Does Savings Affect Participation in the Gig Economy? Evidence from a Tax Refund Field Experiment

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Social Policy Institute Working Paper
November 2019

As the gig economy plays an increasingly important role in the labor market, there is a need to understand the economic factors that influence participation in this sector. In this paper, we investigate how saving the federal tax refund affects gig economy participation for low-income online tax filers in the six months following tax filing. Using longitudinal survey data merged with administrative tax records, we leverage random assignment in a unique refund savings experiment as an exogenous instrument for refund savings to estimate the average effects of saving the tax refund on participation in the gig economy. Although we find no effects of refund savings on gig economy participation for the full sample of low-income filers, we find significant heterogeneous effects of refund savings for households conditional on student status and having access to liquidity prior to tax refund receipt—findings consistent with both life cycle models and the limited literature on the antecedents of gig participation. Specifically, refund savings reduced the likelihood of low-income students working in the gig economy, but increased the likelihood of more economically vulnerable households—non-students with low-incomes and substantial liquidity constraints—to work in the gig economy. These findings hold when estimating the effects of refund savings on the rate of joining the gig economy, suggesting that at least part of this shift in labor supply is occurring at the extensive margin.

I. Introduction

Discourse surrounding the rise of the gig economy takes place amid a broader shift in the structure of the U.S. economy over the past 30-40 years. This shift—the scale of which is a point of debate—has potentially helped drive noteworthy changes to the labor market. Several studies have documented a relative hollowing out of middle-skill employment opportunities, including textile and manufacturing-based jobs, which has coincided with an employment expansion in harder-to-automate services sectors, as well as in sectors requiring higher education; the latter have disproportionately experienced earnings gains over this time period (Autor et al., 2008; Jaimovich and Siu, 2012), while the former are commonly associated with lower, flatter earnings profiles.

Recent labor market changes over the past 10-15 years have ostensibly provided individuals and families with new opportunities to supplement earnings from their primary mode of employment, or to find alternative, flexible employment arrangements using online platforms that previously did not exist for individual contractors (e.g., Uber, AirBnB, Etsy). Our work, which examines the role that liquid savings plays in facilitating access to gig employment, fits broadly within the domain of understanding the behavioral responses of low-income workers who face a changing labor market and opportunities to participate in multiple work arrangements (e.g. Hirsch et al., 2017).

There is currently no broad consensus on a definition of the gig economy. Abraham et al. (2019) discuss these concerns, and put forth a definition of gig economic activity predicated on the worker (a) not receiving a wage or salary; (b) providing work services outside of a formal contractual agreement binding them or the firm to continuous employment; and (c) providing work services without any consistent or predictable schedule or earnings. Using this definition, alternative work arrangements are inclusive of gig economy work, though this conceptualization may admit jobs outside of common conceptions of gig work, including consultants who simply work outside of a standard office environment. These workers would ostensibly be included as self-employed workers, rather than gig workers, within surveys such as the Current Population Survey. While acknowledging the lack of a consensus around the definition of gig employment, for the purposes of our research we consider a person to be a gig worker if they earn money through an online platform or app such as Uber, Etsy, or Postmates.

Even as the definition of what constitutes gig work remains a point of debate, there is also a lack of consensus on both the size and growth of the gig economy. Analyses combining data from the Contingent Worker Supplement of the Current Population Survey and the RAND-Princeton Contingent Worker Survey find that, between the mid-2000s and 2015, participation within the “contingent sector”—a term often synonymous with the “gig economy”—has been rising, especially among women, black individuals, and older workers (Abraham et al., 2018; Katz and Krueger, 2019). This overall work participation increase, evident within the Survey of Household Economics and Decisionmaking, the RAND-Princeton survey, and Schedule C tax records, is generally interpreted as being driven by passenger transportation (Abraham et al., 2019). Alternatively, analysis based solely on the CPS suggests a relatively flat trend in gig economy participation over the 2000s (Katz and Krueger, 2019), and novel data drawing upon banking records from the JP Morgan Chase Institute suggest a relatively low baseline participation rate this type of work (roughly 1 percent of adults earned income through online platforms in a given month) (Farrell and Grieg, 2018).

Participation in the gig economy carries a number of benefits, including greater control over the number of work hours, flexible work scheduling, and increased ability to combine multiple income-earning activities (Hall and Krueger, 2018; Prudential, 2017). Low- and moderate-income households, who tend to be at greater risk of experiencing material hardships like food insecurity or skipped bills are more likely to struggle meeting financial commitments and may stand to disproportionately benefit from working in the gig economy. On average, low-earning workers are more likely to exhibit higher levels of earnings and income volatility (Hardy, 2017; Hardy and Ziliak, 2014). This relatively high level of volatility is thought to be driven by a combination of weaker labor force attachment—entries and exits from work—as well as changes in hours. Consistent with well-established labor market models wherein low- and moderate-income workers are also “first-in and last-out,” those who have low, relatively less predictable income streams may be at higher risk of experiencing drops in consumption levels and other household hardships. The flexibility offered through the technology of the gig economy has been shown to mitigate volatility in consumption (Koustas, 2018) and could therefore be welfare-enhancing for the many low-income workers at risk of experiencing these hardships.

At the same time, life cycle models of consumption and savings (e.g., Modigliani and Brumberg, 1954) as well as the empirical literature on poverty (e.g., McKernan and Ratcliffe, 2002; Cellini et al., 2008; McKay, 2009;) indicate that there are distinct categories of low-income workers who face different financial constraints and opportunities, and thus likely view the gig economy in different ways. For example, a vast majority of working students earn less than $42,000 per year (Carnevale et al., 2015) and qualify as “low-income.” However, the financial experiences of students, who may technically have low incomes, is fundamentally different from those of households who are considered to be chronically poor. By attending classes and developing skills, students are actively investing in their human capital (Becker, 1962) and can reasonably expect to earn substantially more money in the future (Deaton, 2005; Abel and Deitz, 2014). Therefore, these students are likely relatively uninterested in spending their time and effort in working gig jobs rather than, say, focusing on their education.

By contrast, non-student households experiencing short or long spells of low earnings—households that are commonly characterized as chronically or persistently poor—likely view the gig economy quite differently than transitorily poor households like students. Life cycle models suggest that these households should both strive to maximize their earnings over their lifetime while building both a stock of precautionary savings to buffer them against economic volatility and longer-term savings to either invest in themselves or support them in retirement (e.g., Browning and Crossley, 2001). However, these households likely struggle to earn enough to live comfortably or build either short- or long-term savings; they tend to have their limited budgets taken up primarily by expenditures on necessities (Schanzenbach et al., 2016), and thus struggle to build liquid savings (Collins and Gjertson, 2013) or savings for investments like starting a business or pursuing higher education (Beverly and Sherraden, 1999; Sherraden, 1991). This precarious financial situation often places these households at a heightened risk of experiencing material hardship (Despard et al., 2018; Heflin, 2016; Leete and Bania, 2010), indicating that they may stand to benefit from the work opportunities offered through the gig economy. Indeed, the potential income from working in this sector could provide a substantial boost to LMI households' income flows, as an analysis of account-level data for gig workers has found that that the average income for these workers is roughly $10,000 per year (Farrell, et al., 2019).[1]

Yet, there is reason to believe that they may struggle to access this segment of the workforce. Joining and working in the gig economy is typically associated with certain financial costs, such as the purchase of physical capital (e.g., a car for Uber, tools for TaskRabbit, furniture for Airbnb) and recurring operating expenses (e.g., vehicle repairs, raw materials for art sold on Etsy). The fixed costs associated with working in the gig economy can be substantial, and it is typically the responsibility of gig workers to cover these costs on their own (Koustas, 2018). Relatedly, household asset levels and non-gig incomes tend to fall as debt levels rise in the weeks prior to joining the gig economy (Koustas, 2019). While this trend may be coming about as a result of unexpected financial shocks, it may also be driven by workers voluntarily drawing down their assets and perhaps households purchasing the capital needed to work in the gig economy. Liquidity-constrained households, which are likely at greater risk of experiencing material hardship and have an increased incentive to earn money through the gig economy, may be unable to enter this market due to the preventative costs of gig work.

In this paper, we investigate how saving a federal tax refund can affect participation in the gig economy for low-income tax filers in the six months after tax filing. The direction of this effect is not immediately obvious. To the extent that financial costs can be a substantial barrier to gig work, saving a federal tax refund—which is often the single largest payment LMI households receive in a year (Roll et al., 2018; Roll et al., 2019)—may provide households with the liquidity needed to cover the fixed and operating costs of gig work, enabling them to participate in this labor market. Alternatively, increased

[1] To our knowledge, there has been no comprehensive estimates of the start-up costs of gig work, which would allow for a more holistic accounting of the potential benefits of gig work. Hall & Krueger (2018) examine both the costs and earnings specifically for Uber drivers and find that the average earnings in large markets was $19.35 per hour, while costs ranged from $2.94 to $5.34 per hour, depending on the type of car and whether the work was part-time or full-time. This indicates that the benefits of gig work may be substantially larger than the costs, at least for certain types of gig work.

access to liquidity through refund savings may ensure that LMI households will be able to manage future financial shocks and cover necessities like rent and medications without relying on additional labor income, thereby reducing the incentive to do gig work. The strength of these effects may vary across different types of LMI households. For example, we may expect that the positive effects of additional liquidity may be more pronounced for liquidity-constrained households.

Using administrative tax data and two waves of longitudinal survey data, we identify a causal relationship between refund savings and gig economy participation by applying an instrumental variable approach. Specifically, we leverage random variation in savings levels that came about through a low-touch savings experiment administered in 2017 through online tax-filing software that is free to qualifying low-income filers. The experiment tested how low-touch changes to the tax-filing environment affected the decision to deposit the federal tax refund into a savings account. Participating tax filers were randomly assigned to the control group that completed their taxes in the usual way and the treatment group that was exposed to one of five treatment conditions that promoted depositing the tax refund into a savings vehicle, such as a savings account or a U.S. Savings Bond. We used this random assignment to the treatment conditions as an instrument for refund savings to estimate the average effects of saving the tax refund on participation in the gig economy. In addition, due to the heterogeneity in the low-income population, we conducted several subsample analyses to understand how the effects of refund savings may vary across different types of low-income filers.

Our findings indicate no measurable average effect of refund savings on gig work for the full sample of low-income filers. However, we find strong heterogeneous effects of refund savings in different subsamples. Our findings show that refund savings reduced the likelihood of students working in the gig economy in the six months after tax-filing. For liquidity-constrained non-students, however, we consistently find that refund savings increased the likelihood of working in the gig economy in the six months after tax-filing. Additional evidence suggests that the shift in labor supply is also occurring at the extensive margin and the results from the reduced form regression are consistent with the main findings.

We expect that this research will have important implications for policymakers and researchers who are interested in the role of savings in the labor force participation of low-income households.

II. Data and Sample

A. Research Setting

All data used in this study were obtained through the Refund to Savings (R2S) Initiative, a research collaboration between Intuit Inc., the makers of TurboTax, Washington University in St. Louis, and Duke University. From 2012 through 2017, the R2S team implemented a series of unique tax refund savings interventions in TurboTax Freedom Edition (TTFE), an online tax-filing product that is offered through the Internal Revenue Service's Free File Initiative and is free for qualifying low-income households. To qualify for TTFE in 2017, a tax household needed to have no more than $33,000 in adjusted gross income or receive the Earned Income Tax Credit, with looser requirements for households with an active-duty member of the military.[2] Interventions from 2012 through 2016 tested how messaging, anchoring, and various changes in choice architecture of the tax-filing environment affected refund savings decisions, generally finding positive effects (Grinstein-Weiss et al., 2015; 2017a; 2017b; Roll et al., 2019). These interventions have also been shown to increase the rate of having at least part of the refund saved six months after tax-filing (Roll et al., 2018; Roll et al., 2019).

In 2017, the savings intervention focused on exploring how pre-commitment to save and tailored choice architecture can impact refund deposit decisions. As part of this experiment,[3] users of TTFE randomly assigned to the control group went through the usual tax-filing experience and had three options for the method of receiving the federal tax refund: direct deposit into a bank account, a paper check, and U.S. Savings bonds. This refund receipt screen did not explicitly emphasize the option of depositing the tax refund into a savings account (Appendix, Figure 1A). Tax filers in the treatment group were randomly assigned to one of four interventions that, though they varied slightly in design, all focused on

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[2] In practice, over 98 percent of 2017 TTFE filers qualified through the income and Earned Income Tax Credit criteria.
[3] For a more detailed description of the 2017 R2S experiment, see Roll et al. (2019).

encouraging filers to save their full federal tax refund. A random subset of the treatment group was offered an opportunity to pre-commit to saving the tax refund at the beginning of the tax-filing process (Appendix, Figure 2A). Subsequently, all treated tax filers saw savings-focused refund receipt screens, which included an option to deposit the entire refund into a savings account, split the refund into both savings and checking accounts, directly deposit the entire refund into a checking account, and receive a paper check by mail. The option to deposit the entire refund into a savings account was listed first on the refund receipt screen, thereby making it the most salient option; it was also prepopulated for tax filers who, at the beginning of the tax-filing process, pre-committed to save their tax refund. The refund receipt screen seen by the treatment groups also included a reminder about the intention to save (among pre-committers) or a motivational message about the importance of saving for emergencies (among non-pre-committers). Figure 3A in the Appendix shows an example of the refund receipt screen for treated filers who pre-committed to save their tax refund.

Table 1A in the Appendix shows that the control and treatment groups in the savings-focused experiment were well-balanced on observed baseline characteristics. The only statistically significant differences were related to refund savings. Specifically, the rate of depositing the full tax refund into a savings account was nearly twice as high for filers randomized into the treatment group (20.8 percent) as it was for filers randomized into the control group (10.7 percent); likewise, the amount of federal tax refund saved was higher in the treatment than in the control group ($319 and $175, respectively). Given the strong balance between treatment and control groups resulting from random assignment we can assert that the only difference between these groups was the treatment assignment and, therefore, that the differences in refund savings deposits are exogenous. In this paper, we leverage this exogenous shift in the rate of saving the federal refund to identify the effects of refund savings on participation in the gig economy.

B. Data Source

Data for this paper come from administrative tax records and the two waves of the 2017 Household Financial Survey (HFS), which was administered through the R2S Initiative. Survey data were collected on a subset of TTFE users who were randomly invited to participate in the first wave of the 2017 HFS immediately after filing their taxes. Respondents who completed the first wave of the survey were also invited to take a follow-up survey approximately six months after tax filing, thereby allowing us to observe the same respondents over the six-month period. Both survey waves included questions about tax filers' demographic and financial characteristics, asset and debt levels, experiences of hardship and financial shocks, and participation in the gig economy. Administrative tax data, collected through TTFE, contain information on tax filers' household income, federal tax refund, dependents, and tax credits and tax deductions.

The final dataset was obtained by combining longitudinal survey responses with individual-level administrative tax data. In total, 4,680 LMI individuals who received a federal tax refund, completed both waves of the 2017 HFS, and had non-missing data on key demographic and financial characteristics were included in the analytical sample. [4]

C. Variable Description

The purpose of this research is to examine the extent to which savings accumulation can drive participation in the gig economy among LMI tax filers. The key independent variable measured through the administrative tax records at the time of tax filing describes whether tax filers deposited their entire federal tax refund into a savings account. We expect that tax refund deposits in a savings account can be a good proxy for the accumulation of liquid savings for future uses. This expectation is based on the prediction that savings accounts tend to be "stickier" than checking accounts. First, money in savings accounts is usually less liquid than that in checking accounts: while funds in checking accounts can be

[4] Among the roughly 420,000 households who were offered the opportunity to take the survey, 39,305 consented to participate in the study. Among those who consented to participate in the study, 32,305 respondents completed the first wave of the survey. 9,864 respondents who completed the first wave of the survey began taking the second wave of the survey. 9,038 of these respondents completed the second survey wave. Tax data were successfully merged with survey responses for 77.6 percent of these survey takers.

easily accessed through the use of debit cards, ATM withdrawals, or electronic transfers, savings accounts typically have certain frictions associated with them including the inability to pay with a debit card or the extra time and effort required to transfer money from a savings to a checking account. Second, prior work from the field of behavioral economics suggests that the fungibility of money is conditional on the perceived purpose of a given pool of funds—various types of accounts tend to be designated for different consumption and savings purposes and funds placed in various accounts out of which individuals have different marginal propensities to consume (Shefrin and Thaler, 1988; Thaler, 1999). Account types can be organized according to a mental hierarchy, where cash and checking accounts are considered more tempting and easier spendable ("current assets") and savings accounts tend to have lower temptation levels and are less fungible ("current wealth") (Thaler, 1999). Funds kept in savings accounts are expected to be saved for longer time periods than money allocated to checking accounts. Therefore, by encouraging people to allocate money to savings rather than checking accounts, individuals could potentially boost their short or longer-term savings rates. Indeed, as mentioned above, previous low-touch interventions that were designed to encourage low-income filers to deposit their refunds into savings accounts led to a higher rate of having some of the refund saved six months later (Roll et al., 2018; Roll et al., 2019)

The outcome variable was measured through the second wave of the HFS and captured individual participation in the gig economy. Specifically, the survey question asked respondents to indicate whether in the past six months, they earned any income through services offered through a mobile app or website, which may include ride-sharing services like Uber, home-sharing services like AirBnB, and selling crafts through sites like Etsy.

D. Sample Characteristics

Demographic and financial characteristics for our LMI sample measured at the time of tax filing are presented in Table 1. The first column describes the full sample. The majority of respondents were White (75 percent) and had at least a bachelor's degree (58.9 percent) at the time of tax filing while a majority

(58.3 percent) of the sample was female.[5] Almost a third of the sample was enrolled in an educational program at the time of the survey's first wave, and the average respondent was 34.4 years old. In terms of household composition, 20 percent of respondents had dependents and had an average of 1.7 adults living in the household. Almost one-fifth of respondents were unemployed and 46.7 percent worked full-time. In the year prior to filing their taxes, 4.4 percent of survey takers reported participating in the gig economy. Generally, sampled respondents experienced substantial financial hardship. The average adjusted gross income was just $16,691 and 36.2 percent of respondents received the Earned Income Tax Credit (EITC). While vehicle ownership was prevalent in our sample, only 24.3 percent of survey takers owned a home. Sixty percent reported that they would be able to access $2,000 within a month in the event of an emergency using any source available including family, friends, and credit lines—a common measure of liquidity in the household finance surveys (e.g., the National Financial Capability Study, 2018). 17.8 percent reported experiencing unexpected income volatility, and the median level of liquid assets was $1,560 (the mean was $6,605). The average size of the refund was $1,601, and 18.6 percent of the sample deposited their full tax refund into a savings account. On average, sampled tax filers saved $287 of their tax refund, and those who saved any of their refund saved and average of $1,734.

Table 1 also compares the baseline characteristics of students (individuals currently enrolled in an educational program) and non-students (Columns 2 and 3) and individuals with and without access to $2,000 in emergency liquidity (Columns 5 and 6), pointing to significant inter-group differences. To highlight a few, although non-students had higher levels of adjusted gross income, received larger federal tax refunds, and were more likely to own a vehicle and a house, students were more likely to have access to $2,000 in an emergency, had higher levels of median liquid assets, were substantially less likely to have received the Earned Income Tax Credit, and were significantly more likely to save their full federal tax refund. Student and non-students participated in the gig economy in the 12 months prior to tax filing at similar rates. As expected, survey takers with access to $2,000 in liquidity appeared to have

[5] Relative to the rest of the low-income population (as measured through the 2017 American Community Survey), a higher proportion of our sample was female, White, and had higher levels of educational attainment.

substantially higher levels of financial security. Compared to liquidity-constrained respondents (i.e., those without access to $2,000 in emergency liquidity), those with access to liquidity had higher incomes, more money in liquid assets, were more likely to have saved the full tax refund, and own a vehicle and a house. A higher proportion of liquidity-constrained respondents received the EITC and had higher federal tax refunds. The observed heterogeneity in the sample suggests that different groups may face different financial constraints and needs and have different motivations for working in the gig economy, which prompts further investigation as to how savings incentivize gig economy participation across different subgroups.

[Insert Table 1 Here]

III. Analytical Approach

To assess the average effect of savings on gig economy participation among LMI individuals, we estimate a linear probability model of the general form:

$$Y_{ij} = \beta_0 + \beta_1 S_{ij} + \boldsymbol{X}_{ij}\lambda + \delta_j + \gamma_t + \delta_j \cdot \gamma_t + u_{ij} \quad (1)$$

where Y_{ij} is an indicator of whether respondent i from state *j* reported working in the gig economy in the six months after filing their taxes, and S_{ij} is a binary indicator of whether respondent i deposited their entire tax refund into a savings account. A set of control variables measured at the time when respondent *i* filed their taxes ($\boldsymbol{X}_{ij}$) includes respondent i's gender, age, age squared, race/ethnicity, level of educational attainment, employment status, participation in the gig economy in the past year, presence of dependents, the number of adults in a household, the experience of unexpected income volatility,[6] homeownership, vehicle ownership, household adjusted gross income, and refund size. Importantly, this vector of controls also includes an indicator of whether or not respondent *i* worked in the gig economy in the 12 months

[6] This variable was constructed based on responses to two questions. The first question asked respondents how their household income compares from month to month. Anyone who indicated that their incomes varied somewhat or quite a bit from month to month were also asked how predictable the fluctuations in income were. Respondents who indicated that fluctuations in income were unpredictable were coded as having experienced unpredictable income volatility. All other respondents were coded as having not experienced unpredictable income volatility.

prior to tax-filing. State fixed effects are represented by δ_j, the month of tax filing is captured by γ_t, and term ($\delta_j \cdot \gamma_t$) corresponds to the interaction between the two. These fixed effects reduce variance that may be have come about through geographic differences in factors that could influence an individual's propensity to work in the gig economy (e.g., regional economic opportunity, regulations of the gig economy, or demand for gig economy services). Finally, u_{ij} is an error term.

An OLS estimation of Equation 1 may produce biased estimates of β_1, the key parameter of interest, if the decision to save the full tax refund was correlated with other unobserved factors that influence gig economy participation decisions, such as the experience of acute financial hardship at the time of tax filing, family commitments, or individual preferences towards savings and labor force participation. Given the endogeneity issues associated with estimating Equation 1, we conducted two-stage least squares (2SLS) analyses to obtain unbiased estimates of β_1, where the savings decision to deposit the full federal tax refund into a savings account (S_{ij}) was instrumented by a random assignment of savings experiment participants into the treatment and control groups (T_{ij}). The first stage of the 2SLS is shown in Equation 2:

$$S_{ij} = \gamma_0 + \gamma_1 T_{ij} + \boldsymbol{X}_{ij}\lambda + \delta_j + \gamma_t + \delta_j \cdot \gamma_t + v_{ij} \qquad (2)$$

where S_{ij} is defined the same as above and T_{ij} describes whether respondent i was randomly assigned to one of five savings-focused intervention groups in the 2017 R2S experiment. A valid instrument must can be correlated with the outcome only through its correlation with the variable of interest. We expect this assumption to hold in our study, as it is implausible that any changes in gig participation in the months following tax filing would be the direct result of random assignment to the savings-focused intervention through any channel other than the shift in refund savings deposits. At the same time, the comparison of treatment and control groups in Table A1 provides empirical evidence that the randomized savings-focused experiment was effective in driving the rate of refund savings; we further test the strength of our instrument by reporting the results from the first-stage regressions in the next section.

Finally, we estimate a reduced form linear probability model as shown in Equation 3:

$$Y_{ij} = \theta_0 + \theta_1 T_{ij} + \boldsymbol{X}_{ij}\lambda + \delta_j + \gamma_t + \delta_j \cdot \gamma_t + \varepsilon_{ij} \qquad (3)$$

As in Equation 1, the dependent variable in Equation 3 is Y_{ij}, a binary indicator of whether or not respondent *i* worked in the gig economy in the six months after tax-filing. T_{ij} again indicates whether or not respondent *i* was randomly assigned into one of the treatment conditions in the 2017 R2S refund savings experiment. Since T_{ij} was exogenously determined, θ_1 shows the average effect of the 2017 R2S interventions on participation in the gig economy in the six months after tax-filing. Although the refund savings rate of the treated group was nearly twice that of the control group, just over one-fifth of treated households saved their federal refund. Therefore, θ_1 likely gives a more conservative estimate of the effects of refund savings on participation in the gig economy.

In addition to analyzing the average effects of refund savings on participation in the gig economy among LMI individuals, heterogeneity among LMI households in the role of liquid savings and the potential benefits offered through gig employment motivate us to explore heterogeneous impacts on gig economy participation for different types of individuals. To address these differences, we conducted six subgroup analyses for the sample of students and non-students, students with and without access to emergency liquidity at the time of tax-filing, and non-students with and without access to emergency liquidity at the time of tax-filing.[7] Lastly, in order to understand the impact of refund savings on joining the gig economy—as opposed to simply participating in the gig economy—we limited the sample only to tax filers who had reported not working in the gig economy in the 12 months prior to tax-filing.

[7] In both waves of the HFS, respondents were asked how likely it was that they would be able to access $2,000 in liquidity within one month if an emergency arose. Respondents who indicated that they probably could not or definitely could not access $2,000 within one month are considered liquidity-constrained. All other respondents (all of whom indicated that they either probably could or definitely could access $2,000 within a month) are not considered liquidity-constrained.

IV. Results

A. Main Results

For our analysis, we first conducted the OLS regression analysis, by estimating Equation 1, to explore the relationship between refund savings and participation in the gig economy for the full sample and six subgroups (students and non-students as well as liquidity-constrained and liquidity non-constrained students and non-students) (Table 2, Panel A). Because, for reasons stated earlier, this estimation is likely to produce biased results, we then estimated the 2SLS regression model where refund savings was instrumented by random assignment into an experimental group (Table 2, Panels B and C). In order to explore whether refund savings affected the rate of joining the gig economy, each of analyses were repeated with the sample restricted to those who had not worked in the gig economy in the 12 months prior to tax-filing (Table 3). For each instrumental variable regression, we report the results of tests of weak identification (Kleibergen-Paap rk Wald F-Statistic). For space considerations, the output suppresses the coefficients for control variables.

Panel A in Table 2 presents OLS regression results where participation in the gig economy was regressed on refund savings. Column 1 corresponds to the full sample and Columns 2-7 show the findings for the six subgroups. The coefficient on the saved refund was not statistically significant for the full sample as well as for most subgroups, with the exception of students. Among students, saving the full tax refund was associated with an increase in the rate of participation in the gig economy by 3.1 percentage points ($p<.1$). As mentioned earlier, we cannot infer causality from these associations and employ an IV 2SLS approach to establish a causal relationship.

Panels B and C in Table 2 present the IV estimates showing the effects of refund savings on participation in the gig economy for the full sample (Column 1) and the six subgroups (Columns 2-7). Panel B corresponds to the model without any controls and Panel C reflects regression results with a full set of controls. First stage F-test statistics indicate that the identified instrument is strong for all but one model: likely due to sample size issues, the instrument in the analysis of liquidity-constrained students appears to be weak, and thus caution is recommended when interpreting these findings. The full first stage regression results are included in Table 1B of the Appendix.

Coefficients remained insignificant for models without controls (Panel B), with one exception: we observe that saving the full tax refund increased the probability of working in the gig economy for liquidity-constrained non-students by 40.4 percentage points (95% CI: 2.03pp to 78.7pp). Including controls (Panel C) helps improve the precision of estimates. After accounting for demographic and financial characteristics, results for the full sample show that, on average, saving the full refund did not have a statistically significant impact on participation in the gig economy in the six months after tax filing. Despite the lack of significant effects for an average tax filer, our findings indicate substantial heterogeneity in impact estimates. We find that saving the tax refund reduced the rate of participation in the gig economy for students by 35 percentage points (95% CI: -63.0pp to -6.92pp). The negative impact of refund savings on gig economy participation appeared to hold only for students with access to liquidity, but not for those who were liquidity-constrained. For students with access liquidity at the time of tax filing, saving the full tax refund led to a 29.6 percentage point reduction in the probability of working in the gig economy (95% CI: -56.7pp to -2.50pp), and the coefficient was negative and statistically insignificant for students who lacked access to emergency savings. Opposite trends were observed for the subsample of non-students. While saving the full tax refund did not have a statistically significant effect on the rate of participation in the gig economy in our analysis of all non-students, we did find that saving the refund increased the rate of working in the gig economy by 46.9 percentage points among liquidity-constrained non-students (95% CI: 8.46pp to 85.3pp). The effect of refund savings on participation in the gig economy for non-students who could access funds in the case of an emergency was positive but statistically indistinguishable from zero.

Panel D presents the results from reduced form models that estimate the effects of the 2017 R2S savings experiment on participation in the gig economy. Since just over one-fifth of those in the treatment group actually saved their tax refund, the coefficient on this variable likely provides a more conservative estimate on the effects of refund savings on participation in the gig economy. As these results show, we see that random assignment to the treatment group did not have a statistically significant effect on participation in the gig economy for our sample as a whole. However, in our analysis of all students, we see that random assignment into one of the treatment groups reduced the rate of participation in the gig economy by 4.70 percentage points (95% CI: -8.29pp to -1.14pp). In our analysis of students with access to liquidity, we find that the treatment reduced the rate of gig participation by a similar magnitude – 5.2 percentage points (95% CI: -10.0pp to -0.339pp). Again, we do not observe statistically significant effects in our analyses of liquidity-constrained students, non-students as a whole, and non-students who have access to liquidity. However, we find that random assignment to one of the treatment groups increased the rate of gig work by 3.71 percentage points for liquidity-constrained non-students (95% CI: 1.40pp to 6.02pp).

[Insert Table 2 Here]

Our findings so far indicate refund savings decreased the rate of participation in the gig economy for students (including those that had access to emergency liquidity) while increasing the rate of participation for liquidity-constrained non-students. These findings raise an important question: Does saving the full tax refund get workers to work more (or fewer) hours than they otherwise would have worked, or does refund savings encourage people to start (or quit) participating in the gig economy? Due to sample size limitations, we cannot directly test whether refund savings shifts the number of hours gig economy earners work or whether savings affected the rate of quitting gig work, but we can examine the impact of refund savings on joining the gig economy. We do so by restricting our sample to respondents who indicated that they did not work in the gig economy in the 12 months prior to tax-filing and re-estimating the models shown in Table 2. Since these analyses do not include respondents who previously worked in the gig economy in the year prior to tax-filing, the coefficient estimates show the effects of refund savings on the rate of joining the gig economy.

Table 3 presents these results for the OLS and 2SLS estimation models after excluding a small proportion of individuals that did gig work at the time tax filing. The models are identical to the ones presented in Table 2, though they do not control for gig economy participation twelve months prior to tax-filing. With the exception of a single subsample, first stage *F*-test statistics point to the strength of the instrument across the models. The general conclusion is that the OLS, 2SLS, and reduced form results for joining the gig economy are strongly consistent with the results for any gig economy participation in Table 2. In particular, the main results with a full set of controls (Table 3, Panel C) again show no average effects of refund savings on joining the gig economy for the full sample. At the same time, refund savings had a negative and statistically significant effect on the rate of joining the gig economy for all students (95% CI: -56.4pp to -2.11pp). The effect on students who had access to emergency liquidity was similar in magnitude but only marginally significant. The effect of refund savings was statistically insignificant in our analyses of liquidity-constrained students, all non-students, and non-students with access to liquidity. However, we found the opposite effect for liquidity-constrained non-students—a 39.5 percentage point increase in the likelihood of joining the gig economy (95% CI: 9.89pp to 69.2pp). As Panel D shows, the effects of the R2S savings intervention on the rate of joining the gig economy had the same signs and levels of statistical significance as the effects of refund savings (Panel C), but the estimated effects of the savings experiment were smaller in absolute value. Random assignment into the treatment group reduced the rate of joining the gig economy for all students by 3.89 percentage points (95% CI: -7.38pp to -0.397pp). The impact on liquidity-constrained students was similar in magnitude but was only marginally significant. Assignment to the treatment group did not have a statistically significant effect on the rate of joining the gig economy in our analyses on liquidity-constrained students, all non-students, and non-students with access to emergency liquidity. However, random assignment to the treatment group increased the rate of joining the gig economy for liquidity-constrained non-students by 3.71 percentage points (95% CI: 1.40pp to 6.02pp).

[Insert Table 3 Here]

B. Robustness Checks

In this section, we report results from additional analyses examining the validity of our instrument and checking the robustness of our findings to different variable specifications.

A key assumption underlying the validity of our instrument is that the random assignment in the experiment was correlated with working in the gig economy only through its correlation with refund savings. Generally, since the experiment was very low-touch and focused specifically on depositing the refund into savings, we think it is unlikely that the random assignment affected other outcomes which would have driven the changes we observe in gig participation. Although filers did not have the option to open new accounts in the TTFE software, there may be a concern that experiment encouraged filers to open savings accounts and that savings account ownership may be correlated with the changes in gig participation that we observed. For example, opening a savings account may be linked with increases in subsequent savings deposits, and may thus be the mechanism by which households save enough to access gig employment (rather than through saving the refund directly). If random assignment into either a treatment or control group affected account ownership, it could be the case that savings account ownership (rather than saving tax refunds) drove participation in the gig economy. Indeed, Despard et al. (2018) has previously found that a different R2S savings intervention (from a previous year) had a marginally significant impact on savings account opening in the six months after tax-filing.

We tested the validity of this concern by regressing an indicator of whether the respondent reported owning a savings account six months after tax filing on our instrument, the random assignment in the savings experiment. These models contained the same vector of control variables as the earlier 2SLS models (Equation 1, 2, and 3). As shown in Table 4, we found a statistically insignificant relationship between savings account ownership and the savings experiment interventions for the full sample and each subsample, which provides additional evidence that the instrument is valid as it impacts gig participation only through its influence on refund savings decisions.

[Insert Table 4 Here]

We further examined the sensitivity of our findings to alternative measures of refund savings and access to liquidity. First, rather than using an indicator of depositing the entire refund into a savings account, we used an indicator of whether a respondent deposited any portion of the tax refund into a savings account. Over 95 percent of savers in our sample saved their entire refund. The set of respondents who saved any of their refund includes all of those who saved their entire refund as well as those who saved part of their tax refund (and ended up putting aside less in savings). The results reported in Table 5 were very consistent with those in Panel C of Tables 2 and 3. Though coefficients were of a slightly smaller magnitude, they had the same sign and similar levels of statistical significance.

[Insert Table 5 Here]

Second, we explored whether using an alternative measure of liquidity would alter our conclusions. We broke down the subsamples by the reported amount of liquid assets rather than self-assessed access to $2,000 in an emergency and restricted the sample to households that had non-missing information on liquid assets.[8] The full refund savings was used as the key independent variable in this analysis. Table 6 shows that after dropping some observations, the average effects for the full sample resembled those observed earlier. Likewise, the results for non-students were consistent with earlier findings: while we found no statistically significant effects of refund savings on gig work for an average non-student—including an average non-student with higher asset levels—those with fewer assets were 44.1 percentage points more likely to participate in the gig economy (95% CI: 3.34pp to 84.8pp) and 38.1 percentage points more likely to join the gig economy (95% CI: 3.38pp to 72.9pp). For students, while we still found that refund savings reduced the rate of gig economy work for an average student (by 30.4 and 24.0 percentage points for participating and joining gig economy, respectively), the negative coefficient for students with a higher level of liquid assets was statistically insignificant. At the same time, even though the coefficient for students with lower levels of liquid assets was statistically significant for gig economy participation, the low *F*-statistic from the first stage regression (likely due to the small sample size) indicated that the instrumental variable may be weak for this subgroup and thus no valid conclusion can be drawn from this result.

[Insert Table 6 Here]

[8] We used slightly different medians for different subsamples. For the analyses on gig economy participation, the median level of assets was $2,200 for students and $1,115 for non-students. For the analyses on joining the gig economy, the median level of assets was $2,302.50 and $1,180 for students and non-students, respectively.

V. Discussion

In this paper, we explored the degree to which access to savings—as facilitated by an experiment that led to an exogenous increase in tax refund savings deposits among LMI tax filers—influences participation in the gig economy. While our analysis found that tax refund savings did not affect participation in the gig economy for the average LMI tax filer in our sample, we also observed notable and consistent heterogeneous effects across different subsamples of filers. For students, refund savings reduced the rate of working in the gig economy. For non-students facing liquidity constraints, refund savings increased the rate of working in the gig economy. We also found that saving the refund impacted the rate of joining the gig economy in similar ways—increasing the rate of joining the gig economy for liquidity-constrained non-students and decreasing the rate of joining the gig economy for students. These results were robust to an array of different modeling approaches and variable specifications.

These findings are consistent with life cycle models of consumption and savings (e.g., Modigliani and Brumberg, 1954), which predict that households will seek to both maximize and smooth the utility of consumption both over the course of their lives and in the event of economic volatility. Low- and moderate-income students, who are investing in their human capital and can reasonably expect to earn higher incomes in the future (Deaton, 2005), likely have less incentive to participate in gig work to maintain their consumption; contributing labor to the gig economy would necessarily result in less time spent on educational pursuits, and students can often rely on education debt (or their family) to financially support them during their education. As such, students may be more likely to use their refund as a source of "precautionary" savings (Browning and Crossley, 2001)—drawing these savings down in the event of economic volatility—rather than using these savings to invest in the physical capital required for gig work or otherwise facilitate access to the gig economy.

At the same time, LMI households that are non-students and that face substantial liquidity constraints may have different incentives when it comes to gig economy participation. These households, who are likely more in-line with common conceptions of the "poor" than those experiencing transitory spells of low incomes like students (McKernan and Ratcliffe, 2002; Cellini et al., 2008; McKay, 2009;), may wish to earn income through the gig economy to maximize their consumption over their lifetime or to offset economic volatility. However, these households often lack the funds to cover the fixed costs of gig participation. There is a fair bit of reason to believe that these households may be interested in working in the gig economy as 42 percent of respondents in this group reported experiencing income volatility in the six months before tax-filing, which is 50 percent higher than the national average (Board of Governors of the Federal Reserve System, 2018). The flexible work schedules offered through the gig economy may give these households the opportunity to smooth their incomes when their regular employment is less consistent. However, without the liquidity to cover the costs of gig work, it may difficult for these would-be workers to enter the market. The receipt and saving of the tax refund may be one of the few opportunities for such households to actually overcome these costs and access a new source of income through gig labor.

In this, our work may point toward an interesting extension of life cycle models of consumption and savings. Whereas savings is typically characterized as being for precautionary purposes such as saving to buffer against a financial emergency, or for investment purposes such as higher education or starting a business to increase lifetime earnings, our findings may point a somewhat different role for savings. Specifically, limited amounts of savings may be used either directly for precautionary purposes (as we observed in our student sample) or to provide access to new streams of income by overcoming the low costs of gig employment relative to, say, starting a business or pursuing higher education (as we observed in our liquidity-constrained non-student sample). In essence, this potential extension of these models may only emerge in the specific context of gig work or other similar labor arrangements, in which small amounts of liquid savings can translate into additional income streams through access to gig jobs.

However, there is an open question about the precise mechanisms underlying the results observed in our paper. Limitations in our data and sample size prevent us from conducting a comprehensive exploration into the ways by which liquid savings facilitate participation in the gig economy for LMI households. It may be that allocating money into accounts designated for savings purposes shifts how households view that money (e.g., Thaler, 1988), making them more likely to use these funds for investment or income-generating purposes. This mechanism could function directly by simply shifting the way in which households spend their funds, or indirectly by encouraging households to further build on accumulated savings in order pay for the costs of gig work. It may also be that households who have money placed in savings may respond to the experience of economic volatility (e.g., the loss of a job) by using those savings to create an additional income stream through gig employment. In this way, households could use their precautionary savings to create alternative income streams to finance consumption rather than financing consumption directly through those savings. Finally, it is possible that our intervention, which encourages households to explicitly allocate their tax refund for savings purposes, may increase households' needs to consume out of their current income. A household that allocates an additional $1,000 of their tax refund into savings may be less willing to consume out of those funds, and may respond to this reduced pool of consumption-allocated funds by taking on gig work to meet their consumption needs. Prior research has shown that interventions encouraging households to earmark funds for savings can also increase consumer debt usage (Sussman and O'Brien, 2016), and it may be possible that similar savings interventions also encourage pursuing income streams with low barriers to entry, such as gig labor, as an alternative to consuming out of savings.

An additional limitation of our paper is that we are unable to examine the differential impacts of savings on different categories of gig work. Prior research (Farrell et al., 2018) has defined gig work as being either capital-focused (e.g., renting properties or making and selling goods) or labor-focused (e.g., ride-sharing or pet care). It is likely the case that the impacts of savings on gig participation differ by the type of gig work. Additional savings may help households repair or detail a car to drive for Uber or purchase supplies to produce goods for sale on Etsy, but may be less effective at preparing a property to rent on AirBnB. By comparing the effects of savings on capital-intensive gig work with those on labor-intensive gig work, we may be able to test some of our hypotheses about the mechanisms at play.

Unfortunately, due to sample size limitations, we were unable to test this hypothesis empirically. This was another limitation of our study and an avenue for future research.

A final limitation of our study is that, while we were able to estimate the relationship between savings and participation in the gig economy (i.e. the shifts in gig work at the extensive margin), we were unable to estimate the ways in which refund savings shifts the number of hours people work in the gig economy (i.e., the shifts in gig work at the intensive margin) due to sample size limitations. This too is a fruitful area for future research.

Despite these limitations, our findings clearly point to the importance of liquid assets in the LMI households. In addition to serving as a buffer against financial hardship, liquid assets appear facilitate access to the gig economy and, potentially, additional income streams for these households. However, given the overall low rates of liquid saving in the U.S. population generally and in LMI households specifically (Board of Governors of the Federal Reserve System, 2016), an implication of this research is that many households may be shut out of gig labor due to low levels of liquidity. In particular, our results show that low-income, liquidity-constrained non-students—those who may stand to benefit the most from additional income streams—are often unable to access those opportunities without access to additional liquidity. If ensuring equitable access to this labor market is a goal of policy, policymakers and practitioners working in financial security-related fields should examine ways of either increasing liquidity access in LMI households (for example, by pairing federal or state EITC expansions with savings incentives) or by finding ways of lowering the costs of accessing gig jobs.

Disclaimer

Statistical compilations disclosed in this document relate directly to the bona fide research of, and public policy discussions concerning, financial security of individuals and households as it relates to the tax filing process and more generally. Compilations follow Intuit's protocols to help ensure the privacy and confidentiality of customer tax data.

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Tables and Figures

Table 1. Baseline Sample Characteristics

Characteristic	Full sample	Mean/Proportion: Student	Mean/Proportion: Non-student	Sig.	Mean/Proportion: Access to $2,000	Mean/Proportion: No access to $2,000	Sig.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Participated in gig economy (past 12 months) (%)	4.4	4	4.6		3.7	5.5	
Male (%)	41.7	43.4	40.9		45.2	36.4	
Age (years)	34.4	26.1	38.4	***	33.9	35.1	***
White (%)	75	69.8	77.6	***	76.2	73.2	**
Black (%)	5.6	4.9	6		3.9	8.1	***
Hispanic/Latino (%)	7.7	9.6	6.8	***	6.3	9.9	***
Asian (%)	6	9.6	4.2	***	7.7	3.4	***
Other (%)	5.6	6.2	5.4	***	5.8	5.4	***
College degree or greater (%)	58.9	49.2	63.7	***	65.8	48.5	***
Enrolled in school at time of survey (%)	32.9	100	0	***	35.8	28.6	***
Any dependents (%)	20	11.6	24.2	***	16.3	25.7	***
Number of adults in the household	1.7	1.8	1.6	***	1.7	1.7	*
Unexpected income volatility (%)	17.8	13.7	19.8	***	11.8	26.9	***
Employed full-time (%)	46.7	26.59	56.6	***	47.6	45.3	
Employed part-time (%)	33.7	54.7	23.3	***	32.8	35	
Not employed (%)	19.7	18.7	20.1		19.6	19.7	
Adjusted gross income ($)	16,691	12,696	18,654	***	17,180	15,950	***
Could come up with $2,000 in an emergency (%)	60.2	65.4	57.7	***	100	0	***
Liquid assets (median, $)	1,560	2,300	1,200		3,778	300	
Liquid assets ($)	6,605	6,987	6,419		10,157	1,220	***
Owns a car (%)	72.6	65.4	76.2	***	75.3	68.6	***
Owns a home (%)	24.3	19.1	26.9	***	27.2	20	***
Any income volatility (%)	33.8	33.7	33.9		42.1	28.4	***
Federal tax refund amount ($)	1,601	1,308	1,745	***	1,518	1,727	***
Received Earned Income Tax Credit (%)	36.2	25.3	41.5	***	30	45.6	***
Saved full federal tax refund (%)	18.6	23.7	16.1	***	22.2	13.2	***
Amount of federal refund saved ($)	287	291	285		331	219	***
Amount of federal refund saved among savers ($)	1,734	1,194	1,653	***	1,430	1,549	***
N	4,680	1,542	3,138		2,819	1,861	

Notes: Statistical significance: ***p<.01, **p<.05, *p<.1.

Table 2: Effects of Savings on Participation in the Gig Economy

	Full Sample	All Students	Students With Liquidity	Liquidity-Constrained Students	All Non-Students	Non-Students With Liquidity	Liquidity-Constrained Non-Students
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS regression (Full controls)							
Saved Full Refund	0.00489	0.0313*	0.0332	0.0125	-0.0148	-0.0137	-0.00788
95% CI	[-0.013, 0.023]	[-0.003, 0.066]	[-0.011, 0.077]	[-0.062, 0.087]	[-0.028, 0.008]	[-0.032, 0.012]	[-0.037. 0.030]
Observations	4,680	1,542	1,009	533	3,138	1,810	1,328
R-squared	0.186	0.235	0.279	0.449	0.231	0.232	0.312
Panel B: IV regression (No controls)							
Saved Full Refund	0.0136	-0.213	-0.213	-0.209	0.142	-0.00800	0.404**
95% CI	[-0.154, 0.181]	[-0.526, 0.101]	[-0.530, 0.104]	[-1.218, 0.801]	[-0.066, 0.349]	[-0.250, 0.234]	[0.020, 0.787]
Kleibergen-Paap rk Wald Statistic	75.00	24.01	23.39	2.35	50.82	31.42	22.29
Observations	4,680	1,542	1,009	533	3,138	1,810	1,328
Panel C: IV regression (Full controls)							
Saved Full Refund	-0.0221	-0.350**	-0.296**	-0.495	0.149	0.0419	0.469**
95% CI	[-0.166, 0.121]	[-0.63, -0.069]	[-0.567, -0.025]	[-2.14, 1.15]	[-0.035, 0.332]	[-0.174, 0.258]	[0.085, 0.853]
Kleibergen-Paap rk Wald Statistic	87.50	26.66	24.31	0.67	51.40	31.92	16.97
Observations	4,680	1,542	1,009	533	3,138	1,810	1,328
Panel D: OLS Reduced Form Regression (Full controls)							
Treated	-0.0025	-0.0470***	-0.0520**	-0.0201	0.0149	0.00479	0.0395***
95% CI	[-0.019, 0.014]	[-0.083, -0.011]	[-0.100, -0.003]	[-0.086, 0.046]	[-0.004, 0.034]	[-0.022, 0.031]	[0.011, 0.067]
Observations	4,680	1,542	1,009	533	3,138	1,810	1,328

Notes: Statistical significance: ***p<.01, **p<.05, *p<.1.
Confidence intervals were calculated using robust standard errors. Control variables measured at the time of tax filing include respondent's gender, age, age squared, race/ethnicity, level of educational attainment, employment status, presence of dependents, the number of adults in a household, experience of unexpected income volatility, homeownership, vehicle ownership, household adjusted gross income, and refund size. State fixed effects, month of tax filing fixed effects, and their interaction are also included.

Table 3: Effects of Refund Savings on Joining the Gig Economy

	Full Sample	All Students	Students With Liquidity	Liquidity-Constrained Students	All Non-Students	Non-Students With Liquidity	Liquidity-Constrained Non-Students
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS regression (Full controls)							
Saved Full Refund	0.00683	0.0314*	0.0338	0.0155	-0.00986	-0.00987	-0.00340
95% CI	[-0.009, 0.023]	[-0.002, 0.065]	[-0.009, 0.076]	[-0.053, 0.084]	[-0.027, 0.007]	[-0.030, 0.011]	[-0.034, 0.027]
Observations	4,475	1,480	974	506	2,995	1,742	1,253
Panel B: 2SLS regression (No controls)							
Saved Full Refund	-0.0258	-0.242	-0.205	-0.451	0.0897	-0.0114	0.260*
95% CI	[-0.174, 0.123]	[-0.553, 0.068]	[-0.516, 0.106]	[-1.605, 0.704]	[-0.080, 0.259]	[-0.229, 0.206]	[-0.005 - 0.526]
Kleibergen-Paap rk Wald Statistic	69.68	20.85	20.61	1.85	49.28	28.33	24.20
Observations	4,475	1,480	974	506	2,995	1,742	1,253
Panel C: 2SLS regression (Full controls)							
Saved Full Refund	-0.0167	-0.292**	-0.232*	-0.470	0.134	0.0239	0.395***
95% CI	[-0.154, 0.120]	[-0.564, -0.021]	[-0.491, 0.028]	[-2.18, 1.24]	[-0.034, 0.301]	[-0.188, 0.236]	[0.099, 0.692]
Kleibergen-Paap rk Wald Statistic	80.33	24.15	22.58	0.51	48.02	27.60	20.18
Observations	4,475	1,480	974	506	2,995	1,742	1,253
Panel D: OLS Reduced Form Regression (Full controls)							
Treated	-0.00186	-0.0389**	-0.0404*	-0.0177	0.0133	0.00263	0.0371***
95% CI	[-0.018, 0.014]	[-0.074, -0.004]	[-0.088, 0.007]	[-0.081, 0.046]	[0.003, 0.030]	[0.021, 0.026]	[0.014, 0.0602]
Observations	4,475	1,480	974	506	2,995	1,742	1,253

Notes: Statistical significance: ****p*<.01, ***p*<.05, **p*<.1.
Confidence intervals were calculated using robust standard errors. Control variables measured at the time of tax filing include respondent's gender, age, age squared, race/ethnicity, level of educational attainment, employment status, presence of dependents, the number of adults in a household, experience of unexpected income volatility, homeownership, vehicle ownership, household adjusted gross income, and refund size. State fixed effects, month of tax filing fixed effects, and their interaction are also included.

Table 4: Effect of the R2S Intervention on Savings Account Ownership

	Full Sample	All Students	Students With Liquidity	Liquidity-Constrained Students	All Non-Students	Non-Students With Liquidity	Liquidity-Constrained Non-Students
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.0182	-0.0187	-0.0184	-0.0518	0.0305	0.0126	0.0471
95% CI	[-0.011, 0.047]	[-0.068, 0.031]	[-0.076, 0.040]	[-0.168, 0.064]	[-0.007, 0.068]	[-0.033, 0.059]	[-0.020, 0.114]
Observations	4,678	1,540	1,009	531	3,138	1,810	1,328
R-squared	0.114	0.171	0.213	0.374	0.145	0.157	0.213

Notes: Statistical significance: ****p*<.01, ***p*<.05, **p*<.1.
Confidence intervals were calculated using robust standard errors. Control variables measured at the time of tax filing include respondent's gender, age, age squared, race/ethnicity, level of educational attainment, employment status, presence of dependents, the number of adults in a household, experience of unexpected income volatility, homeownership, vehicle ownership, household adjusted gross income, and refund size. State fixed effects, month of tax filing fixed effects, and their interaction are also included.

Table 5: Effect of Refund Savings (Saved Any Refund) on Gig Economy Participation and Joining Gig Economy

	Full Sample	All Students	Students With Liquidity	Liquidity-Constrained Students	All Non-Students	Non-Students With Liquidity	Liquidity-Constrained Non-Students
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Gig economy participation: 2SLS regression (Full controls)							
Saved Any Refund	-0.0204	-0.335**	-0.286**	-0.477	0.132	0.0396	0.375**
95% CI	[-0.152, 0.112]	[-0.601, -0.068]	[-0.546, -0.026]	[-2.04, 1.09]	[-0.031, 0.295]	[-0.164, 0.244]	[0.086, 0.665]
Kleibergen-Paap rk Wald Statistic	99.28	28.42	25.93	0.71	61.38	34.14	25.06
Observations	4,680	1,542	1,009	533	3,138	1,810	1,328
Panel B: Joining gig economy: 2SLS regression (Full controls)							
Saved Any Refund	-0.0154	-0.280**	-0.225*	-0.431	0.118	0.0224	0.319***
95% CI	[-0.141, 0.110]	[-0.538, -0.022]	[-0.475, 0.026]	[-1.949, 1.087]	[-0.030, 0.266]	[-0.176, 0.221]	[0.093, 0.545]
Kleibergen-Paap rk Wald Statistic	91.37	25.80	23.84	0.60	57.61	29.96	29.20
Observations	4,475	1,480	974	506	2,995	1,742	1,253

Notes: Statistical significance: ****p*<.01, ***p*<.05, **p*<.1.
Confidence intervals were calculated using robust standard errors. Control variables measured at the time of tax filing include respondent's gender, age, age squared, race/ethnicity, level of educational attainment, employment status, presence of dependents, the number of adults in a household, experience of unexpected income volatility, homeownership, vehicle ownership, household adjusted gross income, and refund size. State fixed effects, month of tax filing fixed effects, and their interaction are also included.

Table 6: Effect of Refund Savings on Gig Economy Participation and Joining Gig Economy, by Liquid Assets

	Full Sample	All Students	Students (Below Median)	Students (Median and Above)	All Non-Students	Non-Students (Below Median)	Non-Students (Median and Above)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Gig economy participation: 2SLS regression (Full controls)							
Treated	-0.0164	-0.304**	-0.467*	-0.190	0.137	0.441**	-0.0120
95% CI	[-0.157, 0.124]	[-0.567, -0.040]	[-1.003, 0.070]	[-0.456, 0.077]	[-0.0442, 0.318]	[0.033, 0.848]	[-0.190, 0.166]
Kleibergen-Paap rk Wald Statistic	93.06	28.80	6.82	21.11	54.38	17.73	39.86
Observations	4,545	1,496	744	752	3,049	1,524	1,525
Panel B: Joining gig economy: 2SLS regression (Full controls)							
Treated	-0.0104	-0.240*	-0.240	-0.185	0.118	0.381**	-0.0240
95% CI	[-0.144, 0.123]	[-0.494, 0.013]	[-0.715, 0.234]	[-0.467, 0.098]	[-0.047, 0.283]	[0.034, 0.729]	[-0.200, 0.152]
Kleibergen-Paap rk Wald Statistic	86.23	26.18	6.88	17.95	51.38	18.51	33.37
Observations	4,345	1,436	718	718	2,909	1,453	1,456

Notes: Statistical significance: ***p<.01, **p<.05, *p<.1.
Confidence intervals were calculated using robust standard errors. Control variables measured at the time of tax filing include respondent's gender, age, age squared, race/ethnicity, level of educational attainment, employment status, presence of dependents, the number of adults in a household, experience of unexpected income volatility, homeownership, vehicle ownership, household adjusted gross income, and refund size. State fixed effects, month of tax filing fixed effects, and their interaction are also included.

Appendix

Savings-Focused Experiment: Treatment and Control Conditions

Figure 1A: Refund Receipt Screen (Control Group)

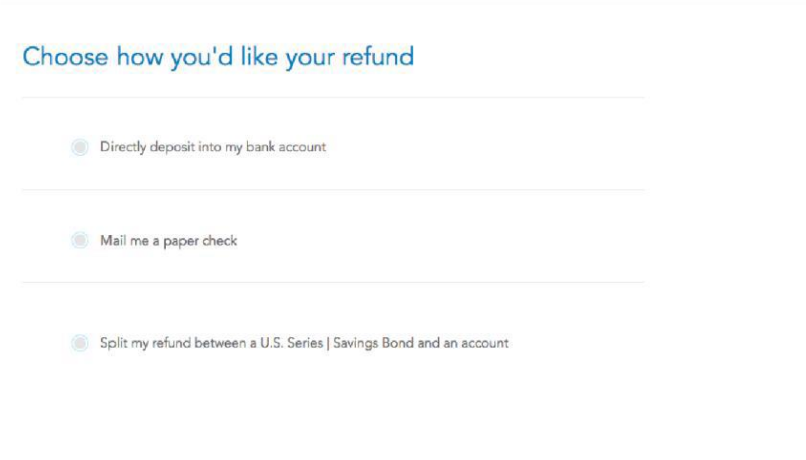

Figure 2A. Pre-Commitment Screen (Treatment Group)

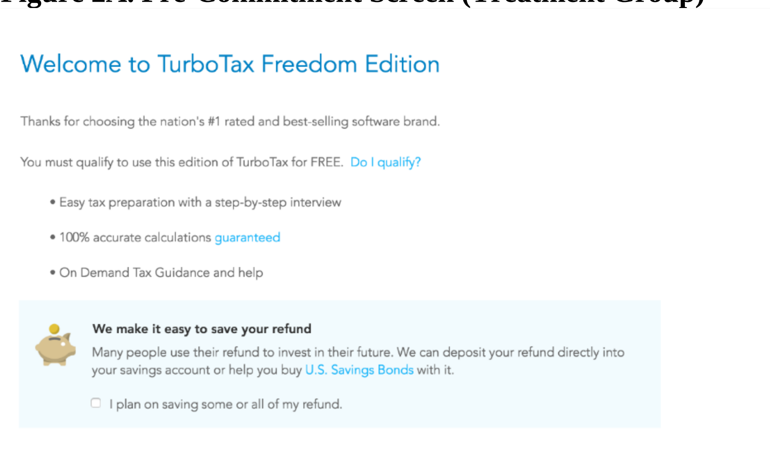

Figure 3A. Refund Receipt Screen for Pre-Committers (Treatment Group)

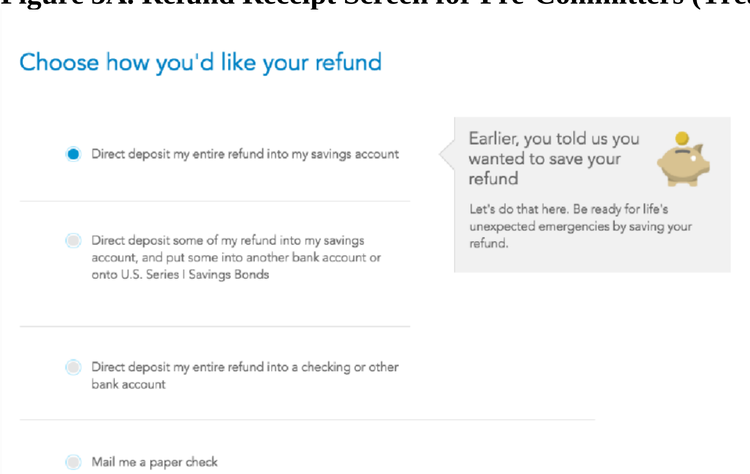

Table 1A: Baseline Sample Characteristics, Treatment and Control Groups

Characteristic	Mean/Proportion Full sample (1)	Treatment (2)	Control (3)	Sig.
Participated in gig economy (past 12 months) (%)	4.4	4.6	3.5	
Male (%)	41.7	41.8	41.3	
Age (years)	34.4	34.4	34.2	
White (%)	75	74.9	75.5	
Black (%)	5.6	5.7	5.3	
Hispanic/Latino (%)	7.7	7.7	7.7	
Asian (%)	6	6.2	5.5	
Other (%)	5.6	5.5	6.1	
College degree or greater (%)	58.9	58.7	59.8	
Enrolled in school at time of survey (%)	32.9	33.2	32.0	
Any dependents (%)	20	19.7	21.2	
Number of adults in the household	1.7	1.7	1.7	
Unexpected income volatility (%)	17.8	18.2	16.4	
Employed full-time (%)	46.7	46.4	47.9	
Employed part-time (%)	33.7	33.9	32.9	
Not employed (%)	19.7	19.8	19.2	
Adjusted gross income ($)	16,691	16,598	17,025	
Could come up with $2,000 in an emergency (%)	60.2	60.0	61.3	
Liquid assets (median, $)	1,560	1,463	4,694	
Liquid assets ($)	6,605	4,575	1,669	
Owns a car (%)	72.6	72.4	73.6	
Owns a home (%)	24.3	23.9	25.8	
Any income volatility (%)	33.8	34.0	33.2	
Received federal tax refund ($)	1,601	1,582	1,669	
Received Earned Income Tax Credit (%)	36.2	36.0	36.6	
Saved full federal tax refund (%)	18.6	20.8	10.7	***
Amount of federal refund saved ($)	287	319	175	***
Amount of federal refund saved among savers ($)	1,734	1,447	1,589	
N	4,680	3,657	1,023	

Notes: Statistical significance: ****p*<.01, ***p*<.05, **p*<.1.

Table 1B: Effect of the R2S Intervention on Saving Full Refund

	Full Sample (1)	All Students (2)	Students With $2,000 (3)	Students Without $2,000 (4)	All Non-Students (5)	Non-Students With $2,000 (6)	Non-Students Without $2,000 (7)
Panel A: Gig economy participation: First stage regression (No Controls)							
Treated	0.102***	0.114***	0.146***	0.0540	0.0946***	0.106***	0.0822***
	[0.079, 0.125]	[0.068, 0.160]	[0.0867, 0.205]	[-0.015, 0.123]	[0.067, 0.121]	[0.067, 0.143]	[0.048, 0.116]
Observations	4,680	1,542	1,009	533	3,138	1,810	1,328
R-squared	0.012	0.012	0.018	0.004	0.011	0.013	0.011
Panel B: Gig economy participation: First stage regression (Full Controls)							
Treated	0.113***	0.135***	0.175***	0.0406	0.100***	0.114***	0.0841***
	[0.089, 0.137]	[0.084, 0.186]	[0.105, 0.244]	[-0.057, 0.138]	[0.073, 0.127]	[0.075, 0.154]	[0.044, 0.124]
Observations	4,680	1,542	1,009	533	3,138	1,810	1,328
R-squared	0.089	0.152	0.217	0.360	0.110	0.150	0.174
Panel C: Joining gig economy: First stage regression (No Controls)							
Treated	0.101***	0.109***	0.140***	0.0495	0.0960***	0.104***	0.0876***
	[0.077, 0.125]	[0.062, 0.156]	[0.079, 0.200]	[-0.022, 0.121]	[0.069, 0.123]	[0.066, 0.142]	[0.053, 0.122]
Observations	4,475	1,480	974	506	2,995	1,742	1,253
R-squared	0.011	0.011	0.016	0.003	0.012	0.012	0.012
Panel D: Joining gig economy: First stage regression (Full Controls)							
Treated	0.111***	0.133***	0.175***	0.0378	0.0995***	0.110***	0.0938***
	[0.087, 0.136]	[0.080, 0.186]	[0.102, 0.247]	[-0.066, 0.142]	[0.071, 0.128]	[0.069, 0.151]	[0.053, 0.135]
Observations	4,475	1,480	974	506	2,995	1,742	1,253
R-squared	0.088	0.152	0.219	0.359	0.113	0.153	0.186

Notes: Statistical significance: ***$p<.01$, **$p<.05$, *$p<.1$.
Confidence Intervals were calculated using robust standard errors. Control variables measured at the time of tax filing include respondent's gender, age, age squared, race/ethnicity, level of educational attainment, employment status, presence of dependents, the number of adults in a household, experience of unexpected income volatility, homeownership, vehicle ownership, household adjusted gross income, and refund size. State fixed effects, month of tax filing fixed effects, and their interaction are also included.