POLICY RESEARCH WORKING PAPER 8633

The Limits of Commitment

Who Benefits from Illiquid Savings Products?

Niklas Buehren
Markus Goldstein
Leora Klapper
Tricia Koroknay-Palicz
Simone Schaner

WORLD BANK GROUP

Africa Region
Gender Innovation Lab
October 2018

Abstract

Working with a private bank in Ghana, this study examines the impacts of a commitment savings product designed to help clients taking repeated overdrafts break their debt cycles. Overall, the product significantly increased savings with the bank without increasing overdrafts. However, after accounting for other sources of savings, the study finds that clients with above-median baseline overdraft histories do not accrue new savings during the commitment period. Rather, they draw down other savings to offset the committed amount and take on new debt. In contrast, individuals with below-median overdraft histories significantly increase savings during and after the commitment period.

The Limits of Commitment: Who Benefits from Illiquid Savings Products?[*]

Niklas Buehren   Markus Goldstein   Leora Klapper

Tricia Koroknay-Palicz   Simone Schaner[†]

1 Introduction

Temporary short-term loans, such as checking account overdrafts or payday credit, are double-edged swords: while easy access to such credit may help individuals make ends meet when faced with shocks (Morse, 2011; Islam and Maitra, 2012), there is also a risk that high-cost debt triggers further financial distress (Melzer, 2011) and draws people into debt cycles. The risk of negative consequences is particularly acute when individuals exhibit behavioral biases like hyperbolic discounting, which could lead individuals to continuously put off paying down high-cost debt in order to enjoy more consumption in the present (Angeletos et al., 2001). When agents are time inconsistent, there is scope for financial products that offer commitment to restrict current spending and improve long-run welfare.

Yet the success of commitment in such a context is by no means assured: individuals who are (partially) naïve about their self-control problem may not sign up, or sign up for too little commitment (Laibson, 2015). Furthermore, individuals who do choose to commit may take costly measures to undo their commitments in the future. This could take the form of drawing down other savings, paying fees to opt out of the commitment product, or taking on additional debt to support current-period consumption. Ultimately, assessing the extent to which commitment products increase welfare and build net savings is an empirical question. Given the risk that commitment crowds out other savings or crowds in debt, it is particularly important to collect comprehensive data on consumer balance sheets when evaluating commitment products – yet rigorous evaluations that do this are surprisingly rare, especially in developing country contexts.[1]

We conducted a randomized trial with salaried workers in Ghana to fill this evidence gap and shed light on whether commitment has the potential to lift individuals out of debt cycles. All the workers had access to an overdraft facility on the bank account linked to their salary payments, and many made regular use of the facility at a substantial cost. Half the workers were randomly offered a novel savings product that automatically deducted a pre-specified savings contribution from their monthly paycheck. The contribution cycle lasted for 18 months, after which all contributions, plus a "completion bonus" equal to one month's contribution, were released to the saver. Workers faced a penalty if they withdrew any money before the end of 18 months. The product was designed with debt cycles in mind: by building up a sizable lump sum with the partner bank, users could release themselves from an overdraft debt cycle at the end of the commitment period.

---

[1]Accounting for crowd out is important, as even the very poor typically have access to some type of liquidity (Banerjee and Duflo, 2007; Collins et al., 2009). Note that access to liquidity in and of itself is not a sign that commitment will fail, especially when individuals have precautionary savings motives – in this case, people can be pushed to a corner before drawing down their liquidity (Laibson, 1997).

We combine administrative data from the bank with six waves of detailed survey data to study how commitment impacted respondents' financial behavior – both at the partner bank and more broadly. First, we find robust demand for commitment. Seventy-two percent of individuals in the treatment group signed up for the product, and just 13 percent of those who signed up dropped out before the 18-month savings cycle ended. Second, the product more than doubled balances at the partner bank both during and after the commitment period. We also observe a marginally significant 6 percentage point (18 percent) decline in overdrafts in the six months following the release of the commitment amount. In the longer run, however, treatment effects on overdrafts disappear.

Our third main result underscores the importance of taking a broader view of consumers' finances when evaluating savings products. Although the commitment product did not crowd out other cash savings, we do observe a significant increase in the share of respondents who took on new debt during the commitment period. Point estimates on debt stocks are roughly equal to point estimates on total savings, and as a result our treatment estimates on net savings (savings less debt) are close to zero and not statistically significant.

This interpretation changes dramatically when studying heterogeneity in treatment effects by baseline overdrafter status, however. "Heavy overdrafters" (those with above median overdraft histories at baseline) did save more at the partner bank during the commitment period, but this treatment effect disappears after the commitment amount is released. Moreover, the product significantly *decreased* heavy overdrafters' other formal sector savings during the commitment period while increasing the rate of debt taking. These results are consistent with the hypothesis that heavy overdrafters are naïve hyperbolic discounters, who end up undoing their commitment by borrowing. "Light overdrafters" (those with below-median histories) exhibited a very different response: the commitment product significantly increased other formal sector savings both during and after the commitment period, with no significant impact on debt. This suggests that light overdrafters benefited from the savings product through a channel other than time inconsistency.

The main contribution of our paper is to explore the extent to which commitment is crowded out by other sources of liquidity, while directly exploring the pitfalls of commitment. To date, much of the research on behavioral savings products in development economics focuses on savings held at partner financial institutions, or savings without accounting for debt (for a review of recent work see Karlan et al. 2014). Our findings highlight the need for future work to cast a broader net when assessing the promise of commitment for improving the financial lives of the poor.[2] Our work also builds on John (2017), who finds that over

---

[2]In this way our paper is related to a growing literature that seeks to understand the net effects of retirement savings programs in developed country contexts (Poterba et al., 1996; Benjamin, 2003; Engelhardt

half of individuals who sign up for a similar contribution-based commitment product end up paying a fee to drop out early. We go beyond this to show that negative effects need not be limited to program dropout – in our case, agents turn to debt to offset savings commitments, which is costly and runs the risk of propagating debt cycles. We also demonstrate that negative side effects are concentrated among people who exhibit behavior most consistent with self-control problems at baseline. This suggests that when individuals are naïve about the extent of their self-control problem, commitment may do more harm than good.

Another contribution of our paper is to show that illiquid savings products *can* improve individuals' financial lives for reasons unrelated to a direct commitment effect. Here, our work relates to Brune et al. (2016), who find that a commitment savings product increased savings, agricultural investment, agricultural output, and consumption of Malawian farmers even though amounts actually committed were quite low. We build on this by showing that benefits accrue to individuals least likely to be liquidity constrained, and by showing that the commitment product crowds in other savings both during and after the commitment period.

The remainder of the paper proceeds as follows: We first describe our sample and the experiment in Section 2. We present our results in Section 3, which includes an analysis of overall impacts, heterogeneity by baseline overdraft status, and discussion. Section 4 concludes.

2 Experimental Context and Design

2.1 Partner Bank and Study Sample

The study sample is comprised of 320 salaried workers receiving electronic salary payments into bank accounts at the North Volta Rural Bank (NVRB).[3] NVRB is a small bank that mostly serves individuals living in rural areas, where the majority of economically active individuals are self-employed in agriculture (Ghana Statistical Service, 2013).

By virtue of being salaried workers, our sample is better off than the average citizen in the bank's catchment area. Column 1 of Appendix Table A1 illustrates baseline characteristics in the control group: three-quarters of study participants were men, two-thirds had a post-secondary education, and 90 percent were government employees, with teacher the most common occupation.[4] In comparison, across Ghana and in the Volta Region, approximately

---

and Kumar, 2007; Gelber, 2011; Chetty et al., 2014; Beshears et al., 2017).

[3]Appendix A provides additional detail on sampling and inclusion/exclusion criteria.

[4]The remaining 10 percent of workers are NVRB employees. Our results are unchanged if we drop these individuals from the analysis.

6 percent of the economically active population is employed by the government and just 8 percent of individuals aged 15 and older have a post-secondary education (Ghana Statistical Service, 2013, 2012). Our respondents reported an average monthly income of GHS 770, or $358 at a 2013 exchange rate of GHS 2.15 per $1, had GHS 240-250 in savings with NVRB, and GHS 1,628 in cash savings across all sources.[5] Although our respondents have desirable jobs, they are still economically vulnerable: one-third of the respondents reported that children in their care miss school due to late school fees "often" or "sometimes", 71 percent reported that they often or sometimes worry about having enough money to pay monthly bills, and 61 percent stated that they are "socially taxed", in that they often or sometimes have difficulty saying no to requests from others.

Salaried workers are valuable bank customers in Ghana. Regularly-recurring salary payments allow banks to reliably deduct loan fees, interest, and principal directly from the customer's account, and bank accounts offering salary advances or overdraft facilities to customers who receive electronic payments are common. At NVRB, salaried customers may receive an overdraft by completing a requisition form at the bank branch. NVRB then permits the customer to take a cash withdrawal equal to the overdraft amount, which turns the customer's bank balance negative. At the time of the experiment, overdrafts incurred a fixed GHS 5 service fee plus a variable fee equal to 18 percent of the overdraft amount. Since most overdrafts are cleared in less than one month, these loans have exceptionally high effective interest rates.[6] Workers in our sample made extensive use of overdrafts at baseline: Fifty-eight percent of respondents overdrafted at least once in the year before baseline, with the average overdrafter paying GHS 226 ($105) in fees and interest over the same period. Appendix Figure A1 graphs the distribution of overdrafts.

Individuals who use costly, short-term credit are particularly likely to have time inconsistency problems (Meier and Sprenger, 2010; Gathergood, 2012). As such, we stratified our randomization by baseline overdrafting behavior (as well as gender and bank branch) because we were interested in studying heterogeneity in treatment effects by overdrafter status.[7] Before randomizing we constructed an index of overdrafting behavior and defined those

---

[5]Total baseline cash savings include savings at NVRB, savings at other banks, microfinance institutions, and credit unions, savings with group savings clubs, savings with agricultural co-ops, savings held by susu collectors, money lenders, other individuals, and cash kept at home.

[6]Seventy-six percent of the overdrafts taken out by our study sample were cleared within 31 days and 90 percent were cleared in 93 days. As a matter of policy, NVRB only formally approves overdrafts for one or two months.

[7]The baseline survey also asked respondents to make hypothetical choices between different amounts of money at different times, but there were only two opportunities for respondents to make a quasi-hyperbolic preference reversal and choices were unincentivized. In practice, just 3.8 percent of respondents made two reversals, while 20 percent of respondents made one reversal. Given the weaknesses of the survey-based measure, we do not use it in the main analysis and instead focus on heterogeneity in overdrafting behavior.

with an above-median score to be "heavy overdrafters".[8]

Table 1 illustrates differences in baseline characteristics by overdrafter status. The first row shows that, on average, light overdrafters only took an overdraft in 6 percent of the months in the year prior to the baseline, while heavy overdrafters were in overdraft more than 50 percent of the time. Heavy overdrafters were more likely to be men, more likely to be teachers, less likely to work in the private sector, and had more children and lived in larger households. Although they had similar incomes to light overdrafters, they reported roughly GHS 400 less in savings and GHS 1,200 more in total debt and were 12 percentage points more likely to report difficulty paying bills.

2.2 Experimental Design

NVRB worked with the research team to design a new savings product called Salary Susu Plus (SSP) for the purposes of the study. Similar services were not offered by other banks in the study area at the time. SSP participants specified a monthly contribution amount, which would be deducted from future salary deposits for a period of 18 months. The monthly contribution had to be equal to at least GHS 30 and could not be changed after it was set. Customers could not access their contributions during the savings cycle unless they opted out of the program at a cost of one month's contribution. Customers who completed the commitment cycle received all their contributions plus an additional bonus payment equal to one month's contribution (or 5.6 percent of the total contribution amount). This amounts to an annual percentage yield (APY) of 7 percent, assuming monthly compounding. By way of comparison, NVRB offered no interest on current accounts and a 3 percent APY on savings accounts at the time of the baseline. While SSP offered an attractive rate of return relative to NVRB's other offerings, the real return on savings was still negative, since annual inflation ranged from 10-19 percent over the course of our study period (Bank of Ghana, 2014, 2017).

Half the sample in each overdrafter×branch×gender stratum was assigned to the treatment group, which was offered the opportunity to sign up for SSP. Appendix Table A1 confirms that baseline characteristics are balanced across treatment and control groups, both overall and when the sample is split by overdrafter status.

Appendix Figure A2 illustrates the timeline of all study activities. SSP debits began in

---

[8]Overdraft scores were constructed for each person using their current account transaction records available at the time of the randomization. The score was calculated as the fraction of balance entries that were negative over the life of the account. The sample was sorted from lowest score (never had a negative balance) to highest (negative balance 66 percent of the time).

December 2013, continued through May 2015, and payouts were made to SSP clients in June 2015.[9]

2.3   Data

Our analysis combines administrative data from NVRB with six waves of survey data. The administrative data contain information on all transactions posted to respondents' NVRB accounts from the time of account opening to one year after the end of the intervention (32 months after baseline).

The baseline took place before SSP was introduced. The first three follow-ups took place while SSP was ongoing – during program months 6, 10, and 14. The objective of these rounds was to collect data on savings and debt during the commitment period, and assess how commitment impacted expenditure. The last two follow-ups took place 3 and 5 months after the SSP payout. These surveys were designed to assess how respondents' financial lives had changed after the commitment amount was released.[10]

In total, 91 percent of study participants participated in all survey rounds and 98 percent of study participants participated in at least one follow-up survey round. Appendix Table A2 verifies that attrition is uncorrelated with treatment status.

3   Main Results

3.1   Take Up

Seventy-two percent of individuals in the treatment group signed up for SSP. The median monthly contribution was GHS 30, while the average contribution was GHS 42. Completion rates were also high, with just 13 percent of respondents who signed up for SSP dropping out before the savings cycle was over. Appendix Table A3 studies correlates of take-up (in the treatment group) and dropout (among those in the treatment group who signed up). The first two columns study pairwise correlations between take-up/dropout and baseline characteristics, while the last two columns present results of regressions where all baseline

---

[9]All study participants – in both the treatment and the control group – who did not have a savings account with NVRB were given the opportunity to open one in May 2015. Treatment group individuals were significantly more likely to open a savings account, largely because the bank stated to customers that SSP deposits were meant to be sent to a savings account. As a result the treatment group had 0.45 more accounts with NVRB than the control group by endline. This should be kept in mind when interpreting treatment effects in the post-intervention period.

[10]Additional details on each survey round, including timing, participation, and modules contained, can be found in Appendix A.

characteristics are entered simultaneously. SSP had broad appeal, with few baseline characteristics predicting take-up and dropout. Strikingly, while heavy overdrafters were just as likely as light overdrafters to sign up for SSP, they were 12 percentage points more likely to drop out (column 2). This is consistent with the hypothesis that some heavy overdrafters are partially-naïve and underestimate their future desire to undo commitments.

3.2   Direct Effects: Results from Administrative Data

We now analyze how SSP impacted study participants' use of NVRB accounts. Here, we focus on use of overdrafts, overdraft fees paid, net deposits (total deposits minus total debits, a measure of savings flow), and the average daily account balance (a measure of savings stock).[11] Our administrative data span the commitment period, the post-commitment period covered by follow-up surveys, and a post-commitment, post-survey period. We therefore estimate separate treatment effects for these three periods to understand how effects evolve over time.[12] We use the following regression specification:

$$y_{it} = \beta_1 treat_i \times during_t + \beta_2 treat_i \times after1_t + \beta_3 treat_i \times after2_t + \beta_4 y_{i0} + \gamma_s + \delta_t + \varepsilon_{it} \quad (1)$$

Where y_{it} is the outcome of interest for account holder i in month t, $treat_i$ is a dummy variable equal to one if account holder i is in the treatment group, $during_t$ identifies the commitment period, $after1_t$ identifies the post-commitment, follow-up survey period, $after2_t$ identifies the post-commitment, post-survey period, γ_s are strata fixed effects, δ_t are month fixed effects, and y_{i0} is the average outcome of interest, measured in the year before the survey.

The first two columns of Table 2 show that during the commitment period SSP had no impact on overdrafts. After SSP savings were released, the share of accounts in overdraft declined by 6 percentage points (18 percent), while overdraft charges declined by GHS 3.77 (21 percent) per month – both significant at the 10 percent level. In the longer run, however, participants slid back into debt cycles: we find no treatment effect on overdrafts 7-12 months after the commitment amount was released.

During the commitment period, SSP increased net deposits by GHS 19.5 (column 3 of Table 2). The 72 percent takeup rate implies a treatment-on-the-treated effect of GHS 27, roughly two-thirds of the average SSP contribution of GHS 42 – thus, there is evidence that participants were able to partially undo their commitments by drawing down other

---

[11]When constructing variables we sum across all accounts each respondent holds with NVRB. Note that average daily balances can be negative, e.g. when an account holder takes an overdraft.

[12]The commitment period lasted from December 2013 to May 2015, the post-commitment, follow-up survey period covered June to November 2015, and the post-commitment, post-survey period covers six months from December 2015 to May 2016.

savings at NVRB. Column 3 also shows that the treatment effect on net deposits dropped to zero during the follow-up, post-commitment period. This is not surprising, since many participants withdrew their SSP savings during this period. Interestingly, in the post-survey, post-commitment period the treatment effect on net deposits (GHS 32.5) exceeds the commitment period treatment effect. This suggests that SSP had persistent effects on savings behavior at NVRB, subject to the caveat that the point estimate is not significantly different from zero.

Finally, column 4 shows that SSP generated a significant and highly persistent increase in NVRB balances: the average daily balance during the commitment period increased by GHS 244, and remained GHS 140-141 higher during the two post-commitment periods. These treatment effects are large relative to control groups means ranging from GHS 90-111 across the three focal time periods.

3.3 Overall Effects: Results from Survey Data

We now turn to the survey data to assess whether impacts on NVRB balances reflect new savings or crowd out. We use a regression specification that mirrors equation 1, except t now references survey round, the interaction between treatment and the post-commitment, post-survey period is dropped, and y_{i0} references values measured at the baseline survey.

The first two columns of Table 3 present treatment effects on NVRB savings. The first column uses the same administrative data used in the earlier section, but focuses on NVRB savings on the day of the relevant survey. In order to make the measure comparable with survey reports, negative NVRB account balances are coded as zero NVRB savings. The second column studies savings based on respondent self-reports. Both variables paint a similar picture: SSP more than doubled NVRB savings during the commitment period. The administrative data also show a marginally significant impact on NVRB savings in the post-commitment period, while the point estimate for the survey data is smaller and not significantly different from zero. Columns 3 and 4 show that SSP had no significant impact on other formal or informal savings.

Individuals may also offset commitments by taking on more debt. Columns 6 and 7 of Table 3 study SSP's impacts on debt.[13] Here, we find a significant, 7.6 percentage point (40 percent) increase in the likelihood of taking on new debt during the commitment period. This could reflect increased financial strain driven by the regular monthly commitment.[14] Recall that we do not see any parallel impacts on the overdraft rate or overdraft fees at

---

[13]Appendix Table A4 shows impacts on debt by source.

[14]We do not, however, find that SSP increased self-reported markers of financial strain, like food insecurity and difficulty paying bills (Appendix Table A5).

NVRB during the commitment period (Table 2). This could be for one of two reasons: either marginal debtors have access to cheaper forms of credit than overdrafts, or marginal debtors are already maxed out on overdrafts.

While the effect of SSP on the debt stock is not significantly different from zero, the point estimate is economically meaningful and very close in magnitude to the treatment effect on savings. As a result, we find no evidence that SSP increased savings net of debt (Table 3, column 8). One caveat here is that our survey-based estimates – while close to zero – are noisy. The 95 percent confidence interval for the impact of SSP on savings net debt easily includes the analogous point estimate on savings with NVRB.

3.4 Heterogeneity by Baseline Overdrafting

The main effects mask strikingly different treatment effects by baseline overdrafter status. This can be seen in Figure 1, which graphs monthly treatment effects on the average daily balance at NVRB by overdrafter status. Here, the first vertical dashed line demarcates the payout month and the second vertical line demarcates the end of survey coverage. Panel B shows that the treatment effect for heavy overdrafters peaked roughly six months *before* the SSP payout. This could reflect a combination of program dropout (19 percent of heavy overdrafters dropped out early, as compared to 7 percent of light overdrafters) and draw-down of liquid, non-committed NVRB balances. In contrast, the treatment effect for light overdrafters peaked the month before the payout (Panel A). Moreover, treatment effects for light overdrafters re-emerge in the post-endline period after dropping immediately after commitment amounts are released. These patterns are consistent with the hypothesis that light overdrafters became accustomed to making smaller withdrawals from their NVRB accounts and continued with this behavior after the SSP program ended.[15]

These stark differences in treatment effects by baseline overdraft behavior are also apparent in the survey data. We examine heterogeneous treatment effects by augmenting equation 1 to include interactions between the different treatment effects and either a "heavy overdrafter" (above median overdraft score at baseline) or "light overdrafter" (below median overdraft score) dummy.[16] Table 4 shows that during the commitment period, light overdrafters saved more both with NVRB *and* with other banks and credit unions – commitment crowded additional savings into the banking sector. While there is some crowd out of informal savings during the commitment period, the point estimate is small compared to treatment effects on formal savings. Consequently, we reject the null of no impact on total

---

[15]In contrast, treatment effects on overdrafts are similar for the two groups – see Appendix Table A6 for detail.

[16]We also include interactions between overdrafter status and time period.

savings at the 5 percent level, both during and after the commitment period. The point estimates for both periods are just short of GHS 600, very close to total program savings of the median SSP participant (GHS 570).

In contrast, heavy overdrafters saved significantly less in other formal sector accounts, more than offsetting the additional savings stored with NVRB – as a result, point estimates on total savings are negative and not statistically significant. We reject equality of treatment effects on total savings for heavy and light overdrafters at the 1 percent level both during and after the commitment period.

Columns 6-8 of Table 4 show that this heterogeneity persists when accounting for debt. Only heavy overdrafters took on significantly more debt during the commitment period. As a result, treatment effects on savings net debt are large and negative for heavy overdrafters, while treatment effects on savings net debt are similar to treatment effects on total savings for light overdrafters. Here, we reject equality of effects by overdrafter status at the 5 percent level during and after the commitment period.

Given that SSP helped light overdrafters save more without taking on new debt, where did the additional money come from? Appendix Table A8 presents impacts on income and expenditure. Given the modest monthly changes needed to generate our point estimates, we cannot definitively identify consumption or income as the source of the new savings, although We do find some significant survey-based evidence that light overdrafters in the treatment group earn more. We view this result with caution, as we only collected income data in one follow-up survey. Table A8 also helps rule out the hypothesis that heavy overdrafters leverage SSP payouts and new debt to make lumpy investments in the post-commitment period: SSP had no significant impact on investment for either group.

3.5 Mechanisms and Discussion

Overall, our results for heavy overdrafters are consistent with a model of partially-naïve hyperbolic discounting, e.g. where workers sign up for SSP expecting that it will help with their commitment problems, but fail to account for the fact that they will resort to costly borrowing to undo their commitment in the future. The behavior of light overdrafters is more difficult to rationalize. What is clear is that their response to SSP is not consistent with standard models of hyperbolic discounting, where savings outside SSP should weakly decrease during the commitment period. How, then, did SSP catalyze savings among light overdrafters?

One possibility is that SSP helped light overdrafters manage financial demands from extended family or community members. This type of "other control" problem could be

particularly important in our context, where salaried workers are often asked to support less well-off relatives – recall that roughly 60 percent of our sample reported difficulty saying no to requests from others at baseline. In theory, SSP could have had a direct effect on transfer requests, e.g. if SSP reduced liquidity available for transfers, and an indirect effect, e.g. if signing up for SSP gave respondents an accepted justification to deny transfer requests. In practice, the indirect channel is needed to explain our results for light overdrafters, given that the treatment crowds in other formal sector savings and that treatment effects persist into the post-commitment period. We find no strong evidence that other control is driving heterogeneity by overdrafter status: Appendix Table A8 shows that the treatment had no significant effect on money spent on others, and heterogeneity by other control does not mirror heterogeneity by overdrafter status (Appendix Table A9).

Alternatively, psychological mechanisms could have catalyzed the SSP treatment effects for light overdrafters. For example, the product may have helped light overdrafters build better savings habits – the fact that treated light overdrafters continued to save more with NVRB after withdrawing the SSP lump sum is consistent with this channel. A related hypothesis is that SSP helped individuals "learn" how to save, or helped them learn that saving was less difficult than they initially thought. The spillover effects on other types of saving are consistent with these theories, which could transition individuals to an entirely new savings equilibrium (Becker and Murphy, 1988).

An alternative way to rationalize crowd in would be if individuals have lumpy savings goals with high returns/marginal utility (e.g. a business asset or a high-value durable good like a vehicle), but there is some uncertainty over whether the savings goal can be met. If SSP increased the probability of reaching the savings goal, this could increase the return to saving in other places as well. We do have some suggestive evidence that SSP helped respondents reach savings goals. First, 69 percent of people who signed up for SSP reported that they did so with a specific goal in mind. Second, experience with SSP led participants to positively update their assessment of the product's usefulness for meeting goals.

Appendix Table A10 reports on how treatment impacted respondents' post-commitment assessments of SSP, both overall and by baseline overdrafter status.[17] In the control group, 21 percent of respondents strongly agreed that SSP would help to meet savings goals and 19 percent of respondents strongly agreed SSP would help build money for a large expense. Assignment to the treatment group increased these assessments by 14 and 19 percentage

---

[17]In our final survey round, we asked all respondents whether they thought SSP was useful for a range of purposes. SSP users answered based on their experience, while the control group had the product explained to them and were then asked to speculate as to whether the product would be useful for the given purposes. Here we focus on the share of respondents who "strongly agree" that SSP helps with a given aim, since the default response for most respondents was "agree".

points respectively, with similar effects for both heavy and light overdrafters. SSP also significantly increased the share of individuals who strongly agreed that the product helps manage transfer requests from people within the household (column 1) – this is broadly consistent with results in Appendix Table A9.

However, we cannot reject the null that the effects in Appendix Table A10 are the same for heavy and light overdrafters. Why are effects on downstream outcomes so different by overdrafter status when perceived benefits of the product are so similar? One possibility is that differential treatment effects are not driven by underlying behavioral mechanisms so much as they are driven by differences in financial fundamentals. Consider, for example, the "lumpy savings goal" hypothesis: in order for SSP to have an impact on the likelihood of meeting a savings goal, a saver's baseline probability of meeting a goal would have to be neither too low nor too high. It may be that light overdrafters, by virtue of being in a better savings position to begin with, were more likely to be marginal savers.

Ultimately, data constraints prevent us from testing the hypotheses discussed in this section more directly – we therefore leave further investigation of these questions to future research.

4   Conclusion

We use six rounds of detailed survey data as well as administrative bank data to study the impact of a commitment savings product for salaried workers in Ghana. Overall, the product increased savings at the partner bank, both during and after the commitment period, with limited crowd out of cash savings held in other places. These savings gains are offset by an increase in debt – as a result, the point estimate on net savings is small and insignificant. This suggests that people with access to liquidity may fail to benefit from commitment savings products because they can undo their commitment with other financial instruments.

However, we find divergent results when we examine heterogeneity in treatment effects by baseline propensity to take overdrafts with our partner bank. Heavy overdrafters – who had less liquid savings and more debt at baseline – see (marginally significant) declines in net savings when offered the product, while light overdrafters – who had considerably better access to liquidity – save more, both with the partner bank and in other savings devices, and do not take on more debt. Strikingly, these treatment effects manifest both during and after the commitment period.

Our results for heavy overdrafters are consistent with the hypothesis that time inconsistent agents make inefficient savings commitments, especially when they have access to liquidity that allows them to unwind their commitments. The results for light overdrafters

present a much bigger puzzle, especially because commitment crowded in liquid savings *during* the commitment period. Although optimally chosen commitment can increase the savings of time inconsistent agents, theory predicts that this will be at the expense of liquidity held elsewhere. Hence, our light overdrafter results underscore that commitment savings products can benefit consumers for reasons unrelated to time inconsistency.

What remains less clear is *why* commitment products benefit savers for other reasons. While we cannot isolate a single mechanism for our effects, we do identify several potential channels, including habit formation and learning. Ultimately, additional research is needed to explore the multiple pathways through which illiquid savings products transform individuals' financial lives.

References

Angeletos, G.-M., D. Laibson, A. Repetto, J. Tobacman, and S. Weinberg (2001). The Hyperbolic Consumption Model: Calibration, Simulation, and Empirical Evaluation. *The Journal of Economic Perspectives 15*(3), 47–68.

Banerjee, A. V. and E. Duflo (Winter 2007). The Economic Lives of the Poor. *The Journal of Economic Perspectives 21*(27), 141–167.

Bank of Ghana (2014). Statistical Bulletin: March 2013. Technical report, Bank of Ghana Statistics Department.

Bank of Ghana (2017). Monthly Statistical Bulletin: December 2016. Technical report, Bank of Ghana Statistics Department.

Becker, G. S. and K. M. Murphy (1988). A Theory of Rational Addiction. *The Journal of Political Economy*, 675–700.

Benjamin, D. J. (2003). Does 401 (k) Eligibility Increase Saving?: Evidence from Propensity Score Subclassification. *Journal of Public Economics 87*(5), 1259–1290.

Beshears, J., J. J. Choi, D. Laibson, B. C. Madrian, and W. L. Skimmyhorn (2017). Borrowing to Save? The Impact of Automatic Enrollment on Debt.

Brune, L., X. Gine, J. Goldberg, and D. Yang (2016). Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi. *Economic Development and Cultural Change 64*(2), 187–220.

Chetty, R., J. N. Friedman, S. Leth-Petersen, T. H. Nielsen, and T. Olsen (2014). Active vs. Passive Decisions and Crowd-Out in Retirement Savings Accounts: Evidence from Denmark. *The Quarterly Journal of Economics 129*(3), 1141–1219.

Collins, D., J. Morduch, S. Rutherford, and O. Ruthven (2009). *Portfolios of the Poor: How the World's Poor Live on Two Dollars a Day.* Princeton University Press.

Engelhardt, G. V. and A. Kumar (2007). Employer Matching and 401 (k) Saving: Evidence from the Health and Retirement Study. *Journal of Public Economics 91*(10), 1920–1943.

Gathergood, J. (2012). Self-Control, Financial Literacy and Consumer Over-Indebtedness. *Journal of Economic Psychology 33*(3), 590 – 602.

Gelber, A. M. (2011). How Do 401(k)s Affect Saving? Evidence from Changes in 401(k) Eligibility. *American Economic Journal*.

Ghana Statistical Service (2012). 2010 Population and Housing Census: Summary of Final Results.

Ghana Statistical Service (2013). 2010 Population and Housing Census: Regional Analytical Report: Volta Region. Technical report.

Islam, A. and P. Maitra (2012). Health Shocks and Consumption Smoothing in Rural Households: Does Microcredit Have a Role to Play? *Journal of Development Economics 97*(2), 232 – 243.

John, A. (2017). When Commitment Fails - Evidence from a Field Experiment.

Karlan, D., A. L. Ratan, and J. Zinman (2014). Savings by and for the Poor: A Research Review and Agenda. *Review of Income and Wealth 60*(1), 36–78.

Laibson, D. (1997). Golden Eggs and Hyperbolic Discounting. *Quarterly Journal of Economics 112*(2), 443–477.

Laibson, D. (2015). Why Don't Present-Biased Agents Make Commitments? *The American Economic Review 105*(5), 267–272.

Meier, S. and C. Sprenger (2010). Present-Biased Preferences and Credit Card Borrowing. *American Economic Journal: Applied Economics 2*(1), 193–210.

Melzer, B. T. (2011). The Real Costs of Credit Access: Evidence from the Payday Lending Market. *The Quarterly Journal of Economics 126*(1), 517–555.

Morse, A. (2011). Payday Lenders: Heroes or Villains? *Journal of Financial Economics 102* (1), 28–44.

Poterba, J. M., S. F. Venti, and D. A. Wise (1996). How Retirement Saving Programs Increase Saving. *The Journal of Economic Perspectives 10* (4), 91–112.

Table 1: Demographic Differences By Baseline Overdrafter Status

	(1)	(2)	(3)
	Light Overdrafter Mean	Heavy- Light Difference	N
Fraction Months in Overdraft: Past Year	0.0578	0.481***	318
	[0.141]	(0.0242)	
Female	0.3	-.129***	318
	[0.46]	(0.0472)	
Age	45.5	4.53***	318
	[9.63]	(0.945)	
Teacher	0.469	0.202***	318
	[0.501]	(0.0545)	
Other Govt. Employee	0.35	-.0272	318
	[0.478]	(0.0531)	
Formal Private Sector Worker	0.188	-.181***	318
	[0.392]	(0.0316)	
Education: Secondary Graduate	0.344	-.0526	318
	[0.476]	(0.0523)	
Education: Postsecondary	0.606	0.0583	318
	[0.49]	(0.054)	
Married	0.85	0.0487	318
	[0.358]	(0.0372)	
Biological Children Ever Born	3.14	1.32***	318
	[2.1]	(0.252)	
Householders 14 and Under	2.15	-.0677	318
	[1.73]	(0.19)	
Householders 15 and Older	3.5	0.69***	318
	[2.04]	(0.237)	
Income Last Month	729	15.4	302
	[372]	(45.3)	
Savings - NVRB (Reported)	324	-186***	283
	[612]	(58.9)	
Savings - NVRB (Adminstrative)	356	-243***	318
	[699]	(63.2)	
Total Cash Savings	1672	-399	249
	[3249]	(358)	
Total Debt	1930	1252***	308
	[2428]	(329)	
Economic Shock	0.481	0.0377	318
	[0.501]	(0.0562)	
Financial Strain: Missed School	0.313	-.00237	318
	[0.465]	(0.0521)	
Financial Strain: Pay Bills	0.675	0.116**	318
	[0.47]	(0.0493)	
Financial Strain: Pay Debt	0.563	0.0957*	318
	[0.498]	(0.0546)	
Socially Taxed	0.619	-.0301	318
	[0.487]	(0.055)	
Any Food Insecurity	0.444	0.0562	318
	[0.498]	(0.0561)	

Notes: Standard deviations in brackets, heteroskedasticity robust standard errors in parentheses. All variables denominated in Ghanaian Cedis top-coded at the 99th percentile. In 2013 GHS 2.15 ≈ USD 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table 2: Impacts on NVRB Account Use, Administrative Data

	(1) Acct. in Over-draft	(2) Overdraft Charges	(3) Net Deposits	(4) Average Daily Balance
Treatment × During	0.0095	0.55	19.5**	243.5***
	(0.020)	(1.36)	(8.11)	(43.9)
Treatment × Endline	-0.060*	-3.77*	-3.96	140.4**
	(0.032)	(2.17)	(26.7)	(63.2)
Treatment × Post-Endline	-0.0056	-0.55	32.5	141.2*
	(0.030)	(2.36)	(23.2)	(84.1)
Control Mean (During)	0.30	16.3	-19.3	89.0
Control Mean (Endline)	0.33	18.3	1.99	110.1
Control Mean (Post-Endline)	0.26	17.5	18.8	110.6
N	9540	9540	9540	9540
Baseline Control?	Yes	Yes	Yes	Yes

Notes: All regressions control for strata and month fixed effects as well as the average outcome in the year before the baseline survey. Robust standard errors clustered at the individual level in parentheses. Data is at the person-month level. Overdrafter is a dummy equal to one if the participant paid either overdraft interest or the fee for an overdraft form in a given month. Monthly overdrafter charges topcoded at the 99th percentile, average daily balance topcoded at the 1st and 99th percentile. Net deposit is the difference between total deposits and withdrawals, both topcoded at the 99th percentile. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Table 3: Impacts on Total Cash Savings, Debt, and Savings Net Debt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Savings			Debt		
	Savings at NVRB (Admin)	Savings at NVRB (Survey)	Other Formal Savings	Informal Savings	Total Savings	Took Any New Debt	Total Debt	Savings Net Debt
Treatment × During	303.1***	288.2***	-47.7	-20.7	215.1	0.076***	283.3	-82.9
	(43.7)	(42.0)	(118.1)	(22.3)	(139.4)	(0.029)	(283.3)	(346.5)
Treatment × After	91.3*	25.0	127.5	2.57	145.0	0.046	195.1	1.38
	(52.8)	(49.3)	(142.6)	(45.6)	(167.7)	(0.031)	(269.1)	(354.6)
Control Mean (During)	164.6	214.2	979.9	172.9	1377.4	0.19	3080.8	-1687.8
Control Mean (After)	199.5	256.7	921.1	236.2	1424.1	0.15	2636.7	-1244.9
N	1528	1499	1518	1527	1488	1528	1519	1481
Baseline Control?	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes

Notes: All regressions include strata fixed effects, time period fixed effects, and the baseline outcome of the dependent variable whenever possible. Robust standard errors clustered at the individual level in parentheses. Administrative NVRB savings is the balance on the relevant survey day, with negative balances coded to zero. All variables in Ghanain Cedis top coded at the 99th percentile. In 2013 GHS 2.15 ≈ USD 1. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Table 4: Impacts on Total Cash Savings, Debt, and Savings Net Debt - Heterogeneity by Baseline Overdraft Behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Savings			Debt		
	Savings at NVRB (Admin)	Savings at NVRB (Survey)	Other Formal Savings	Informal Savings	Total Savings	Took Any New Debt	Total Debt	Savings Net Debt
Treatment × During × Light Overdrafter	322.4***	326.6***	341.7**	-81.2**	573.8***	0.043	72.5	621.8
	(76.8)	(71.4)	(166.5)	(36.4)	(200.2)	(0.028)	(369.9)	(471.3)
Treatment × After × Light Overdrafter	101.2	54.7	567.3***	-8.97	596.4**	0.021	-76.5	794.4*
	(97.4)	(85.7)	(205.1)	(63.9)	(242.9)	(0.036)	(338.1)	(459.1)
Treatment × During × Heavy Overdrafter	284.2***	251.2***	-428.6***	38.7	-136.6	0.11**	492.6	-776.1
	(43.0)	(46.8)	(158.6)	(26.1)	(185.8)	(0.050)	(426.0)	(472.2)
Treatment × After × Heavy Overdrafter	81.0**	-4.04	-312.0*	15.3	-297.4	0.069	463.3	-771.4
	(39.4)	(51.8)	(186.9)	(65.9)	(221.5)	(0.051)	(418.0)	(499.4)
P-value: Heavy=Light, During	0.665	0.382	0.001***	0.009***	0.009***	0.253	0.456	0.031**
P-value: Heavy=Light, After	0.847	0.561	0.002***	0.793	0.006***	0.437	0.316	0.017**
P-value: Treat=0, Light	0.000***	0.000***	0.021**	0.079*	0.011**	0.308	0.892	0.221
P-value: Treat=0, Heavy	0.000***	0.000***	0.026**	0.333	0.406	0.060*	0.440	0.195
Control Mean (During, Light)	292.1	293.2	780.2	234.1	1311.7	0.081	2552.9	-1257.1
Control Mean (After, Light)	336.7	349.4	731.2	246.9	1328.1	0.088	2322.0	-989.5
Control Mean (During, Heavy)	42.0	137.9	1174.4	113.9	1442.0	0.29	3590.2	-2114.5
Control Mean (After, Heavy)	65.9	165.3	1105.9	225.7	1518.8	0.20	2943.2	-1496.9
N	1528	1499	1518	1527	1488	1528	1519	1481
Baseline Control?	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes

Notes: All regressions include strata fixed effects, time period fixed effects, interactions between overdrafter status and time period, and the baseline outcome of the dependent variable whenever possible. Robust standard errors clustered at the individual level in parentheses. Administrative NVRB savings is the balance on the relevant survey day, with negative balances coded to zero. All variables in Ghanain Cedis top coded at the 99th percentile. In 2013 GHS 2.15 ≈ USD 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Figure 1: Administrative Data – Impact on NVRB Balance by Month and Overdrafter Status
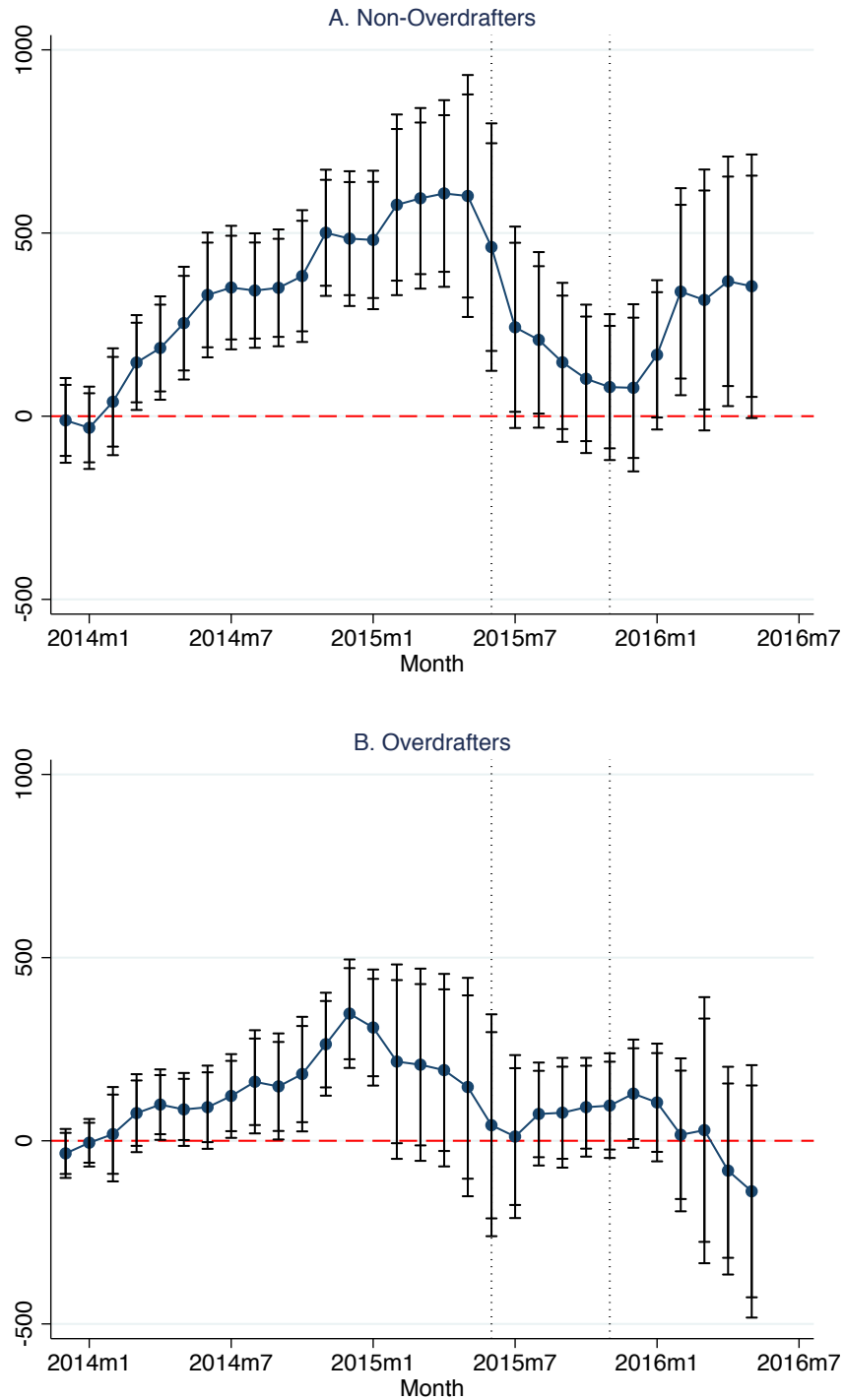
Notes: Whiskers give 90 and 95 percent confidence intervals on estimates. Average monthly balance is top-coded at the 99 percent level by month.

A Appendix

A.1 Sampling

NVRB is a small bank, with eight branches in five districts serving roughly 10,000 account holders at the time of the experiment.[18] We attempted to enroll all workers who received electronic salary payments into an NVRB account. Of the 420 workers we approached, 320 met our inclusion criteria and were willing to participate in the study. Workers were excluded if (a) their electronic payments were not salaries (e.g. pension recipients), (b) they expected to retire over the study duration, (c) they expected to move out of the study area, or (d) they had all deposits to the NVRB account immediately transferred to another bank. Of the 100 individuals from the initial list who were not included in the study sample, 63 met one of the exclusion criteria, 10 were on chronic sick bed or mentally ill, 3 were deceased, and 24 were not interested in participating in the study.

Half of individuals enrolled in the study were offered the opportunity to sign up for SSP. We stratified the randomization by heavy overdrafter×branch×gender, which generated 28 distinct strata. The heavy overdrafter dummy was constructed by first creating an index of overdrafting behavior. Overdraft scores were constructed for each person using their current account transaction records available at the time of the randomization. The score was calculated as the fraction of balance entries that were negative over the life of the account. The sample was sorted from lowest score (never had a negative balance) to highest (negative balance 66 percent of the time), with the top 50 percent of the sample classified as "heavy overdrafters".

A.2 Data Collection

Appendix Figure A2 shows the study timeline. In April 2013 we used administrative data from NVRB to compile a list of 420 salaried workers receiving salary deposits into an NVRB account. We contacted and enrolled these workers between April and September 2013. The baseline followed in September-October 2013, before SSP was marketed to the treatment group. During November 2013, we conducted the randomization and enrolled interested treatment group clients in SSP. The first three follow-up surveys took place while SSP was ongoing – during June 2014, October 2014, and February 2014 (months 6, 10, and 14 of the commitment cycle). SSP savings were released in June 2015. The last two follow-ups took place between September-November 2014 (months 3 and 5 after the SSP payout). The key follow-up survey modules included savings, debt, expenditures, and assets. We collected

---

[18]The districts are Kratchi East, Biakoye, Jasikan, Kadjebi, and Nkwanta South.

data on income by source in the baseline and fourth follow-up. The baseline collected additional detail on respondents' demographic characteristics. Selected follow-up survey rounds also asked about economic shocks, financial well-being, intra-household decision-making, and (unincentivized) time preferences. The final follow-up also asked respondents about perceived benefits of SSP and interest in signing up for future SSP cycles.

A.3   Attrition

Study attrition was very low, with 91 percent of the sample interviewed in each survey round. Appendix Table A2 formally verifies that attrition is uncorrelated with treatment status – here, we see that treatment-control differences in attrition are small in magnitude and almost never statistically significant, for both heavy and light overdrafters.

A.4   Summary of Key Variables

This subsection summarizes some of the key outcome variables in the study, and provides additional detail on how we constructed them. All variables that were winsorized were done so by survey round/month.

Administrative Data

- *Account in Overdraft* – A dummy variable equal to one in months where an individual paid for an overdraft form or paid overdraft interest. Equal to one if an overdraft is taken on any NVRB account.

- *Overdraft Charges* – Charges associated with issuance of an overdraft or overdraft interest. Sums across all NVRB accounts.

- *Net Deposits* – Total monthly deposits less total monthly debits (including withdrawals and fees), summed across all NVRB accounts. Total monthly deposits and withdrawals are each winsorized at the 99th percentile before taking the difference.

- *Average Daily Balance* – The average daily balance across all NVRB accounts. Accounts in overdraft are included with a negative balance. Winsorized at the first and 99th percentile.

- *NVRB Savings at Survey Date* – Total balance across all accounts on date of survey. Accounts with negative balances (e.g. due to an overdraft) are coded to zero before summing. Winsorized at the 99th percentile.

- *Salary Deposited into NVRB Accounts* – Monthly salary deposits made into NVRB accounts, based on account narration text. Winsorized at the 99th percentile.

Survey Data

- *NVRB Savings* – Respondent self-reported NVRB balances. In the survey, respondents were asked to report non-SSP balances. SSP balances were added on top of this amount based on administrative data. Winsorized at the 99th percentile.

- *Other Formal Savings* – Includes self-reported savings at other banks, microfinance institutions, and credit unions. Winsorized at the 99th percentile.

- *Informal Savings* – Includes self-reported savings in cash and in savings clubs. Additional categories covered varied from survey round to survey round. At baseline, this measure also includes savings in agricultural co-ops, savings held by other individuals and susu collectors, and moneylenders. Follow-ups 1, 2, and 3 include mobile money savings and "other informal cash savings", which was collected in a free answer format. This mostly includes money hidden at home and money stored with other individuals. Follow ups 4 and 5 includes the same categories as follow ups 1-3, but explicitly prompted respondents to report savings held with other individuals including friends and relatives. Winsorized at the 99th percentile.

- *Total Savings* – Sum of (winsorized) self-reported NVRB savings, other formal savings, and informal savings.

- *Took Any New Debt* – Dummy variable equal to one if individual reported taking on any new debt in the past 30 days.

- *Debt with NVRB* – Self-reported debt held at NVRB. Winsorized at the 99th percentile.

- *Other Formal Debt* – At baseline includes self-reported debt held at other banks, credit unions, and microfinance organizations. Measures for all follow up surveys include these categories as well as insurance companies and the teachers fund. Winsorized at the 99th percentile.

- *Informal Debt* – At baseline includes self-reported debt with individual and group susus. Follow-up surveys include this, as well as other debt (collected in a free form answer) from informal sources, such as shop keepers, traders, and individuals. Winsorized at the 99th percentile.

- *Total Debt* – Sum of (winsorized) debt with NVRB, other formal, and informal debt.

- *Savings Net Debt* – Difference between total savings and total debt, both as defined above.

- *Child Missed Class for Unpaid School Fees* – Dummy variable equal to one if a child missed school because the respondent was not able to pay their school fees. The way this variable is constructed varies from round to round. At the baseline, the variable is equal to one if the respondent reports that children have to miss school "often" or "sometimes" due to missed school payments. lookback period on this question varied. In the first, second, and third follow up, the dummy is equal to one if a child missed school due to a late payment in the past 30 days. Follow up 4 asked respondents to report on the period between SSP release (June 1, 2015) and the survey and follow up 5 asked about the past 60 days.

- *Concerned About Paying Bills* – Dummy variable equal to one if the respondent reported not having enough money to pay normal monthly expenses. The construction is similar to the "missed school" variable, in that the baseline dummy identifies individuals who report concern often or sometimes, while the follow ups identify respondents who report actual difficulty. The follow up look back periods are the same as they are for the missed school variable.

- *Concerned About Repaying Debt* – Dummy variable equal to one if the respondent reported not having enough money to service scheduled debt payments. The construction is similar to the "missed school" variable, in that the baseline dummy identifies individuals who report concern often or sometimes, while the follow ups identify respondents who report actual difficulty. The follow up look back periods are the same as they are for the missed school variable.

- *Experienced Food Insecurity* – Dummy to identify any kind of food insecurity. Equal to one if respondent reports days when there was not enough food to meet the needs of the family or days when adults cut or skipped meals because there was not enough food rarely, sometimes, or often. The lookback period varied by survey round: it was 12 months at baseline, 30 days for follow ups 1-3, since June 1 2015 for follow up 4, and 60 days for follow up 5.

- *Personal Consumption* – Respondent's spending on his/her self (excludes household expenses such as rent, utilities and food). The lookback period was 30 days for follow ups 1-3, since June 1 2015 for follow up 4, and 60 days for follow up 5. Values are re-normalized to correspond to a 30 day window for each survey round. Winsorized at the 99th percentile.

- *Expenditure on Household and Others* – Respondent's spending on the household and dependents (includes expenses such as rent, utilities, food, and education). The lookback period was 30 days for follow ups 1-3, since June 1 2015 for follow up 4, and 60 days for follow up 5. Values are re-normalized to correspond to a 30 day window for each survey round. Winsorized at the 99th percentile.

- *Expenditure on Investments* – Respondent's spending on investments. The lookback period was 30 days for follow ups 1-3, since June 1 2015 for follow up 4, and 60 days for follow up 5. Values are re-normalized to correspond to a 30 day window for each survey round. Winsorized at the 99th percentile.

- *Monthly Salary and Wages* – Income in the past month from salaried and wage jobs. Winsorized at the 99th percentile. Note that income modules were only administered at baseline and at follow up survey 4.

- *Monthly Self-Employment Income* – Income in the past month from self employment. Winsorized at the 99th percentile. Note that income modules were only administered at baseline and at follow up survey 4.

- *Total Monthly Income* – Sum of salary/wage and self employment income as defined above. Note that income modules were only administered at baseline and at follow up survey 4.

- *SSP Reduces Transfer Pressure in Household* – Dummy variable equal to one if respondent strongly agrees that SSP makes it easier to say no to requests for money within the household.

- *SSP Reduces Transfer Pressure Outside Household* – Dummy variable equal to one if respondent strongly agrees that SSP makes it easier to say no to requests for money coming from outside the household.

- *SSP Helps Meet Savings Goals* – Dummy variable equal to one if the respondent strongly agrees that SSP makes it easier to meet savings goals.

- *SSP Helps Gather Money for Large Expense* – Dummy variable equal to one if the respondent strongly agrees that SSP makes it easier to gather money for a large expense.

- *SSP Helps Build Funds for Emergency* – Dummy variable equal to one if the respondent strongly agrees that SSP makes the respondent more likely to have funds available in case of an emergency.

Table A1: Demographic Characteristics and Randomization Verification

	(1) Whole Sample	(2) Whole Sample	(3)	(4) Heavy Overdrafters	(5) Heavy Overdrafters	(6)	(7) Light Overdrafters	(8) Light Overdrafters	(9)
	Control Mean	Treat-Control Difference	N	Control Mean	Treat-Control Difference	N	Control Mean	Treat-Control Difference	N
Fraction Months in Overdraft: Past Year	0.286	0.0209	318	0.525	0.0274	158	0.0475	0.0204	160
	[0.319]	(0.0362)		[0.274]	(0.043)		[0.121]	(0.0222)	
Female	0.234	0.00332	318	0.165	0.0127	158	0.304	-.0075	160
	[0.425]	(0.0478)		[0.373]	(0.0603)		[0.463]	(0.0729)	
Age	47.3	0.965	318	50.1	-.0633	158	44.5	2.04	160
	[8.98]	(0.979)		[7.04]	(1.12)		[9.84]	(1.52)	
Teacher	0.563	0.0117	318	0.684	-.0253	158	0.443	0.0508	160
	[0.498]	(0.0557)		[0.468]	(0.0752)		[0.5]	(0.0793)	
Other Govt. Employee	0.335	0.00206	318	0.316	0.0127	158	0.354	-.00875	160
	[0.474]	(0.0532)		[0.468]	(0.0749)		[0.481]	(0.0759)	
Formal Private Sector Worker	0.101	-.00752	318	0	0.0127	158	0.203	-.0297	160
	[0.303]	(0.0334)		[0]	(0.0127)		[0.404]	(0.0621)	
Education: Secondary Graduate	0.304	0.0275	318	0.304	-.0253	158	0.304	0.0789	160
	[0.461]	(0.0523)		[0.463]	(0.0727)		[0.463]	(0.0753)	
Education: Postsecondary	0.665	-.0583	318	0.671	-.0127	158	0.658	-.103	160
	[0.474]	(0.054)		[0.473]	(0.0756)		[0.477]	(0.0773)	
Married	0.861	0.0267	318	0.886	0.0253	158	0.835	0.0288	160
	[0.347]	(0.0373)		[0.32]	(0.0483)		[0.373]	(0.0568)	
Biological Children Ever Born	3.84	-.0793	318	4.51	-.0759	158	3.18	-.0661	160
	[2.34]	(0.262)		[2.39]	(0.379)		[2.1]	(0.334)	
Householders 14 and Under	2.2	-.159	318	2.23	-.291	158	2.16	-.0288	160
	[1.78]	(0.19)		[1.73]	(0.262)		[1.84]	(0.276)	
Householders 15 and Older	3.78	0.128	318	4.16	0.0506	158	3.39	0.213	160
	[2.27]	(0.24)		[2.28]	(0.348)		[2.21]	(0.324)	
Income Last Month	770	-67.1	302	746	-3.33	148	794	-128**	154
	[421]	(45)		[425]	(68.1)		[419]	(59.4)	
Savings - NVRB (Reported)	249	-33.2	283	125	26.3	139	368	-89.5	144
	[528]	(60.3)		[330]	(59.1)		[645]	(102)	
Savings - NVRB (Adminstrative)	243	-14.9	318	106	14.4	158	380	-46.8	160
	[614]	(64.9)		[411]	(61.7)		[743]	(111)	
Total Cash Savings	1628	-315	249	1314	-83.3	124	1937	-543	125
	[3269]	(357)		[1825]	(423)		[4231]	(572)	
Total Debt	2506	82.3	308	3127	111	152	1886	86.7	156
	[2877]	(336)		[3108]	(535)		[2495]	(390)	
Economic Shock	0.468	0.0629	318	0.494	0.0506	158	0.443	0.0755	160
	[0.501]	(0.0561)		[0.503]	(0.0799)		[0.5]	(0.0793)	
Financial Strain: Missed School	0.342	-.0605	318	0.329	-.038	158	0.354	-.0828	160
	[0.476]	(0.052)		[0.473]	(0.074)		[0.481]	(0.0735)	
Financial Strain: Pay Bills	0.709	0.0474	318	0.747	0.0886	158	0.671	0.00813	160
	[0.456]	(0.0497)		[0.438]	(0.0647)		[0.473]	(0.0745)	
Financial Strain: Pay Debt	0.589	0.0426	318	0.633	0.0506	158	0.544	0.0359	160
	[0.494]	(0.0548)		[0.485]	(0.0758)		[0.501]	(0.0789)	
Socially Taxed	0.614	-.0202	318	0.557	0.0633	158	0.671	-.103	160
	[0.488]	(0.055)		[0.5]	(0.0786)		[0.473]	(0.0768)	
Any Food Insecurity	0.456	0.0318	318	0.481	0.038	158	0.43	0.0264	160
	[0.5]	(0.0561)		[0.503]	(0.08)		[0.498]	(0.079)	

Notes: Standard deviations in brackets, heteroskedasticity robust standard errors in parentheses. All variables denominated in Ghanaian Cedis top-coded at the 99th percentile. In 2013 GHS 2.15 ≈ USD 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table A2: Tests for Differential Attrition by Treatment Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Whole Sample			Heavy Overdrafters			Non-Heavy Overdrafters		
	Control Mean	Treat-Control Difference	N	Control Mean	Treat-Control Difference	N	Control Mean	Treat-Control Difference	N
In Baseline	0.994	0.0000781	320	0.988	0	160	1	0	160
	[0.0793]	(0.00884)		[0.112]	(0.0177)		[0]	(0)	
In Endline 1	0.962	0.00668	320	0.975	0	160	0.949	0.0136	160
	[0.191]	(0.0204)		[0.157]	(0.0248)		[0.221]	(0.0326)	
In Endline 2	0.962	-.00574	320	0.975	-.025	160	0.949	0.0136	160
	[0.191]	(0.0221)		[0.157]	(0.0302)		[0.221]	(0.0326)	
In Endline 3	0.937	0.0194	320	0.95	0	160	0.924	0.0389	160
	[0.244]	(0.0252)		[0.219]	(0.0347)		[0.267]	(0.0367)	
In Endline 4	0.943	0.0255	320	0.95	0	160	0.937	0.0509*	160
	[0.232]	(0.0229)		[0.219]	(0.0347)		[0.245]	(0.0302)	
In Endline 5	0.943	0.00691	320	0.95	-.0125	160	0.937	0.0263	160
	[0.232]	(0.0252)		[0.219]	(0.0366)		[0.245]	(0.0347)	
In All Survey Rounds	0.906	0.00738	320	0.913	-.0125	160	0.899	0.0272	160
	[0.293]	(0.0322)		[0.284]	(0.0464)		[0.304]	(0.045)	

Notes: Standard deviations in brackets, heteroskedasticity robust standard errors in parentheses. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table A3: Predictors of SSP Take Up and Drop Out

	(1)	(2)	(3)	(4)
	Pairwise		Joint	
	Take Up	Drop Out	Take Up	Drop Out
Heavy Overdrafter	0.005	0.124**	-0.001	0.063
	(0.072)	(0.063)	(0.082)	(0.072)
Female	0.024	-0.078	0.090	0.003
	(0.083)	(0.062)	(0.090)	(0.071)
Age	0.013***	0.002	0.016***	-0.003
	(0.004)	(0.004)	(0.006)	(0.007)
Teacher	-0.054	0.093	-0.361***	0.033
	(0.072)	(0.061)	(0.132)	(0.141)
Other Govt. Employee	0.033	-0.085	-0.337**	-0.043
	(0.075)	(0.060)	(0.154)	(0.153)
Education: Secondary Graduate	-0.003	-0.038	-0.176	0.049
	(0.076)	(0.064)	(0.158)	(0.078)
Education: Postsecondary	-0.019	0.072	-0.181	-0.014
	(0.073)	(0.061)	(0.196)	(0.135)
Married	0.059	0.053	0.118	0.014
	(0.118)	(0.087)	(0.135)	(0.107)
Biological Children Ever Born	0.038**	0.024	-0.002	0.022
	(0.017)	(0.016)	(0.021)	(0.021)
Householders 14 and Under	0.039*	-0.009	0.042	-0.026
	(0.022)	(0.021)	(0.027)	(0.025)
Householders 15 and Older	0.014	0.024	0.001	0.009
	(0.019)	(0.017)	(0.024)	(0.024)
Income Last Month / 1000	0.065	0.223**	0.143	0.100
	(0.103)	(0.105)	(0.149)	(0.121)
Total Cash Savings / 1000	0.002	-0.012*	-0.010	-0.013
	(0.014)	(0.007)	(0.017)	(0.009)
Total Debt / 1000	-0.007	0.035**	-0.005	0.022
	(0.013)	(0.015)	(0.013)	(0.018)
Economic Shock	0.023	-0.003	0.043	0.004
	(0.072)	(0.064)	(0.080)	(0.082)
Financial Strain: Missed School	0.113	0.012	0.157*	0.009
	(0.074)	(0.069)	(0.084)	(0.079)
Financial Strain: Pay Bills	0.103	-0.191**	0.188*	-0.187*
	(0.087)	(0.096)	(0.098)	(0.107)
Financial Strain: Pay Debt	-0.123*	-0.031	-0.161*	-0.006
	(0.071)	(0.066)	(0.088)	(0.075)
Socially Taxed	0.019	0.072	0.036	0.098
	(0.073)	(0.061)	(0.075)	(0.068)
Any Food Insecurity	-0.077	0.003	-0.065	0.019
	(0.071)	(0.064)	(0.078)	(0.072)
DV Mean	0.719	0.130	0.719	0.130
N	160	115	160	115

Notes: Heteroskedasticity robust standard errors in parentheses. The first two columns show results of pairwise regressions (each cell represents a separate regression). The last two columns show results from regressions where all demographic variables are jointly included as covariates. All variables denominated in Ghanaian Cedis top-coded at the 99th percentile. In 2013 GHS 2.15 ≈ USD 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table A4: Impacts on Debt by Source

	(1) Debt With NVRB	(2) Other Formal Debt	(3) Informal Debt
Treatment × During	29.6	240.6	5.68
	(174.6)	(216.4)	(5.97)
Treatment × After	-153.5	338.5	0.96
	(157.1)	(225.6)	(8.81)
Control Mean (During)	1563.7	1490.6	10.6
Control Mean (After)	1471.6	1150.1	15
N	1520	1527	1528
Baseline Control?	Yes	Yes	Yes

Notes: All regressions include strata fixed effects, time period fixed effects, and the baseline outcome of the dependent variable whenever possible. Robust standard errors clustered at the individual level in parentheses. All variables in Ghanain Cedis top coded at the 99th percentile. In 2013 GHS 2.15 ≈ USD 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table A5: Impacts on Financial Strain

	(1) Child Missed Class for Unpaid School Fees	(2) Concerned About Paying Bills	(3) Concerned About Repaying Debt	(4) Experienced Food Insecurity
Treatment × During	-0.012	0.050	0.020	-0.019
	(0.034)	(0.037)	(0.032)	(0.038)
Treatment × After	0.021	0.0084	0.030	-0.0015
	(0.037)	(0.044)	(0.038)	(0.041)
Control Mean (During)	0.27	0.49	0.22	0.40
Control Mean (After)	0.21	0.35	0.22	0.28
N	1516	1510	1497	1528
Baseline Control?	Yes	Yes	Yes	Yes

Notes: All regressions include strata fixed effects, time period fixed effects, and the baseline outcome of the dependent variable whenever possible. Robust standard errors clustered at the individual level in parentheses. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table A6: Impacts on NVRB Account Use, Administrative Data - Heterogeneity by Baseline Overdraft Behavior

	(1) Acct. in Over- draft	(2) Overdraft Charges	(3) Net Deposits	(4) Average Daily Balance
Treatment × During × Light Overdrafter	0.018	0.62	35.7***	339.0***
	(0.019)	(1.06)	(12.5)	(66.9)
Treatment × Endline × Light Overdrafter	-0.058*	-3.85**	-11.0	213.6**
	(0.029)	(1.89)	(44.8)	(103.8)
Treatment × Post-Endline × Light Overdrafter	-0.020	-1.19	89.5**	278.2**
	(0.029)	(2.32)	(35.1)	(132.0)
Treatment × During × Heavy Overdrafter	0.0015	0.48	2.87	147.1***
	(0.036)	(2.52)	(10.2)	(55.7)
Treatment × Endline × Heavy Overdrafter	-0.062	-3.72	3.16	65.9
	(0.057)	(3.93)	(28.0)	(71.0)
Treatment × Post-Endline × Heavy Overdrafter	0.0078	0.089	-25.3	2.22
	(0.052)	(4.13)	(29.3)	(102.8)
P-value: Heavy=Light, During	0.691	0.959	0.044**	0.028**
P-value: Heavy=Light, Endline	0.948	0.976	0.788	0.241
P-value: Heavy=Light, Post-Endline	0.643	0.788	0.012**	0.100
P-value: Treat=0, Light	0.030**	0.052*	0.003***	0.000***
P-value: Treat=0, Heavy	0.312	0.527	0.837	0.072*
Control Mean (During, Light)	0.070	3.22	-20.1	263.0
Control Mean (Endline, Light)	0.12	6.57	3.56	322.3
Control Mean (Post-Endline, Light)	0.099	5.96	-13.9	288.9
Control Mean (During, Heavy)	0.54	29.3	-18.4	-85.0
Control Mean (Endline, Heavy)	0.54	30.0	0.42	-102.1
Control Mean (Post-Endline, Heavy)	0.43	29.0	51.5	-67.7
N	9540	9540	9540	9540
Baseline Control?	Yes	Yes	Yes	Yes

Notes: All regressions control for strata and month fixed effects, interactions between overdrafter status and period (endline, post-endline), and the average outcome in the year before the baseline survey. Robust standard errors clustered at the individual level in parentheses. Data is at the person-month level. Overdrafter is a dummy equal to one if the participant paid either overdraft interest or the fee for an overdraft form in a given month. Monthly overdrafter charges topcoded at the 99th percentile, average daily balance topcoded at the 1st and 99th percentile. Net deposit is the difference between total deposits and withdrawals, both topcoded at the 99th percentile. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Table A7: Impacts on Debt by Source - Heterogeneity by Baseline Overdraft Behavior

	(1) Debt With NVRB	(2) Other Formal Debt	(3) Informal Debt
Treatment × During × Light Overdrafter	-38.4	135.8	-5.11
	(269.8)	(277.2)	(7.82)
Treatment × After × Light Overdrafter	-386.9	313.4	3.67
	(234.6)	(255.2)	(14.2)
Treatment × During × Heavy Overdrafter	98.0	344.7	16.3*
	(219.6)	(328.8)	(9.03)
Treatment × After × Heavy Overdrafter	78.7	361.3	-1.81
	(205.1)	(372.4)	(10.3)
P-value: Heavy=Light, During	0.694	0.626	0.075*
P-value: Heavy=Light, After	0.135	0.915	0.755
P-value: Treat=0, Light	0.115	0.445	0.659
P-value: Treat=0, Heavy	0.883	0.500	0.176
Control Mean (During, Light)	1504.3	1021.6	17.6
Control Mean (After, Light)	1598.3	709.0	14.7
Control Mean (During, Heavy)	1621.1	1941.4	3.83
Control Mean (After, Heavy)	1348.2	1579.7	15.3
N	1520	1527	1528
Baseline Control?	Yes	Yes	Yes

 Notes: All regressions include strata fixed effects, time period fixed effects, interactions between overdrafter status and time period, and the baseline outcome of the dependent variable whenever possible. Robust standard errors clustered at the individual level in parentheses. All variables in Ghanain Cedis top coded at the 99th percentile. In 2013 GHS 2.15 ≈ USD 1. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Table A8: Impacts on Expenditure and Income - Heterogeneity by Baseline Overdraft Behavior

	(1)	(2)	(3)	(4)	(5)	(6)
	Expenditure			Income		
	Personal Con-sumption	Spending on House-hold and Others	Expenditure on Invest-ments	Salary and Wages	Self Em-ployment	Total
Treatment × During × Light Overdrafter	-11.2	0.65	-24.8			
	(18.4)	(83.4)	(42.7)			
Treatment × After × Light Overdrafter	-2.12	-102.5	0.93	114.3**	29.8	161.9**
	(13.6)	(80.7)	(43.1)	(55.2)	(27.3)	(66.3)
Treatment × During × Heavy Overdrafter	31.9	144.2	7.65			
	(20.1)	(100.8)	(36.4)			
Treatment × After × Heavy Overdrafter	-0.48	93.8	-1.36	4.81	23.3	27.5
	(12.7)	(79.6)	(38.5)	(81.8)	(28.8)	(83.1)
P-value: Heavy=Light, During	0.115	0.273	0.563			
P-value: Heavy=Light, After	0.930	0.084*	0.968	0.270	0.867	0.206
P-value: Treat=0, Light	0.831	0.311	0.818	0.039**	0.276	0.015**
P-value: Treat=0, Heavy	0.262	0.294	0.973	0.953	0.420	0.740
Control Mean (During, Light)	139.2	789.9	155.2			
Control Mean (After, Light)	81.8	732.0	106.3	712.8	54.1	766.9
Control Mean (During, Heavy)	128.9	842.2	128.5			
Control Mean (After, Heavy)	83.4	658.4	87.0	843.7	32.3	876.0
N	1528	1528	1528	306	306	306
Baseline Control?	No	No	No	Yes	Yes	Yes

Notes: All regressions include strata fixed effects, time period fixed effects, interactions between overdrafter status and time period, and the baseline outcome of the dependent variable whenever possible. Robust standard errors clustered at the individual level in parentheses. All variables in Ghanain Cedis top coded at the 99th percentile. In 2013 GHS 2.15 ≈ USD 1. Survey data on income only available at baseline and in follow up survey 4. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table A9: Impacts on Overall Cash Savings by Baseline Other Control

|	Savings					Debt		
	(1) Savings at NVRB (Admin)	(2) Savings at NVRB (Survey)	(3) Other Formal Savings	(4) Informal Savings	(5) Total Savings	(6) Took Any New Debt	(7) Total Debt	(8) Savings Net Debt
Treatment × During	320.669***	307.723***	-70.619	-33.662	178.277	0.057	11.572	173.230
	(65.958)	(67.313)	(160.979)	(26.211)	(187.036)	(0.038)	(356.207)	(404.700)
Treatment × During × Other Control	-24.511	-20.091	77.412	21.291	89.924	0.033	526.606	-465.372
	(76.088)	(77.384)	(181.515)	(30.331)	(207.053)	(0.045)	(415.209)	(459.911)
Treatment × After	215.657**	127.123	145.120	-74.361	168.866	0.054	-59.809	304.677
	(87.049)	(84.844)	(183.638)	(51.846)	(215.301)	(0.043)	(347.078)	(407.878)
Treatment × After × Other Control	-210.609**	-160.434*	4.668	130.586**	-9.620	-0.009	472.037	-523.364
	(90.014)	(86.444)	(220.950)	(64.381)	(250.668)	(0.047)	(399.027)	(466.946)
P-Val, During=0, Other Control Problem	0.000***	0.000***	0.963	0.654	0.110	0.011**	0.121	0.477
P-Val, After=0, Other Control Problem	0.923	0.493	0.402	0.335	0.439	0.218	0.205	0.607
DV Mean (Control, No Problem)	185.542	242.651	1212.544	204.877	1662.356	0.165	2792.135	-1073.859
DV Mean (Control, Other Control Problem)	175.536	226.206	803.076	195.684	1238.958	0.172	2969.259	-1770.425
N	1519	1490	1509	1518	1479	1519	1510	1472

Notes: Robust standard errors clustered at the individual level in parentheses. All regressions control for the baseline value of the outcome, strata fixed effects, time period fixed effects, an other control dummy, and interactions between the other control dummy and time period. The other control dummy is equal to one if the respondent reports difficulty saving no to transfer requests from others at baseline. All variables in Ghanain Cedis top coded at the 99th percentile. In 2013 GHS 2.15 ≈ USD 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table A10: Does Experience Impact Respondents' Assessments of SSP?

|	(1) Reduce Transfer Pressure in Household	(2) Reduce Transfer Pressure Outside Household	(3) Helps Me Meet Savings Goals	(4) Helps Me Gather Money for a Big Expense	(5) Helps Build Funds for an Emergency
Panel A. Overall Impacts					
Treatment	0.13***	0.039	0.14***	0.19***	0.076
	(0.047)	(0.046)	(0.051)	(0.052)	(0.052)
Control Mean (After)	0.15	0.18	0.21	0.19	0.24
Panel B. Impacts by Baseline Overdraft Behavior					
Treatment × Light Overdrafter	0.17***	0.047	0.15**	0.13*	0.052
	(0.064)	(0.070)	(0.071)	(0.074)	(0.068)
Treatment × Heavy Overdrafter	0.085	0.030	0.14*	0.25***	0.099
	(0.069)	(0.059)	(0.073)	(0.072)	(0.078)
P-value: Non-OD=OD	0.386	0.855	0.922	0.214	0.651
Control Mean (After, Non-OD)	0.12	0.23	0.19	0.22	0.20
Control Mean (After, OD)	0.18	0.13	0.22	0.16	0.28
N	303	303	303	303	303
Baseline Control?	No	No	No	No	No

Notes: All regressions control for strata fixed effects and the baseline outcome of the dependent variable whenever possible. Heteroskedasticity robust standard errors in parentheses. All regressions control for strata fixed effects. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Figure A1: Administrative Data – Overdrafts in Year Before Experiment
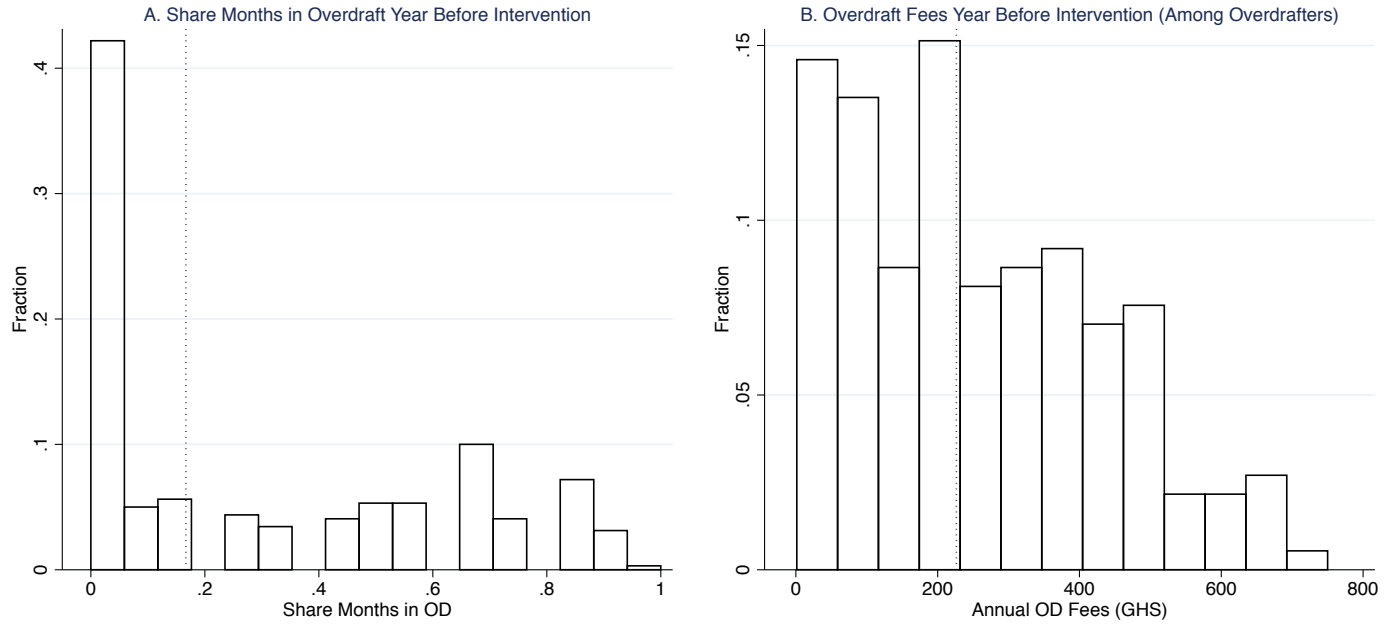
Note: Dashed lines mark median values.

Figure A2: Study Timeline
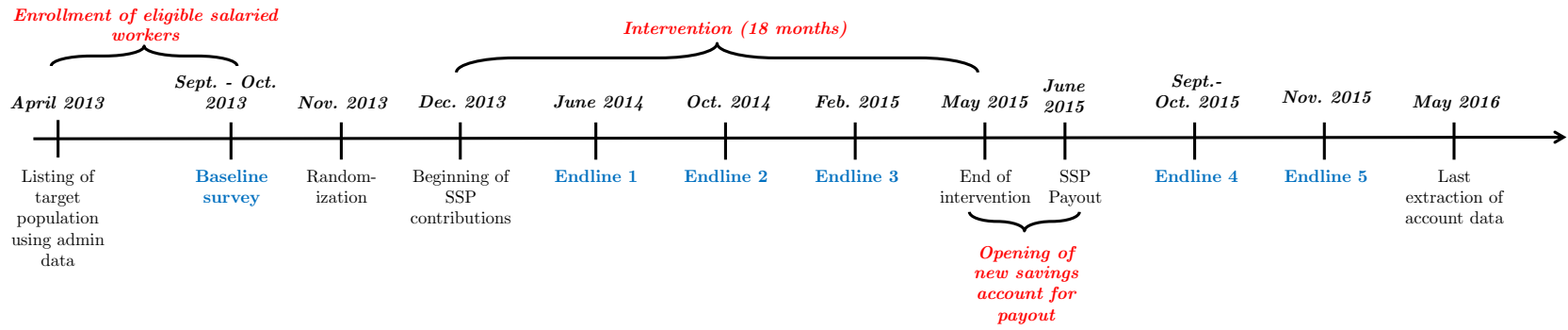
Notes: Activities took place in sequential order from left to right.