In The Effect of an Emergency Savings Program on Employee Savings and Work Performance: A Two-Year Field Intervention

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Abstract

Financial precarity – the persistent worry about one's financial situation – can have detrimental effects on individuals' cognitive, emotional, and social functioning. It can also interfere with work performance. In this paper we report on a two-year field intervention aimed at addressing financial precarity through the implementation of an employee emergency savings program. The program was employer-sponsored but required voluntary employee participation in the form of a weekly payroll deduction. Using propensity score matching of program participants with non-participants, results showed that program participants accrued more in emergency savings than non-participants, and that for more financially precarious employees, participation in the program was associated with improved work performance. Supplemental analysis showed that participation in the program, and the enhanced savings associated with it, buffered employees from financial shocks that might otherwise have interfered with their performance at work. Thus, the program offered benefits to employees in the form of enhanced short-term savings, and to the employer in the form of enhanced work performance. We discuss the implications of the findings for employee financial wellness initiatives.

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There is mounting evidence that people in the United States are financially vulnerable. A recent national survey (National Payroll Week 2021) documents that nearly two-thirds of employees would find it difficult or somewhat difficult to keep up with their rent, mortgage and other financial responsibilities if their paycheck was delayed for just one week. Of particular concern is that workers do not have enough savings to cover expenses for unexpected events such as medical or other emergencies (Salary Finance 2022).

Employers have taken notice, and many companies are offering their employees financial wellness programs including cash and other incentives to put away money into emergency savings funds. There is little evidence, however, of the efficacy of such programs (Beshears et al. 2020): We do not know whether they "work" and what it means to "work" – work for whom and in what ways? Do they generate returns to employers by some measure of worker performance? Do these programs improve the performance of the financially precarious workers who need them most? And do employees themselves benefit directly through improvements in their financial positions?

In this paper we report on a 2-year study of an employer-sponsored emergency savings program and discuss its potential benefits to employers and employees. Following an Industry Studies methodology, we immersed ourselves in understanding the unique job context of short-haul delivery, and the financial situation of those who work as drivers in this industry and, in particular, for this company. Our sample was comprised of short-haul truck drivers, who received good benefits from their employer, and whose pay was in the middle quintile in the U.S. Nonetheless, surveys that we conducted before the emergency program was offered indicated that many were persistently worried about their finances. In addition, over a third

(34%) reported having no emergency savings and a further 20% only had sufficient savings to cover one month of expenses.

To address these concerns, the company instituted a voluntary emergency savings program. Employees contributed to the program weekly through an automatic payroll deduction, and if they stayed with the program for a year, the company provided an incentive match. The goal of our research was to assess the effects of the program on both the employees and their employer. Since the program was voluntary, we used propensity score matching to match program participants with non-participants on a large number of baseline characteristics. Our findings indicate that the program had positive effects on levels of employee emergency savings overall, and on driving safety for the most financially precarious drivers.

Our research contributes to recent literature in several ways. First, there are numerous studies about the negative effects of financial precarity on well-being (e.g., Netemeyer, Warmath, Fernandes, and Lynch 2018) and existing evidence of the detrimental effects of precarity on work performance (Meuris and Leana 2018; Kaur, Mullainathan, Oh, and Schilbach 2021). Here we describe an intervention aimed at counteracting the detrimental effects of financial precarity that can be beneficial for employees (enhanced emergency savings) and employers (enhanced work performance) alike. Moreover, our measure of work performance is an objective one. Second, despite the growing popularity of employer-sponsored financial wellness programs, our understanding of their effectiveness is in a nascent stage (Beshears et al. 2020). The research described here is one of the few rigorous tests of such an intervention. Third, our research has practical significance in that the program described is low cost and easy to scale, and thus can be a model for other employers wishing to enhance employee financial wellness through short-term savings initiatives. Finally, our study was conducted with truck drivers, a profession characterized by high turnover and difficult working conditions, yet one that is vital to the effective flow of goods in an economy. Because of this national economic interest, there are policy initiatives in the U.S. aimed at strengthening this workforce (e.g., The White

House 2021). The program we describe here can have the further benefit of enhancing the stability of this workforce.

Background and Literature

Financial precarity is the persistent worry about one's financial situation. It is a psychological state, rather than a material one. Although clearly the two are related (Leana and Meuris 2015), objective and subjective financial standing are only moderately correlated and influenced by a variety of other factors (Gasiorowska 2014). Prior research has established that financial precarity can have detrimental effects on individuals' cognitive, emotional, and social functioning (Mani Mullainathan, Shafir, and Zhao 2013; Leana and Meuris 2015; Viseau et al. 2018; Leana 2022) as well as on their performance at work (Meuris and Leana 2018; Kaur, Mullainathan, Oh, and Schilbach 2021).

Research in economics and psychology has suggested that persistent worry about money can cause people to "tunnel" in on their financial problems, leaving less cognitive bandwidth to attend to their work, health, or family (Mullainathan and Shafir 2013). Indeed, numerous studies have established the link between financial precarity and cognitive burden (Mani et al. 2013; Zhao and Tomm 2017; Meuris and Leana 2018; Kaur et al. 2021). The study by Kaur et al. (2021) is particularly useful here as, like Meuris and Leana (2018), these authors measured work performance rather than just cognitive load. In this study design, workers' payments were timed such that some were paid more quickly than others, thus dampening their immediate financial strain. In addition, the researchers were able to rule out other potential explanatory factors for enhanced daily performance like changes in work motivation, perceptions of fairness, and even enhanced nutritional intake when workers' financial strain was addressed through early payments. They conclude that reduction in financial strain, rather than other factors, explain the enhanced productivity and reduction in work errors associated with the relative financial stability afforded by early payments. Using both experimental and field

research methods, Meuris and Leana (2018) provide earlier evidence of the role of financial precarity in adversely affecting employee ability to perform at work.

In addition to its potential incursions on cognitive ability, persistent financial worry can have high emotional and social costs. Financial precarity is associated with a range of negative emotions, from anxiety to fear to anger (Catalano et al. 2011). Such emotions are aversive so people are motivated to suppress them. But emotional suppression is itself cognitively taxing and, in this regard, it can further erode performance (Meuris and Leana 2018). People are often motivated to hide their financial problems from others, which can lead to social isolation and further anxiety and other negative emotions (Leana 2022), again increasing the cognitive load on individuals.

This emergent research on financial precarity and performance has caught the attention of employers. In the U.S., 97% of employers reported that they feel responsible for their employees' financial wellness (Bank of American 2022), up from 41% in 2013. Thus, employers are increasingly offering an array of financial wellness initiatives to employees, including emergency savings programs – the subject of our current study.

The goal of emergency savings programs is to shore up employees' liquid assets that can be used to meet unexpected expenses. National surveys suggest that such assistance may be needed. In the most recent Federal Reserve Board survey, approximately a third of U.S. adults could not meet a $400 unexpected expense without incurring debt – a level that has remained fairly consistent over several years (Board of Governors of the Federal Reserve System 2022). The same survey found that more than one-fourth of adults reported that they could not pay at least one of their monthly bills on time. And because of limited liquidity, many people will dip into retirement savings accounts to meet unexpected expenses in the present (Beshears et al. 2020). Argento, Bryant, and Sabelhaus (2015) report that for every dollar individuals invest in defined contribution retirement accounts like 401(k)'s, 30 to 40 cents flow out of such accounts before retirement, suggesting that individuals are using their retirement

savings to fund present-day expenses due to the absence of an emergency fund. Payday loans and other predatory forms of credit are even more damaging to individuals' financial stability.

A second rationale for setting up separate emergency savings accounts rests with Thaler's (1999) concept of mental accounting – or how individuals organize and keep track of their financial matters. When people earmark funds for special uses – such as covering an unexpected expense – they are less likely to spend that money on other things (Zhang and Sussman 2017) and thus be better prepared to cover such unexpected expenses. Thus, employees who enroll in an emergency savings program should report having higher levels of emergency savings after completing the program than will employees who do not enroll in the program.

With regard to the potential value of the program for work performance, we expect that employees who are most worried about their finances will realize the strongest benefits. Individuals have limited cognitive bandwidth and financial worry is cognitively taxing. When employees are persistently worried about their finances, this can take cognitive resources away from other aspects of their lives, including their performance at work (Mullainathan and Shafir 2013; Leana and Meuris 2015; Kaur et al. 2021). And the previously cited survey from Morgan Stanley showed that 78% of workers who are stressed about their finances report that it distracts them at work. Thus, a further outcome of enrollment in the program could be one that benefits both employees and their employers through enhanced work performance.

At the same time, emergency savings programs are not equally beneficial to all who may enroll in them. Indeed, the rationale for employer sponsorship of such a program is to assist those employees who need it most. One would expect that those employees who are most worried about their finances (i.e., most financially precarious) would benefit most from an emergency savings program. Employees who are not financially precarious and thus not incurring the cognitive load associated with financial precarity will not be similarly distracted by financial worries. Thus, we expect that financial precarity will moderate the relationship between

enrollment in the program and work performance such that employees who feel more financially precarious will show greater improvement in their work performance compared to those who are less financially precarious.

It is important to re-emphasize that, following Mullainathan and Shafir's (2013) general propositions about the negative effects of scarcity, here we are making a psychological argument for financial precarity's potentially negative effects on work performance, and the possibility of an emergency savings program to offset these negative effects. While objective financial insecurity (e.g., in the form of limited income or savings) is associated with subjective financial worry, objective financial standing is filtered through a subjective lens, and research shows that the subjective construal of one's financial situation, rather than the objective state (e.g., actual financial resources), is a more powerful predictor of outcomes such as quality of life (Ackerman and Paolucci 1983).

The beneficial effects of the savings program on performance should be mediated by the enhanced emergency savings accrued by those who participate, i.e., enhanced emergency savings is the mechanism by which this enhanced performance will be realized. Prior research has suggested that lack of emergency savings is a key driver of financial well-being (Anvari-Clark and Amsong 2022). A program aimed at directly improving emergency savings should address this concern. Thus, employees who participate in the program should report higher emergency savings after enrollment. This improvement in one's financial standing (i.e., more in emergency savings) should, in turn, have a beneficial effect on driving performance because employees will be better positioned to cover unexpected expenses and thus be less bothered by them. Further, following the same logic as articulated above, we expect these benefits to be most pronounced for employees who are most financially precarious at the start of the program.

Our research model is illustrated in Figure 1.

[[Figure 1 near here]]

Method

Participants and Procedure

We conducted our research within a medium-sized transportation company operating out of 22 terminals across nine states. We focused on short-haul truck drivers who make deliveries to distances that do not require them to stay overnight on the road. The drivers in our study can be described as solidly "middle class" with an average household income of approximately $65,000 per year, putting them in the middle-income quintile in the U.S. In addition to wages, the company provided benefits including health insurance, retirement savings contributions, and profit sharing. Despite this, a significant number of drivers in our initial interviews expressed concerns about their financial situations.

In an attempt to improve employee financial wellness, the company implemented a Rainy Day Savings (RDS) program. The program was voluntary and those who chose to participate contributed $19 a week[1] into a credit union account. After a driver signed up for the program, the contribution was automatically deducted from their paycheck and deposited into a personal credit union account. After 6 months, participants could receive a 12% match from the employer if they had continued to contribute each week and did not make any withdrawals from the account. Those who participated for an additional 6 months without making withdrawals received an additional 12% match. This means that after one year a driver could accumulate approximately $1,100 in an emergency savings account.

Although random assignment of drivers to the RDS program would have been desirable, there were several reasons why we instead used an active choice strategy (Carroll et al. 2009) whereby voluntary enrollment in the program was offered to all drivers. First and foremost, the RDS savings program was an employee benefit in which the company provided an incentive to participants. Depriving some employees of the opportunity to participate in a program that was

[1] Initial interviews with drivers suggested that even a $20 per week deduction would have dampened participation in the RDS program. Thus, we settled on $19 per week.

expected to benefit them would have been ethically questionable. In addition, although federal law does not require firms to offer all employees the same benefits, the employees in this study did not differ in their occupational or employment class (i.e., all were full-time truck drivers in good standing), making it questionable whether randomly distinguishing among them to receive the benefit would be allowable under the ERISA nondiscrimination rule (Gordon 2021). Finally, although employers are permitted to automatically enroll employees in retirement savings programs (so-called "opt out" programs), which could allow for a random assignment design (i.e., some employees could be randomly enrolled while others are not), automatic enrollment in "stand alone" emergency savings programs like this one is not currently allowed.[2] We address the lack of random assignment in more depth below.

Drivers signed up for the RDS program during group meetings at the terminals. To enroll, they were required to show a picture ID and sign forms establishing the credit union account and the automatic payroll deduction. Drivers were free to cancel their participation in the program at any time by notifying the HR department and/or withdrawing their funds and closing the account. All but a small number of the drivers who signed up for the program stayed with it for the entire 12 months (fewer than 10 drivers dropped out of the program).

We asked all drivers to complete a survey (Time 1 survey) prior to introducing the RDS program, regardless of whether they chose to sign up for the program. Surveys were administered during terminal meetings, which all drivers were expected to attend unless they were absent from work that day. We conducted a follow-up survey (Time 2 survey) six months

[2] In December 2022 Congress passed the SECURE 2.0 ACT, which allows employers to automatically enroll employees in emergency savings programs, but only if those programs are tied to employees' 401(k) program (so called "side car" programs). Stand-alone emergency savings programs that are deliberately separated from 401(k) programs are not included in this legislation so programs such as the one described here are not covered by this legislation and, in any case, the RDS program was implemented prior to the Act. (All Info - S.1870 - 117th Congress (2021-2022): Enhancing Emergency and Retirement Savings Act of 2021 | Congress.gov | Library of Congress). Note that stand-alone programs like the RDS described here are far more common and considered more desirable than sidecar programs since they are easier and cheaper for employers to implement, and they can be implemented independent of whether an employer offers a 401(k) program, or an employee chooses to participate in it.

after the program was implemented. We also collected driving safety data for all drivers for 12 months prior to the introduction of the RDS program, and for 12 months afterwards. Thus, the driving performance data spanned two years: one year prior to the program initiation to establish a baseline of performance, and one year after the program initiation to test for the potential effects of program participation on driving performance.

The population of 1,415 drivers consisted of those who were employed at the company just prior to the initiation of the RDS program. In the population, 98% of the drivers were male and the average age and tenure at the time of program initiation were 48.37 years (*SD* = 9.88) and 11.12 years (*SD* = 8.45) respectively. Of the population of 1,415 drivers, 1,086 participated in the first survey (a 76.8% response rate). Of these, 847 also participated in the second survey (a 78.0% response rate from Time 1). We further limited the sample to drivers who were continuously employed full time for the two-year duration of the study. The final sample consisted of 772 drivers, who were 98% male with an average age of 47.61 years (*SD* = 9.76) and an average tenure of 10.71 years (*SD* = 8.49) at the time of the initiation of the RDS program. There were no significant differences between the final sample and the population of drivers on gender, age ($t = 1.73$, $p = 0.17$), or tenure ($t = 1.08$, $p = 0.56$).

Pre-Treatment Measures

In order to conduct propensity score matching of program participants and non-participants, we measured a large number of baseline covariates in the pre-treatment survey, including demographics, health and wellness measures, indicators of financial standing and financial behavior, affective satisfaction, self-efficacy, and feelings of financial precarity. The means, standard deviations, and correlations for all measures are reported in Table 1.

[[Table 1 near here]]

Financial Precarity. Financial precarity was assessed using the four-item scale developed by Meuris and Leana (2018). On a five-point scale (1 = Never, 5 = Always), participants indicated the frequency over the past month that they felt worried about their

finances, felt overwhelmed by financial obligations, felt that they did not have enough money, and felt hopeless about their financial situation (α = 0.87). On average, participants reported a moderate level of financial precarity (M = 2.59, SD = 0.89) at Time 1.

Emergency Savings. Participants indicated their level of emergency savings on a seven-point scale with 0 = no emergency savings and 6 = emergency savings equal to at least six months' worth of expenses. In other words, each unit on the scale is equivalent to roughly one month of emergency savings. On average, participants reported having emergency savings that could cover about two months (or 64 days) of expenses at Time 1 (M = 2.12, SD = 2.17).

Credit Confidence. On a five-point scale (1 = not confident at all; 5 = very confident), participants reported how confident they felt that their application would be approved if they were to apply for a credit card or a loan that day. Participants reported a moderately high level of credit confidence (M = 3.89, SD = 1.35) at Time 1.

Credit Card Debt. Participants indicated the amount of credit card debt they currently had on a 12-point scale (0 = no credit card debt, 11 = credit card debt equal to $10,000+). Participants reported a moderate amount of credit card debt (M = 3.14, SD = 3.88) that is slightly above the $2,000 mark on the scale (an average of about $2,140).

Life Satisfaction and Job Satisfaction. On a six-point scale (1 = very unsatisfied, 6 = very satisfied), participants indicated the extent to which they were satisfied with their life overall and their job, respectively. On average, participants reported moderately high life satisfaction (M = 4.80, SD = 1.07) and job satisfaction (M = 4.78, SD = 1.13).

General Self-efficacy. We measured participants' general self-efficacy using items from Chen, Gully, and Eden (2001). On a six-point scale (1 = strongly disagree, 6 = strongly agree), participants indicated their level of agreement on three items: "I will be able to achieve most of the goals that I have set for myself", "When facing difficult tasks, I am certain that I will accomplish them", and "In general, I think that I can obtain outcomes that are important to me"

(α = 0.90*).* With a mean score of 4.51 (*SD* = 0.89), participants indicated a moderately high level of self-efficacy.

Health Indicators. Because physical and mental well-being could affect driving performance, we measured employee health problems in the pre-treatment survey using multiple indicators. Physical health symptoms were measured using the Somatic Symptom Scale-8 (SSS-8) (Gierk et al. 2014). This scale is comprised of symptoms of common health problems (e.g., stomach problems, dizziness, headaches, or trouble sleeping); it is used by medical practitioners to quickly evaluate the severity of a patient's somatic burden and has been validated as a measure of general health status (Gierk et al. 2014). Participants reported how often, in the past month, they have experienced each of eight symptoms on a scale from 0 (not at all) to 4 (very much) (α = 0.80). In addition, we used seven items from the Patient Health Questionnaire-9 (PHQ-9) (Kroenke, Spitzer, and Williams 2001), which asks people to indicate how often they have experienced different symptoms of depression over the past month on a four-point scale from 0 (Not at all) to 3 (Nearly every day) (α = 0.86). In general, the data indicate that participants experienced few of the symptoms listed and were physically (*M* = 0.65, *SD* = 0.56) and psychologically (*M* = 0.41, *SD* = 0.50) healthy. Finally, we asked employees to report how many hours of sleep they got on average each night, how many times they exercise each week, how many alcoholic drinks they had on average each week, and the average number of cigarettes they smoked daily. According to the reported means at Time 1, participants typically got about 6.47 hours of sleep per night (*SD* = 0.98), smoked 2.92 cigarettes per day (*SD* = 7.84), exercised about 2.86 times per week (*SD* = 2.46), and consumed an average of 3.33 alcoholic drinks per week (*SD* = 5.66). Note that other than sleep, there was high variability in all of the behavioral measures.

In addition to the variables listed above, in the pre-treatment survey, participants reported their annual household income on a nine-point scale from 1 ($10,000-$19,999) to 9 ($100,000 or more), whether they borrowed money against their 401(k) in the past year (1 =

yes, 0 = no), whether they took out a payday or cash advance loan in the past 6 months (1 = yes, 0 = no), their number of dependent children (*M* = 0.85, *SD* = 1.16), their age (*M* = 47.61, *SD* = 9.76), and their job tenure (*M* = 10.71, *SD* = 8.49). The average household income reported was 6.53 (*SD* = 1.95), which translates into approximately $65,300. Additionally, before the program was implemented, 15% of the participants reported that they borrowed money against their 401(k) in the past year and 2% of the participants reported taking out a payday or cash advance loan.

Measure of Performance

We collected data on all participants' driving performance 12 months prior to the start of the RDS program and for 12 months afterwards. Specifically, we calculated the total number of driving safety citations each driver received before and after the program was implemented. Safety citations are "tickets" issued by law enforcement for violations such as speeding, failure to come to a complete stop at a traffic light, etc. They are a key indicator of driving safety and of critical importance to the company in assessing driver performance. As noted by the company's COO, "we are in the safety business first, and the transportation business second." All driving citations are recorded by the company and stored in the employee record.

Other possible indicators of driving performance were considered but not used because either they are not viewed by the company and/or by drivers as clear indicators of driving safety, they were too infrequent in occurrence, or the record-keeping was not consistent. For example, the company collects data on what they label "violations", which are more minor infractions of company policy that do not result in a traffic ticket and many do not even involve the actual driving of the truck. These include everything from tardiness to inadequate tire inflation or improper loading of the truck, and they varied considerably in both their severity and their relevance to driving safety, and thus are difficult to interpret in the aggregate. Another indicator could be actual traffic accidents. We did not use this indicator because of its relative infrequency, particularly the more serious accidents. A final indicator could have been

supervisors' assessments of driving performance but generally such subjective performance indicators are not considered as desirable as objective indicators because they are prone to bias and inflation (Bommer et al. 1995; Forth and McNabb 2008). In addition, in this setting, such assessments were unevenly completed by supervisors. Thus, in consultation with the company, safety citations were tracked as our dependent variable because they are a clear indicator of driving performance, they occur more frequently, and records of citations were consistently kept as safety violations which indicate the potential for an accident and thus substandard performance.

Results

Analytic Method

Random assignment of drivers to the RDS program is desirable as it can ensure a balanced set of driver characteristics between the treated and control groups, and it enables causal statements regarding the treatment. However, as described earlier, because of ethical and legal concerns, the company offered the program benefit to all drivers rather than randomly assigning them to treatment or control groups. As described below, while we are not able to achieve that gold standard of randomization, our research design and rich data on driver characteristics enable us to derive associations between treatment and outcomes after accounting for other factors that may plausibly affect selection into treatment and/or driving safety.

Non-randomized studies have been widely used as an alternative for evaluating treatment effects when randomized experiments are not available, although such designs are not without threats to internal validity. One of the concerns is that the treated and control groups might differ in some systematic ways unrelated to differences in their treatment. To address this "sorting" issue, we adopted the propensity score matching method that enables us to quasi-experimentally match treated drivers (those who elected to participate in the RDS program) and control drivers (those who elected not to participate) on confounding factors that could affect

both program participation and post-treatment outcomes, as well as factors only affecting the outcomes (Rosenbaum and Rubin 1983; Austin 2011). Ong (2022) suggests this approach is appropriate for naturally occurring treatment groups in field settings. It enables us to rule out credible alternative explanations for differences in the outcomes (e.g., health status, prior driving history), facilitates the estimation of credible treatment effects, and strengthens causal inferences. In addition, propensity score matching is preferred to traditional regression analysis in that it more closely mimics randomized control trials by creating balanced samples from observed baseline variables across treated and control groups (Shah, Laupacis, Hux, and Austin 2005; Austin 2011).

We used the MatchIt package in R (Ho, Imai, King, and Stuart 2011) to perform propensity score matching. We first estimated the propensity score for each driver using logistic regression, one of the most commonly used models (Stuart and Rubin 2008). The propensity score is an indicator of one's likelihood of participating in the program based on a set of observed covariates or confounding factors. Matching treated and control drivers on their propensity scores ensured that the two groups were similar in their likelihood of participating in the program. In other words, differences in the outcomes can be attributed to the RDS program intervention rather than other confounding factors. In selecting the matching variables, we followed recommendations in prior studies (e.g., Brookhart et al. 2006; Stuart and Rubin 2008; Steiner, Cook, Shadish, and Clark 2010; Stuart 2010) and only focused on pre-treatment covariates or variables that were not affected by the treatment. Specifically, we included variables that were related to *both* the treatment and post-treatment outcomes (emergency savings and driving safety). As recommended in Brookhart et al. (2006) and Stuart (2010), we also included variables only related to the outcomes but not treatment to increase the precision of the estimation of the treatment effect.

We relied on the method developed by Belloni, Victor, and Hansen (2014) to identify variables relevant to treatment assignment and the outcomes. Specifically, we regressed

treatment and the two post-treatment outcomes (emergency savings and safety citations) respectively on all the baseline covariates[3], and classified the regressors with $p \leq 0.10$ as relevant for matching. We also matched on pre-treatment citations to ensure that treated and control drivers were comparable in their baseline driving performance. Baseline covariates that did not significantly predict program participation and/or the outcomes were dropped. A complete list of matching variables used can be found in Table 2.

Because there were more treated drivers than control drivers in the sample (468 treated drivers versus 304 control drivers), the one-to-one nearest neighbor matching algorithm or the matching without replacement procedure would drop at least 164 treated drivers in the sample. This would greatly reduce the power of our estimates and could reduce the quality of the match by forcing us to match treated drivers with control drivers who are different. As a result, a two-to-one nearest neighbor matching algorithm, a type of matching with replacement procedure, was used (Austin 2010).[4] For each driver in the treated group, the algorithm identified up to two drivers in the control group who provided the closest propensity scores to that of the treated driver. Doing so allowed us to keep all of the treated drivers in the matched sample. Additionally, to ensure that the matched drivers were similar enough to each other, we further applied a caliper of width equal to 0.20 SD of the logit of the propensity score for the treated drivers, and were able to achieve balance between treated and control drivers across all covariates (Austin 2008). In other words, there is a substantial overlap in the propensity scores between the treated and control drivers, indicating good common support. A caveat with this procedure is that not all of the control drivers are used in the match. In total, 265 out of 304

[3] The baseline covariates are financial precarity, credit confidence, credit card debt, emergency savings, health issues, depression, sleep hours, alcohol use, household income, life satisfaction, general self-efficacy, age, and job tenure. All baseline covariates were measured before the implementation of the RDS program. Six variables (number of dependent children, job satisfaction, exercise, smoking, borrowed against 401(k), and took out payday loan) were excluded from this process as they contained redundant information. Instead, they served as "additional controls" later in our robustness checks.

[4] Austin (2010) recommends using either a two-to-one or three-to-one match with replacement. We confirmed that the results were robust with a three-to-one match with replacement.

control drivers (87%) were used in the match, and all 468 treated drivers were successfully matched in the final sample.

In order to check for balance, we used standardized mean differences (SMDs). Because SMDs are the standardized differences in covariates between treated and control groups, they allow us to compare covariates measured with different units (Austin 2009). Using the strict and well accepted criterion proposed by Austin (2011), we consider a covariate balanced when the absolute SMD is smaller than 0.10. Table 2 presents the balance table for the final list of matching variables for control and treated drivers before and after the match. Table 2 indicates that before matching, the absolute difference in SMDs between treated and control drivers was greater than 0.10 for seven of the nine matching variables. The treated drivers had higher financial precarity, more credit card debt, lower credit confidence, less in emergency savings, more health issues, higher household income, and a shorter average job tenure compared to the control drivers. As shown in Table 2, all absolute SMDs in the matched sample are well below 0.10, which suggests the sample is well balanced and plausibly random.

[[Table 2 near here]]

Results

We conducted simple mediation, moderation, and moderated mediation analyses using the matched sample to test our hypotheses. We estimated the impact of the treatment (RDS program participation) on driving performance and emergency savings using weighted linear regressions to adjust for clustering in matched datasets (Ho et al. 2011). Regression results are reported in Table 3. To facilitate the interpretation of regression coefficients, we standardized the financial precarity variable.

We expected that drivers who participated in the RDS program would accumulate more emergency savings post-treatment than non-participants. We found a significant treatment effect on emergency savings (b = 0.36, SE = 0.15, p = 0.017), reported in Table 3 column (7). In practical terms, the coefficient of 0.36 suggests that treated drivers, on average, were able to

cover an additional 0.36 months or about 11 days' worth of expenses compared to the control drivers. This represents a 17% increase over the baseline average savings in just the first six months of the program.

With regard to the effects of the program on driving performance, we predicted that the effect of treatment on driving performance would be greater for those experiencing higher levels of financial precarity. Table 3 reports the effect of the RDS program on participants' driving performance twelve months after the treatment was implemented, captured by the number of citations accrued. As expected, no significant average effect on the treated (ATT) was found (b = -0.01, SE = 0.03, p = 0.659). However, pre-treatment financial precarity was found to moderate the relationship between participation in the program (treatment) and post-treatment citations. In other words, the impact of the program on participants' driving performance depends on the drivers' level of pre-treatment financial precarity, and the program appears to be particularly beneficial for those with higher levels of pre-treatment financial precarity. Specifically, drivers who were more financially precarious (i.e., more worried about their financial situations) before signing up for the RDS program (treatment) showed a greater reduction in the total number of driving citations they received after treatment compared to drivers who were less financially precarious pre-treatment (b = -0.08, SE = 0.03, p = 0.023).

To understand how the effect of treatment varied by different levels of financial precarity, we compared group means and assessed the significance of group differences for drivers with high versus low levels of financial precarity (see Table 4 and Figure 2). Specifically, we found significant group differences in the number of citations for those who were high in financial precarity (1 SD above the mean) (est. = -0.09, SE = 0.03, p < .001) and drivers low in financial precarity (1 SD below the mean) (est. = 0.06, SE = 0.03, p = 0.011). This indicates that for drivers experiencing high financial precarity (at a financial precarity level 1 SD above the mean), those in the treated group were expected to have approximately 0.09 fewer citations than those in the control group. Although a 0.09 reduction in the number of citations received might seem

small, it represents a meaningful percentage decrease relative to the baseline citation level among the financially precarious drivers. The average number of baseline safety citations among the highly financially precarious was 0.103, and the percentage decrease is approximately 87%. In other words, among the more financially precarious drivers, participation in the program leads to an impressive 87% reduction in the number of safety citations received post-treatment.

For drivers experiencing low financial precarity (at a financial precarity level 1 SD below the mean), however, those in the treated group showed approximately 0.06 more citations than those who did not receive the treatment. This finding was unexpected, and we can only speculate about the reason for it. Perhaps financially secure drivers perceived the purpose and utility of the emergency fund differently from those who felt more precarious. For example, financially precarious drivers may view it as a safety net and thus use the funds only to cover unexpected expenses. Financially secure drivers are already able to absorb unexpected expenses and thus might perceive the fund more as a surplus and allocate it more broadly. Overall, however, as the level of financial precarity increases, participating in the program is more beneficial. Among the highly financially precarious drivers, those who participated in the program had significantly fewer citations post-treatment compared to those who did not participate.

We predicted that the treatment would have an indirect effect on safety citations via emergency savings. That is, level of emergency savings was predicted to mediate the relationship between treatment and driving performance. We followed the steps proposed by Baron and Kenny (1986) and used bootstrapping (Preacher and Hayes 2004) for significance testing. The first step of Baron and Kenny's (1986) technique involves determining whether the independent variable (treatment) significantly predicts the dependent variable (post-treatment citations) (Table 3 column 1). As noted above, we did not expect nor find a significant treatment effect in our analysis. Step 2 involves regressing the independent variable on the mediator,

post-treatment emergency savings (Table 3 column 7). As predicted, we found a significant treatment effect on the mediator, representing the value of path *a* in a simple mediation model. In step 3, we regressed both the independent variable and the mediator on the dependent variable (Table 3 column 3). The coefficient associated with the mediator in this model represents the value of path *b* in a simple mediation model. The product of the two coefficients, *a* and *b*, represents the indirect effect (MacKinnon, Fairchild, and Fritz 2010), which is reported in Table 4. We used bootstrapping to test the significance of the indirect effect and found no support for the mediating effect of emergency savings on the relationship between financial precarity and driving performance (indirect effect = 0.002, $p = 0.460$, 95% C.I. [-.002, .010]).

To examine whether pre-treatment financial precarity would moderate the indirect effect of participation in the program on post-treatment driving performance through emergency savings, we conducted a moderated mediation analysis using the *lavaan* package in R (Rosseel 2012). We first tested in a simple moderation if the effect of emergency savings on post-treatment citations depends on drivers' financial precarity and found the interaction effect to be non-significant ($b = -0.00$, $SE = 0.01$, $p = 0.953$) (Table 3 column 4). Hence, financial precarity did not moderate the effect of emergency savings on post-treatment driving performance in the full moderated mediation model (Table 3 columns 5 and 6). We still report additional statistics in Table 4, including the conditional indirect effects, which are the indirect effects of the treatment on driving performance via emergency savings at high and low levels of financial precarity (1 standard deviation above and below the mean). Our analysis reveals that while there is a slight positive indirect effect of the treatment on driving performance at low levels of financial precarity (estimate = 0.005, $p = 0.069$, 95% C.I. [.001, .011]), this effect diminishes and becomes non-significant as financial precarity increases. We also report the index of moderated mediation or the difference between the conditional indirect effects (Index = -0.004, $p = 0.256$, 95% C.I. [-.012, .004]), which suggests a lack of moderated mediation. In other words, either emergency

savings do not have a strong role in influencing participants' driving performance or the effect of emergency savings does not vary by participants' financial precarity.

[[Table 3, Table 4 near here]]

Robustness Checks

As noted earlier, an important assumption in the propensity score method is that observed covariates drive selection into treatment. The idea is that once we match drivers on observed covariates, there are no unobserved characteristics that affect the drivers' decision to participate in the RDS program. However, some scholars note that because the set of variables used for matching does not always account for all potential factors affecting participation in treatment, there should be some validation of this strong assumption (Caliendo, Mahlstedt, and Mitnik 2017). We acknowledge that in a field study it is almost impossible to control for all potential unobservables. However, we anticipated this in the design and have taken steps to address this concern by matching the participants on a large number of observable characteristics that are arguably relevant for program participation and outcomes.

To check the robustness of the findings, we first attempted to find an instrumental variable – i.e., an instrument that is relevant and predicts program participation and also satisfies the exclusion restriction. That is, it should only influence the dependent variable through treatment (Angrist and Krueger 2001). Several of our baseline (pre-treatment) driver-level variables are relevant and are strongly associated with subsequent program participation (e.g., measures of financial precarity, emergency savings, credit confidence, credit card debt and health issues). While we have collected many variables that are potentially relevant here, they could all affect people's driving performance through unmeasured pathways other than the program itself. As a result, none of the variables satisfies the exclusion restriction.

To further address the issue of selection and non-random assignment to treatment, we also conducted the analyses including additional control variables. This analysis and a full list of the additional control variables are available in the online Appendix. With the additional control

variables, treated drivers showed a significant increase in emergency savings and financial precarious drivers showed a significant improvement in their driving performance. These estimated effects did not differ significantly from the original estimates reported earlier. As a further robustness check, we tested our models using Poisson regressions to examine the effect of treatment on post-treatment citations, in which the post-treatment citations count was the outcome variable that took non-negative values. These results are also reported in the online Appendix. These analyses support our earlier conclusion: The program is particularly effective in reducing citation counts for drivers with high financial precarity.

Supplemental Analysis

We conducted further analysis to better understand how program participation influenced post-treatment citations using a sub-sample of 610 drivers (79% of our sample) who completed a short (one page) survey 12 months after the program was implemented (Time 3 survey). Here, drivers reported whether they had experienced a financial setback during the prior 12 months (1 = yes, 0 = no).[5] Almost one in six (15.4%) of the participants indicated that they had experienced a financial setback during this time. The sub-sample is not significantly different from the sample we used for the main analyses in terms of all the baseline covariates. As indicated in the online Appendix, the matched sub-sample is also well balanced on the relevant covariates.

In the supplemental analysis, we explored whether a financial setback was a potential moderator of the relationship between emergency savings and safety citations. Our results indicated that there was a significant interaction between post-treatment emergency savings and financial setback (b = -0.09, SE = 0.05, p = 0.051), after controlling for the treatment effect. A simple slope analysis revealed that for drivers who had a financial setback, having more emergency savings was associated with a decrease in the number of safety citations (slope =

[5] Examples of financial setbacks included health emergencies, unexpected home or car repairs, etc.

-.08, *SE* = 0.02, 95% CI [-.114, -.039]). Specifically, for each unit increase in emergency savings, there was a reduction of 0.08 safety citations. Thus, the increased emergency savings associated with enrollment in the RDS program appeared to act as a buffer against financial setbacks, which had a positive effect on driving performance. Full results associated with the sub-sample analysis can be found in the online Appendix.

Summary of Results

To summarize, enrollment in the RDS program resulted in higher levels of emergency savings accrued 6 months later. In addition, for those drivers who were more worried about their financial situations (i.e., more financially precarious) before the program was initiated, their participation in the program resulted in an 87% decrease in driving citations over the year following enrollment in the program. Increased emergency savings did not mediate the relationship between enrollment in the program and driving performance for the full sample or for the more financially precarious drivers. However, supplemental analysis showed that the increased emergency savings associated with the program did act as a buffer for those who experienced a financial setback over the previous year. This, in turn, had a beneficial effect on their driving performance.

Discussion

In this paper we describe an emergency savings intervention that resulted in benefits for both employees and their employer. For employees, those who enrolled in the program realized significant increases in their short-term emergency savings funds. For the employer, there were significant improvements in driving performance attributable to the program for those drivers who were more financially precarious. Moreover, the program was low in direct costs (the 12% match totaled less than $119 per employee over the course of the year), and once employees enrolled in the program, it was simple to administer. Altogether the company estimated that it spent under $100,000 in direct and indirect costs associated with the program over the course of a year. This can be compared to the cost of traffic accidents – which presumably traffic laws,

and citations for breaking them, are meant to avoid. The average cost of just one commercial truck accident that includes an injured person is over $148,000. If a fatality occurs, the costs range upwards of $7 million (Antich 2018; Finance Monthly 2022).

Short-term emergency savings are a key to financial stability, yet many employees do not have such funds (Salary Finance 2022). Indeed, in our sample, nearly one in three drivers had no emergency savings at the beginning of the program. Other researchers have pointed to the longer-term problems individuals may encounter when they are unprepared for unexpected financial setbacks. These include borrowing from retirement savings, running up credit card debt, and using predatory lending outlets (Argento et al. 2015; Beshear et al. 2020; Salary Finance 2022). An emergency savings fund can buffer against unexpected financial setbacks and, indeed, in our supplemental analysis we find evidence for this in the current study.

Although it is not surprising that we found that those who enroll in an emergency savings program report more emergency savings accrued over time, this finding lends face validity to our model and suggests that the drivers who enrolled in the program treated it as a vehicle for savings rather than as a source of money to cover day-to-day – i.e., anticipatable - expenses. The effect of the intervention on driving performance are at least as interesting. Essentially, enrolling in the program resulted in better driving performance over time for those participants who reported being more financially precarious at the program's inception. This finding is predicted by the larger theory of psychological scarcity (Mullainathan and Sherif 2013), which suggests that worry imposes a cognitive tax that can hinder cognitive functioning and hence work performance. Meuris and Leana (2018) provided empirical support for this prediction in both laboratory and field settings, whereby driving was worse for those who were financially precarious. In the current work, we extend these previous findings to test an intervention – the Rainy Day Savings Program – that was found to reverse these negative effects of precarity.

This is an important set of findings for research as well as for practice. There is burgeoning interest among employers in assisting employees with financial wellness, with

nearly 70% of U.S. employers offering some form of financial wellness benefits in 2020. Thus, benefits such as emergency savings programs are being implemented, but without much evidence of their effectiveness (Beshears et al. 2020). The current study is one of the first attempts to provide such evidence. Moreover, the outcomes that are the focus of our study are changes in actual financial standing and work behavior, rather than just affective measures like satisfaction with one's job or employer. In this regard, we provide objective evidence for the benefits of the program for both employees and their employers.

Future Research

While the current research provides a rigorous test of an emergency savings intervention, it is on a relatively small scale and thus future research is needed to confirm and/or expand upon our findings. In particular, there are many decisions that are made in designing such programs. These include facets such as the degree of liquidity and portability of the program, as well as features such as how individuals are enrolled in such programs. Based on the evidence from 401(k) retirement plans, clearly an automatic enrollment option can be very beneficial in terms of program uptake. At the same time, an automatic enrollment option for stand-alone emergency savings programs does not comply with current federal regulations. Perhaps that should change as the benefits of emergency savings programs become clearer, but for now the program described here is one that is necessarily based on "opt-in" rather than "opt-out" enrollment. Similarly, there is little data on what form or amount of employer match in such programs is optimal in terms of inducing employees to sign up for the program or stay with it. Beshears et al. (2020) provide a thoughtful summary of the issues entailed in such decisions but, again, there is little objective evidence to date which tests these different models and features. The intervention described here provides evidence supporting one such model. Future research should test it against others.

Finally, a question that can be addressed is why more employers are not offering emergency savings as part of their employee benefits portfolio. Perhaps they are concerned

about the costs of such a program or worry that it will be unwieldy to manage. Our results suggest that these costs are more than outweighed by the benefits in terms of measurable employee performance and that once initiated, such programs can be relatively easy to administer (for a discussion of barriers to adoption of emergency savings programs, see Nicora, Sprick and Gilmore 2024). We are hopeful that the current study will spur other rigorous research that can used to build a case for why emergency savings programs are beneficial for employers as well as employees.

Conclusion

Lack of emergency savings is a serious problem for many in the U.S. workforce. The program described here is a low-cost intervention that is mutually beneficial for employees and their employer. Most notably, enrollment in the Rainy Day Savings intervention resulted in increased stocks of short-term liquid savings for employees and markedly improved work performance for the more financially precarious drivers. We are hopeful that the current study will spur other rigorous tests of interventions to address financial precarity.

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Table 1. Mean, Standard Deviations, and Correlations

Variables	M	SD	1	2	3	4	5	6	7	8	9	10
1. Treated	0.61	0.49	--									
2. T1 Financial precarity	2.59	0.89	.12***	--								
3. T1 Credit confidence	3.89	1.35	-.12***	-.51***	--							
4. T1 Credit card debt	3.14	3.88	.09**	.25***	-.08**	--						
5. T1 Emergency savings	2.12	2.17	-.16***	-.49***	.49***	-.27***	--					
6. T1 Health issues	0.65	0.56	.07*	.33***	-.20***	.01	-.19***	--				
7. T1 Depression	0.41	0.50	.06	.41***	-.23***	.02	-.21***	.61***	--			
8. T1 Sleep hours	6.47	0.98	.02	-.15***	.10***	-.06	.15***	-.30***	-.29***	--		
9. T1 Alcohol	3.33	5.66	.04	-.03	.01	.02	.03	.08**	.10***	.00	--	
10. T1 Exercise	2.86	2.46	-.08**	-.17***	.18***	-.01	.21***	-.22***	-.23***	.09**	-.04	--
11. T1 Smoking	2.92	7.84	-.01	.07**	-.07*	.03	-.03	.11***	.06	-.01	.04	-.10***
12. T1 Children	0.85	1.16	.06	.23***	-.18***	.03	-.20***	.08**	.10***	-.04	-.03	-.08**
13. T1 Household income	6.53	1.95	.05	-.15***	.23***	.14***	.18***	-.08**	-.11***	-.04	.02	.05
14. T1 401(k)	0.15	0.36	.11***	.18***	-.22***	.16***	-.18***	.09**	.06	-.07**	-.02	-.00
15. T1 Payday loan	0.02	0.15	.03	.18***	-.19***	.06	-.12***	.15***	.19***	-.12***	.01	-.08**
16. T1 Life satisfaction	4.80	1.07	-.01	-.26***	.14***	-.04	.08**	-.26***	-.41***	.10***	-.06*	.18***
17. T1 Job satisfaction	4.78	1.13	.05	-.04	-.07**	-.01	-.08**	-.13***	-.14***	.05	.01	.06
18. T1 GSE	4.51	0.89	-.03	-.39***	.30***	-.07*	.25***	-.26***	-.34***	.13***	-.02	.22***
19. T1 Age	47.61	9.76	-.04	-.13***	.20***	-.03	.24***	-.14***	-.17***	.05	-.07**	.09**
20. T1 Tenure	10.71	8.49	-.07*	-.18***	.28***	-.04	.25***	-.10***	-.13***	.00	.02	.13***
21. Male	0.98	0.13	-.04	-.08**	.10***	.01	.07*	-.11***	-.08**	.01	.07*	-.00
22. T1 Citations	0.11	0.34	.02	.01	.00	.01	.03	.00	.00	.01	.07*	.01
23. T2 Citations	0.10	0.33	.00	.00	-.06*	-.02	-.03	-.04	.00	-.04	-.01	-.04
24. T2 Emergency savings	2.37	2.08	-.07*	-.43***	.40***	-.26***	.74***	-.14***	-.17***	.09**	.03	.21***

Table 1. (Continued)

Variables	11	12	13	14	15	16	17	18	19	20	21	22	23
11. T1 Smoking	--												
12. T1 Children	-.07*	--											
13. T1 Household income	-.00	.00	--										
14. T1 401(k)	.04	.11***	.02	--									
15. T1 Payday loan	.02	.20***	-.05	.11***	--								
16. T1 Life satisfaction	-.02	.02	.05	-.04	-.05	--							
17. T1 Job satisfaction	-.05	.05	-.03	.07*	.01	.55***	--						
18. T1 GSE	-.09**	-.06	.09**	-.07*	-.05	.31***	.19***	--					
19. T1 Age	.03	-.38***	.20***	.01	-.08**	.02	-.02	-.02	--				
20. T1 Tenure	-.02	-.17***	.26***	-.00	-.08**	.06	-.04	.06*	.43***	--			
21. Male	.00	.04	.02	-.06	.02	.00	.01	.02	.02	.09**	--		
22. T1 Citations	-.05	.00	-.00	-.02	-.05	.01	.02	.00	-.06*	-.08**	.01	--	
23. T2 Citations	.03	.09**	-.01	.00	.06*	.01	.00	-.04	-.06*	-.09**	.01	.01	--
24. T2 Emergency savings	-.02	-.15***	.17***	-.15***	-.09**	.10***	-.05	.19***	.19***	.19***	.03	.01	.06

Notes: N = 772. T1 = Time 1 (pre-treatment), T2 = Time 2 (post-treatment). Treated (1 = yes, 0 = no); Children = number of dependent children; 401(k) = borrowed money against 401(k) in the past year (1 = yes, 0 = no); payday loan = took out a payday or cash advance loan in the past 6 months (1 = yes, 0 = no); GSE = General self-efficacy; Male (1 = yes, 0 = no); Citations = total number of safety citations.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table 2. Sample Balance Before and After the Match

Matching Variables	Balance of the Unmatched Sample			Balance of the Matched Sample		
	Treated	Control	SMD	Treated	Control	SMD
Financial precarity	2.68	2.46	0.25	2.68	2.69	-0.01
Credit card debt	3.41	2.72	0.17	3.41	3.49	-0.02
Credit confidence	3.75	4.09	-0.24	3.75	3.72	0.03
Emergency savings	1.83	2.57	-0.37	1.83	1.80	0.02
Health issues	0.68	0.60	0.15	0.68	0.72	-0.08
Sleep hours	6.49	6.45	0.04	6.49	6.43	0.06
Household income	6.60	6.41	0.10	6.60	6.49	0.06
Job tenure	10.26	11.40	-0.14	10.26	10.43	-0.02
Safety citations	0.12	0.10	0.05	0.12	0.14	-0.07

Notes: Unmatched sample N = 772 (304 control drivers and 468 treated drivers). Matched sample N = 733 (265 control drivers and 468 treated drivers). Means of the covariates in the control and treated groups are presented. A covariate is considered balanced when the absolute value of SMD (standardized mean difference) is smaller than 0.10. All matching variables were measured pre-treatment.

Table 3. Regressions Results on Post-treatment Safety Citations and Emergency Savings

Independent variable	Post-treatment Safety Citations						Post-treatment Emergency Savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	-0.01 (0.03)	-0.01 (0.03)	-0.02 (0.03)		-0.02 (0.03)	-0.01 (0.03)	0.36** (0.15)
T1 Financial precarity		0.06** (0.03)		0.03 (0.02)	0.03 (0.02)	0.07* (0.03)	
Treated x T1 Financial precarity		-0.08** (0.03)				-0.08** (0.03)	
T2 Emergency savings			0.00 (0.01)	0.01 (0.01)	0.01 (0.01)	0.01 (0.01)	
T2 Emergency savings x T1 Financial precarity				-0.00 (0.01)	-0.00 (0.01)	0.00 (0.01)	
Intercept	0.12*** (0.03)	0.11*** (0.02)	0.11*** (0.03)	0.09*** (0.02)	0.10*** (0.03)	0.09*** (0.03)	1.89*** (0.14)
R^2	0.00	0.02	0.00	0.01	0.01	0.02	0.01

Notes: N = 733. To enhance interpretability, T1 Financial precarity is standardized (mean = 0, standard deviation = 1). Cluster-robust standard errors are reported in parentheses. Treated (1 = yes, 0 = no); safety citations = total number of safety citations. T1 = Time 1 (pre-treatment), T2 = Time 2 (post-treatment).

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table 4. Summary of Indirect Effects and Conditional Effects

Paths and effects	Pre-treatment Financial Precarity	Estimate	95% C.I.
Treatment → Safety Citations (H2)	Low	.064	[.014, .113]
	High	-.091	[-.140, -.042]
Treatment → Emergency Savings → Safety Citations (H3a)	--	.002	[-.002, .010]
Treatment → Emergency Savings → Safety Citations (H3b)	Low	.005	[.001, .011]
	High	.001	[-.005, .009]
Index of moderated mediation	--	-.004	[-.012, .004]

Notes: To enhance interpretability, T1 Financial precarity is standardized. Low and high financial precarity correspond to 1SD below and above average financial precarity.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Figure 1. Theoretical Model

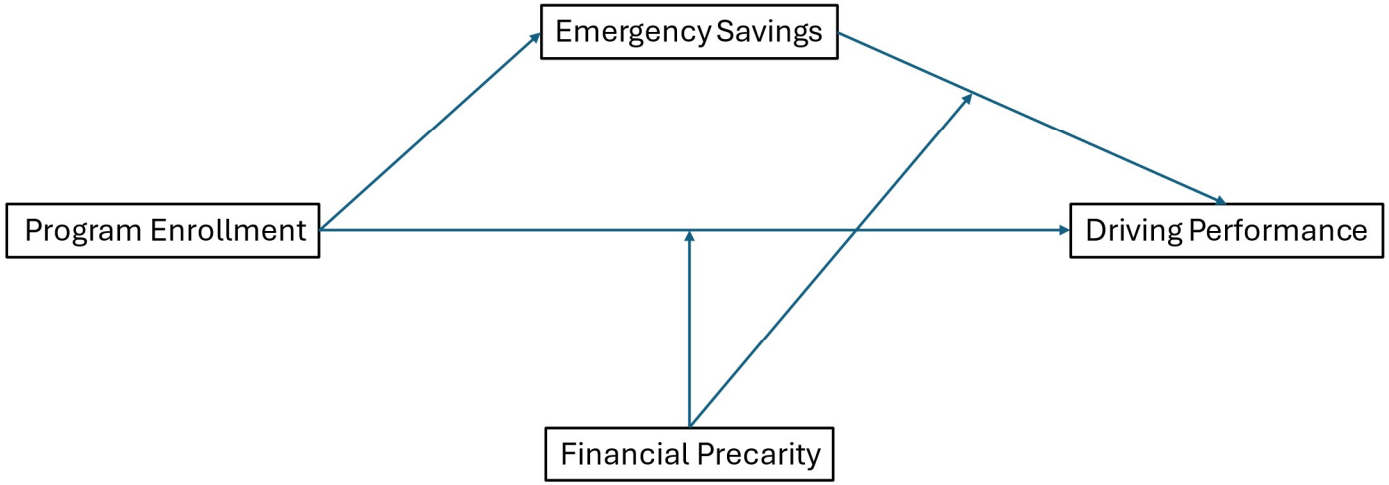

Figure 2. The Moderating Effect of Financial Precarity on the Relationship Between Treatment and Post-treatment citations