using *CBEbirr*, a system operated by Commercial Bank of Ethiopia (CBE), the country's largest bank. The system is similar to Kenya's popular M-Pesa system and allows users to receive and transfer money and to purchase goods and services using simple text messages. Transfers are immediate and can be precisely timed. Users can withdraw money from any CBE branch or one of many authorized CBEbirr agents. CBE is a well-known and widely-trusted institution, even in more rural areas. All subjects in the study had phones that supported CBEbirr.[26] This equalizes and minimizes costs of receiving and accessing the payments. Mobile money has previously been used for experimental payments from a laboratory CTB task by Balakrishnan et al. (2017).

Second, I follow Andersen et al. (2008) and Giné et al. (2017) in prioritizing symmetry of payments over the opportunity to pay subjects immediately. Even though mobile money payments allow for truly immediate payments as in Balakrishnan et al. (2017), making the earlier payment while enumerators are still physically present with the respondents could favor the earlier over the later allocations if respondents do not have full confidence in the later payment being delivered as promised. I instead choose to make the earliest payment on the next day before noon. While the front-end delay equalizes perceived or real transaction costs, it comes at the cost of not being able to study present bias with respect to truly immediate consumption. It is likely that the small delay attenuates any present bias.[27] Given that present bias is a key input to my empirical analysis, I prefer this more conservative approach over one that increases my ability to detect present bias at the risk of undermining identification.

Third, I follow previous implementations of the CTB task (Andreoni & Sprenger, 2012) in making sure that both experimental payments always happen on the same day of the week. This avoids differential weekday effects between sooner and later payments. Subjects know that all payments are made before noon.

Fourth, respondents who win the payout are given a written confirmation or voucher that repeats the payment amounts, payment times and dates, and information on how the payment is delivered. This information serves to reduce the cognitive costs of keeping track of payments.[28] It is is printed on high-quality paper with a watermark of the principal investigator's university, signed by the principal investigator and the enumerator, and includes the private cell phone number of the survey coordinator. Similar to Andreoni and Sprenger (2012), who include a business card of one of the authors and encourage student subjects to reach out to if the payment is not delivered, we encourage subjects to reach out to us should there be any problems with the payment. This is intended to increase trust in the study team and confidence

[26] Even though all respondents could receive payments, 4 percent of respondents who won the coin flip preferred to receive their payments using *hawala*, a widely-used money transfer system that is more comparable to a money order. There are several formal and informal hawala operators in Ethiopia. For the small number of cases in which subjects did not want to receive their payments through CBEbirr, we sent formal hawala payments through CBE. Dropping these observations from the analysis does not affect results.

[27] This is indeed one of they key findings of Balakrishnan et al. (2017). Using a CTB task in a laboratory experiment in Nairobi, they find evidence of present bias over money when payments are truly immediate. When payments are slightly delayed until the end of the day, they do not observe aggregate present bias.

[28] Both Andreoni and Sprenger (2012) and Balakrishnan et al. (2017) take such steps to reduce the cognitive transaction costs that may result from keeping track of future payments. Haushofer (2015) explores theoretically how the cognitive cost of keeping track may explain stylized facts of temporal discounting in the literature.

that the payment will be delivered. Appendix Section C.3 provides an English translation of the confirmation.

Results of Time Preference Elicitation

While aggregate time preferences are not the focus of this study, it is useful to consider aggregate results in order to check if subjects understood the task and to compare results with the existing literature. Figure 3 shows the mean fraction of beans allocated to the earlier dish for each experimental interest rate r, separately by front-end delay t and delay between earlier and later allocation k. Error bars indicate the standard error clustered at the individual level. It is clear that in all three choice sets subjects on average respond to the increase in interest rate in line with the law of demand. As the interest rate increases and the price of consumption later decreases, subjects monotonically decrease the share of beans allocated to earlier. Appendix Section C.4 assesses individual consistency with the law of demand, which compares favorably to previous experiments in the literature. For a given interest rate subjects increase the share of beans allocated to earlier as the delay between earlier and later increases. Appendix Table C.1 presents an overview of all subject allocations, including the percentage of corner solutions, for each of the choice sets and each interest rate.

Panel (a) of Figure 3 suggests that there is no evidence for aggregate present bias. When comparing the two choice sets with delay $k = 14$ and different front-end delay t, one would expect present-biased subjects to allocate a larger fraction of their budget to earlier when $t = 1$. On aggregate this does not appear to be the case.

For a more rigorous test of aggregate present-bias I follow the parametric assumptions of Andreoni and Sprenger (2012) and Augenblick et al. (2015) to structurally estimate aggregate time preference parameters. I assume CRRA utility and, in line with the previous literature, make varying assumptions about background consumption. Appendix Section C.5 provides a detailed discussion of identification and estimation via maximum likelihood. Appendix Table C.2 presents parameter estimates recovered through non-linear combinations of regressions coefficients from a two-limit Tobit. (β-δ) parameters are precisely estimated and relatively stable across the three assumptions over background consumption, while estimated CRRA curvature varies more strongly with background consumption assumptions. Assuming no background consumption, I estimate present bias $\hat{\beta} = 1.006$ (SE 0.018) and weekly discounting $\hat{\delta} = 0.959$ (SE 0.008). Utility is concave but with relatively limited curvature at $\hat{\alpha} = 0.886$ (SE 0.006). This suggests that money is reasonably fungible between payment dates.

Importantly, in all specifications I fail to reject the null hypothesis of $\beta = 1$. Overall, aggregate parametric results for time preference and utility curvature closely mirror previous estimates by Andreoni and Sprenger (2012), Augenblick et al. (2015), Balakrishnan et al. (2017), and Giné et al. (2017).

Having established that subject choices are consistent with the law of demand and that parametric estimates of aggregate time preferences are in line with the literature, I can now turn to individual-level time preference estimates. I use the same parametric assumptions as for the aggregate estiamtes to estimate two-limit Tobit regressions for each subject. As before, I make three different assumptions about background consumption. Table 2 summarizes the distribution of parameter estimates for individual-level discount parameter $\hat{\delta}_i$, present-bias parameter $\hat{\beta}_i$, and CRRA curvature $\hat{\alpha}_i$. Appendix Figure C.9 plots the correlation of present bias and discounting for all three assumptions over background consumption. As in Andreoni and Sprenger (2012) for ease of exposition I focus the rest of my analysis on the assumption of no background consumption (Table 2, panel a).

First, the estimation strategy appears to produce reasonable parameter estimates for most subjects, though 21.32 percent of $\hat{\beta}_i$ estimates fall out of the range of $[0.75, 1.5]$. While this share of extreme values is larger than comparable estimates in the literature based on laboratory subjects, it is in line with previous work from the field. Using a different estimation strategy and time preferences over work effort, Andreoni, Callen, Hussain, Khan, and Sprenger (2017) find that 20.3 percent of their sample has $\hat{\beta}_i$ estimates fall out of this range. For the remainder of this paper, I follow Andreoni et al. (2017) in trimming extreme estimates. I trim the top and bottom 5 percent of the sample.[29]

Second, it is worth noting that in line with the aggregate estimates, the individual-level parameter indicate that neither the mean subject nor the median subject are present-biased. CRRA utility curvature for the median subject is different from 1, but relatively close to linear. This differs from previous experimental estimates that do not use the CTB method, which find significantly more curvature Andersen et al. (2008), but is in line with previous estimates based on the CTB method (Andreoni & Sprenger, 2012; Augenblick et al., 2015).

While there is no theoretical reason for using a binary measure of present bias, most of the literature has done so because experimental results did not permit parameter estimation (Ashraf et al., 2006; Meier & Sprenger, 2010). To compare my estimates with existing work and facilitate presentation of results, I will define an indicator for present bias, which takes the value 1 if $\hat{\beta}_i < 1$ and 0 otherwise. I find that 37.8 percent of all subjects are present-biased. This shares is in line with proportions reported in the literature.[30]

[29] This is more conservative than Andreoni et al. (2017), who trim the top and bottom 1 percent of their sample.

[30] Augenblick et al. (2015) report that 33 percent of subjects have $\hat{\beta}_i < 0.99$. In my data, 36.5 percent of subjects have $\hat{\beta}_i < 0.99$.

5. Empirical Analysis

5.1. Sample Description and Comparison to National Household Survey

Table 1 reports baseline means and standard errors on the random sample of 460 workers (column 2). It also reports selected demographic data from firm personnel records on the workers that were randomized out of the sample (column 1). On the three observable characteristics available, workers that were randomized out of participation are not significantly different from workers who participated in the study.

As reported in more detail in Section 2 above, workers in the sample are exclusively female and tend to be young, low-skill, rural-urban migrants with little to no previous work experience. Table 1 presents a comparison of the study sample with household survey data from women in the same age range in Addis Ababa (column 3), the wider population of Addis Ababa (column 4) and Ethiopia (column 5) based on the 2015-16 Ethiopian Socioeconomic Survey (ESS). The ESS is representative for Addis Ababa and at the national level.[31] Compared with women in the same age range in Addis Ababa, workers in the study sample have less education (completed 6th grade versus completed 10th grade), fewer are married (19 percent versus 30 percent), and slightly fewer identify as Ethiopian Orthodox (73 percent versus 77 percent).

In terms of living standards measured by consumption and asset ownership (Table 1, panel b), the study sample is poorer than women in the same age range in Addis Ababa. Workers in the sample spend 129.88 birr (US$ 4.72) per person per week on food consumption, compared to 158.03 birr (US$ 5.74) for the average women in the same age range in Addis Ababa and 149.39 birr (US$ 5.23) for the average person in Addis Ababa. For comparison, the local food poverty line in Addis Ababa is 109.66 birr (US$ 3.98) per person per week.[32] Workers in the study sample also live in households that are notably poorer in assets as measured by a simple additive index. Section B.4 provides details on household asset data in my sample.

[31] I use publicly available microdata from the World Bank's Living Standards Measurement Survey (LSMS) program (Central Statistical Agency and World Bank, 2017). While sampling methods and survey protocols are inherently different from my survey, I aimed to harmonize concepts across survey instruments. The age range of the study population and for column 6 of Table 1 is 18 to 31.

[32] For further comparison, total average consumption expenditures at the national level in the 2015-16 ESS was 246.16 birr per person (US$ 8.94). Food consumption from the 2015-16 ESS is adjusted for inflation using the national GDP deflator. Recall period in both survey instruments is seven days. The ESS food aggregate is adjusted by adult equivalent household size. The poverty line is based on the official 2015/16 food poverty line of 3,772 birr per adult equivalent per year, adjusted to current values using the GDP deflator and adjusted for local prices using the spatial food price indices reported in National Planning Commission (2017).

5.2. Correlations of Present Bias with Savings and Job Search

Results are presented in three subsections that follow the predictions derived from the theoretical framework in Section 3. Each subsection includes a range of robustness checks. In a fourth subsection I discuss alternative explanations.[33]

Simple correlations between present bias and the outcome of interest may be biased if present bias is correlated with omitted individual characteristics such as work experience, liquidity, cognitive, and non-cognitive factors that may affect the outcome of interest. Therefore I try to control for a wide range of individual differences. I include controls in four groups, measured in the baseline interview when workers join the firm: Personal characteristics and human capital, liquidity and access to finance, cognitive control, and non-cognitive skills and stress.

Personal characteristics include respondent age, an indicator for respondent marital status, an indicator for whether the respondent has children, an indicator of whether the respondent is a rural-urban migrant, a set of indicators for the respondent's mother tongue and religion, an indicator for whether the respondent has a working spouse, and indicators for the highest education level of the worker. Access to finance includes an indicator for whether the respondent holds any savings at baseline and indicators for how difficult the respondent would find it to take out a small loan to cover an emergency. Cognitive control governs impulse control and affects how well individuals can formulate, maintain, and execute plans and goals. I measure cognitive control with a fully-incentivized spatial Stroop task that respondents complete on a tablet computer. Non-cognitive skills and stress are measured with scores on three psychological scales: Generalized Self-Efficacy, Locus of Control, and the Perceived Stress Scale. Appendix B provides details on the survey and explains the construction of these variables. Summary statistics on all variables used in the empirical analysis are presented in Appendix Table E.2.

Present Bias and Savings

Prediction 1 states that asset accumulation is increasing in present bias parameter β. Holding everything else constant, present-biased workers should save less. We can use data from the baseline CTB experiment and follow-up surveys for a reduced-form test of this prediction.

[33] I filed a pre-analysis plan (PAP) with the AER RCT Registry (#AEARCTR-0002555) after piloting the survey instrument and before any data was collected. The current paper deviates from the PAP in several ways. The PAP was build on the assumption of having daily attendance and productivity data. The firm could unfortunately not provide in time for the analysis. The PAP excluded savings and focused on correlations of present bias with job search effort and labor supply. The current paper does not consider the administrative data but instead includes self-reported savings in the analysis. The full administrative data is analyzed in a companion paper (Hardy et al., 2018).

Data on savings per week is a non-negative response variable with a continuous distribution over strictly positive values and most observations at a corner of zero. To take this into account, I model the participation decision and the quantity decision jointly in a latent regression model, estimated using Tobit (Wooldridge, 2010). I pool observations from all follow-up calls and include dummies for each panel wave. To ease interpretation of results, I use a binary measure of present bias, which takes the value 1 if $\hat{\beta}_i < 1$ and 0 otherwise.

The estimation problem can be written as

$$Y_{i,t} = \gamma_0 + \gamma_1 \mathbb{1}_{\{\hat{\beta}_i < 1\}} + \gamma_2 \hat{\delta}_i + \sum_{s=0}^{8} \kappa_s \mathbb{1}_{\{t=s\}} + \theta' \mathbf{X}_i + \epsilon_{i,t} \tag{9}$$

where for each subject i in period t, $Y_{i,t}$ is the amount of money saved over a seven-day period, $\mathbb{1}_{\{\hat{\beta}_i < 1\}}$ is an indicator for whether subjects are present-biased in the baseline CTB experiment, $\hat{\delta}$ is the discounting parameter from the baseline CTB experiment, $\mathbb{1}_{\{t=0,..,8\}}$ are indicators for each survey wave, $\mathbf{X}_i$ is a vector of respondent observable characteristics and indicators for the enumerator who administered the survey, and ϵ is the error term clustered at the individual level.

Columns 1 and 2 of Table 3 present maximum likelihood estimates of Equation (9) with and without controlling for the full set of baseline covariates. I report coefficient estimates, which in the Tobit model give the marginal effect of each independent variable on the expected value of the latent variable.

Baseline present-bias appears to reduce savings in line with the theoretical prediction, though the effects are small and not statistically significant at any conventional level. This is true irrespective of whether or not I control for the full set of baseline characteristics. To assess the economic significance of the coefficient one can calculate the average marginal effect of present bias on the expected value of savings. Holding all else constant and using coefficient estimates from column (2) that includes the full set of baseline covariates, present-biased individuals save about 7.7 percent less per week than time-consistent individuals. The predicted savings per week of present-biased individuals is 83.58 birr (US$2.93) compared to 90.57 birr (US$ 3.26) for time-consistent individuals, an insignificant difference of 6.99 birr, $\chi^2(1, N = 1,832) = 0.39$, $p = 0.533$. Panel (a) of Appendix Figure D.4 illustrates.

While the theoretical prediction only speaks to the causal link between present bias and savings, it is instructive to also consider consumption expenditure. In particular I consider the amount of consumption expenditures devoted to what Banerjee and Mullainathan (2010) call "temptation goods," that is goods that yield utility in the present as opposed to the future. The surveys collect consumption expenditure data on various categories of goods: food, alcoholic beverages, phone credit, transportation, clothing and shoes, soaps, cosmetics and beauty products, and gifts and donations. I categorize alcoholic beverages, clothing and shoes, and cosmetics and beauty products as temptation goods and use the sum as an alternative dependent variable of Equation (9).[34]

Table 3 columns 3 and 4 present Tobit coefficient estimates. After controlling for baseline covariates, I find no relationship between baseline present-bias and spending on temptation goods. Using the average marginal effect of present bias on the unconditional expected value of temptation good spending, I find an insignificant difference of 0.4 birr per week, $\chi^2(1, N = 2,028) = 0.00$, $p = 0.967$. Panel (b) of Appendix Figure D.4 illustrates.

Results remain qualitatively unchanged under a range of different specifications. First, I replicate the analysis above using $\hat{\beta}_i$ as continuous variable instead of an indicator of present bias. Appendix Table E.3 and Table E.4 provide coefficient estimates for savings and temptation goods spending, respectively. Second, I consider only the extensive margin, i.e. the decision to accumulate any savings or spend any money on temptation goods in any given week. I estimate Equation (9) using a probit model, Table E.5 presents estimated average marginal effects. The direction of effects remains unchanged and estimates remain statistically insignificant at any conventional level.

Present Bias and Search

Prediction 2 states that search effort is increasing in present bias parameter β. Holding everything else constant, present-biased workers should search for work less intensively. For a reduced form test of this prediction I use data from the baseline CTB experiment and my follow-up surveys. Over the whole panel of 3,041 observations, individuals report looking for work in 520 periods (390 on the job search, 130 job search while unemployed). For those 520 observations, I have detailed data on job search effort in three dimensions: Hours spent looking for work, phone calls made in order to look for work, and a subjective assessment of search intensity on a three-point scale ("not very intensively"; "intensively"; "very intensively").[35] Like for all high-frequency data in my panel, the recall period for these three measures is seven days.

Like savings data, data on job search intensity comes in the form of non-negative response variables with continuous distributions over strictly positive values and most observations at a corner of zero. To take this into account, I model the participation decision and the quantity decision jointly in a latent regression model, estimated using Tobit. I pool observations from all follow-up calls and include dummies for each panel wave. To ease interpretation of results, I use a binary measure of present bias, which takes the value 1 if $\hat{\beta}_i < 1$ and 0 otherwise.

[34] This result remains qualitatively unchanged when using the share of total consumption expenditures devoted to this group of goods (not shown).

[35] Subjects found it hard to make the subjective assessment, so I have fewer observations than for the other two measures.

The estimation problem can be written as

$$Y_{i,t} = \gamma_0 + \gamma_1 \mathbb{1}_{\{\hat{\beta}_i<1\}} + \gamma_2\,\hat{\delta}_i + \sum_{s=0}^{8} \kappa_s \mathbb{1}_{\{t=s\}} + \theta' \mathbf{X}_i + \epsilon_{i,t} \tag{10}$$

where for each subject i in period t, Y is measured job search effort, $\mathbb{1}_{\{\hat{\beta}_i<1\}}$ is an indicator for whether subjects are present-biased in the baseline CTB experiment, $\hat{\delta}$ is the discounting parameter from the baseline CTB experiment, $\mathbb{1}_{\{t=0,..,8\}}$ are indicators for each survey wave, $\mathbf{X}_i$ is a vector of respondent observable characteristics and indicators for the enumerator who administered the survey, and ϵ is the error term clustered at the individual level. Table 4 presents maximum likelihood estimates of Equation (10) with and without controlling for the full set of baseline covariates. As before, I report Tobit coefficient estimates.

I find that baseline estimates of present bias (as well as individual discounting) are statistically and economically significant predictors of subsequent job search behavior. In line with prediction 1, present-biased individuals spend significantly fewer hours looking for work and make significantly fewer phone calls. While the sign on the subjective assessment of search intensity is also in line with the prediction, I cannot reject a null effect at any conventional level of significance.

To assess the magnitude of the coefficients I calculate the average marginal effects (average partial effects) of present bias on the expected value of the observed outcome.[36] All else constant, present-biased individuals make on average 0.32 calls per week while time-consistent individuals make 0.68 calls per week, a significant difference of 0.36 calls, $\chi^2(1, N = 1,939) = 9.87$, $p = 0.0017$. Present-biased individuals spend on average 37.2 minutes on search, compared to 84.6 minutes for time-consistent individuals, a significant difference of 47.4 minutes, $\chi^2(1, N = 1,939) = 10.51$, $p = 0.0012$. Finally, present-biased individuals report a subjective intensity of search of 1.62 on a three-point scale from 1 (“not very intensively”) to 3 (“very intensively”), while time-consistent individuals report an intensity of 1.6, an insignificant difference of 0.02, $\chi^2(1, N = 320) = 0.02$, $p = 0.8757$.[37] Appendix Figure D.5 illustrates the average marginal effects for all three outcome variables.

Results are robust to different specifications. First, I replicate the analysis with $\hat{\beta}_i$ as continuous variable instead of an indicator of present bias. Results remain qualitatively unchanged (Appendix Table E.6). $\hat{\beta}_i$ remains a significant predictor of search intensity measured in the

[36] For each observation, I calculate the difference in expected values at both values of the present-bias indicator while keeping all other covariates unchanged. The average difference over all observations gives the average marginal effect. Wooldridge (2010) provides an exposition of how to estimate average marginal effects of binary variables in a Tobit model.

[37] Alternatively, one can calculate the average marginal effect of present bias conditional on individuals searching for work: Present-biased individuals who search make 3.04 calls while time-consistent individuals who search make 3.54 calls, a significant difference of 0.5 calls, $\chi^2(1, N = 1,939) = 10.15$, $p = 0.0014$. Similarly, present-biased individuals who search spend 338.4 minutes per week, compared to 402.6 minutes for time-consistent individuals, a significant difference of 64.2 minutes per week, $\chi^2(1, N = 1,939) = 10.89$, $p = 0.001$.

number of calls and the time spent per week, though I am marginally less powered to detect effects. The results also hold on the extensive margin. Second, I consider only the extensive margin and abstract from search intensity. I estimate Equation (10) using a probit model for the decision to look for week in any given week. Table E.7 presents results. All else constant, present-biased individuals have a 11.86 percent predicted probability of looking for work, while time-consistent individuals a 20.66 percent predicted probability, a significant difference of 8.8 percentage points, $\chi^2(1, N = 1,919) = 8.84$, $p = 0.0029$.

Even though the theoretical framework is silent on the outcomes of job search, it is instructive to assess empirically whether the negative correlation between present bias and search effort also holds for the relationship between present bias and search outcomes. I focus on two outcome measures from my panel data: The number of offers generated by individuals who search and an indicator for voluntary departures from the firm.[38] The estimation problem can be written as

$$\begin{aligned} Y_{i,t} =& \gamma_0 + \gamma_1 \mathbb{1}_{\{\hat{\beta}_i<1\}} + \gamma_2\,\hat{\delta}_i + \gamma_4\,C_{i,t} + \gamma_5\,H_{i,t} + \gamma_6 I_{i,t} \\ & + \gamma_7 II_{i,t} + \sum_{s=0}^{8} \kappa_s \mathbb{1}_{\{t=s\}} + \theta' \mathbf{X}_i + \epsilon_{i,t} \end{aligned} \tag{11}$$

where for each subject i in period t, Y is one of two search outcomes, $\mathbb{1}_{\{\hat{\beta}_i<1\}}$ is an indicator for whether subjects are present-biased (with $\hat{\beta} < 1$) in the baseline CTB experiment, $\hat{\delta}$ is the discounting parameter from the baseline CTB experiment, $C_{i,t}$ is the number of calls made in search of a job in the same period, $H_{i,t}$ is the number of hours spent on job search in the same period, $I_{i,t}$ and $II_{i,t}$ are indicators for (very) high job search intensity in the same period based on a subjective assessment, $\mathbb{1}_{\{t=0,..,8\}}$ are indicators for each survey wave, $\mathbf{X}_i$ is a vector of baseline controls, and ϵ is the error term clustered at the individual level. I estimate Equation (11) separately for each of two outcome measures. When the outcome variable is the number of offers generated in the same period, I use a Poisson regression. When the outcome variable is an indicator for a voluntary departure in the same period, I use a probit model. Appendix Table E.8 presents maximum likelihood estimates with the same set of controls as in Table 4. For the probit model I report average marginal effects. For the poisson model I report incidence-rate ratio estimates.

Two results are worth highlighting. First, baseline present bias is associated with fewer job offers generated. This relationship, however, only significant when controlling for job search effort in the same period. Second, present-biased subjects are significantly more likely to depart from the firm voluntarily. This relationship holds when controlling for contemporaneous job search effort.

[38] While the survey also asked for wages of job offers generated through search, respondents only reported wages for 65 offers.

Present Bias and Turnover

Prediction 3 states that the survival rate at the firm is decreasing in present bias β. In the theoretical framework, this is because the probability of leaving the firm depends on the probability of receiving an alternative wage offer, and thus directly on search effort. Given that present bias undermines search, present-biased individuals should exhibit a higher rate of survival at the firm.

As a first step, I graphically assess exit rates from the firm. I estimate separate Kaplan-Meier survival functions for present-biased workers with $\hat{\beta} < 1$ and time-consistent workers with $\hat{\beta} \geq 1$ (Figure D.6). Visual inspection of the survival estimates suggests that present-biased workers have a higher rate of survival at the firm in line with prediction 3.

For a more rigorous test that controls for other covariates, I use the Cox (1972) partial likelihood method for the proportional hazard model. Consider the hazard that worker i leaves the firm after t days of work as

$$h_{i,t} = h_0(t)\ \exp\left(\gamma_1 \mathbb{1}_{\{\hat{\beta}_i<1\}} + \gamma_2\, \hat{\delta}_i + \gamma_3\, C_{i,t} + \gamma_4\, H_{i,t} + \theta' \mathbf{X}_i\right) \tag{12}$$

where $h_0(t)$ is the baseline hazard, $\mathbb{1}_{\{\hat{\beta}_i<1\}}$ is an indicator for whether subjects are present-biased (with $\hat{\beta} < 1$) in the baseline CTB experiment, $\hat{\delta}$ is the discounting parameter from the baseline CTB experiment, $C_{i,t}$ is the number of calls made in search of a job in the same period, $H_{i,t}$ is the number of hours spent on job search in the same period, and $\mathbf{X}_i$ is a vector of baseline controls. I do not include subjective assessments of job search intensity due to the small number of observations. The main focus of the analysis is parameter γ_1, which measures the correlation of baseline present bias with the hazard of leaving the firm after t days.

Table 5 presents maximum likelihood estimates of the exponentiated coefficients (hazard ratios) from Equation (12). Columns 1 and 2 only include time preference parameters with and without the same set of controls as above, columns 3 and 4 examine the role of savings and job search in isolation. Columns 5 and 6 combine time preferences and job search effort. My preferred specification for a reduced-form test of prediction 3 is presented in column 2.

In line with the theoretical prediction, present-biased individuals have a lower hazard of leaving the firm. The result is statistically significant and economically large. All else equal, the risk of leaving the firm is 52.3 percent as high for present-biased individuals as it is for individuals who are not present-biased. If this effect operates through search effort, as it does in the theoretical framework, we would expect increased search effort to lead to a higher hazard of leaving the firm. We would also expect that the effect of present bias on the hazard of leaving should be less pronounced when controlling for search effort. Both appear to be the case. Search effort measured in hours and in the number of phone calls indeed significantly increases the hazard of leaving the firm (column 4). One more phone call per week increases the hazard of leaving the firm by 20.1 percent. When including both time preferences and search effort, the coefficient on baseline present bias becomes insignificant.

5.3. Alternative Explanations

The analysis above already included a number of robustness checks and showed that results are robust to the inclusion of a broad range of observable characteristics. In this section, I discuss alternative explanations.

Do Individual Characteristics, Liquidity, and Environmental Factors Explain Experimental Responses?

A focus of this study was to elicit time preferences in a tightly-controlled experiment that eliminates confounders commonly found in the literature (see discussion in Section 4.2). Nevertheless, the correlations presented above may be biased if experimental responses are systematically driven by personal characteristics, liquidity,[39] or environmental factors outside the experiment. To assess if this is the case, I regress an indicator for estimated present bias ($\hat{\beta}_i < 1$) and the estimated present bias parameter $\hat{\beta}_i$ on a range of observable characteristics.

Appendix Table E.9 presents regression coefficient estimates from probit and OLS regressions. I find that estimates of present bias are not significantly predicted by most personal observable characteristics. Importantly, neither human capital measured by previous work experience and education, nor access to finance predict estimated present bias.[40]

In addition to the correlations presented here, I can use randomized experimental payouts from the baseline experiment to assess whether liquidity drive time preference estimates from the endline experiment three months later. Recall that in the convex time budget experiment, individuals have a 50 percent chance of winning their payouts, determined with a coin flip at the end of the experiment. At 20 to 40 percent of the monthly wage, experimental payoffs represent a large cash drop that provides exogenous variation to liquidity. I regress an indicator for estimated endline present bias ($\hat{\beta}_i < 1$) and the estimated endline present bias parameter

[39] Cassidy (2018) uses the MPL approach to show that present bias elicited from poor subjects may in fact represent a rational, time-consistent response to liquidity constraints.

[40] Two additional empirical findings are worth highlighting. First, respondent age is negatively correlated with the estimated present bias parameter $\hat{\beta}$, but not a significant predictor of an indicator of present bias. This stands in contrast to Meier and Sprenger (2010), who find that age is a negative predictor of a present bias indicator. Second, higher cognitive control as measured by the spatial Stroop-like task is a significant positive predictor of the estimated present bias parameter $\hat{\beta}$. This is consistent with findings in a recent literature that has investigated the link between cognitive control, or *bandwidth* more generally, and time preferences (Haushofer & Fehr, 2014; Mani, Mullainathan, Shafir, & Zhao, 2013; Schilbach, Schofield, & Mullainathan, 2016).

$\hat{\beta}_i$ on an indicator of whether respondents won their experimental payout at baseline and a range of observable characteristics.

I do not find evidence that experimental responses at endline change as a result of winning the sizable experimental payout at baseline. Appendix Table E.10 presents regression coefficient estimates from probit and OLS regressions. While the results indicate that liquidity do not appear to have affected endline present bias, individual liquidity three months after the beginning of the study is likely different than at the beginning of the study.

Does Liquidity Explain Search Effort?

In the previous subsection, I provided evidence that individual liquidity does not appear to explain experimental responses. In this subsection, I assess whether financial wealth and liquidity affects search effort.

While my theoretical framework holds initial wealth constant, the effect of liquid savings on search effort isÂătheoretically ambiguous. Lentz and Tranæs (2005) show analytically and with simulations that job search effort is negatively related to initial wealth under the assumption of additively separable utility. This negative relationship is consistent with empirical results in which unemployment spells are positively correlated with initial wealth holdings, e.g. in Algan et al. (2003) and Bloemen and Stancanelli (2001). In my context, however, it is also possible to imagine that liquidity-constrained individuals are not able engage in costly search. If liquidity-constrained subjects were to appear more present-biased, as in Cassidy (2018) but in contrast to the evidence presented above, and if liquidity-constrained subjects also searched less, this would explain the correlational patterns that I find above.

I can assess the link between liquidity and search using randomized experimental payouts from the baseline convex time budget experiment. I do so in two steps. I first show that subjects who won the experimental payouts and those who did not appear balanced on observable characteristics (Table E.11). There is no significant difference between both groups, except that slightly more participants who are native Oromiffa speakers won the coin flip. In a second step, I use the same latent regression model as in Equation (10) to study the correlation of present bias and search effort, but add an indicator for whether the subject won or lost the coin flip. Given that participants appear balanced on observables, I interpret the coefficient estimate for this indicator as the causal effect of experimental payouts on search. Table E.12 presents maximum likelihood estimates of Equation (10) with and without controlling for the full set of baseline covariates. As before, I report Tobit coefficient estimates.

Two findings are worth highlighting. First, winning the experimental payouts causes significantly less search. Second, the significant negative correlation between measured present bias and search effort holds up. The results are consistent with a negative relationship between liquidity and search effort. Subjects with more liquidity search less. Taken together, this suggests that liquidity constraints are unlikely to explain my results above.

Does Human Capital Explain Search Effort?

Individuals tend to choose job search effort in response to economic incentives. In particular, job search effort has been found to increase in the expected returns to search (Christensen, Lentz, Mortensen, Neumann, & Werwatz, 2005). As a result, one would expect that workers with higher earnings potential, i.e. larger human capital, are more likely to search or search more intensively. While I already showed that proxies of human capital do not appear to predict measured present bias, it is worth investigating more explicitly if measures of human capital are positively correlated with search effort.

I run a Tobit regression of job search effort (measured in number of calls and time spent on search) on respondent age, an indicator for whether the individual has any formal work experience, a set of indicators for the highest education level completed, and measures of cognitive control and non-cognitive skills.[41] Appendix Table E.13 provides regression coefficient estimates. Age, which I control for in all results above, is the only significant predictor of search effort. Without controlling for cognitive control and non-cognitive skills, education is a significant predictor of job search only for those individuals who have completed more than grade 10 schooling.

The Role of Reservation Wages

My theoretical and empirical analysis so far has abstracted from workers' reservation wage choices. In the model of DellaVigna and Paserman (2005), individuals with a higher (exponential) discount factor set a higher reservation wage while present bias should essentially be orthogonal to the reservation wage. My data allows for a reduced-form test of this prediction. At each round of the panel, I record self-reported reservation wages using a similar question to the one used by Krueger and Mueller (2016) and in the May 1976 US Current Population Survey.[42]

[41] Abebe et al. (2016) report that young job seekers in Addis Ababa may find it hard to signal their skills and, as a result, firms often use criteria such as whether workers have any previous work experience.

[42] The question text, translated from Amharic, was: "Suppose someone offered you a job today. What is the lowest monthly pay after taxes that you would accept for the type of work you were looking for?" The question in the 1976 CPS was: "What is the lowest wage or salary you would accept (before deductions) for this type of work?."

Appendix Table E.14 reports coefficient estimates from an OLS regression of log self-reported reservation wage on baseline time preference parameters and the same set of controls as before. Neither present bias nor the estimated discount parameter are significant predictors of reservation wages. The findings are in line with the empirical results of DellaVigna and Paserman (2005) and Krueger and Mueller (2016), who do not find evidence that time preferences affect the choice of the reservation wage. This indicates that the reservation wage choice is unlikely to play a large role in the context of this study.

Reliability of Self-Reported Data on Search Intensity

The analysis above hinges on self-reported data on job search effort. The little existing work that analyzes high-frequency data on job search behavior either uses self-reported survey data similar to mine (Faberman, Mueller, Şahin, & Topa, 2017; Krueger & Mueller, 2016), observational data under highly controlled conditions (Belot, Kircher, & Muller, 2018) or administrative data from online job boards (Faberman & Kudlyak, 2018). In the setting of my study, self-reported data is the only feasible option. It is worth asking if this data is reliable.

First, it is important to note that if questions on job search intensity are affected by experimenter demand or Hawthorne effects, these effects would only confound my results if they are systematically correlated with results from the experimental elicitation of time preferences. Experimenter demand effects occur when respondents systematically alter their answers based on what they believe constitutes desirable or appropriate behavior (Zizzo, 2010). Neither the individuals participating in the experiment nor the team that implements the survey is aware of the research questions. All throughout the experiment, I take care to not provide cues to the respondents. Overall, it is not obvious why individuals categorized as present-biased would report systematically lower job search intensity.

Second, because individuals likely find it difficult to exactly quantify how much they look for work in a given week, I use three different measures of intensity (Cronbach's $\alpha = 0.733$). I show results separately for each dimension. Results also hold when using an aggregate measure of all three dimensions generated from factor analysis where I retain the first factor.[43] While all three measures likely suffer from measurement error, it is improbable that this measurement error is systematically correlated with results from the experimental elicitation of time preferences.

Third, the results on turnover do not require self-reported data on search. Firm personnel records indicate that workers truthfully report tenure. It is not clear how the results on tenure could be rationalized in the absence of a search channel.

[43]Not shown, results available upon request.

Taken together, it appears unlikely that systematic biases in search intensity data are a significant driver of the results presented above.

6. Conclusion

Policymakers in Ethiopia and other low-income countries have promoted labor-intensive light manufacturing as an opportunity to generate a large number of formal employment opportunities. For the low-skill rural-urban migrants in this study, industrial work represents a stepping stone into the formal labor market of Addis Ababa. My results suggest that self-control problems in the form of present bias significantly undermine the ability of workers to use these jobs as such a stepping stone.

I show that present bias is a significant predictor of reduced job search effort over a period of three months after starting a low-skill industrial job in peri-urban Addis Ababa. Present-biased workers search less and – as a result – generate fewer alternative job offers, and stay at the firm significantly longer. My results offer the first experimental evidence of a theorized link between present bias and job search effort. I do not find evidence for a link between present bias and reduced savings.

An immediate implication of my findings is that individuals looking for work might benefit from policies or devices that commit their future selves to more search. Whether and under what conditions such a commitment device can be welfare-improving depends on the exact welfare criterion, which is not obvious to define when we observe two individual choices that are in conflict with each other. It is particularly difficult in the context of this study, where individuals likely have imperfect information about their own future efficiency in searching for work. It is easy to imagine how individuals could under-estimate how physically and mentally demanding their new job will be and thus over-estimate how easy it will be for them to set aside time to search for alternatives.[44]

What could a potential commitment device for job search look like? For a discussion, it is useful to consider the difference between hard commitment devices, which involve real economic costs, and soft commitment devices, which mainly work through psychological costs (Bryan et al., 2010). Search effort is hard to monitor directly in the study context, so it is difficult to imagine how the market could provide a hard commitment device that directly contracts on workers' search effort or search outcomes. Through focus group interviews with workers in my sample, I identified three feasible alternatives.[45]

[44] A related design challenge derives from the balance between flexibility and commitment (Amador, Werning, & Angeletos, 2006). For example, an individual taking up a commitment savings plan could demand a provision to withdraw from the contract in case of a medical emergency. The conditions for flexibility are difficult to specify in the context of this study.

[45] Detailed notes are available upon request.

A first option would be a soft commitment device in the form of a personal plan for job search. With such a plan, workers could formalize intentions for search and possibly set individual rules for how much to search, when to search, and how to search. This plan could be implemented through a small notebook that workers can carry. This type of soft commitment device builds on a recent literature in economics and psychology, which has found that prompting people to form concrete implementation plans can increase follow-through (Beshears, Milkman, & Schwartzstein, 2016; Milkman, Beshears, Choi, Laibson, & Madrian, 2011).[46] Abel, Burger, Carranza, and Piraino (2017) show that prompting unemployed South African job seekers to make a plan increases the number of applications and diversified search strategies. Because the plan is unenforced, it avoids the challenge of monitoring search effort. Three months after starting their job, 96 percent of individuals in my sample indicated that they would be interested in such a planning device. 74 percent indicated a positive willingness to pay in an unincentivized question.[47]

A second alternative would be a soft and informal commitment device that operates through social pressure in small groups of women. Individuals indicated that beyond social pressure, they would benefit from exchanging knowledge about search in smaller groups. In the context of savings, Kast, Meier, and Pomeranz (2018) test the impact of peer groups with publicly-announced goals and peer monitoring on individual savings. They find large effects on the number of deposits and the savings balance of individuals. 95 percent of individuals in my sample indicated their interest in such groups, 69 percent indicated a positive (unincentivized) willingness to pay.

A third option would be a hard and formal commitment device that operates through advance payments to a job broker. 9 percent of workers currently rely on job brokers to assist with search. A formal commitment device could involve an upfront payment to a broker or a fixed fraction of the monthly wage to be paid to job broker. 58 percent of individuals in my sample indicated their interest in such an arrangement.

Overall, my results suggest several avenues for future work. First, future research could use exogenous variation to establish a causal link between present-biased preferences and job search effort. A field experiment that tests potential commitment devices for job search, building on the list above, is a natural starting point. This will require addressing difficult questions around welfare implications. Second, the study of commitment prompts the question of whether workers are aware of their self-control problems or not. The empirical analysis above did not consider to what extent workers are aware of their own self-control problems. Third, the null result on savings behavior merits further analysis.

[46] See Bénabou and Tirole (2004) for a theoretical perspective from economics on how unenforced personal plans can serve as a commitment device.

[47] In a companion paper, we implemented a plan that aimed at increasing worker savings, not search. We provide a full evaluation in Hardy et al. (2018).

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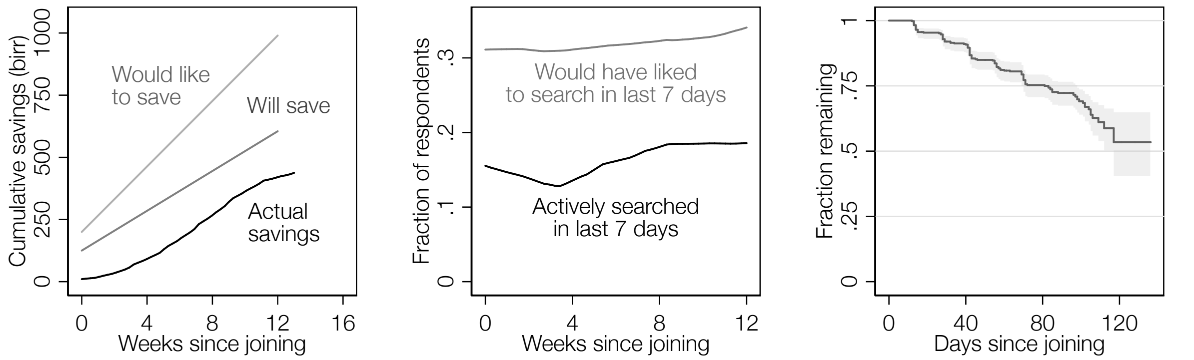
Figures

Figure 1: Job Search, Cumulative Savings, and Survival of Workers During the First Three Months at the Study Firm

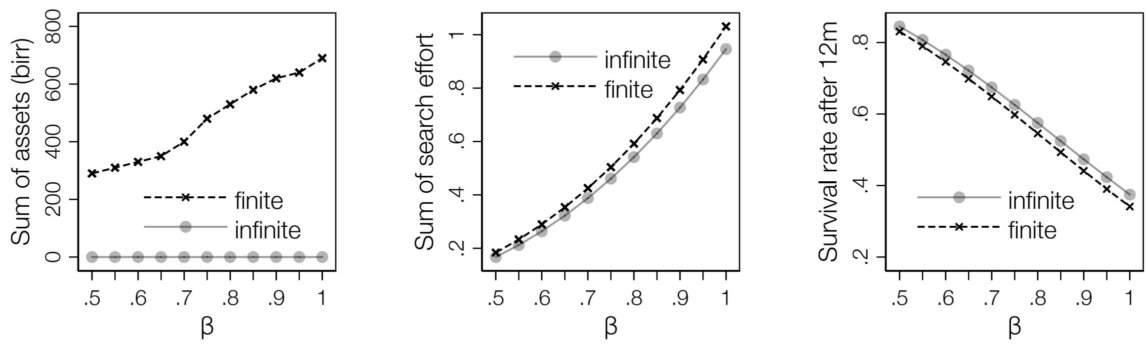
(a) Cumultative savings (birr) (b) Job search (% of sample) (c) Survival (% of sample)



Notes: Panels (a) and (b) are local polynomial smoothers at the weekly level. Panel (c) plots the Kaplan-Meier survivor function for the hazard of leaving the firm at the daily level with a 95 percent confidence interval shaded in gray. Job search intensity in panel (a) is hours of search over the past seven days. Appendix Figure D.2 plots all three dimensions of search effort data (hours, number of phone calls, and subjective intensity) and the extensive margin of search. In panel (b), "would like to save" plots the self-set savings goal set on the day that workers join the firm while "will save" refers to the amount that workers think they can realistically save. By construction both measures are linearly increasing in weeks. For comparison with savings goals, the monthly wages are 1,000 birr (month 1), 1,1075 birr (month 2), and 1,150 birr (month 3 and all months after that).

Figure 2: How Present Bias Affects Job Search, Asset Accumulation, and Survival (Simulation Results, by Present Bias Parameter)

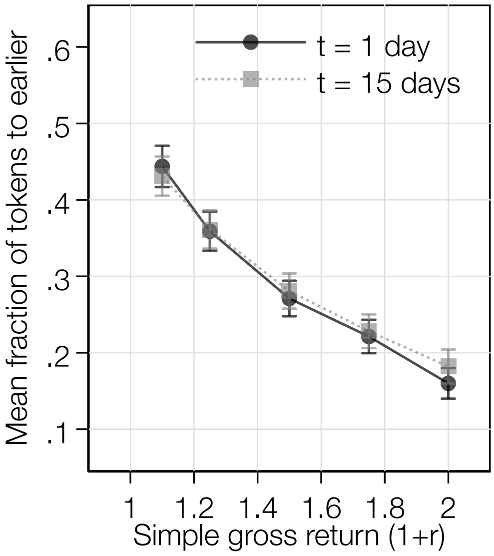
(a) Assets (b) Job search (c) Survival



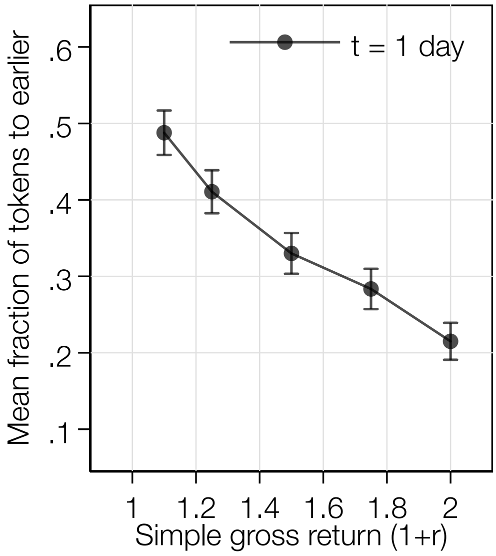
Notes: Finite refers to simulations in which workers know that they will leave the safety net firm after one year, which is equivalent to the median expected tenure in my survey data. Infinite refers to simulations in which workers assume that they never need to leave the safety net firm, so they do not have a precautionary savings motive. Panels (a) and (b) represent the sum of search effort and assets over all simulated periods. Parameter values as given in text. Figure A.1 in Appendix A.2 illustrates the optimal paths of job search and assets and implied consumption and survival for the case of no present bias ($\beta = 1$).

Figure 3: Fraction of Experimental Budget Allocated to Earlier, by Front-End Delay t and Delay Between Earlier and Later k

(a) Delay $k = 14$



(b) Delay $k = 28$



Tables

Table 1: Baseline Summary Statistics and Comparison to National Household Survey (Means and Standard Errors)

	Pure Control	Study Sample	Wider Population: Addis Ababa Young Women	Wider Population: Addis Ababa	Wider Population: Ethiopia
	(1)	(2)	(3)	(4)	(5)
Panel (a) Personal background					
Age	21.44	21.41	24.31	28.20	23.09
	(0.226)	(0.117)	(0.333)	(0.733)	(0.217)
% female	1.00	1.00	1.00	0.56	0.47
	-	-	-	(0.013)	(0.006)
Education (respondent)	8.16	8.17	10.30	6.52	2.33
	(0.135)	(0.243)	(0.496)	(0.298)	(0.078)
% married	0.16	0.19	0.30	0.25	0.26
	(0.028)	(0.018)	(0.033)	(0.012)	(0.003)
% Ethiopian Orthodox faith		0.73	0.77	0.55	0.30
		(0.021)	(0.050)	(0.038)	(0.016)
Panel (b) Living standards					
Food consumption (past 7 days)		129.88	158.03	149.39	103.95
		(6.885)	(14.21)	(11.701)	(2.662)
Non-food consumption (past 7 days)		147.93			
		(9.997)			
Asset index (household)		4.07	8.70	8.45	1.72
		(0.145)	(0.417)	(0.353)	(0.069)
Asset index (self)		1.12			
		(0.077)			
Savings (3 month recall)		384.09			
		(39.582)			
Savings (would like per month)		603.01			
		(14.885)			
Savings (will likely per month)		355.10			
		(8.729)			
N	238	460	183	1,188	27,990
Population represented (using weights)			658,336	4,507,503	117,437,134

Notes: Column 1 presents administrative data from firm personnel records for the group of workers who were randomized out of participation in the study. Columns 3 to 5 present data from the 2015-16 Ethiopian Socioeconomic Survey (ESS) collected as part of the World Bank's Living Standards Measurement Survey program. Column 3 refers to women aged 18 to 31, the age range of the study population. ESS data is weighted and stratified using the provided survey weights. The ESS is representative for Addis Ababa and at the national level. While data in all columns of the table is measured using the same concepts, comparisons between my survey and the ESS should be seen as indicative due to different sampling methods and survey protocols. Food consumption in ESS adjusted for inflation using the national GDP deflator. Recall period for consumption expenditures is seven days both in my survey and in the ESS. The ESS food aggregate is adjusted by adult equivalent household size, while the food aggregate in my survey is per capita. Comparison with non-food data not shown because of differences in survey instrument. Asset index is an additive index of 13 ESS households assets that best predict nominal total household consumption at the national level. Appendix Section B.4 for details.

Table 2: Individual-Level Time Preference Parameter Estimates from Two-Limit Tobit, by Assumptions About Background Consumption

Parameter	N	Mean	5th pctl	50th pctl	95th pctl	Min	Max
Panel (a) No background consumption $\omega_1 = \omega_2 = 0$							
Present bias $\hat{\beta}_i$	406	1.235	0.581	1.011	1.791	0.018	24.554
Discount factor $\hat{\delta}_i$	406	1.076	0.769	0.972	1.408	0.196	22.569
CRRA curvature $\hat{\alpha}_i$	406	0.645	-0.159	0.825	0.984	-7.677	6.042
Panel (b) Sample average background consumption $\omega_1 = \omega_2 = \bar{c}$							
Present bias $\hat{\beta}_i$	404	1.389	0.654	1.017	1.618	0.004	108.916
Discount factor $\hat{\delta}_i$	404	1.023	0.791	0.988	1.212	0.194	6.625
CRRA curvature $\hat{\alpha}_i$	404	0.430	-0.372	0.641	0.922	-13.751	1.640
Panel (c) Individaul background consumption $\omega_1 = \omega_2 = c_i$							
Present bias $\hat{\beta}_i$	404	1.13	0.611	1.017	1.631	0.008	12.99
Discount factor $\hat{\delta}_i$	404	1.05	0.797	0.987	1.247	0.235	20.48
CRRA curvature $\hat{\alpha}_i$	405	0.382	-0.523	0.659	0.944	-34.027	2.108

Notes: Table shows individual-level maximum likelihood estimates of Equation (17) using separate two-limit Tobit models. The three panels reflect different assumptions about background consumption at each point in time (see Equation (13) for details) and correspond to columns (1) to (3) of aggregate estimates presented in Appendix Table C.2. Panel (a) assumes no background consumption ($\omega_1 = \omega_2 = 0$). Panel (b) assumes that background consumption is constant and set at the sample average daily consumption expenditure in the seven days before the baseline survey ($\omega_1 = \omega_2 = \bar{c}$). Panel (c) assumes that background consumption is constant and set at the individual daily consumption expenditure in the seven days before the baseline survey ($\omega_1 = \omega_2 = c_i$). Appendix Figure C.9 provides a scatter plot of β_i and δ_i for the three different assumptions.

Table 3: Savings, Temptation Goods Expenditures, and Present Bias
(Regression Coefficient Estimates and Robust Standard Errors)

	Savings		Tempt. goods exp.	
	(1)	(2)	(3)	(4)
	Tobit	*Tobit*	*Tobit*	*Tobit*
Baseline $\hat{\beta}_i < 1$	-86.20	-25.91	13.85	0.670
	(58.089)	(41.836)	(15.158)	(16.063)
Baseline $\hat{\delta}_i$	445.7	84.99	-30.72	-39.16
	(342.470)	(212.468)	(89.965)	(101.663)
Survey wave dummies	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes
Personal characteristics	No	Yes	No	Yes
Baseline liquidity	No	Yes	No	Yes
Cognitive control	No	Yes	No	Yes
Non-cognitive ability and stress	No	Yes	No	Yes
N	1901	1832	2102	2028

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Time preference parameters trimmed at 5th and 95th percentile. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables. Figure D.4 plots average marginal effects of the present bias indicator in column (2) and (4).

Table 4: Job Search Effort and Present Bias
(Regression Coefficient Estimates and Robust Standard Errors)

	(1) Calls *Tobit*	(2) Calls *Tobit*	(3) Hours *Tobit*	(4) Hours *Tobit*	(5) Subjective *Tobit*	(6) Subjective *Tobit*
Baseline $\hat{\beta}_i < 1$	-2.287**	-2.963***	-4.479**	-6.130***	-0.0149	-0.0224
	(1.016)	(0.950)	(1.965)	(1.894)	(0.148)	(0.143)
Baseline $\hat{\delta}_i$	12.22**	12.41***	22.12**	22.28***	0.685	0.401
	(5.067)	(4.216)	(9.621)	(8.593)	(0.768)	(0.675)
Survey wave dummies	Yes	Yes	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
Personal characteristics	No	Yes	No	Yes	No	Yes
Baseline liquidity	No	Yes	No	Yes	No	Yes
Cognitive ability	No	Yes	No	Yes	No	Yes
Non-cognitive ability and stress	No	Yes	No	Yes	No	Yes
N	2008	1939	2008	1939	325	320
Log likelihood	-1263	-1166	-1555	-1467	-361	-327

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Time preference parameters trimmed at 5th and 95th percentile. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables. Figure D.5 plots average marginal effects of the present bias indicator in column (2), (4), and (6).

Table 5: Hazard of Leaving the Firm
(Cox Hazard Ratios and Robust Standard Errors)

	(1)	(2)	(3)	(4)	(5)	(6)
Baseline $\hat{\beta}_i < 1$	0.554^{**}	0.516^{***}			0.815	0.741
	(0.129)	(0.124)			(0.228)	(0.236)
Baseline $\hat{\delta}_i$	18.162^{***}	9.907^{**}			3.743	2.104
	(18.353)	(11.485)			(4.592)	(3.195)
Search effort, last 7 days (hours)			1.060^{***}	1.078^{***}	1.076^{***}	1.105^{***}
			(0.009)	(0.018)	(0.009)	(0.016)
Search effort, last 7 days (# calls)			1.191^{***}	1.206^{***}	1.183^{***}	1.182^{***}
			(0.025)	(0.041)	(0.026)	(0.043)
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
Personal characteristics	No	Yes	No	Yes	No	Yes
Baseline liquidity	No	Yes	No	Yes	No	Yes
Cognitive control	No	Yes	No	Yes	No	Yes
Non-cognitive ability and stress	No	Yes	No	Yes	No	No
N	1830	1767	2267	2195	1792	1732

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Table shows exponentiated coefficients. Robust standard errors in parentheses, clustered at individual level. Time preference parameters trimmed at 5th and 95th percentile. Subjective assessments of job search effort are not included as independent variable due to the small number of observations. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables.

Additional Online Appendices

Appendix A Theoretical Framework

A.1 Functional Form Specification

To simulate the model from Section 3, I make additional functional form assumptions in line with the previous literature. As in Paserman (2008) and DellaVigna et al. (2017) I use search costs of the form $k(s) = \psi s^{1+\gamma}/(1+\gamma)$ with $\gamma > 0$, where γ determines the convexity of the cost function and ψ is a scaling parameter.[48] ψ is equivalent to the cost of obtaining the outside option job with probability one. I set the curvature parameter of the search cost function $\gamma = 0.4$, taken from Paserman (2008). I assume that workers exhibit constant relative risk aversion (CRRA) and derive utility from consumption in the form of $u(c_t) = log(c_t)$.

I use survey data from my experiment to calibrate the remaining parameters of the model. Income at $t = 0$ before joining the safety net firm is set equal to the median cash income of 458 birr (US\$16.61).[49] Wages at the safety net firm then follow the wage regime of the firm in my experiment: Workers receive 1,000 birr (US\$ 36.30) in the first month, 1,075 birr (US\$ 39.02) in the second month, and 1,150 birr (US\$ 36.30) in all months after that. The income of workers who leave the firm after 12 months drops back the the pre-employment level of 458 birr. The outside option wage $\widetilde{w}$ is set to equal to 1,835 birr (US\$ 66.61), the median wage offer generated by workers after joining the experimental firm.

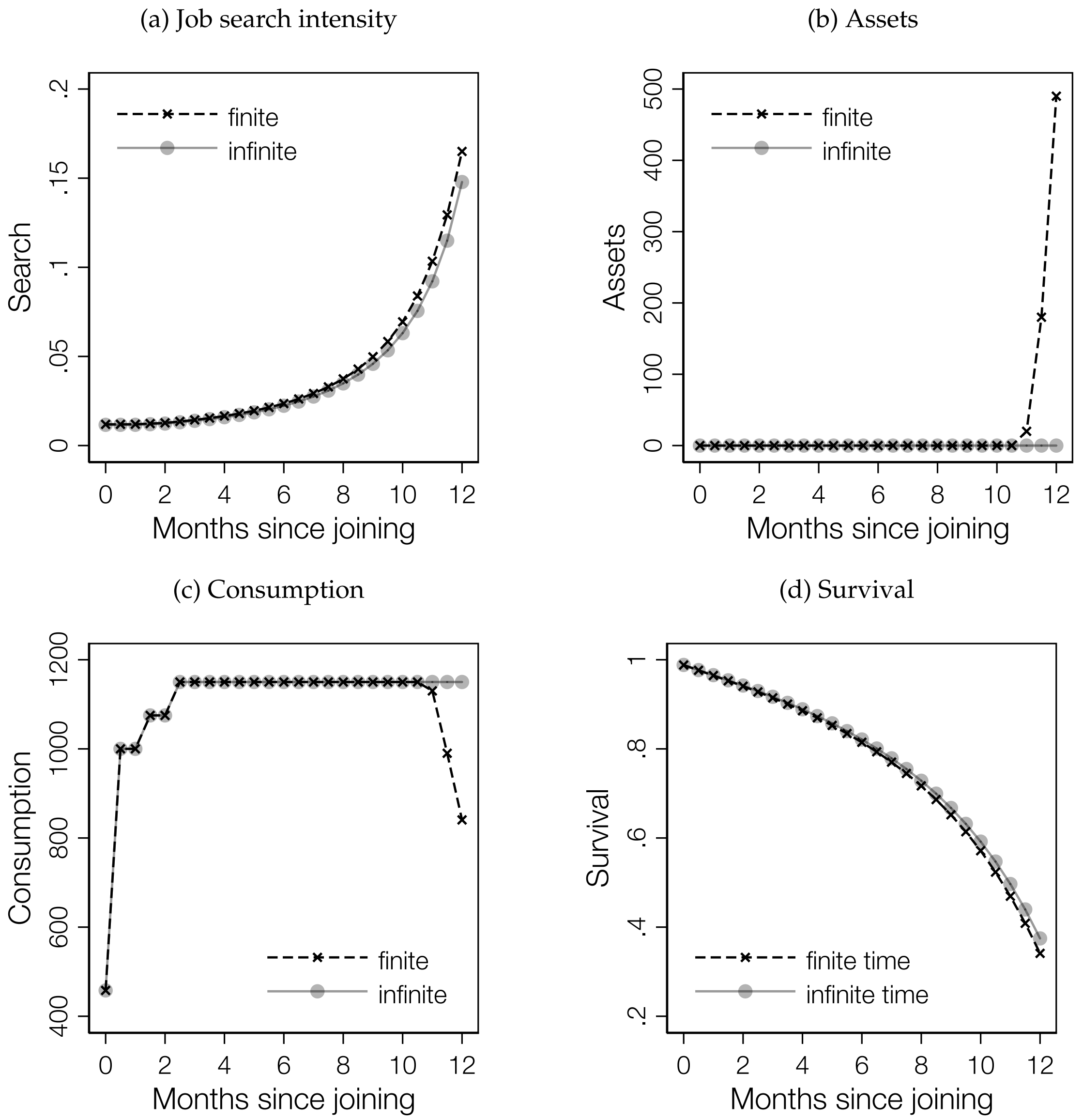
One period in my simulation represents 15 days. The discount factor δ is based on aggregate estimates from a convex time budget experiment with workers in my sample, conducted on the day before they start their job. My preferred estimate for discounting over 15 days is $\hat{\delta} = 0.92$ (SE 0.008). Assets pay a 15-day return of 0.003 based on the Ethiopian deposit interest rate of 7 percent per year.

[48]As noted by DellaVigna et al. (2017), γ is equal to the elasticity of search effort with respect to the net value of employment. To see this denote $\left[V^O_{t+1}(A_{t+1}) - V^F_{t+1}(A_{t+1})\right] = \Phi$. Using the first order condition with respect to search effort (4), rewrite $c'(s^*) = \Phi$. Plugging in the cost function above, we obtain $s^* = (\Phi/\psi)^{1/\gamma}$.

[49]The median wage income over the four weeks before joining the firm is 0 birr. 458 birr is the total cash income including transfers from family members and friends.

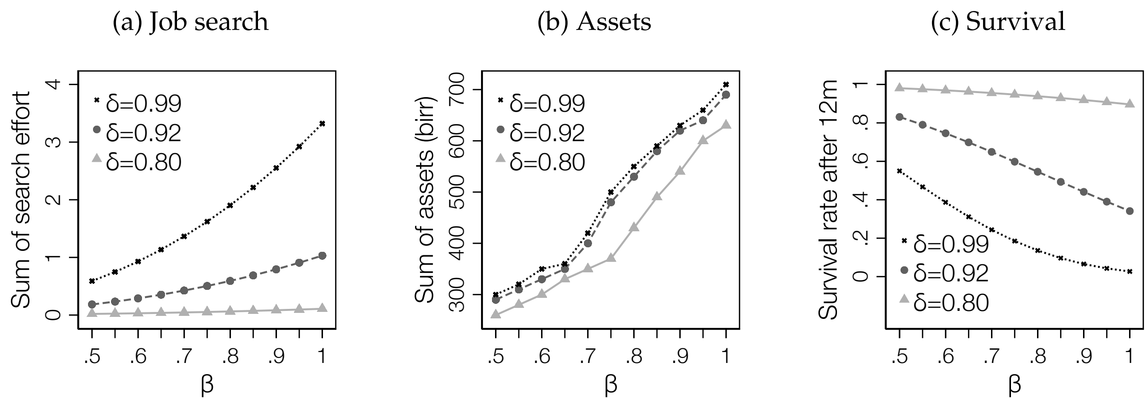
A.2 Additional Simulation Results

Figure A.1: Optimal Path of Search and Assets (Panels a and b) and Implied Consumption and Survival (Panels c and d)



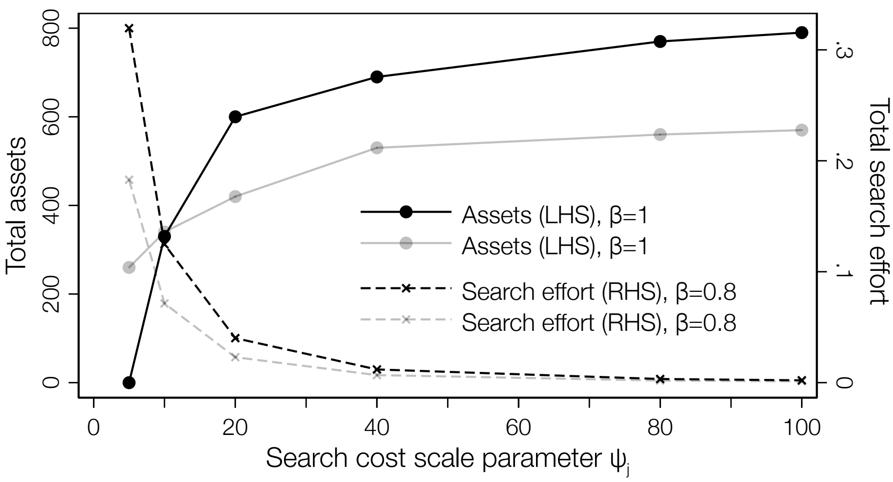
Notes: Assuming exponential discounting ($\beta = 1$). Other parameters are set as described in the text.

Figure A.2: How Discounting and Present Bias Affect Job Search, Asset Accumulation, and Survival (Simulation Results, by Present Bias and Discount Parameter, Finite Case)



Notes: Simulation results for the "finite" case in which workers know that they will leave the safety net firm after one year, which is equivalent to the median expected tenure in my survey data. Panels (a) and (b) represent the sum of search effort and assets over all simulated periods. Parameter values as given in text.

Figure A.3: How Search Cost Affects Search Effort and Asset Accumulation (Simulation Results, by Search Cost Type and Present Bias)



Notes: Sum of search effort and assets over all simulated periods. Based on simulations allowing for heterogeneity in job search costs and assuming finite tenure at the firm of one year.

Appendix B Procedures and Methods for Survey Data Collection

B.1 Survey Procedures

B.1.1 Randomization

Randomization is done using a combination of Excel spreadsheets in the survey office, ad-hoc randomization by the survey software, and a coin flip by the respondent.

Sample selection: Every evening, the field coordinator receives a list with names and contact information of the workers that were hired on that day and that are scheduled to return on the next day to begin employment. This list is entered into an Excel spreadsheet and shuffled in random order using the built-in random number generator. The random order defines whether respondents are approached for an interview at the factory, an interview at home, or are part of a control group that is not interviewed. The randomized list is distributed to all enumerators via email and hard copy. In one case the enumerator did not comply with the survey assignment and the observation was dropped.

Survey and time preference order: The questionnaire modules were administered in the same order to reduce complexity for the enumerator. The three sets of the convex time budget experiment are presented in random order based on a random number generated by the survey software.

Incentives from time preference elicitation: Subjects have a 50 percent chance of receiving one out of the 15 convex time budget allocations that they make. Whether or not respondents win is determined using a coin flip at the end of the experiment. The coin is thrown by the respondent under supervision of the enumerator. This was done to maximize transparency for respondents. If respondents win the coin flip, they are asked to make a draw from a bowl with folded cards numbered 1 to 15 that represent the choices of the experiment. Enumerators then enter the drawn number into the survey software.

B.1.2 Survey Protocols

Each respondent is randomly assigned to one of 15 enumerators, all but three of which female. Randomization to enumerators is done using the document described in the previous section. All enumerators have several years of work experience in data collection with the Ethiopian Development Research Institute (EDRI) or the Ethiopian Central Statistics Agency (CSA). The survey field coordinator and I conducted six days of study-specific training with all enumerators. In addition, all enumerators conduct training interviews with real respondents for one week. This data is discarded and enumerators are brought back for another day of training and feedback. Care is taken to minimize observer bias and enumerators do not know the specific hypotheses being tested. Training materials are available from the author upon request.

Each respondent is paired with the same enumerator throughout the study to maximize trust and reduce attrition. Informed consent, all interviews, and the time preference elicitation are administered in Amharic or Oromiffa, the most commonly spoken local languages. 67 percent of respondents in the sample report Amharic as their mother tongue, another 20 percent report Oromiffa as their first language. All workers at the firm are required to speak Amharic, but 4.8 percent of the sample still prefer to conduct interviews in Oromiffa. Enumerators are trained in both and are instructed to refuse consent if workers are not comfortable in either of the two languages.

All survey questions were carefully translated from English to Amharic. Where available, the translation was compared to official translations of survey instruments by the Ethiopian Central Statistics Agency. The survey team discussed each question in English and Amharic as a group to make sure that the meaning is correctly translated and understood by all team members. In addition, an Amharic native speaker who was not involved in the study reviewed the translations.

Data for the baseline survey, time preference elicitation, treatments, high-frequency phone survey, and endline survey is collected using computer-assisted personal interviewing (CAPI) with Android tablets running Open Data Kit / SurveyCTO. The interview GPS location and a randomly-selected 10-second audio segment of each interview are recorded for auditing purposes.

The data presented in this paper was collected from March 27 to September 28, 2018. In-person interviews are conducted seven days a week, Monday through Saturday at the firm and on Sunday in the homes of respondents. On average, the team of enumerators conducts 7 baseline interviews per day and 29 per week. Phone interviews are conducted every day, mostly in the evening when workers have returned from work. On average, enumerators conduct 108 phone interviews per week.

B.1.3 Ensuring Confidentiality

Workers may fear retaliation by the firm for example if they report that they are not planning to stay for long or that they dislike the working conditions. They may also feel the need to respond in a way that is socially or otherwise desirable. In addition to addressing these concerns during the informed consent procedures, I take a number of precautions to alleviate these concerns and maximize respondent privacy.

First, I select a random subset of workers to be interviewed at home instead of at the factory. These interviews follow the exact same protocol as the interviews in the factory, but they happen on Sundays in the privacy of the respondent's home or a safe space in the community. If workers systematically conceal the truth in work-related questions during interviews at the factory, interviews at home should give me a sense of the size and sign of the bias. Similarly, if workers interviewed at home systematically conceal the truth in personal questions (for example about intra-household allocation decisions or gender norms), interviews at the factory should help me assess the bias.

Second, the physical interview location is chosen to maximize privacy. In the factory, baseline interviews are conducted in a cafeteria not visible to other firm staff. For interviews at home, enumerators offer to meet respondents on the property of the local church – a location that is commonly seen as a safe space in the community.

Third, the team of enumerators is instructed to keep their distance from firm management. When working on factory premises, enumerators wear ID badges that identify them as not belonging to the firm.

B.2 Elicitation of Subjective Expectations

Throughout the survey when asking subjects to assess various quantities and subjective expectations I use beans as visual aids. Delavande, Giné, and McKenzie (2011) review studies that have used such visual aids and discuss advantages and disadvantages of various methods. In particular, I follow Delavande and Kohler (2009) in explicitly linking beans to probabilities.

My instructions read: *"I want to ask you one question about the chance or likelihood that a certain event is going to happen. There are 10 beans in the bowl [show bowl]. I would like you to choose some beans out of these 10 beans and put them in the empty bowl to express what you think the likelihood or chance is of a specific event happening. One bean represents one chance out of 10. If you do not put any beans in the bowl, it means you are sure that the event will NOT happen. As you add beans, it means that you think the likelihood that the event happens increases. For example, if you put one or two beans, it means you think the event is not likely to happen, but it is still possible. If you put 5 beans, it means that it is just as likely it happens as it does not happen (fifty-fifty). If you pick 6 beans, it means the event is slightly more likely to happen than not to happen. If you put 10 beans in the plate, it means you are sure the event will happen. There is not a right or wrong answer, I just want to know what you think".*

I use their method when asking respondents to assess the probability of reaching their self-set savings goal or their subjective job finding probabilites.

B.3 Measures of Self-Regulation, Stress, and Well-being

To measure constructs in the areas of self-regulation and stress, I use measures validated as part of the Science of Behavior Change (SOBC) framework.[50] In particular, I build on Esopo et al. (2018), who have adapted and validated psychological scales that measure self-efficacy and executive control with laboratory subjects in Kenya.

B.3.1 Cognitive Control

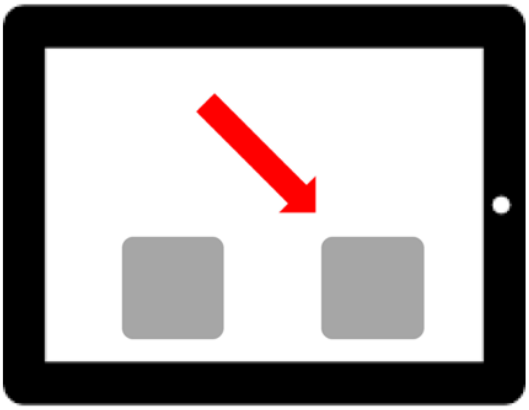
Cognitive control – sometimes called executive control – is a broad construct in cognitive neuroscience that refers to the processes that organize information for goal-driven decision-making (Mackie, Van Dam, & Fan, 2013). Cognitive control affects how well we can control our impulses and our working memory, and how well we can formulate, maintain, and execute plans and goals. From the viewpoint of economics, Mullainathan and Shafir (2013) see cognitive control as one key component of what they call "bandwidth", that is the mental capacity required to engage in what is sometimes termed "System 2" thinking. In contrast to quick and intuitive System 1 thinking, decision-making under System 2 is "slow, forgetful, deliberate, and costly but typically produces more unbiased and accurate results" (Schilbach et al., 2016, pg. 435). Esopo et al. (2018) provide a summary recent contributions in economics.

The context of the study, goal-directed behavior of workers that engage in physically demanding and repetitive industrial work with long hours, suggests that bandwidth is an important factor to take into account.

To measure cognitive control I adapt a Stroop-like arrows task for use in the field (Baldo, Shimamura, & Prinzmetal, 1998). Closely following Esopo et al. (2018), subjects are shown red and blue arrows and must press a gray rectangle either in the same direction of the arrow when it is red or the opposite direction direction of the arrow when it is blue. Arrow direction (left or right) and color (red or blue) are randomized. This task is preferable over measures that use numbers because it does not require literacy. Subjects complete 20 arrows as quickly as possible on an Android tablet computer that is also used to administer the survey questionnaire. Figure B.1 illustrates the tablet screen during the test.

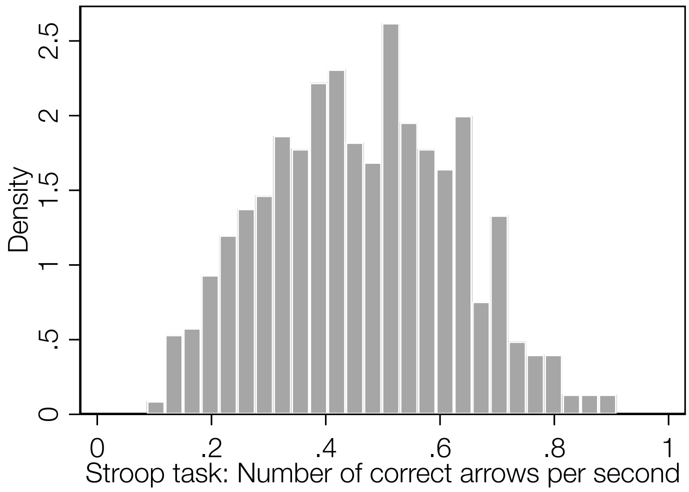
[50] SOBC is a large US National Institutes of Health (NIH) program, which aims to improve our understanding of behavior change across a broad range of (mostly health-related) behaviors. SOBC maintains a measures repository at scienceofbehaviorchange.org/measures.

Figure B.1: Example Screen of Cognitive Control Measure (Red Arrow: Touch Same Side)



I define correct answers per second as measure of executive control. The task is fully incentivized. Subjects are given 3 birr per correct answer. 1 birr is subtracted per second. The minimum payoff is 0 birr. Figure B.2 plots the distribution of scores.

Figure B.2: Cognitive Control Task: Histogram of Scores



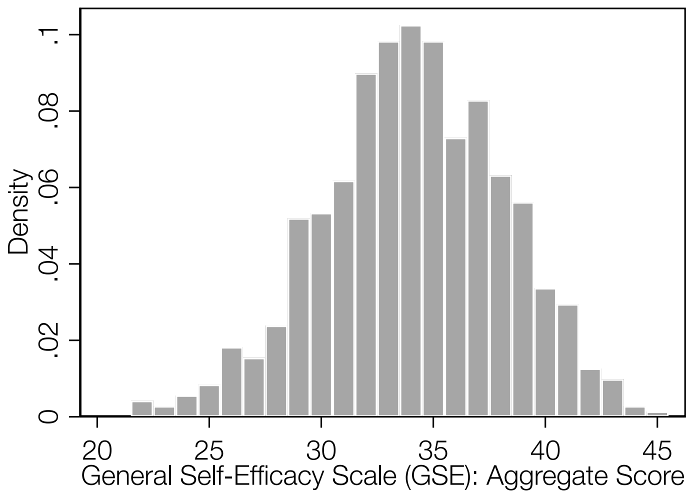
B.3.2 Perceived Self-Efficacy

Perceived self-efficacy broadly refers to an individuals belief in his or her own ability to perform well in a specific situation (Bandura, 1997). I hypothesize that self-efficacy could affect the ability of study participants to follow through on their plans. Esopo et al. (2018) review recent evidence that low measures of self-efficacy are correlated with low adherence to exercise regimes and health behaviors.

To measure perceived self-efficacy, I use an adapted version of the General Self-Efficacy (GSE) scale (Schwarzer & Jerusalem, 1995), which is designed to "assess a general sense of perceived self-efficacy with the aim in mind to predict coping with daily hassles as well as adaptation after experiencing all kinds of stressful life events." I use the 12-item scale from the SOBC repository. Responses are anchored on a four-point scale with responses ranging from 1 (strongly disagree) to 4 (strongly agree). The final score is calculated by adding all items, which yields a scale with a range from 12 to 48.

The GSE scale has been successfully used in across different cultural contexts (Luszczynska, Scholz, & Schwarzer, 2005), but to the best of my knowledge not in Ethiopia. I translated and back-translated the English scale to Amharic and piloted it with a focus group before deployment in the baseline survey. Figure B.3 plots the distribution of scores.

Figure B.3: GSE Measure: Histogram of Scores



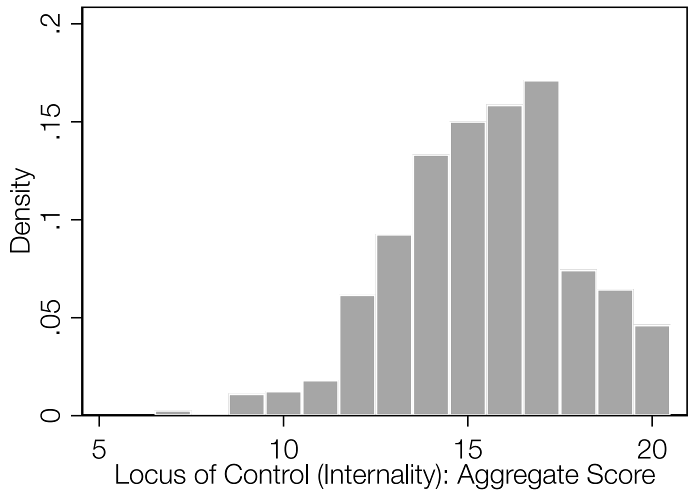
B.3.3 Locus of Control

Locus of control is a concept from personality psychology. Individuals with strong internal locus of control tend to believe that events in their lives are based on their own decisions, actions, and behaviors. People with an external locus of control believe that events in their life are beyond their control.

Locus of control has been hypothesized to affect job search behavior. Locus of control could impact an individual's subjective assessment of her own ability to influence job search outcomes (Caliendo, Cobb-Clark, & Uhlendorff, 2015; Falk, Huffman, & Sunde, 2006a, 2006b; McGee & McGee, 2016). To measure locus of control, I use five items from the Internality, Powerful Others, and Chance (IPC) scale developed by Levenson (1981), which is commonly used in applied work. Responses are anchored on a four-point scale ranging from 1 (strongly disagree) to 4 (strongly agree). The final score is calculated by adding all items, which yields a scale with a range from 5 to 20.

As with the GSE scale, I translated and back-translated the English scale to Amharic and piloted it with a focus group before deployment in the baseline survey. Figure B.4 plots the distribution of scores.

Figure B.4: Locus of Control Measure: Histogram of Scores



B.3.4 Physical Health

To measure physical health, I rely on self-reported ability to perform "activities daily life" (ADL). ADL scales are a widely used to measure health in various domains in developing and developed countries, originally by clinicians to assess fitness for work, eligibility for disability insurance, or claims for accidents and injuries (McDowell, 2006), and more recently in development program evaluation (Thomas & Strauss, 2007).

ADL scales are preferably over measures that are endogenous to the labor supply decision such as sick days, but come with all the problems of self-reported scales including different interpretation of questions by respondents, endogeneity of self-perceived health to the work experience of respondents, and potential experimenter demand effects.

I create an additive scale of four ADL measures used by Blattman and Dercon (2018) in context of Ethiopian manufacturing workers: walk for 2 kilometers, work outside on your feet for a full day, carry a 20 liter carton of water for 20 meters, and standing at a workbench for 8 hours. Each of the four measures is scored on a four-point scale from 1 (unable) to 4 (easily).

B.3.5 Psychological Well-Being

There is evidence that the psychological consequences of poverty can lead to stress and negative affect, which in turn can influence decision-making (Haushofer & Fehr, 2014). To assess if stress and negative affect – feeling unhappy or anxious – are potentially confounding my results, I use two measures.

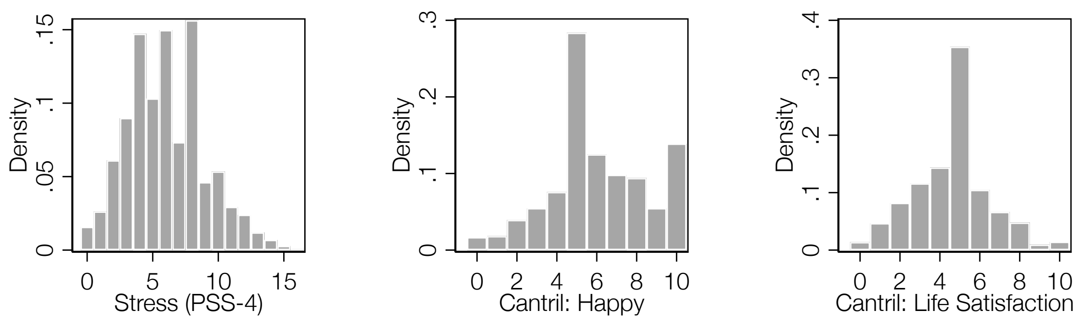
To measure stress, I follow Haushofer and Shapiro (2016) and use the Perceived Stress Scale by Cohen, Kamarck, and Mermelstein (1983). While their original scale contains 14 items, I use the same four items as Haushofer and Shapiro (2016). Respondents are asked how often they felt in certain ways. Answers are anchored on a five-point scale with responses ranging from 0 (never) to 4 (very often). The final score is calculated by adding all items, which yields a scale with a range from 0 to 16.

To measure happiness and life satisfaction, I use Cantril's Self-Anchoring Scale (Cantril, 1965), which is commonly used in applied work and global opinion surveys such as Gallup's World Poll (Kahneman & Deaton, 2010). The scale asks respondents to imagine a staircase or ladder with numbered steps, where the top of the ladder represents the best possible or happiest possible life. Respondents are then asked to place themselves on one step of the ladder ("The top of the ladder represents the best possible life for you, and the bottom of the ladder represents the worst possible life for you. On which step of the ladder would you say you personally feel you stand nowadays?") In the survey, enumerators show respondents a picture of a ladder on the tablet to aid visualization.

Figure B.5 plots the distribution of scores for all three measures.

Figure B.5: Psychological Well-Being: Histogram of Scores

(a) Stress (b) Cantril: Happy (c) Cantril: Life Satisfaction



B.4 Construction of Other Survey Measures

Assets: Asset indices are commonly used in welfare and poverty analyses. They are particularly useful to assess living standards in settings where expenditure and income data is absent or unreliable. This is often the case in urban or peri-urban areas like the study setting, where consumption expenditures may be underreported because consumption happens outside the household. To construct an index of household and individual assets, I take three steps. First, I use data from the 2015-2016 Ethiopian Socioeconomic Survey (ESS) to identify 13 assets that best predict nominal total household consumption expenditures. I do this using dominance analysis, an approach sometimes used in psychology that calculates the marginal contribution of each independent variable in predicting an outcome by testing all possible combinations of independent variables (Budescu, 1993). I identify the following household assets: shelf for storing goods, energy saving stove, kerosene stove, sofa set, refrigerator, electric stove, electric mitad (an appliance to prepare injera bread), radio, television, CD/DVD player, satellite dish, wristwatch, and water pump. Second, I ask each respondent for the number of these assets owned by the household where the respondent sleeps and the number of these assets owned by the respondent. I use the exact question phrasing of the LSMS survey. Third, I calculate two additive indices of all assets: One for assets owned by the household where the respondent sleeps and one for the respondent herself. Additive indices are attractive due to their simplicity. Filmer and Scott (2012) show that under most conditions the method of aggregation does not significantly affect household rankings.

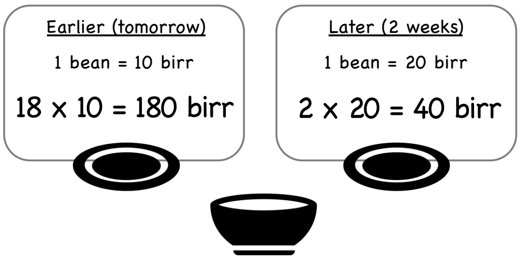
Appendix C Time Preference Elicitation and Estimation

C.1 Implementation in the Field

Figure C.6: Convex Time Budget Implementation

(a) Picture of Training Interview

(b) Illustration of Whiteboards



Notes: The picture in panel (a) is taken during the training week of the enumerator pictured, so the data from this respondent was not used. This decision shows choice set $(1, 14)$ with interest rate $r = 1.00$. In this case, the respondent chose to allocate 18 beans to the earlier dish and 2 beans to the later dish.

C.2 Selected Experimental Instructions

I provide English translations of key experimental instructions. These are read by the enumerator to the respondent from a tablet screen. The tablet screen shows both the English and the Amharic version, so enumerators can go over the text with respondents. Amharic originals are available upon request.

Introduction

For the next part of our conversation, I would like to ask you to make 15 different decision about how to divide money between two different dates: "earlier" and "later". The money will be represented with beans. I will give you a bowl with 20 beans and ask you to divide the beans between two dishes: one that represents the earlier date and one that represents the later date. The beans that you allocate to "later" will always be worth more than the beans you allocate to "earlier".

It's important that you listen carefully to understand how this exercise works, because you will make decisions about real money. At the end of our conversation, we will flip a coin to decide if you will be paid out one of the decisions. If you win the coin flip, we will have a lottery in which you draw one of the 15 decisions that you made. We will then send you the earlier and later payments for the decision that you draw. You will get each payment exactly on the date specified, not earlier. We will send you the money to your cell phone as CBE Birr payment [a payment system by Commercial Bank of Ethiopia].

After you received the text message you can redeem the money at any CBE branch or any CBE Birr agent. I will now give you an example so that you can better understand the exercise.

Example and Comprehension Check

Let's look at an example together. In this example, the earlier dish represents the amount of money you would like to get tomorrow. The later dish represents the amount you would like to get in 4 weeks. For each dish, I wrote how much one bean is worth. On the earlier dish for money tomorrow, one bean is worth 10 birr. On the later dish for money in 4 weeks, one bean is worth 15 birr.

You can put any number of beans on the earlier dish and on the later dish, but you must use all of your beans. If you decide to put all 20 beans on the earlier dish, this means that you would like to get 200 birr tomorrow and nothing in 4 weeks. If you put 10 beans on the earlier dish and 10 beans on the later dish, this means that you would like to get 100 birr tomorrow (10 beans x 10 birr per bean = 100 birr) and 150 birr in 4 weeks. Notice that beans on the later dish are always worth more than beans on the earlier dish, so putting beans on the later dish means you would get more money in total. If you decide to put all 20 beans on the later dish, this means that you would like to get nothing tomorrow, and wait 4 weeks to get 300 birr (20 x 15 birr per bean = 300 birr).

After you put your beans on the two dishes, I will write down the total amount of money you would get at each time. If you are not happy with the amounts, you can take beans again and change your mind. We can do this as often as you like until you are happy.

Let's try it out. Please go ahead and divide up the 20 beans between the two dishes. Remember you don't need to put all beans on one dish, but you can divide them up between earlier and later as you like.

[ENUMERATOR: Let participant allocate beans. Calculate total in each dish, write on board. Read out to participant.]

Would you be happy with these amounts tomorrow and in 4 weeks?

[ENUMERATOR: Revise if necessary]

Remember this was just an example. We will now go through 15 such decisions between earlier and later. In these 15 decisions, I will change when the earlier dish will be paid out and when the later dish will be paid out. For each combination of earlier and later, I will increase how much one bean in the later dish is worth. At the end of our conversation, we will flip a coin to see if you will receive one of your decisions in the form of two payments.

Do you understand everything so far?

[ENUMERATOR checkpoint. You must ask the following three questions]

Please explain to me when you would get paid the amount on each dish, should you win the coin flip.

Suppose that you put beans on both dishes and win the coin flip, how many payments will you receive?

Please explain to me how much one bean is worth on each dish.

[The respondent needs to answer all three of these questions correctly. If not, please go back and explain again]

Did the respondent correctly answer al questions?

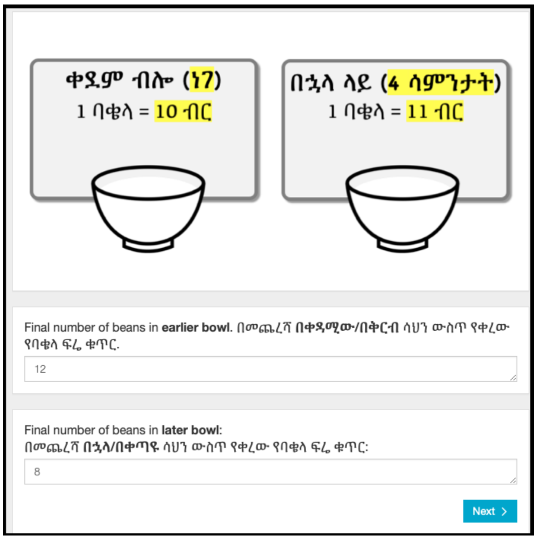
Thank you for listening to my explanations. Let's get started now with your decisions.

Please keep in mind that this is not a test. There are no right or wrong answers. However, it is important to keep in mind that you make decisions over a substantial amount of real money. If you win the coin flip at the end of our conversation, we will pay out one of your decisions. Each of the 15 decision has the same chance of being chosen, so you should think carefully about each of them.

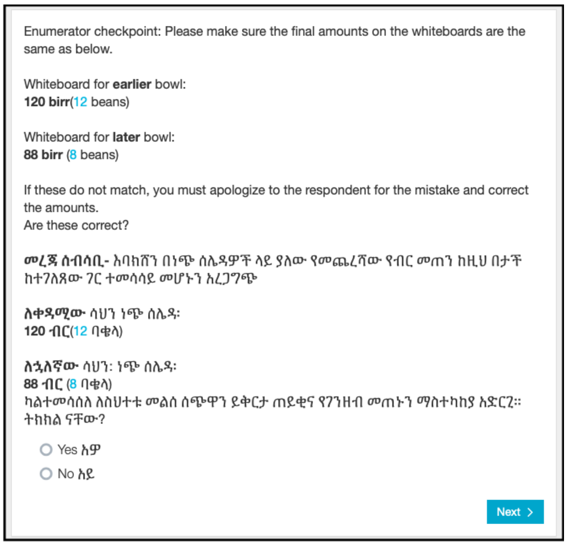
The 15 decisions are done interactively using the beans and the whiteboards. The tablet computer aids the enumerator by visualizing the decision. After illustrating the setup of the white boards, the survey software asks for the final number of beans allocated. Before moving to the next decision, the survey software calculates the total amounts and asks the enumerator to confirm that these were correctly indicated on the white board.

Figure C.7: Screenshots of Survey Software During CTB Task

(a) Setup of Dishes and Whiteboards



(b) Confirmation of Decision



C.3 Text of Payment Confirmation

This shows an English translation of the payment confirmation that subjects receive if they win the coin flip of the CTB experiment. Subjects only receive the original in Amharic. This confirmation is indented to increase confidence in the payment being delivered and reduce the cognitive costs of keeping track. It is printed on high-quality paper with a European University Institute watermark.

Payment Confirmation

As part of our academic study, you made several decisions about whether you would prefer amounts of money earlier or later. Because you won the coin flip at the end of our conversation, you receive the following two amounts:

Earlier payment: *Later Payment:*

[Two large boxes with amount and date for each of the payments]

On the date indicated for each of the payments (but not earlier), you will receive the money to your cell phone as CBEbirr payment. We will send this payment to you before 12 noon on the day indicated. CBEbirr is a payment system by Commercial Bank of Ethiopia. You will receive an SMS from the CBEbirr payment system by the date specified above. The SMS will contain the amount of money that you will receive. The sender will be [survey coordinator name] and the sending phone number will be [survey coordinator phone number]. After you received the text message you can redeem the money at any CBE branch or any CBEbirr agent. When you go to the bank or the CBEbirr agent, please do not forget to bring your Kebele ID and your cell phone with the CBEbirr text message.

If you have any questions, please contact the study coordinator [name] at [coordinator cell phone number].

Recipient name: [subject name]

Date: [date]

Signature Enumerator [signed] *Signature Lead Researcher [signed]*

C.4 Consistency and Comparison with Random Choice

Consistency with the law of demand in the CTB experiment can serve as an indicator of whether subjects understood the instructions of the experiment. This is particularly true because the subject pool has relatively lower numeracy and literacy skills than student laboratory subjects.

The average allocations summarized in Table C.1 below and visual inspection of Figure 3 indicate that on aggregate subject decisions are consistent with the law of demand. As the interest rate increases and the price of consumption later decreases, subjects monotonically decrease the share of beans allocated to earlier.

Table C.1: Baseline CTB Allocations to Later (in Ethiopian birr), by Front-End Delay t, Delay k, and Interest Rate r

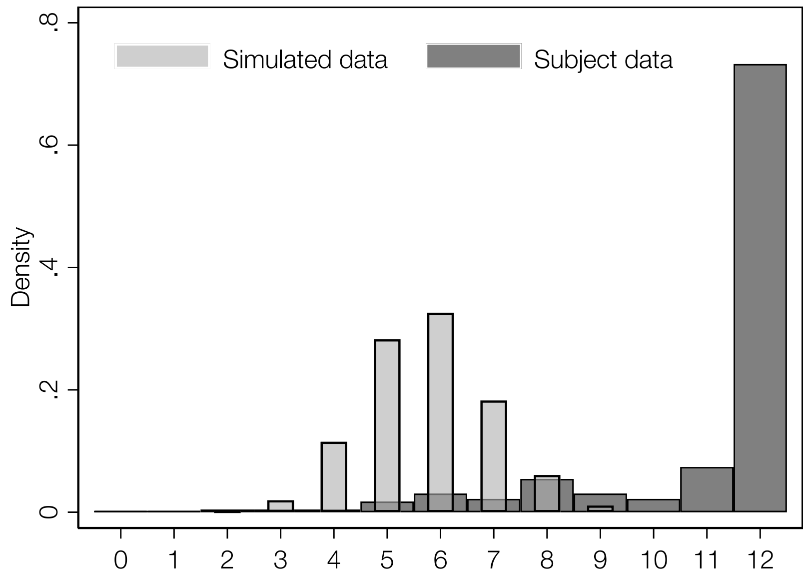
					Percentiles					
t	k	$1+r$	Mean	Std. dev.	10th	25th	50th	75th	90th	Fraction at corner
1	14	1.10	122.24	64.87	0.00	88.00	132.00	165.00	214.50	0.10
1	14	1.25	160.30	69.59	62.50	125.00	175.00	212.50	250.00	0.15
1	14	1.50	218.72	76.00	120.00	180.00	225.00	285.00	300.00	0.23
1	14	1.75	272.64	83.24	175.00	245.00	297.50	350.00	350.00	0.27
1	14	2.00	336.14	87.46	220.00	300.00	380.00	400.00	400.00	0.43
1	28	1.10	112.79	69.76	0.00	55.00	121.00	165.00	209.00	0.10
1	28	1.25	147.48	76.73	0.00	112.50	162.50	200.00	250.00	0.13
1	28	1.50	201.21	87.37	75.00	150.00	225.00	270.00	300.00	0.19
1	28	1.75	251.00	100.61	105.00	210.00	280.00	332.50	350.00	0.23
1	28	2.00	314.19	105.49	180.00	280.00	360.00	400.00	400.00	0.37
15	14	1.10	125.37	61.59	0.00	110.00	132.00	165.00	220.00	0.10
15	14	1.25	159.87	68.37	62.50	125.00	175.00	200.00	250.00	0.15
15	14	1.50	215.95	75.33	120.00	180.00	225.00	270.00	300.00	0.20
15	14	1.75	270.32	84.14	175.00	227.50	280.00	350.00	350.00	0.28
15	14	2.00	327.33	96.83	200.00	300.00	360.00	400.00	400.00	0.42

Notes: Data from all 460 subjects in the baseline survey at $t = 0$.

I follow Giné et al. (2017) in quantifying adherence to the law of demand at the individual level. In each of the three choice sets $(t, k) \in [(1, 14), (1, 28), (15, 14)]$, subjects make five decisions over the same dates but with increasing interest rates. These five decisions can be grouped in four pairs of experimental interest rates between earlier and later where $r' < r''$. In each of these four pairs, subjects should allocate weakly more money to later under r'' than under r'. With 460 subjects in the baseline CTB task, the data has $4 \times 3 \times 460 = 5,520$ such interest rate pairs. Out of those, only 463 (8.4 percent) are not consistent with the law of demand. The median deviation is one bean. This compares favorably to 81 percent of pairs in Giné et al. (2017) and suggests that subjects largely understood the experiment.

Finally, one can compare the number of pairs that are consistent with the law of demand to simulated data in which subjects choose allocations randomly (drawn from a uniform distribution). Figure C.8 plots the number of pairs in which subjects allocate weakly more money to later under r'' than under r' for simulated and real baseline data. The comparison suggests that subjects in my experiment indeed made choices that are significantly more consistent than random chance.

Figure C.8: Consistency with Law of Demand, Subject Decisions Compared to Simulated Random Choice



Notes: In each of three choice sets, subject make five decisions over the same dates but with increasing interest rates. This results four interest rate pairs where $r' < r''$. In total, I can compare $3 \times 4 = 12$ decisions per subjects. The figure plots the number of those decisions that are consistent with the law of demand, such that subjects allocate weakly more money to later under r'' than under r'. Simulated data assumes that for each decision subjects simply allocate their budget based on a random from a uniform distribution.

C.5 Theoretical Framework for Parameter Estimation

In this subsection, I outline a simple theoretical framework to estimate time preference parameters based on the convex time budget experiment. I replicate the approach and parametric assumptions of Andreoni and Sprenger (2012) and Augenblick et al. (2015).

In the lab-in-the-field CTB experiment, each subject chooses to allocate an experimental budget $m > 0$ between an amount c_t available at an earlier time t and a another amount c_{t+k} available after a delay $k > 0$, i.e. paid out at point $t + k$. Let $(1 + r)$ be the simple gross interest rate to be paid over period k. The unit of time are days since the experiment. All monetary amounts are measured in experimental tokens.

Experimental subjects maximize an additively-separable utility function with quasi-hyperbolic (β-δ) preferences (Laibson, 1997; O'Donoghue & Rabin, 1999) in the form:

$$U(c_t, c_{t+k}) = u(c_t - \omega_t) + \beta^{\mathbf{1}_{t=0}} \delta^k u(c_{t+k} - \omega_{t+k}) \tag{13}$$

where β is the parameter of present bias, $\mathbf{1}_{t=0}$ is an indicator that is 1 when the earlier payoff is realized in period 0, and δ is the long-run discounting parameter. With $\beta = 1$ the framework nests the standard exponential discounting model. ω_t and ω_{t+k} are Stone-Geary (Geary, 1950; Stone, 1954) background consumption or subsistence consumption levels at each point in time. Subjects maximize (13) subject to their experimental budget constraint

$$(1+r)c_t + c_{t+k} = m. \tag{14}$$

The first order conditions for c_t and c_{t+k} yield the familiar intertemporal Euler equation that must be satisfied by the optimal allocation (c_t^*, c_{t+k}^*)

$$\frac{u'(c_t - \omega_t)}{\beta^{\mathbf{1}_{t=0}} \delta^k u'(c_{t+k} - \omega_{t+k})} = (1+r). \tag{15}$$

I assume constant relative risk aversion (CRRA) utility in the form $u(c) = 1/\alpha c^\alpha$ or, equivalently, $u(c) = c^{1-\theta}/(1-\theta)$ with θ as the coefficient of relative risk aversion. With that (15) can be written as follows:

$$\frac{c_t - \omega_t}{c_{t+k} - \omega_{t+k}} = \left(\beta^{\mathbf{1}_{t=0}} \delta^k (1+r)\right)^{1/(\alpha-1)} \tag{16}$$

Assuming ω_t and ω_{t+k} to be fixed, non-estimated values, we can take logs on (16) and obtain

$$\ln\left(\frac{c_t - \omega_t}{c_{t+k} - \omega_{t+k}}\right) = \left(\frac{\ln\beta}{\alpha-1}\right)\mathbf{1}_{t=0} + \left(\frac{\ln\delta}{\alpha-1}\right)k + \left(\frac{1}{\alpha-1}\right)\ln(1+r) \tag{17}$$

where $(c_t - \omega_t)\,/\,(c_{t+k} - \omega_{t+k}) > 0$ by assumption so that the log-transformation is well-defined.

When including an additive error term, Euler equation (17) can be estimated by regression at the level of each subject or in aggregate over all experimental subjects. Because the consumption ratios on the left-hand side are censored by corner solutions, estimation by two-limit Tobit is more appropriate than OLS.

Again following Andreoni and Sprenger (2012) but slightly changing their notation, assume each subject i makes her P budget decisions $j = 1, 2, ..., P$. The estimation problem can be written as follows:

$$\ln\left(\frac{c_t - \omega_t}{c_{t+k} - \omega_{t+k}}\right)_{ij} = \gamma_1\,\mathbf{1}_{t=0} + \gamma_2\,k + \gamma_3\,\ln(1+r) + \epsilon_{ij} \tag{18}$$

where ϵ_{ij} is a mean-zero error. By stacking all P observations for subject i, we obtain

$$\ln\left(\frac{\mathbf{c_t} - \boldsymbol{\omega_t}}{\mathbf{c_{t+k}} - \boldsymbol{\omega_{t+k}}}\right)_{\mathbf{i}} = \gamma_1\,\mathbf{1}_{t=0} + \gamma_2\,\mathbf{k} + \gamma_3\ln(\mathbf{1}+\mathbf{r}) + \boldsymbol{\epsilon}_i \tag{19}$$

By estimating (19) for each subject i we can obtain the individual-level parameters of interest from non-linear combinations

$$\hat{\beta}_i = \exp(\hat{\gamma}_1/\hat{\gamma}_3)$$
$$\hat{\delta}_i = \exp(\hat{\gamma}_2/\hat{\gamma}_3)$$
$$\hat{\alpha}_i = (1/\hat{\gamma}_3) + 1$$

with standard errors from the delta method.

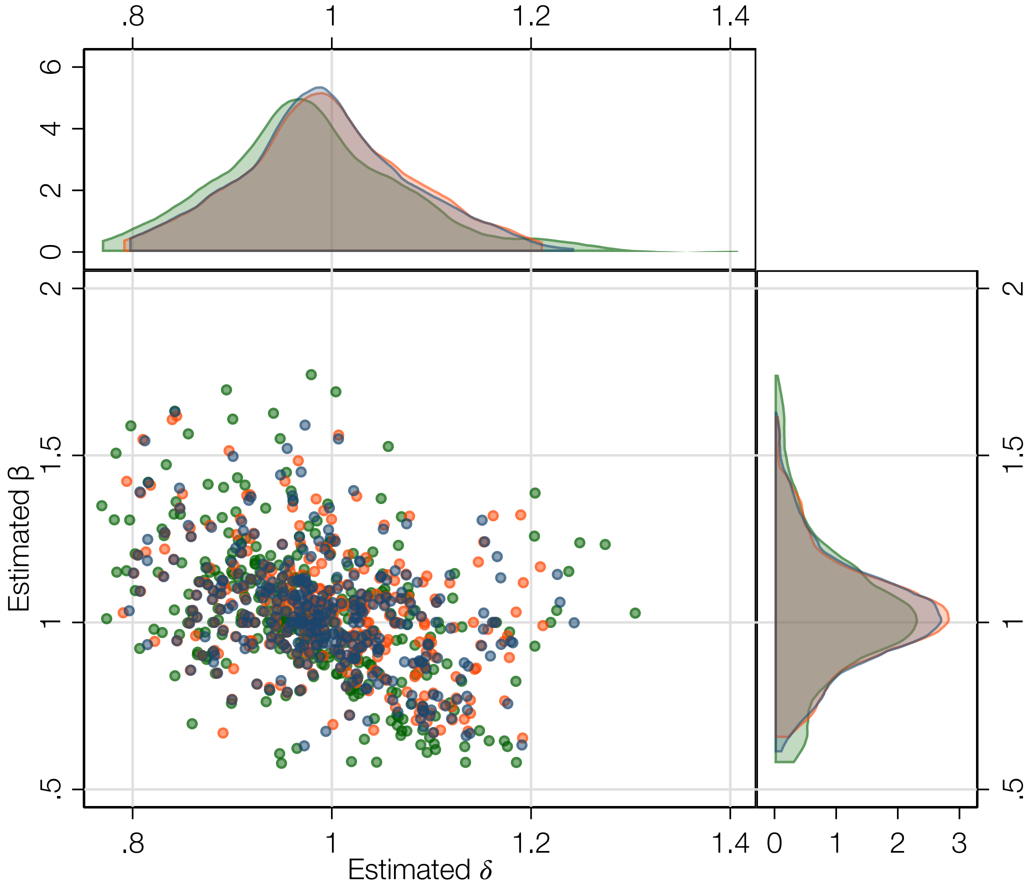
C.6 Additional Estimation Results

Table C.2: Aggregate Time Preference and CRRA Curvature Estimates
(Coefficient Estimates and Standard Errors)

	(1) Tobit	(2) Tobit	(3) Tobit
Present bias $\hat{\beta}$	1.006	1.008	1.002
	(0.018)	(0.013)	(0.014)
Discounting $\hat{\delta}$	0.959	0.977	0.976
	(0.008)	(0.007)	(0.007)
CRRA curvature $\hat{\alpha}$	0.886	0.677	0.734
	(0.006)	(0.011)	(0.012)
$H_0 : \beta = 1$	.119	.338	.0301
p-value	.73	.561	.862
$H_0 : \delta = 1$	23.3	12.4	11.9
p-value	1.39e-06	.000424	.000562
Log likelihood	-17173	-10439	-11902
N	6899	6899	6858
N (uncensored)	4832	4832	4832
Clusters	460	460	459

Notes: Table shows maximum likelihood estimates of Equation (17) using a two-limit Tobit model over the whole sample. Standard errors are clustered at individual level. The three models differ in their assumptions about background consumption at each point in time (see Equation (13) for details). Model (1) assumes no background consumption ($\omega_1 = \omega_2 = 0$). Model (2) assumes that background consumption is constant and set at the sample average daily consumption expenditure in the seven days before the baseline survey ($\omega_1 = \omega_2 = \bar{c}$). Model (3) assumes that background consumption is constant and set at the individual daily consumption expenditure in the seven days before the baseline survey ($\omega_1 = \omega_2 = c_i$).

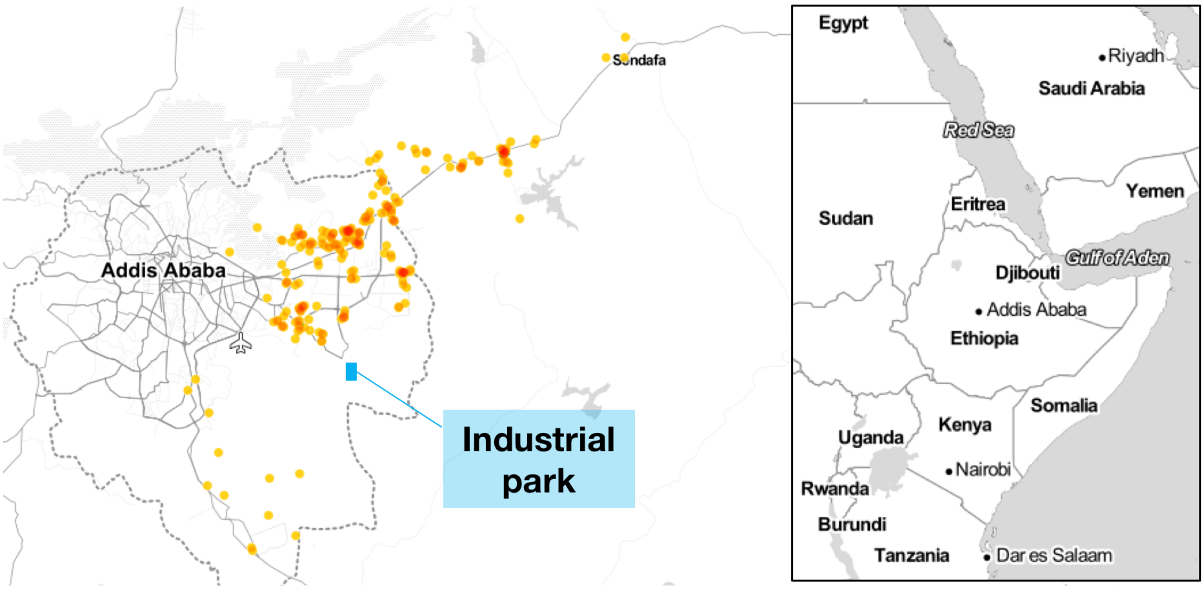
Figure C.9: Comparison of Individual-Level Parameter Estimates Using Two-Limit Tobit, by Assumptions About Background Consumption



Notes: Figure shows maximum likelihood estimates of Equation (17) using a two-limit Tobit model at the individual level. Each dot represents one estimate. Different colors represent different assumptions about background consumption: Green markers: assumed zero background consumption; red markers: using the sample average consumption expenditure as background consumption; blue markers: using individually-reported consumption expenditure as background consumption. Sample size differs from the full 460 subjects due to failure of the MLE to converge.

Appendix D Additional Figures

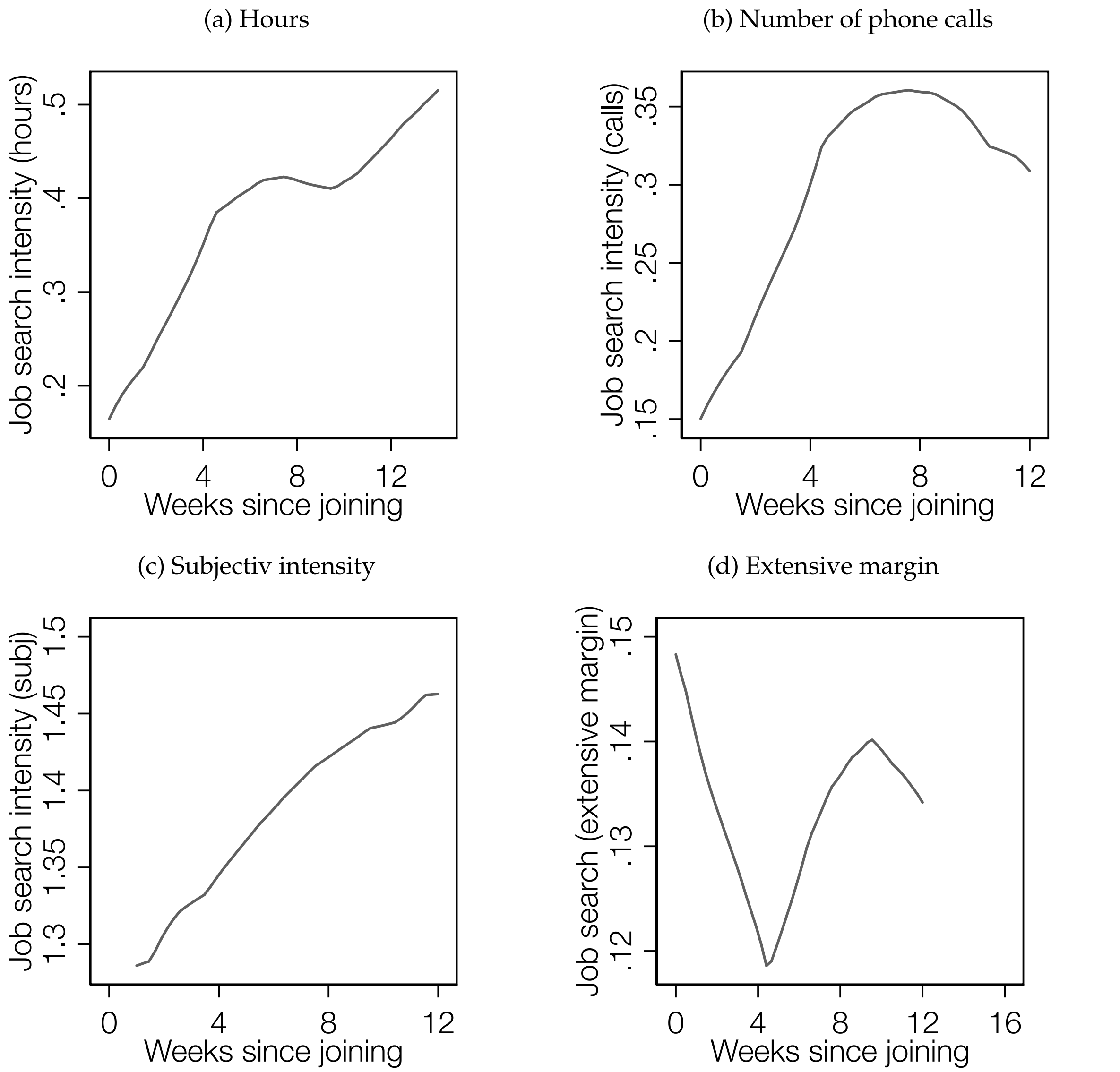
Figure D.1: Locations of Survey Firm and Respondent Households



Notes: Study firm is located in Bole Lemi Industrial Park, highlighted in light blue. Travel time from the industrial park to Addis Ababa city center is about 45 minutes to 1 hour, depending on means of transportation and traffic conditions. Yellow dots represent worker households. The more workers live in one locations, the darker the color of the dot.

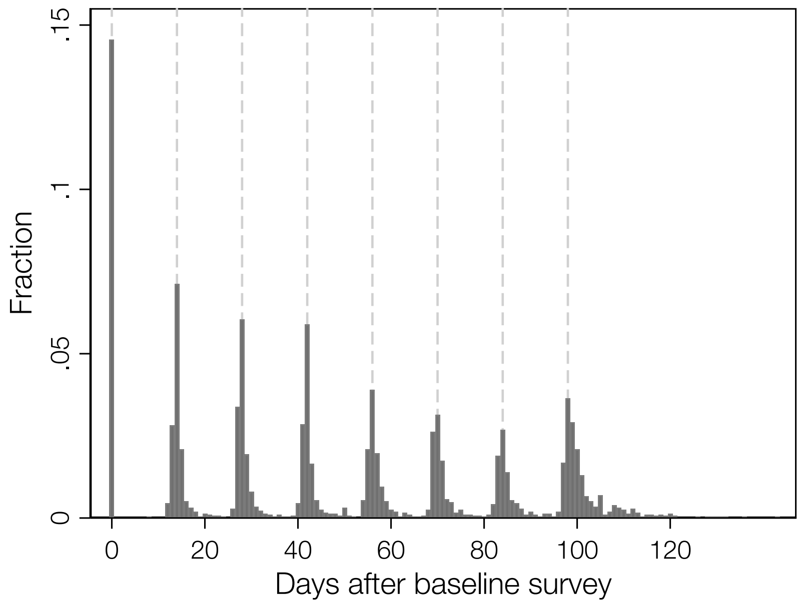
Sources: Base map data by OpenStreetMap, used under ODbL. Map tiles by Stamen Design, used under CC BY 3.0.

Figure D.2: Job Search Intensive and Extensive Margin over Time



Notes: All panels are local polynomial smoothers at the weekly level. All indicators are collected using high-frequency phone surveys and have a reference period of seven days prior to the phone call.

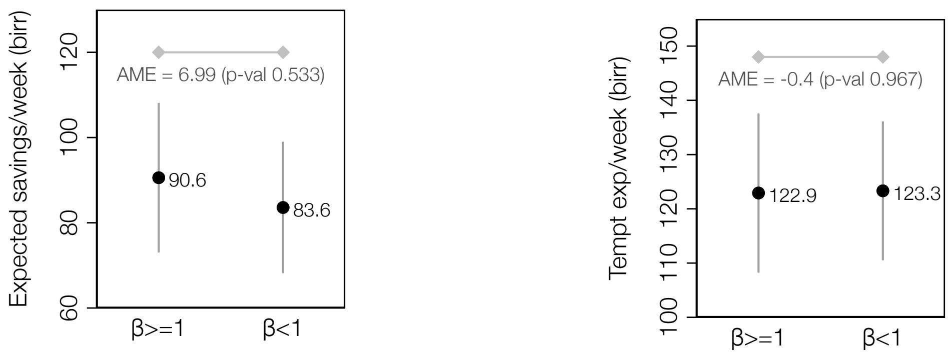
Figure D.3: Histogram of Panel Survey Dates After Baseline



Notes: Dashed lines refer to scheduled survey dates after baseline.

Figure D.4: Expected Value of Savings and Temptation Good Expenditure, by Present Bias Indicator

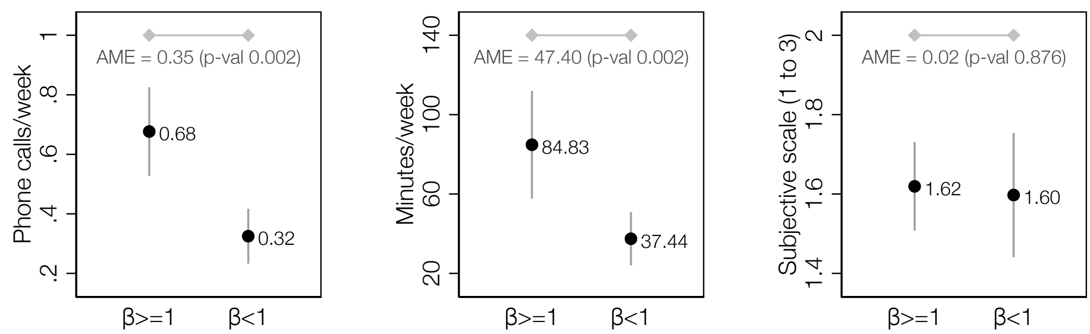
(a) Savings/week (birr) (b) Temptation good expenditures/week (birr)



Notes: Plots show the expected value of the observed outcome, based on columns (2) and (4) of Table 3. Thin bars indicate 90 percent confidence intervals. AME indicates the average marginal effect for discrete change in present bias indicator, i.e. the difference between the two plotted values.

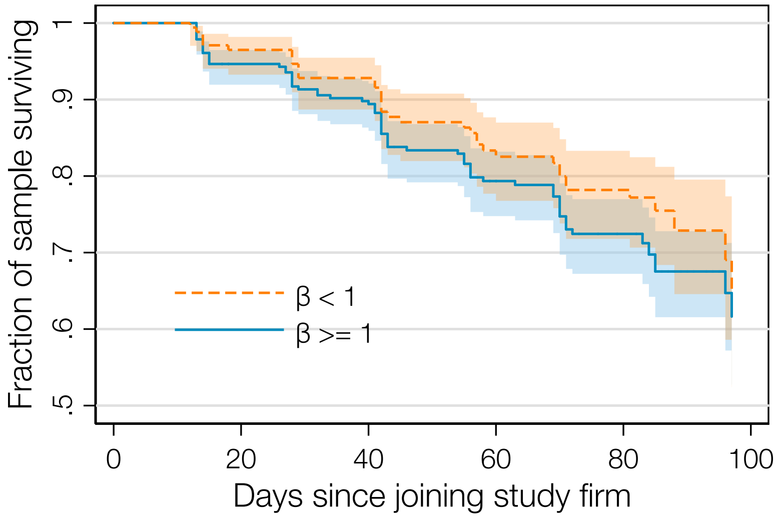
Figure D.5: Expected Value of Search Effort, by Present Bias Indicator

(a) Number of calls (b) Minutes (c) Subjective intensity



Notes: Plots show the expected value of the observed outcome, based on columns (2), (4), and (6) of Table 4. Subjective search intensity is measured on a three-point scale (1 – "not very intensively"; 2 – "intensively"; 3 – "very intensively"). Thin bars indicate 90 percent confidence intervals. AME indicates the average marginal effect for discrete change in present bias indicator, i.e. the difference between the two plotted values.

Figure D.6: Kaplan-Meier Survivor Function of Staying at the Firm, by Present Bias Parameter



Notes: Shaded area shows 90 percent pointwise confidence band.

Appendix E Additional Tables

Table E.1: On the Job Search and Reasons for Not Searching (Fractions of Sample)

	Baseline	Week 2	Week 4	Week 6	Week 8	Week 10	Week 12	Week 14	Endline
Searching for work	0.20	0.10	0.12	0.12	0.16	0.14	0.13	0.15	0.14
Would like to search	0.31	0.31	0.33	0.35	0.31	0.31	0.37	0.33	0.33
It takes too much time	0.10	0.13	0.17	0.17	0.14	0.17	0.18	0.15	0.12
It costs too much money	0.03	0.01	0.01	0.02	0.02	0.00	0.01	0.01	0.01
I don't know how/where to look	0.06	0.04	0.02	0.01	0.03	0.03	0.01	0.01	0.04
Other constraints	0.12	0.13	0.13	0.15	0.13	0.11	0.17	0.15	0.16
Not searching	0.49	0.60	0.55	0.53	0.53	0.55	0.49	0.53	0.52
N	460	425	394	321	275	237	209	163	153

Note: The numbers in this table represent workers who are searching while on their job at the study firm, so the decreasing sample size reflects both workers leaving the firm as well as attrition from the panel. Approximately 90 percent of the reasons given as answers under "Other constraints" relate to health problems. To avoid averaging, survey weeks here refer to survey waves of the panel, not the calendar date that the surveys were conducted. Appendix Figure D.3 compares scheduled and actual survey dates.

Table E.2: Summary Statistics of Baseline Observable Characteristics

Variable	N	Mean	Percentiles					Min	Max
			5th	25th	50th	75th	95th		
Panel (a) Personal characteristics									
Age	460	21.41	18	20	21	23	26	18	31
Married (indicator)	460	0.19	0	0	0	0	1	0	1
Has children (indicator)	460	0.06	0	0	0	0	1	0	1
Has a working spouse (indicator)	460	0.73	0	0	1	1	1	0	1
Rural-urban migrant (indicator)	460	0.67	0	0	1	1	1	0	1
Mother tongue: Amharic (indicator)	460	0.20	0	0	0	0	1	0	1
Mother tongue: Oromiffa (indicator)	460	0.38	0	0	0	1	1	0	1
Religion: Ethiopian Orthodox (indicator)	460	0.73	0	0	1	1	1	0	1
Religion: Muslim (indicator)	460	0.15	0	0	0	0	1	0	1
Education: Has completed 8th grade (indicator)	460	0.17	0	0	0	0	1	0	1
Education: Has completed 10th grade (indicator)	460	0.34	0	0	0	1	1	0	1
Panel (b) Consumption expenditures and savings									
Consumption expenditures: Food	460	141.77	0	0	100	200	500	0	1,200
Consumption expenditures: Non-food	460	147.93	10	36.5	70	155	575	0	1,680
Consumption expenditures: Temptation goods	460	77.96	0	0	20	50	364	0	1,600
Savings	460	469.59	0	0	0	300	2,350	0	30,000
Panel (c) Liquidity and access to finance									
Has bank account (indicator)	460	0.70	0	0	1	1	1	0	1
Ease of obtaining credit (4-point Likert scale)	460	2.24	1	1	2	3	4	1	4
Panel (d) Cognitive control, non-cognitive skills, stress									
Stroop task score	460	0.43	0.18	0.31	0.42	0.54	0.70	0.10	0.89
Generalized Self-Efficacy index	460	34.50	28	32	34	37	41	23	45
Locus of Control index	460	15.59	12	14	16	17	19	7	20
Perceived Stress Scale	460	7.17	3	5	7	9	12	0	16

Notes: Consumption and savings measured with a seven-day recall. Appendix B provides details on survey protocols and the measurement of control variables.

Table E.3: Savings and Present Bias (Continuous Variable)
(Regression Coefficient Estimates and Robust Standard Errors)

	Savings					
	(1)	(2)	(3)	(4)	(5)	(6)
	Tobit	*Tobit*	*Tobit*	*Tobit*	*Tobit*	*Tobit*
Baseline $\hat{\beta}_i$	44.68	75.95		26.97		-42.95
	(101.285)	(170.208)		(91.115)		(111.177)
Baseline $\hat{\delta}_i$		168.5	274.4		35.05	-322.1
		(490.736)	(288.374)		(197.700)	(298.173)
Survey wave dummies	Yes	Yes	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
Personal characteristics	No	No	No	Yes	Yes	Yes
Baseline liquidity	No	No	No	Yes	Yes	Yes
Cognitive control	No	No	No	Yes	Yes	Yes
Non-cognitive ability and stress	No	No	No	Yes	Yes	Yes
N	1890	1803	1901	1814	1832	1734

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Time preference parameters trimmed at 5th and 95th percentile. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables.

Table E.4: Temptation Good Expenditures and Present Bias (Continuous Variable)
(Regression Coefficient Estimates and Robust Standard Errors)

	Temptation goods expenditure					
	(1)	(2)	(3)	(4)	(5)	(6)
	Tobit	*Tobit*	*Tobit*	*Tobit*	*Tobit*	*Tobit*
Baseline $\hat{\beta}_i$	-4.541		-19.30	20.72		7.973
	(37.355)		(47.697)	(40.736)		(52.269)
Baseline $\hat{\delta}_i$		-1.786	-67.42		-37.85	-90.08
		(86.102)	(123.162)		(96.017)	(134.853)
Survey wave dummies	Yes	Yes	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
Personal characteristics	No	No	No	Yes	Yes	Yes
Baseline liquidity	No	No	No	Yes	Yes	Yes
Cognitive control	No	No	No	Yes	Yes	Yes
Non-cognitive ability and stress	No	No	No	Yes	Yes	Yes
N	2090	2102	1995	2009	2028	1921

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Time preference parameters trimmed at 5th and 95th percentile. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables.

Table E.5: Savings and Temptation Goods (Extensive Margin) and Present Bias (Estimated Average Marginal Effects and Robust Standard Errors)

	(1)	(2)	(3)	(4)
	Savings	*Savings*	*Tempt.*	*Tempt.*
	Probit	*Probit*	*Probit*	*Probit*
Baseline $\hat{\beta}_i < 1$	-0.0351	-0.0192	0.0506*	0.0308
	(0.031)	(0.030)	(0.029)	(0.030)
Baseline $\hat{\delta}_i$	0.108	0.00367	-0.0855	-0.131
	(0.173)	(0.166)	(0.157)	(0.171)
Survey wave dummies	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes
Personal characteristics	No	Yes	No	Yes
Baseline liquidity	No	Yes	No	Yes
Cognitive control	No	Yes	No	Yes
Non-cognitive ability and stress	No	Yes	No	Yes
N	2093	2019	2098	1996
Log likelihood				

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Time preference parameters trimmed at 5th and 95th percentile. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables.

Table E.6: Job Search Effort and Present Bias (Continuous Variable)
(Regression Coefficient Estimates and Robust Standard Errors)

	(1) Calls *Tobit*	(2) Calls *Tobit*	(3) Calls *Tobit*	(4) Hours *Tobit*	(5) Hours *Tobit*	(6) Hours *Tobit*
Baseline $\hat{\beta}_i$	0.817		3.992*	0.566		6.640*
	(1.985)		(2.268)	(3.212)		(3.965)
Baseline $\hat{\delta}_i$		6.599*	12.72**		10.32	24.64**
		(3.988)	(5.290)		(7.914)	(9.950)
Survey wave dummies	Yes	Yes	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
Personal characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Baseline liquidity	Yes	Yes	Yes	Yes	Yes	Yes
Cognitive control	Yes	Yes	Yes	Yes	Yes	Yes
Non-cognitive ability and stress	Yes	Yes	Yes	Yes	Yes	Yes
N	1919	1939	1833	1919	1939	1833
Log likelihood	-1194	-1176	-1140	-1510	-1480	-1453

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Time preference parameters trimmed at 5th and 95th percentile. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables.

Table E.7: Job Search Decision and Present Bias
(Estimated Average Marginal Effects and Robust Standard Errors)

	(1)	(2)
	Looking	Looking
	Probit	*Probit*
Baseline $\hat{\beta}_i < 1$	-0.0706**	-0.0879***
	(0.031)	(0.030)
Baseline $\hat{\delta}_i$	0.339**	0.325**
	(0.151)	(0.137)
Survey wave dummies	Yes	Yes
Enumerator dummies	Yes	Yes
Personal characteristics	No	Yes
Baseline liquidity	No	Yes
Cognitive control	No	Yes
Non-cognitive ability and stress	No	Yes
N	2004	1919

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Time preference parameters trimmed at 5th and 95th percentile. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables.

Table E.8: Job Search Outcomes, Search Effort, and Present Bias
(Incidence-Rate Ratios and Average Marginal Effects with Robust Standard Errors)

	(1)	(2)	(3)	(4)
	# offers in t	# offers in t	Vol dep in t	Vol dep in t
	Poisson	*Poisson*	*Probit*	*Probit*
Baseline $\hat{\beta}_i < 1$	0.442	0.385*	-0.0257*	-0.189***
	(0.260)	(0.219)	(0.013)	(0.036)
Baseline $\hat{\delta}_i$	178.6**	243.2**	0.0497	0.803***
	(400.895)	(522.120)	(0.065)	(0.274)
Job search effort (hours) in t		0.989		0.00473**
		(0.026)		(0.002)
Job search effort (# calls) in t		1.001		0.0113**
		(0.048)		(0.005)
Job search effort (intensively) in t		1.350		-0.0550
		(0.600)		(0.046)
Job search effort (very intensively) in t		2.129		-0.00128
		(1.148)		(0.065)
Survey wave dummies	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes
Personal characteristics	Yes	Yes	Yes	Yes
Baseline liquidity	Yes	Yes	Yes	Yes
Cognitive control	Yes	Yes	Yes	Yes
Non-cognitive ability	Yes	Yes	Yes	Yes
N	320	320	1946	235
Log likelihood	-112	-110		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Columns 1 and 2 report incidence-rate ratios from the poisson coefficient estimates while columns 3 and 4 report average marginal effects from probit coefficient estimates. Time preference parameters trimmed at 5th and 95th percentile. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables.

Table E.9: Measured Baseline Present Bias and Observable Characteristics (Regression Coefficient Estimates and Robust Standard Errors)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\mathbb{1}_{\{\hat{\beta}_i<1\}}$	$\mathbb{1}_{\{\hat{\beta}_i<1\}}$	$\mathbb{1}_{\{\hat{\beta}_i<1\}}$	$\hat{\beta}_i$	$\hat{\beta}_i$	$\hat{\beta}_i$
	Probit	*Probit*	*Probit*	*OLS*	*OLS*	*OLS*
Panel (a) Personal characterstics and human capital						
Age	0.0263	0.0284	0.0333	-0.00923**	-0.00956**	-0.0104**
	(0.028)	(0.029)	(0.030)	(0.005)	(0.005)	(0.005)
Married (=1)	0.0871	0.0851	0.0839	-0.0439	-0.0415	-0.0409
	(0.185)	(0.187)	(0.193)	(0.031)	(0.032)	(0.033)
Kids (=1)	0.0831	0.0787	0.0824	-0.0340	-0.0342	-0.0342
	(0.285)	(0.287)	(0.287)	(0.050)	(0.050)	(0.050)
Ethiopian Orthodox faith (=1)	0.347	0.363	0.350	-0.0112	-0.0162	-0.0186
	(0.244)	(0.247)	(0.254)	(0.040)	(0.041)	(0.041)
Muslim faith (=1)	0.142	0.179	0.167	-0.000415	-0.0162	-0.0167
	(0.301)	(0.308)	(0.313)	(0.048)	(0.049)	(0.049)
Amharic mother tongue (=1)	-0.394*	-0.391*	-0.415*	-0.0112	-0.0123	-0.0103
	(0.233)	(0.234)	(0.239)	(0.044)	(0.044)	(0.045)
Oromiffa mother tongue (=1)	0.177	0.162	0.139	-0.101**	-0.0941*	-0.0905*
	(0.249)	(0.251)	(0.253)	(0.047)	(0.048)	(0.048)
Rural-urban migrant (=1)	-0.0178	-0.0256	-0.0801	-0.00642	-0.00428	0.00325
	(0.164)	(0.164)	(0.169)	(0.029)	(0.029)	(0.030)
8th grade education completed (=1)	0.210	0.215	0.153	-0.0421	-0.0437	-0.0358
	(0.180)	(0.179)	(0.184)	(0.031)	(0.030)	(0.031)
Previous formal work experience (=1)	-0.0130	-0.00174	-0.0959	-0.000846	-0.00377	0.00719
	(0.150)	(0.152)	(0.157)	(0.025)	(0.026)	(0.027)
Panel (b) Liquidity and access to finance						
Has bank account (=1)		0.0219	0.0468		-0.0192	-0.0225
		(0.159)	(0.159)		(0.027)	(0.027)
Easy to obtain credit (=1)		-0.118	-0.0885		0.0418	0.0426
		(0.156)	(0.166)		(0.027)	(0.029)
Panel (c) Cognitive control, self-regulation, stress						
Cognitive control score			-1.050**			0.150*
			(0.494)			(0.080)
Self-efficacy score			-0.0362*			0.00503
			(0.022)			(0.003)
Locus of control score			-0.00893			-0.00115
			(0.037)			(0.006)
Stress score			0.00813			0.000494
			(0.025)			(0.005)
Panel (d) Other						
Constant	-0.657	-0.668	1.207	1.255***	1.257***	1.025***
	(0.673)	(0.674)	(1.026)	(0.106)	(0.106)	(0.177)
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	366	366	366	366	366	366

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Dependent variable in columns (1) to (3) is an indicator variable for present bias while the dependent variable in columns (4) to (6) is the untransformed present bias parameter estimate. Appendix B provides details on survey protocols and the measurement of control variables.

Table E.10: Measured Endline Present Bias and Cash Drop
(Regression Coefficient Estimates and Robust Standard Errors)

	(1)	(2)	(3)	(4)
	$\mathbb{1}_{\{\hat{\beta}_i<1\}}$	$\mathbb{1}_{\{\hat{\beta}_i<1\}}$	$\hat{\beta}_i$	$\hat{\beta}_i$
	Probit	*Probit*	*OLS*	*OLS*
Won coin payoff at baseline (=1)	-0.274	-0.246	0.0113	0.00306
	(0.218)	(0.217)	(0.033)	(0.031)
Enumerator dummies	Yes	Yes	Yes	Yes
Personal characteristics	Yes	Yes	Yes	Yes
Baseline liquidity	No	Yes	No	Yes
Cognitive ability	No	Yes	No	Yes
Non-cognitive ability and stress	No	Yes	No	Yes
N	194	194	198	198

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Dependent variable in columns (1) and (2) is an indicator variable for present bias estimated in the endline CTB experiment while the dependent variable in columns (3) and (4) is the untransformed present bias parameter estimate at endline. Appendix B provides details on survey protocols and the measurement of control variables.

Table E.11: Balance by Experimental Payouts
(Means and Standard Errors of the Mean in Parentheses)

Variable	Full sample	Experimental payout		
		No	Yes	Diff p-val
Panel (a) Personal characteristics				
Age	21.41	21.37	21.45	0.735
	(0.117)	(0.186)	(0.149)	
Married (=1)	0.19	0.21	0.17	0.340
	(0.018)	(0.029)	(0.024)	
Kids (=1)	0.06	0.07	0.05	0.369
	(0.011)	(0.019)	(0.014)	
Ethiopian Orthodox faith (=1)	0.73	0.70	0.76	0.149
	(0.021)	(0.033)	(0.027)	
Amharic mother tongue (=1)	0.67	0.69	0.66	0.427
	(0.022)	(0.033)	(0.030)	
Oromiffa mother tongue (=1)	0.20	0.15	0.23	0.039
	(0.019)	(0.026)	(0.026)	
Ethiopian Orthodox faith (=1)	0.73	0.70	0.76	0.149
	(0.021)	(0.033)	(0.027)	
Muslim faith (=1)	0.15	0.17	0.12	0.128
	(0.016)	(0.027)	(0.020)	
Rural-urban migrant (=1)	0.73	0.74	0.72	0.581
	(0.021)	(0.031)	(0.028)	
8th grade education completed (=1)	0.17	0.17	0.17	0.929
	(0.017)	(0.027)	(0.023)	
Previous formal work experience (=1)	0.60	0.59	0.60	0.890
	(0.023)	(0.035)	(0.031)	
Panel (b) Liquidity and access to finance				
Has bank account (=1)	0.70	0.69	0.72	0.463
	(0.021)	(0.033)	(0.028)	
Easy to obtain credit (=1)	0.67	0.68	0.65	0.513
	(0.022)	(0.033)	(0.030)	
Panel (c) Cognitive control, self-regulation, stress				
Cognitive control score	0.43	0.43	0.43	0.943
	(0.007)	(0.011)	(0.010)	
Self-efficacy score	34.50	34.34	34.62	0.446
	(0.182)	(0.260)	(0.252)	
Locus of control score	15.59	15.53	15.64	0.605
	(0.109)	(0.159)	(0.149)	
Stress score	7.17	7.30	7.07	0.405
	(0.138)	(0.213)	(0.182)	
N	460	201	259	

Notes: Experimental payouts are determined by a coin flip at the end of the experiment. "Diff. p-val" refers to the p-value of an t-test for equality of means between subjects who won the coin flip and subjects who did not. Appendix B provides details on survey protocols and the measurement of control variables.

Table E.12: Job Search Effort and Present Bias Controlling for Experimental Payouts (Regression Coefficient Estimates and Robust Standard Errors)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Calls	Calls	Calls	Calls	Hours	Hours	Hours	Hours
	Tobit	*Tobit*	*Tobit*	*Tobit*	*Tobit*	*Tobit*	*Tobit*	*Tobit*
Baseline $\hat{\beta}_i < 1$	-2.287**	-2.370**	-2.963***	-3.138***	-4.479**	-4.615**	-6.130***	-6.337***
	(1.016)	(1.007)	(0.950)	(0.947)	(1.965)	(1.948)	(1.894)	(1.894)
Baseline $\hat{\delta}_i$	12.22**	10.84**	12.41***	11.46***	22.12**	19.11**	22.28***	19.56**
	(5.067)	(4.901)	(4.216)	(4.102)	(9.621)	(9.057)	(8.593)	(8.118)
Won experimental payout (=1)		-2.049**		-2.128***		-3.502**		-3.763**
		(0.851)		(0.764)		(1.658)		(1.627)
Survey wave dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal characteristics	No	No	Yes	Yes	No	No	Yes	Yes
Baseline liquidity	No	No	Yes	Yes	No	No	Yes	Yes
Cognitive ability	No	No	Yes	Yes	No	No	Yes	Yes
Non-cognitive ability and stress	No	No	Yes	Yes	No	No	Yes	Yes
N	2008	2008	1939	1939	2008	2008	1939	1939
Log likelihood	-1263	-1257	-1166	-1159	-1555	-1549	-1467	-1460

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Time preference parameters trimmed at 5th and 95th percentile. Controls for personal characteristics are age, marital status, religion, mother tongue, education, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Controls for baseline liquidity are amount of savings held when starting employment and easy of obtaining credit assessed on a 4 point Likert scale. Control for cognitive control is the final score achieved in the baseline Stroop task. Controls for non-cognitive control and stress are baseline score on the General Self-Efficacy scale, baseline locus of control index, and baseline score on the Perceived Stress Scale (PSS-4). Appendix B provides details on survey protocols and the measurement of control variables.

Table E.13: Job Search Effort and Human Capital
(Regression Coefficient Estimates with Robust Standard Errors)

	(1) Calls *Tobit*	(2) Calls *Tobit*	(3) Calls *Tobit*	(4) Hours *Tobit*	(5) Hours *Tobit*	(6) Hours *Tobit*
Age	0.506***	0.508***	0.493***	0.812***	0.809**	0.771**
	(0.170)	(0.172)	(0.169)	(0.315)	(0.314)	(0.304)
Formal work experience (=1)	-0.415	-0.423	-0.294	-1.992	-1.980	-1.786
	(0.856)	(0.857)	(0.857)	(1.634)	(1.625)	(1.636)
Education level completed (=1)						
(Omitted: Grade 5 or less)						
Grade 6	1.652	1.656	1.476	3.548	3.540	3.343
	(1.672)	(1.672)	(1.684)	(3.219)	(3.216)	(3.189)
Grade 7	-0.605	-0.594	-0.489	-1.320	-1.342	-1.249
	(1.628)	(1.632)	(1.662)	(3.211)	(3.210)	(3.270)
Grade 8	1.476	1.480	1.643	3.498	3.490	3.797
	(1.590)	(1.591)	(1.623)	(3.156)	(3.160)	(3.237)
Grade 9	2.350	2.364	2.471	2.185	2.164	2.145
	(2.186)	(2.191)	(2.249)	(3.397)	(3.395)	(3.428)
Grade 10	0.189	0.205	-0.0109	2.827	2.799	2.502
	(1.467)	(1.473)	(1.495)	(2.874)	(2.862)	(2.865)
More than Grade 10	3.252*	3.280*	3.013	6.571*	6.525*	6.107*
	(1.913)	(1.952)	(1.881)	(3.368)	(3.420)	(3.333)
Cognitive control		-0.376	-0.509		0.568	0.344
		(2.831)	(2.819)		(5.466)	(5.520)
Self-efficacy			0.122			0.136
			(0.126)			(0.255)
Locus of control			0.184			0.488
			(0.223)			(0.410)
Personal characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
Survey wave dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	2215	2215	2215	2215	2215	2215
Log likelihood	-1347	-1347	-1343	-1594	-1594	-1590

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Controls for personal characteristics are age, marital status, religion, mother tongue, number of children, whether or not respondent moved from a rural area, whether respondent has a working spouse. Appendix B provides details on survey protocols and the measurement of control variables.

Table E.14: Log Reservation Wage and Time Preference Parameters
(OLS Regression Coefficient Estimates with Robust Standard Errors)

	(1)	(2)	(3)	(4)
Baseline $\hat{\beta}_i < 1$	0.969	0.971	0.975	0.977
	(0.028)	(0.028)	(0.027)	(0.028)
Baseline $\hat{\delta}_i$	0.983	0.980	0.984	0.993
	(0.149)	(0.147)	(0.146)	(0.147)
Survey wave dummies	Yes	Yes	Yes	Yes
Enumerator dummies	Yes	Yes	Yes	Yes
Personal characteristics	Yes	Yes	Yes	Yes
Baseline liquidity	No	Yes	Yes	Yes
Cognitive control	No	No	Yes	Yes
Non-cognitive ability	No	No	No	Yes
N	2423	2423	2423	2423
R^2	0.404	0.406	0.407	0.409

Time preference parameters trimmed at 5th and 95th percentile.

Robust standard errors clustered at individual level

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, clustered at individual level. Appendix B provides details on survey protocols and the measurement of control variables.