LIQUIDITY AFFECTS JOB CHOICE: EVIDENCE FROM TEACH FOR AMERICA*

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Abstract

Can access to a few hundred dollars of liquidity affect the career choice of a recent college graduate? In a three-year field experiment with Teach For America (TFA), a prestigious teacher placement program, we randomly increase the financial packages offered to nearly 7,300 potential teachers who requested support for the transition into teaching. The first two years of the experiment reveal that while most applicants do not respond to a marginal $600 of grants or loans, those in the worst financial position respond by joining TFA at higher rates. We continue the experiment into the third year and self-replicate our results. For the highest need applicants, an extra $600 in loans, $600 in grants, and $1,200 in grants increase the likelihood of joining TFA by 12.2, 11.4, and 17.1 percentage points (or 20.0%, 18.7%, and 28.1%), respectively. Additional grant and loan dollars are equally effective, suggesting a liquidity mechanism. A follow-up survey bolsters the liquidity story and also shows that those pulled into teaching would have otherwise worked in private sector firms. *JEL* Codes: I21, J22, J45, J62, J68.

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1 Introduction

Taking a new job can come with large financial costs. While many private sector firms offer signing bonuses or travel reimbursement to help cover these costs, the typical public service job is unlikely to offer such benefits, leaving workers to finance their own transitions.[1] For example, an aspiring teacher who graduates college in May and starts teaching in September will spend a few months without a paycheck while potentially facing additional expenses associated with moving and getting ready to teach. A key feature of many of these transition costs is that they demand immediate liquidity at the time of transition.

To what extent does the need for liquidity affect whether individuals take public service jobs like teaching? If all workers had access to credit at a reasonable expense, concerns about liquidity would be mitigated, and those who wanted to become teachers (or work in other public service jobs) would be able to finance their transitions. Evidence suggests, however, that many Americans—even college graduates—are both illiquid and credit constrained.[2]

In this paper, we investigate the role liquidity plays in the choice to become a teacher by running a large, three-year field experiment with a highly selective non-profit teacher placement program, Teach For America (TFA). TFA draws many of its potential teachers from highly ranked colleges and universities. Given the caliber of those admitted to TFA, one might expect that they are not subject to liquidity constraints; consequently, finding these constraints are important for even a subset of those admitted suggests that such concerns may be more widely prevalent.

We run our experiment in the context of TFA's "transitional grants and loans" (TGL) program. The program invites prospective teachers to apply for funding to support their transitions into TFA by providing a battery of financial information that TFA uses to assess need. TFA then offers a personalized package of grants and no-interest loans to each applicant based on its estimate of what the applicant needs to make the transition into teaching. Applicants who accept the funds from TFA receive them in late May or June to cover costs faced during the summer before they begin.

[1] For example, the transition into teaching—the focus of this paper—is unlikely to be supported by such benefits. The most recent Schools and Staffing Survey (SASS) estimates that only 3.8% of school districts in the United States offer teachers signing bonuses and only 2.6% offer funding to help cover expenses related to relocation (Hansen, Quintero, and Feng 2018).

[2] According to the New York Federal Reserve's 2017 Survey of Consumer Expectations, 32% of American adults (and 18% of college graduates) believe there is less than a 50% chance that they could come up with $2,000 in the next month. See also Hayashi (1985); Zeldes (1989); Jappelli (1990); Gross and Souleles (2002); Johnson, Parker, and Souleles (2006); and Brown, Scholz, and Seshadri (2011).

Our experiment randomly varies the grant and loan packages offered to TGL program applicants. Applicants in our experiment either receive a control package or, in our main treatments, a package that randomly includes an additional \$600 in loans or \$600 in grants. Other treatments, added partway through the experiment, randomly offered some applicants an additional \$1,200 in grants or an additional \$1,800 in loans or grants. Across all treatments, "additional" funds were not tagged as special—TGL applicants randomized to our treatment groups were simply offered larger packages than they would have been offered if randomized to our control group.

We find that for the majority of TGL applicants, additional funding does not impact their decision to become a teacher for TFA. However, for the "highest need" applicants (the 10 percent predicted by TFA to be unable to provide any funding for their transitions), both the additional grants and the additional loans have large, statistically significant, positive effects on becoming a teacher for TFA.

The first two years of data revealed a heterogeneous treatment effect. While there are numerous methods to address the empirical validity of heterogeneous treatment effects, we had the opportunity to run our experiment for a third year, which gave us the chance to "self-replicate" our results. As discussed in Section 3, after the first two years of the experiment, we adjusted our experimental design to account for the heterogeneous treatment effects we had found, highlighting the role of the third year as a replication. This self-replication succeeded, generating results nearly identical to those from the first two years.

Across the three years of the experiment, we estimate that providing an extra \$600 in loans, \$600 in grants, and \$1,200 in grants increases the likelihood the highest need applicants become teachers for TFA by 12.2, 11.4, and 17.1 *percentage points*, respectively. These treatment effects represent 20.0%, 18.7%, and 28.1% increases in joining TFA on a base rate of 0.61 in the control group. These large treatment effects arise even though the highest need applicants are offered substantial grant and loan packages (averaging around \$5,000 per applicant) in the control group.

The pattern of our experimental results, institutional details of the TGL program, and results from a post-experiment survey all strongly suggest that our treatments work through the liquidity they provide to applicants. First, consistent with a liquidity mechanism, we find that additional grants and loans are equally effective at inducing the highest need applicants to join TFA (even though loans have to be repaid over the course of the TFA program and grants do not). Second, as described in Section 2, the formula TFA uses to determine TGL awards has a kink that systematically offers control awards below estimated liquidity need to the highest need applicants. Indeed, in a post-experiment survey of our experimental subjects—described in Section 3.1—a large majority of the highest need applicants receiving the TGL control award report needing additional liquidity. Third, the highest need applicants have difficulty accessing credit markets. While the vast majority of the these applicants reported applying for credit when needed, over a quarter of those who applied for credit said they were denied. The latter few points also help explain why applicants with less need do not respond to the treatments—they are not subject to the kink, they are less likely to report having unmet liquidity need, and they are denied credit less often.

The post-experiment survey also reveals that applicants induced to join TFA by our treatments would have otherwise ended up in private sector jobs. That additional funding generated more teachers overall suggests that liquidity need may be preventing workers from becoming teachers or otherwise entering public service. It also suggests that loans may be a particularly cost-effective policy lever to mitigate this barrier. We discuss the policy implications of our findings—and how they compare to existing programs to attract and retain teachers—in Section 7.

Along with having policy implications, our results make contributions to two related literatures. First, we add to the literature that investigates how liquidity constraints affect important life decisions, such as consumption choices (Agarwal, Liu, and Souleles 2007; Johnson, Parker, and Souleles 2006) and educational investments (Lochner and Monge-Naranjo 2012). The closest related work on how finances affect job choice considers unemployment insurance (UI), which necessarily focuses on older workers whose decisions are on the margins of both unemployment duration and job choice. This work finds evidence that liquidity can indeed affect unemployment duration (Chetty 2008). However, there is not a consensus on whether the liquidity provided by UI affects post-unemployment earnings or job match quality.[3] Our experiment involves relatively young workers and finds that giving them access to liquidity affects the type of jobs they take early in their careers. This margin may be particularly important, since evidence suggests that first jobs can have life-long consequences. For example, graduating in a recession not only affects short-run wages, but has modest long-run effects on careers and earnings—effects that may be due to the quality of job match (see Kahn 2010; Oreopoulos, von Wachter, and Heisz 2012; Altonji, Kahn, and Speer 2016; and Zhang and de Figueiredo 2018).

[3] Card, Chetty, and Weber (2007) finds that while UI benefits and severance pay affect the duration of unemployment, they do not affect the job eventually accepted; however, Herkenhoff, Phillips, and Cohen-Cole (2016) finds that unemployed individuals with more access to credit return to employment less quickly and, when they do, earn higher wages. See also Centeno and Novo (2006), Ours and Vodopivec (2008), and Addison and Blackburn (2000).

Second, we provide new evidence that speaks, albeit indirectly, to the open question of why student loan burden affects early career choices of college graduates. Existing literature shows that an increased loan burden leads fewer students to take lower-paying jobs in the public interest. Field (2009) provides evidence that debt aversion could be one factor behind these results. Rothstein and Rouse (2011) finds suggestive evidence that liquidity and credit constraints could be driving these patterns (see also Zhang 2013). Our results provide evidence that liquidity constraints are a first-order concern for some individuals. As discussed in Section 5, our results are also inconsistent with debt aversion and with a lack of awareness of credit market opportunities.

The rest of the paper proceeds as follows. Section 2 provides institutional details about Teach For America and the TGL program. Section 3 describes the experimental design and post-experiment survey. Section 4 presents results from the field experiment. Section 5 discusses evidence on the mechanism driving these results. Section 6 explores the counterfactual jobs of the teachers induced to join TFA by our treatments. Section 7 concludes. Additional material can be found in the Online Appendix; its URL is listed at the end of this article.

2 Setting

2.1 Teach For America (TFA)

Teach For America is a non-profit organization that places roughly 4,000 to 6,000 teachers per year at schools in low-income communities throughout the United States. Prospective TFA teachers apply and are admitted between September and April of a given academic year to begin teaching at the start of the following academic year. Before beginning teaching, accepted applicants must attend a roughly six-week, intensive teacher training program (called "Summer Institute"), usually held in June and July in a city near the school district where they have been assigned. The school year begins around the start of September, and TFA teachers are meant to remain in the program for two school years. TFA administrators estimate that 55 to 60 percent of those who join TFA continue teaching in K–12 schools beyond their initial two-year commitment. TFA recruits its teachers from highly ranked colleges and universities across the United States, and admission to TFA is very selective. During the three years of our experiment, roughly 40,000 to 50,000 people applied to TFA in each year and acceptance rates varied between 12% and 15%.

2.2 Transitional Grants and Loans (TGL) Program

To help cover the costs of the transition into teaching, TFA offers a Transitional Grants and Loans program to which prospective TFA teachers can apply.[4] Those who want TGL funding must complete an extensive application, which requires providing portions of federal tax returns and the Free Application for Federal Student Aid (FAFSA); pay stubs; information about any dependents; and documentation of checking accounts, savings accounts, and debts.

The timing of TGL program application is related to the timing of TFA admission, which occurs in four waves during the academic year before applicants would begin teaching. The first wave of applicants can apply as early as August and be admitted as early as October, while the last wave must apply by February and can be admitted in April. Applications to the TGL program are submitted on a rolling basis during the admission season, with final deadlines associated with—but later than—the admissions deadline for the wave in which an individual applied to TFA. Applicants can submit a TGL application as early as their first invitation to an interview with TFA; however, since preparing an application is time intensive, most applicants only do so after they have accepted their TFA offers.[5]

The TGL program aims to provide offers soon after applications are submitted—they are calculated and sent to applicants in approximately weekly batches. Regardless of when applications are submitted and offers are communicated to applicants, however, almost all funds allocated through the TGL program are disbursed in late May and June of the summer before applicants begin teaching (a small amount of funding is dispersed as early as March for applicants who face transition costs that arise in the spring). The funds are ostensibly for the expenses associated with transitioning into teaching—the TFA website states: "Packages are designed to assist with some transitional costs, including travel, moving, testing, and certification fees"—although an applicant's use of the funds is not restricted (Teach For America 2019).

The package of grants and loans the TGL program offers to each applicant depends on two key variables. The first is the applicant's "expected expense," which is a function of the cost of living where she has been assigned to teach, the location of the Summer Institute she has been assigned to attend, and whether she must move to a new city. The second is the applicant's "expected contribution" (EC), which is a function

[4] According to TFA leadership, the goal of the TGL program is to help attract a "broad and diverse coalition of people" particularly "those who may represent the low income background of the students and communities" where TFA teachers work.

[5] In the years of our experiment, only 9% of TGL applicants declined TFA upon initial admission to the program.

of her cash-on-hand (i.e., money in checking and savings accounts); her credit card and other debts (excluding federal student loans, which AmeriCorps funding places in forbearance during TFA); her income (if working); the amount of financial support she received from parents for educational expenses; her number of dependents; and whether she is about to graduate college or is changing careers. Note that EC can be negative. While we are not permitted to share the specific function that is used to calculate EC, Online Appendix Table A.5 reports how much variation in EC each component listed above can explain. Cash-on-hand is by far the most important factor.

For almost all applicants, the sum of grants and loans that the TGL program offers—called the "award"—is equal to the applicant's expected expense minus her expected contribution. The only exception to this rule occurs when the award would exceed expected expense, in which case the award is capped at the expected expense. This introduces a kink in the award schedule that, *ceteris paribus*, gives an applicant with EC < \$0 the same award as an applicant with EC = \$0. Assuming TFA's estimates of expected expense and expected contribution are reasonable proxies for what they are meant to measure, an applicant with EC < \$0 is more likely than an applicant with EC ≥ \$0 to have insufficient funding to transition into teaching after receiving a TGL award.[6] Almost exactly 10% of our sample have EC < \$0, which gives us a reason to believe applicants in this 1st decile of EC will have more unmet liquidity need than others in the experiment (see Figure A.3 in the Online Appendix for the full histogram of EC). We pay special attention to this decile in our analysis.

Each TGL award is offered as a specific combination of grants and loans. Grants do not need to be repaid if an applicant is teaching on October 1st of the year they join TFA; otherwise, they must be repaid in full. Loans are offered at a 0% interest rate and are expected to be repaid in 18 equal monthly payments starting six months after an applicant begins teaching for TFA. Applicants who fail to make on-time payments are put on adjusted, personalized repayment plans. How the award is split between grants and loans is determined by financial need and the constraint that the loan amount stay below a limit set by TFA. During the three years of our experiment, TFA offered its TGL applicants an average of \$5.5 million a year in grants and \$6.2 million a year in loans.

[6]Consistent with this explanation, if we consider applicants in the control group in the first two years of the experiment, only 61.5 percent of those in 1st decile of EC join TFA, which is substantially lower than the 74.3 percent who join in the pooled 2nd–10th deciles ($p = 0.002$). Figure A.2 in the Online Appendix shows the percentage of applicants in the control group who join TFA in the first two years of the experiment, broken down by decile.

3 Experimental Design

Our experiment was embedded into the TGL program for three years. It includes 7,295 individuals who applied to the TGL program in anticipation of beginning teaching in the fall of 2015, 2016, or 2017.[7] For the years of our experiment, we used TFA's algorithm to construct a control award for each applicant.[8] This control award is what would be offered to an applicant if she were randomized into our control group.

Figure 1 summarizes control awards by showing the distribution of grants, loans, and total awards across the three years of our experiment. These control awards are often quite substantial: the means of grants and loans are each roughly $2,000. Everyone in the experiment has at least $500 in loans in their control award, and the total control award can be in excess of $8,000.

FIGURE 1 ABOUT HERE

As described in detail in Section 4, we analyze the applicants in our experiment separately by decile of expected contribution. Figure 2 shows the distribution of control awards by decile of EC. Applicants with lower EC have substantially larger control awards—and grant money comprises a larger proportion of their awards—than those with higher EC. For example, applicants in the 1st decile of EC (i.e., those with the lowest EC and hence the highest estimated need) have control awards of almost $5,000 on average, while applicants in the 10th decile of EC have control awards of roughly $2,000 on average.

FIGURE 2 ABOUT HERE

The experiment began as a three-arm study in which we randomized TGL applicants into a control group or one of two treatment groups, each with one-third probability. Those in the control group were each offered their control award. Applicants in the two treatment groups were each offered an award that was $600 more

[7] Roughly 6,000–7,000 applicants were admitted to TFA in each of the years of our experiment, of whom approximately 40% apply to the TGL program. The experimental sample includes all TGL applicants across the three years who were offered an award, except for the 15 percent whose expected contribution was greater than 80% of expected expense. TFA deemed these applicants to be ineligible to receive grants, so we excluded them from our experiment (see Section A.1 of the Online Appendix, in which we discuss a mini-experiment run with these applicants during the first two years of our study). In addition, 2% of applicants are deferrals who reapply for TGL funding in a subsequent year of our experiment. We only include an applicant in our experiment the first time she applies for TGL funding during the years of our experiment.

[8] In the years of the experiment, control awards were calculated in the manner described in Section 2.2 and additionally lowered by a small amount—the same for all applicants in our experiment—to maintain budget balance with the introduction of our experimental treatments.

than the control award. In the *$600 Grant* treatment, this additional $600 came in the form of grants, while in the *$600 Loan* treatment, it came in the form of loans. Applicants in the treatment groups did not know that they had been offered more than they would have been offered if they had been randomized to the control group. That is, nothing about the experimental increase was highlighted; applicants were simply offered a larger financial package.

In March of the second year of our experiment—after roughly half of the applicants from the second year of the study had received offers—TFA increased the TGL program's budget. As a result, we added an additional treatment group, the *$1200 Grant* treatment, in which applicants were offered an award that was $1,200 larger than the control award, with this additional funding coming in the form of grants. Starting when the *$1200 Grant* treatment was introduced, we randomized TGL applicants to the control group or one of the three treatment groups, each with one-quarter probability.

As described in detail in Section 4, the first two years of the experiment revealed heterogeneous treatment effects based on the need of the applicant: the treatments only influenced the decision to become a TFA teacher for applicants in the 1st decile of expected contribution. To mitigate concerns that typically accompany the reporting of heterogeneous treatment effects, after analyzing the data from the first two years, we ran a modified version of the experiment for a third year to self-replicate our positive treatment effects and to stress test our null results.[9]

The design of the third year of the experiment makes clear its purpose as a replication and stress test. In particular, the third year of the experiment varied interventions by decile of expected contribution. To replicate the positive treatment effects for only the highest need applicants, we left the treatments unchanged for those in the 1st and 2nd deciles of EC. While our results from the first two years only appeared in the 1st decile of EC, we chose to continue the experiment with both the 1st and 2nd deciles to test whether the pattern of treatment effects across those deciles would also replicate. To stress test the null results found for the rest of the experimental population, we dramatically increased the experimental variation for the other deciles of EC. In particular, applicants in the 3rd–10th deciles of EC were randomly assigned to a control group or to one of two treatments that added $1,800 to the control award—an *$1800 Grant* treatment or an *$1800 Loan* treatment—each

[9]Self-replication, when feasible, is a useful companion to other methods for dealing with heterogeneous treatment effects, such as committing to a pre-analysis plan ex ante or correcting for multiple hypothesis testing ex post (see, e.g., Kling, Liebman, and Katz 2007).

with one-third probability. This variation was quite large, even relative to the control packages offered: the $1,800 treatments increased the average award offer by 59%.[10]

Table 1 shows how applicants were distributed across treatments during the three years of the experiment.

TABLE 1 ABOUT HERE

Since TGL applications arrived on a rolling basis, and because we did not know in advance who would apply to the TGL program, applicants were randomized only when they were included in a TGL awards processing batch. Since the point of randomization is the batch, all analysis conducted in Section 4 includes batch fixed effects. These fixed effects also control for any potential differences in the applicant pool that might arise either across years or within years of the experiment.

It is worth noting that while we can randomize the amount of award offered, we cannot control whether an applicant accepts the grant or loan funding offered.[11] However, the award offer is the relevant variable both for exploring the role of liquidity and for making policy prescriptions. The offer itself provides liquidity—how much funding applicants accept from TFA simply reflects their preference for funding from TFA relative to funding from other sources—and the offer of funding is what a policy maker can control.

3.1 Post-Experiment Survey

After the experiment, we attempted to survey all applicants in the experiment (both those who joined TFA and those who did not) concerning their access to credit and their employment. We were able to link survey responses to TGL data at the individual level. We asked about credit to investigate its role in our treatment effect. We asked about employment to establish whether our intervention produced new teachers or merely convinced those who would have taught independently to teach with TFA instead.

In May 2018, TFA emailed the survey invitation to all 7,295 applicants from the three years of our experiment. The survey was framed as providing data to Wharton

[10] Since we did not know in advance the distribution of EC in the experiment's third year, we used the empirical cutoff between the 2nd and 3rd decile of EC in the first two years of the experiment (i.e., EC = $220), to sort applicants into the two versions of the experiment in the third year.

[11] Most applicants who join TFA accept the entire award offered. Ninety-eight percent of applicants accept the entire grant offered and over 80% of applicants accept the entire loan offered. Those who choose not to accept the entire grant or loan almost always accept none of it (only 0.5% take a partial grant and only 3.2% take a partial loan).

researchers about the TGL program, so that even those who did not join TFA would feel comfortable responding. We offered completion incentives to all applicants, but offered substantially larger incentives to applicants in the 1st decile of EC, since we had a particular interest in that group. We also introduced some random variation in incentives to help assess selection bias. Further details can be found in Section A.2 of the Online Appendix. In total, 38.5% of the applicants in our experiment took the survey. Because we provided stronger incentives to participate for those in the 1st decile of expected contribution, this includes 52.5% of those in the 1st decile of EC and 36.8% of those in the 2nd–10th deciles. Response rates were 32% and 40.6% for those who did and did not ultimately join TFA (respectively) and 38.4% and 38.5% for those who were and were not in the control group (respectively).

3.2 Hypotheses

Before we present results, it is useful to discuss potential hypotheses and what they would predict in our data. Our initial three-arm experiment is designed to test the effect of offering applicants an additional \$600 in liquidity—provided by both the grant and loan treatments—and of offering \$600 in higher effective earnings—provided by the grant treatment only.

Table 2 lays out the theoretical possibilities for our experiment. For instance, if we believe that the amounts in our treatments are too small to work through the earnings channel (since even \$1,800 is small relative to the lifetime earnings of a teacher—see footnote 17), then we expect our results to match the left column of the table. If we further believe that a TGL applicant has liquidity constraints, then we expect our results to match the lower-left cell: grants and loans work equally well to relieve such constraints. If we instead think an applicant has full access to a credit market, our results should match the upper-left cell, since neither channel should be active.

In the next section, we explore which cell of Table 2 best describes our data. Ultimately, we will find evidence for both of the scenarios discussed in the previous paragraph for different subsets of TGL applicants.

TABLE 2 ABOUT HERE

4 Results

4.1 Summary Statistics and Balance

Table 3 reports on our sample of applicants, overall and in relevant deciles of expected contribution. Our sample is mostly female and non-white. Consistent with our sample needing funding to make their transition into TFA, applicants have on average more credit card debt than funds in their checking and savings accounts. Interestingly, applicants in the 1st decile of expected contribution have more in checking and savings than the 2nd decile; however, the 1st decile also has significantly more credit card and private loan debt. Randomization was successful overall and in relevant deciles of expected contribution. Online Appendix Table A.6 reports p-values of balance tests on our demographic characteristics; there are no more significant differences than one would expect by chance.

TABLE 3 ABOUT HERE

4.2 Joining Teach For America: Initial Results (2015–2016)

In this section, we investigate how additional funding offered in TGL packages affects whether applicants become teachers for TFA. Our outcome measure is whether an applicant is teaching for TFA on the first day of the school year for which they applied for TGL funding. We call this outcome "joining TFA."

As described in Section 3, we ran the first two years of the experiment, fully analyzed our results, and then designed an additional year of the experiment—with a modified design—as a self-replication and stress test. Consequently, we present initial results from the first two years of the experiment here in Section 4.2, results from the third year in Sections 4.3 and 4.4, and then pooled results in Section 4.5.

How did the treatments affect the likelihood that applicants began teaching for TFA? To answer this question, we consider regression specifications 1a and 1b:

$$Join\,TFA_i = \textstyle\sum_T \beta_T \cdot Treatment_i^T + \sum_j \gamma^j \cdot Batch_i^j + \boldsymbol{\delta} \cdot \mathbf{X}_i + \varepsilon_i, \tag{1a}$$

$$\begin{aligned} Join\,TFA_i = {} & \textstyle\sum_{d=1}^{10} \sum_T \beta_T^d \cdot Treatment_i^T \cdot Decile_i^d + \sum_{d=1}^{9} \varphi^d \cdot Decile_i^d + \\ & \textstyle\sum_j \gamma^j \cdot Batch_i^j + \boldsymbol{\delta} \cdot \mathbf{X}_i + \varepsilon_i. \end{aligned} \tag{1b}$$

In these specifications (as well as those that follow), $Join\,TFA_i$ is a dummy for whether applicant i is teaching for TFA on the first day of school, and $Treatment_i^T$ is a dummy for whether applicant i was randomized into treatment T. The summation over T is taken for the relevant set of treatments (for instance, it does not cover the *$1800 Grant* treatment if we are only considering data from 2015–2016). Each $Batch^j$ denotes a batch of applicants in the TGL program, which is the level at which randomization into treatment occurred; $Batch_i^j$ is a dummy for applicant i being in $Batch^j$. Similarly, $Decile_i^d$ is a dummy for applicant i being in $Decile^d$. In some specifications, we include a vector of demographic controls, $\mathbf{X}_i$.[12]

Figure 3 shows the treatment effects (as measured with specifications 1a and 1b) on joining TFA, first across all applicants and then by decile of expected contribution.[13] The two bars on the left show the overall effect of the treatments on joining TFA. While both treatment effects are directionally positive, neither is statistically significant: the effect of an additional $600 in loans is 1.61 percentage points ($p = 0.293$) and the effect of an additional $600 in grants is 0.66 percentage points ($p = 0.669$). The next 10 pairs of bars show the impact of the grant and loan treatments on applicants in each decile of expected contribution. Looking across the deciles, only one—the 1st decile—shows significant treatment effects. Both the loan and grant treatment effects are statistically significantly positive. The effect of the *$600 Loan* treatment is 12.1 percentage points ($p = 0.020$), and the effect of the *$600 Grant* treatment is 9.7 percentage points ($p = 0.062$). The difference between these treatments is not statistically significant ($p = 0.614$). The two treatments fail to have a significant effect in any of the other deciles.

FIGURE 3 ABOUT HERE

To more precisely estimate the effect of marginal grant and loan dollars, we combine variation across treatments using regression specifications 2a and 2b, whose estimates

[12] This vector includes all variables about applicants provided to us by TFA, excluding variables that determine expected contribution or are otherwise related to applicants' finances. In particular, the controls include a linear age term, dummies for race, gender, assigned region, whether the applicant was assigned to his or her most preferred region, whether the applicant was assigned to his or her most preferred subject, and a linear term for the applicant's "fit" with TFA. This last measure is a composite of scores from the application, phone interviews, and in person interviews about how well an applicant aligns with TFA's organizational objectives. The latter three measures are known to predict likelihood of joining TFA (see discussion in Coffman, Featherstone, and Kessler 2017). Following Cohen and Cohen (1975), we also include a missing data dummy for each demographic variable that is sometimes missing (age is missing in 103 observations, race in 10, and fit in 2).

[13] Recall that the *$1200 Grant* treatment was only run in the second half of the second year of the experiment. Given the small sample and associated imprecision, for visual simplicity we do not show the *$1200 Grant* treatment effects in Figure 3, although the treatment is included in all regression results. See Table A.7 in the Online Appendix for a full report of the regression underlying Figure 3

are reported in Table 4.

$$Join\,TFA_i = \beta_G \cdot Extra\,Grants_i + \beta_L \cdot Extra\,Loans_i + \sum_j \gamma^j \cdot Batch_i^j + \boldsymbol{\delta} \cdot \mathbf{X}_i + \varepsilon_i, \tag{2a}$$

$$\begin{aligned} Join\,TFA_i = {} & \sum_{d=1}^{10} \beta_G^d \cdot Extra\,Grants_i \cdot Decile_i^d + \sum_{d=1}^{10} \beta_L^d \cdot Extra\,Loans_i \cdot Decile_i^d + \\ & \sum_{d=1}^{9} \varphi^d \cdot Decile_i^d + \sum_j \gamma^j \cdot Batch_i^j + \boldsymbol{\delta} \cdot \mathbf{X}_i + \varepsilon_i. \end{aligned} \tag{2b}$$

In these specifications, $Extra\,Grants_i$ is the randomly assigned amount of additional grant funding offered to the applicant, in hundreds of dollars (i.e., $Extra\,Grants_i$ is either 0, 6, 12, or in the third year of the experiment, 18), and $Extra\,Loans_i$ is the randomly assigned additional loan amount offered to the applicant in hundreds of dollars (i.e., $Extra\,Loans_i$ is either 0, 6, or in the third year of the experiment, 18). In regression specification 2a, the coefficients of interest are β_G and β_L. In regression specification 2b, the coefficients of interest are those same coefficients for each decile d, β_G^d and β_L^d. These coefficients represent the estimated treatment effect of offering an additional \$100 in grants or an additional \$100 in loans. The coefficients are estimated under two parallel linearity assumptions: each additional \$100 of grants is equally effective, and each additional \$100 of loans is equally effective. While they are unlikely to strictly hold, these assumptions allow us to combine variation across treatments (e.g., we can include variation from the *\$1200 Grant* treatment that is imprecisely estimated on its own when examining the first two years of data).

TABLE 4 ABOUT HERE

The first four columns of Table 4 look at the effect of grants and loans over the first two years of the experiment. Column 1 shows results from specification 2a and column 3 shows results from specification 2b, reporting coefficients for the 1st decile and suppressing the rest (full regression results are shown in Online Appendix Table A.8). Columns 2 and 4 report the results of these regression specifications when the demographic controls (i.e., $\mathbf{X}_i$) are included.

Pooling across deciles in columns 1 and 2, we see that neither additional grants nor additional loans affect whether applicants join TFA. However, as shown in column 3, applicants in the 1st decile of EC are estimated to be 1.35 percentage points more likely to join TFA for every \$100 in additional grants offered ($p = 0.022$) and 1.93 percentage points more likely to join TFA for every \$100 in additional loans offered ($p = 0.020$). Column 4 includes demographic controls and finds that the estimates for both grants and loans are directionally larger and have stronger p-values ($p = 0.003$ and $p = 0.010$, respectively). The bottom two rows of Table 4 show that no other decile of expected contribution has a significant treatment effect in 2015–2016 for either grants or loans, regardless of whether demographic controls are included.[14]

4.3 Joining Teach For America: Replication (2017)

In the third year of the experiment, we kept the treatments the same for the 1st and 2nd deciles to see if we could replicate the results from the first two years. Among applicants in the 1st and 2nd deciles of EC, Figure 4 compares the estimated treatment effects (including the *$1200 Grants* treatment) from the first two years of the experiment (2015–2016) to those from the third (2017). Results are strikingly similar across years of the experiment. The effect of additional funding is again concentrated in the 1st decile of expected contribution, and loans and grants are similarly effective at increasing the likelihood that applicants join TFA. In the third year of the experiment, the estimated treatment effects for the 1st decile are 9.8 percentage points for the *$600 Loan* treatment ($p = 0.277$), 14.8 percentage points for the *$600 Grant* treatment ($p = 0.065$), and 21.9 percentage points for the *$1200 Grant* treatment ($p = 0.004$). The latter point estimate, though larger than the point estimate for the two $600 treatments, is not statistically distinguishable from either. The pattern and sizes of the treatment effects in the 2nd decile also look identical between the first two years and the third.

FIGURE 4 ABOUT HERE

4.4 Joining Teach For America: Stress Test (2017)

In the third year of the experiment, we increased the experimental variation for the 3rd–10th deciles of expected contribution as a stress test of our null results in the first two years. Applicants in these deciles were randomly assigned to the control group, an *$1800 Loan* treatment, or an *$1800 Grant* treatment. Figure A.4 in the Online Appendix shows the results by treatment and decile of EC. Looking across the deciles, we see no systematic pattern. This analysis suggests that our null results in these deciles from the first two years of the experiment were not a result of insufficient experimental variation: providing dramatically larger grant and loan increases to applicants in these deciles does not increase the likelihood that they join TFA.

[14] Additional unreported regressions that pool the 2nd–10th deciles, reveal that the treatment effects for grants and for loans among applicants in the 1st decile are each statistically significantly larger than the corresponding (null) effects observed for grants and loans in the 2nd–10th deciles, both with and without controls ($p < 0.05$ for all tests).

4.5 Joining Teach For America: Pooled Results

Given the similar pattern of treatment effects across the three years of the experiment, we now pool the data to get the most precise estimates possible. Figure 5 shows the results from all years of the study graphically. It reports treatment effects for the 1st decile and for the 2nd–10th deciles pooled (estimated with a variant of specification 1b in which there is one unified dummy for deciles 2–10 instead of one dummy for each of those deciles). Among applicants in the 1st decile, over all three years of the study, the *$600 Loan*, *$600 Grant*, and *$1200 Grant* treatments increase the percentage of applicants joining TFA by 12.2, 11.4, and 17.1 percentage points, respectively ($p < 0.01$ for all tests). These treatment effects represent 20.0%, 18.7%, and 28.1% effects on a base rate of joining TFA in the control group of 0.61 across the three years of the experiment. Meanwhile, the results for the 2nd–10th deciles are relatively precisely estimated zeros for all treatments.

Columns 9 through 12 of Table 4 present regressions estimated using the specifications in 2a and 2b, reporting coefficients for the 1st decile and suppressing the rest. Columns 9 and 10 show that, averaging across all years of the experiment and across all applicants, neither additional grants nor additional loans increase the likelihood that applicants join TFA. Columns 11 and 12, however, show that if we interact additional grants and loans with decile of expected contribution, both grants and loans have large, statistically significant effects in the 1st decile. The most precise estimates (from column 12, which includes demographic controls) suggest that applicants in the 1st decile of EC are 1.8 percentage points more likely to join TFA for every $100 of additional grants and 2.1 percentage points more likely to join TFA for every $100 of additional loans provided to them by the experiment.[15] These estimates are not statistically different; in fact, with 95% confidence, we can rule out that the effect of grants (per $100) is more than 0.67 percentage points larger than the effect of loans.

FIGURE 5 ABOUT HERE

[15]As in the first two years, additional results (regression unreported) reveal that the treatment effects for grants and for loans among applicants in the 1st decile are each statistically significantly larger than the (null) effects observed in the 2nd–10th deciles, both with and without controls ($p < 0.01$ for all tests). In addition, we can rule out with 95% confidence that the effects of grants and loans for the 2nd–10th deciles are greater than 0.22 and 0.14 percentage points per $100, respectively.

4.6 Randomization Inference and Multiple Hypothesis Correction

For the estimates reported at the end of the previous section, the p-values based on standard parametric asymptotics (i.e., robust standard errors) are 0.000016 and 0.0035 for grants and loans in the 1st decile, respectively. To get a non-parametric joint p-value for these two estimates, we can use randomization inference (see Athey and Imbens 2017 and Young 2019, among others). This approach uses a "sharp null", which in our context would be: *none of our treatments affect the likelihood of any TGL applicant joining TFA*. This null assumes the results presented above are a result of chance, not treatment. How likely is this? Randomization inference answers in a non-parametric way by asking: "If the meaningless treatment markers are randomly permuted, how often do we get a false positive?" For our setting, a natural definition of false positive is for the same regression specification to yield p-values on the effect of grants and loans in the 1st decile such that the smaller p-value is weakly less than 0.000016 and the larger is weakly less than 0.0035. When we drew 100,000 random permutations of the treatment markers, only 1 produced a false positive for the 2015–2017 sample by this definition. Hence, the joint p-value for our main result is 0.00001.

Of course, the test we just reported does nothing to address multiple hypothesis testing, which should be a major concern given that the treatments only have an effect in a subpopulation. Our self-replication is one way to address this concern. A complementary approach (inspired by Chetty, Hendren, and Katz 2016) is to show that our main result stands up to a randomization inference test that takes multiple hypothesis testing concerns into account. As will become clear, the test we use is exceedingly conservative.

Mathematically, one randomization inference test is more conservative than another if a false positive in the former is also a false positive in the latter. So, to make an exceedingly conservative test, we must come up with an exceedingly permissive definition of false positive. In particular, there are three dimensions on which one might be worried about multiple hypothesis testing: (1) which of our two treatments are significant, (2) which direction they go in, and (3) where we find them. We construct an exceedingly permissive definition by allowing the test to trigger a false positive: if only one treatment (i.e., grants or loans) has an effect, rather than both as in our experimental results; if the effect is either positive or negative, rather than both treatments being positive as in our results; and by allowing it to fall in any subgroup on the EC spectrum (i.e., we search for the treatment in the whole population, above and below the median, in each tercile, quartile, and so on, all the way up to searching in each decile, for 55 total tests). We consider the permutation a false positive if in any of these tests we get a weakly lower p-value than 0.000016 (the stronger of the two p-values from our main result). Note that almost all false positives by this criterion would be exceptionally difficult to write a paper about (e.g, all interactions being insignificant except for a negative effect of grants in the 3rd septile of EC). This inclusiveness is exactly what makes our test so conservative.[16] Of the 100,000 treatment permutations we randomly considered, only 293 could clear the bar just described. Hence, an *exceedingly conservative* p-value for our main result is 0.0029.

5 Liquidity Mechanism

Results from Section 4 point to a liquidity channel for the highest need applicants in the 1st decile of EC. Marginal grants and loans have a large, significant effect on whether applicants in the first decile of EC join TFA. What's more, these effects are not statistically different. Looking back to Table 2, this is exactly what one should expect if applicants in the first decile have binding liquidity constraints, but are not otherwise affected by a marginal bit of compensation.[17] We also showed that for applicants in the other nine deciles, the effects of both grants and loans are statistically indistinguishable from zero. Again looking to Table 2, this is what one should expect from applicants *without* binding liquidity constraints who are not affected by a marginal bit of compensation.

In addition to our main results, the kink in TFA's awards formula (first mentioned in Section 2.2) also suggests that those in the first decile of EC are more likely to have binding liquidity constraints. Recall that, *ceteris paribus*, an applicant to the TGL program whose EC is negative gets exactly the same award as an applicant whose EC is zero. Assuming TFA's estimates of expected expense and expected contribution are reasonable proxies for what they are meant to measure, this means that applicants with EC < \$0 are more likely to find the TGL control award insufficient to fund the transition into teaching. This group turns out to be almost identical to the group

[16] Another, less conservative test, might only consider a result to be a false positive if it were "publishable" in some sense (i.e. only results with an explanation count). While we originally constructed such a test, ran it, and got a strikingly low p-value, this approach was problematic as the definition of "publishable" was too open to interpretation. Instead, we report the most conservative test we can construct with the hope that any test a reader might consider would be less conservative and thus have a lower p-value (where *less conservative* is meant in the technical sense described at the beginning of the paragraph in the main text).

[17] Even an \$1,800 grant (which is 4.2% of the average salary reported by those teachers who responded to the survey described in Section 3.1) is small relative to the lifetime earnings of a career in teaching.

of applicants whose EC is in the 1st decile: 10.4% of admits across all years have a negative EC.

Although compelling, this evidence for a liquidity mechanism is circumstantial. Fortunately, our post-experiment survey (described in Section 3.1) directly assesses the liquidity constraints faced by applicants. Specifically, it asked all of them (both those who joined TFA and those who did not) if they needed funds (beyond their TGL award) to make the transition to teaching. If a respondent answered *yes*, then the survey asked whether she tried to make up the difference by applying for a credit card (or an increase in the limit of a credit card), applying for a loan (or an increase in the limit of an existing loan), or seeking an informal loan or gift from friends or family. For each of these credit request types, the survey also asked whether her request was successful or why she chose not to make it. Responses are reported in Table 5.

TABLE 5 ABOUT HERE

We begin by looking at the fraction of respondents in the control group who said that they needed extra funds. In the 1st decile and the 2nd–10th deciles, these fractions are 60.8% and 56.1%, respectively, a difference that is consistent with a higher prevalence of binding liquidity constraints in the first decile. What's more, extra TGL funding mitigates this difference: respondents in the 1st decile of EC are 1.26 percentage points less likely to report needing funds for every \$100 of grant or loan given to them by experimental treatment ($p = 0.055$, regression unreported).

The follow-up questions about credit access allow us to present an even more nuanced picture. In both the 1st decile and 2nd–10th decile control groups, among those who stated that they needed more funds, the overwhelming majority report applying for some form of credit (88.0% and 88.3%, respectively). The similarity and magnitude of these two numbers suggest that the difference in access to credit is not driven by lack of awareness of credit markets or debt aversion.

This similarity disappears, however, when we examine the degree to which the two groups are able to actually access credit. Among applicants in the 1st decile control group who needed funds, 24.0% were denied in at least one of their attempts to access credit, and this number jumps to 40.0% if we include discouragement (i.e., applicants who fail to apply for credit due to a belief that they will be rejected). Compared to the corresponding figures for the 2nd–10th deciles (14.0% denied, 26.6% including discouragement), we see that the 1st decile simply has less access to the credit market.[18]

[18]Online Appendix Table A.9 shows why respondents did not apply for particular sources of credit,

Taken together, our survey results provide direct evidence that the 1st decile and the 2nd–10th deciles are different in the degree to which their liquidity constraints bind. This difference supports the story that a liquidity mechanism is driving our main results.

6 Occupations Outside of TFA

The results presented in Section 4 show that our treatments induced applicants in the 1st decile to join TFA. Where do those teachers come from? Do we generate more teachers overall or just more teachers for TFA?

To answer these questions, we report on responses to questions that we asked in our post-experiment survey (described in Section 3.1). In particular, we asked all respondents (both those who joined TFA and those who did not) their occupation in the fall after they applied to the TGL program—which is working as a teacher for TFA for those who joined TFA—and their occupation (actual or expected) two years later, immediately after their original commitment to TFA has ended.[19] The survey then asked follow-up questions about respondents' jobs (e.g., about industry and salary) and educational pursuits (e.g., about degree sought).

As reported in Table 6, the survey results suggest that applicants induced to become TFA teachers by our treatments were pulled out of private sector jobs (see table notes for a list of such jobs). The table shows the effect of additional funding provided by the experiment—combining the grant and loan treatments to maximize power—on employment sector for respondents in the 1st decile of EC. Column 1 replicates the main finding of the paper for survey respondents: in the 1st decile of EC, extra funding has a large and statistically significant effect on joining TFA. Column 2 shows that \$100 of extra funding increases the likelihood that respondents are teaching at any school (TFA or otherwise) by 1.11 percentage points. Thus, our treatments created additional teachers overall, not just more teachers for TFA. Column 3 shows that the effect also persists on the two-year time horizon, after their time with TFA has concluded. This result lines up well with Dobbie and Fryer (2015), which provides quasi-experimental evidence that after two years, TFA participants are around 40 percentage points more likely than non-participants to teach at a K–12 school or to

while Online Appendix Table A.10 breaks down acceptance rates by credit type.

[19] For the 2015 cohort, more than two years had elapsed since the fall after they applied to the TGL program, so the "two years later" question was about their actual occupation at that time. For the 2016 and 2017 cohorts, the question was prospective. The survey also attempted to measure aspirational career goals by asking about plans 10 years later. As shown in Online Appendix Table A.11, we find no significant differences on the 10-year outcomes.

remain in education more broadly.[20] This mitigates worries that the teachers induced to join Teach For America by our treatments are short-timers that do not teach for long enough to become effective.[21]

Column 4 shows that \$100 of extra funding decreases the likelihood that the applicant is in a private sector job by 1.13 percentage points. This suggests that the funding is pulling applicants out of private sector jobs and into teaching. While these teachers are coming out of private sector jobs, the jobs they are giving up are not particularly lucrative. Survey respondents in the 1st decile report that their private sector jobs pay on average \$42,692 and report that teaching for TFA pays on average \$43,268. These private sector jobs, however, may start earlier and have smaller transitional costs than teaching with TFA. Column 5 shows that the effect on private sector jobs fails to persist on the two-year horizon. Finally, Columns 6 and 7 suggest that the treatments do not pull applicants out of school initially but may be pulling applicants out of school and into teaching on the two-year horizon.

TABLE 6 ABOUT HERE

7 Discussion

In this paper, we investigate whether liquidity constraints affect job choice. We randomly increase the size of transitional grant and loan packages offered to potential Teach For America teachers who apply for them and find that these small increases—\$600 or \$1,200—can dramatically increase the rate at which the highest need applicants join TFA. Our results suggest that the treatment effects arise due to liquidity constraints and that marginal teachers come from private sector jobs, so the funding generates more teachers overall.

TFA is a highly selective program—the applicants in our experiment are talented college graduates. One might think they would be able to access credit markets effectively and thus not need liquidity provided by TFA. Indeed, most of our applicants do not respond to treatment, suggesting they are able to finance any unmet liquidity

[20] In our experiment, of the roughly two percentage points of TFA teacher created by \$100 of liquidity (see Table 4), our survey tells us that roughly one percentage point remains in teaching after two years. Dividing the two numbers, we find TFA participants are about 50 percentage points more likely than non-participants to be teaching after two years, which is similar to the Dobbie and Fryer (2015) result.

[21] In addition, there is some evidence that even TFA short-timers are good teachers. In contrast to previous non-experimental studies (Raymond, Fletcher, and Luque 2001; Darling-Hammond et al. 2005), the experiment run in Glazerman, Mayer, and Decker (2006) suggests that newly hired TFA teachers outperform newly hired non-TFA teachers and are roughly equivalent in quality to more veteran teachers.

need on their own. However, the highest need applicants in our sample are 1.5 to 2.1 percentage points more likely to join TFA for every $100 in additional funding (either grants or loans) they receive as part of our experiment, suggesting liquidity is a first-order concern for their job choice.

That liquidity affects the decision to become a teacher—and to enter public service more generally—has a number of important policy implications. The United States is facing a growing teacher shortage (Goldring, Tale, and Riddles 2014; Sutcher, Darling-Hammond, and Carver-Thomas 2016), which has been a serious concern for policymakers. A natural implication of our findings is that easing the liquidity constraints for young people transitioning into teaching could prove a low-cost means of attracting teachers into the profession. Our estimates suggest that, in expectation, it only costs TFA $186 in additional interest payments to attract one additional teacher from the 1st decile of EC into TFA using loans.[22]

To be clear, some care must be taken in extrapolating our results to other contexts. Obviously, the population of TGL applicants was not selected to be representative of all new teachers. In addition, our estimates reflect the marginal effect of a dollar of liquidity, not the average effect (recall that TGL applicants in the 1st decile of EC are offered a control award of roughly $5,000 in grants and loans).

These caveats should be viewed in light of two facts. First, at its most general level, our experiment shows that liquidity can be a first-order concern for job choice. This conclusion seems likely to hold much more broadly than the specific numerical values of our estimates. Second, even if our specific numerical estimates are different for some other recruiting context, a liquidity intervention still seems likely to be quite cost effective, especially in contrast to other recent policy approaches geared towards recruiting and retaining teachers. These include conditional student aid grants, e.g., the federal TEACH Grant program and the California Governor's Teaching Fellowship (see Steele, Murnane, and Willett 2010); signing bonuses, e.g., the Massachusetts Signing Bonus Program (see Liu, Johnson, and Peske 2004); retention bonuses, e.g., North Carolina Bonus Program (see Clotfelter et al. 2008); and conditional loan forgiveness, e.g., the Florida Critical Teacher Shortage Program (see Feng and Sass 2018). Such interventions are more akin to our grant treatments than our loan treatments, because they put cash in teachers' pockets without asking it to be repaid. Our results suggest that loan-based policies could be more cost effective, especially

[22] This number is calculated using the estimate from column 12 of Table 4 that each additional $100 in loans increases the rate at which first decile applicants join TFA by 2.06 percentage points. It assumes a 3% interest rate and that all marginal loans are paid back on the standard timetable of 18 equal monthly payments starting six months into the TFA program.

when targeted towards teachers with credit constraints and timed to provide funds when transition costs are incurred.

In short, even if the costs were higher in other contexts, a program that offered bridge loans to prospective teachers (or prospective workers in other public service industries) might be a cost-effective strategy to increase the size of the candidate pool. By mitigating an existing market friction, such a program could simultaneously help both firms and potential workers in these industries. More broadly, increasing applicant pools could also improve job match—even outside the public sector—when job transitions (or even jobs, such as unpaid internships) require upfront liquidity.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at The Quarterly Journal of Economics online. Data and code replicating tables and figures in this article can be found in Coffman et al. (2018), in the Harvard Dataverse, doi:not.sure.yet.

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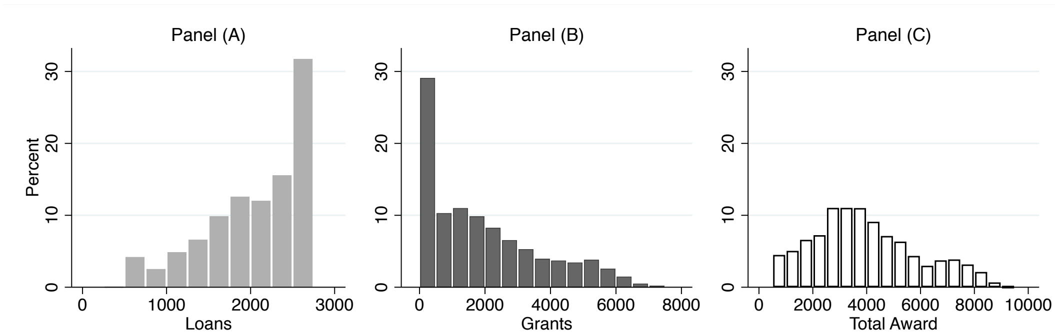
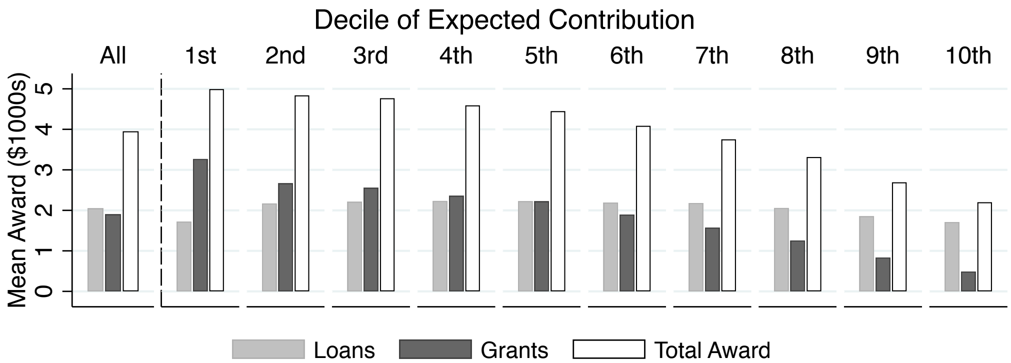
Figures and Tables

Figure 1: Control Awards (2015–2017)



Control awards are the awards that would be offered to applicants randomized into our control group and to which additional funding from our experimental treatments was added. Panel (A) shows a histogram of the amount of loans in the control awards. Panel (B) shows a histogram of the amount of grants in the control awards. Panel (C) shows a histogram of total control awards (i.e., loans plus grants). Bin width is $250 for loans and $500 for grants and total control awards.

Figure 2: Control Awards, by Decile of Expected Contribution (2015–2017)



Control awards are the awards that would be offered to applicants randomized into our control group and to which additional funding from our experimental treatments was added. Figure shows the mean loan, mean grant, and mean total control award, both across the entire sample (leftmost group of bars) and broken down by decile of expected contribution (all other groups of bars).

Figure 3: Treatment Effects of Additional Grants and Loans (2015–2016)

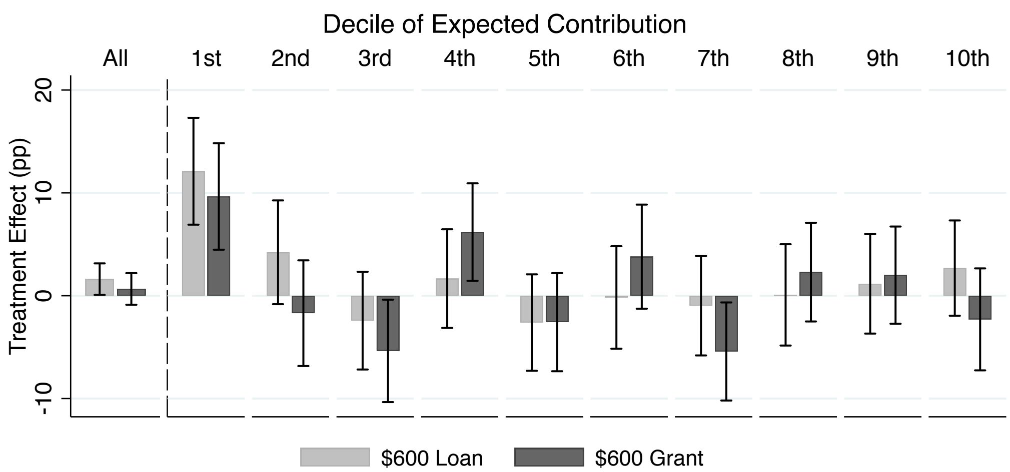


Figure shows treatment effects of offering \$600 in additional loans or \$600 in additional grants on whether applicants join TFA, estimated with specifications 1a and 1b, described in Section 4.2. The two leftmost bars show the effect pooled across all applicants. The other pairs of bars show the effect by decile of expected contribution. **Error bars show standard errors.** All estimates from the regression underlying the figure are reported in Table A.7 of the Online Appendix. Figure only includes applicants from the first two years of the experiment and suppresses estimates from the *\$1200 Grant* treatment, which was only introduced halfway through the second year of the experiment. See footnote 13.

Figure 4: Replication of Treatment Effects

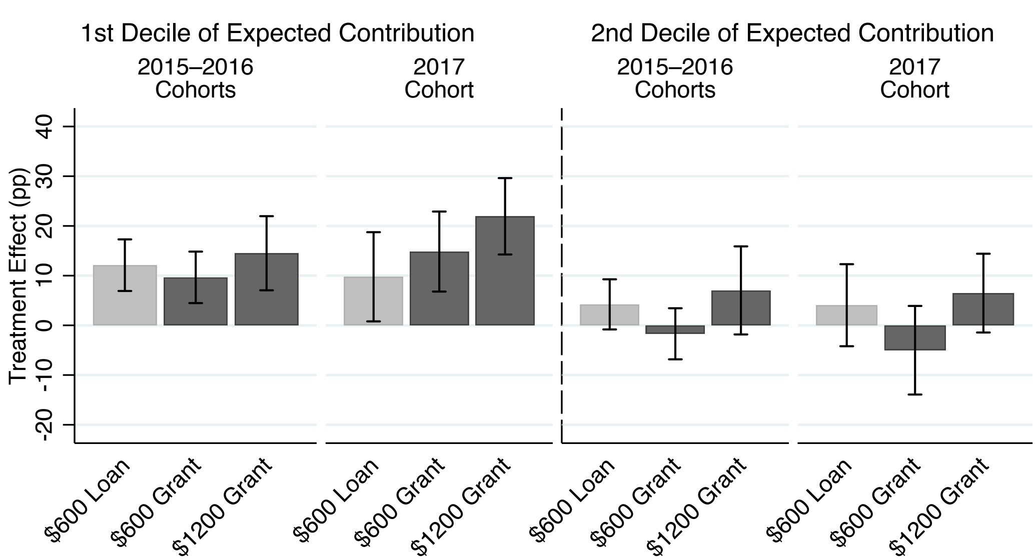


Figure compares treatment effects observed in the first two years of the experiment to treatment effects observed in the third year, estimates with specification 1b, described in Section 4.2. The left panel shows the treatment effects estimated for the 1st decile of expected contribution and the right panel shows the treatment effects estimated for the 2nd decile of expected contribution. The three bars on the left of each panel report results from the first two years of the experiment (2015–2016). The three bars on the right of each panel report results from the third year of the experiment (2017). **Error bars show standard errors.** All estimates from the regressions underlying the figure are reported in Table A.7 of the Online Appendix.

Figure 5: Treatment Effects in 1st Decile and in 2nd–10th Deciles (2015–2017)

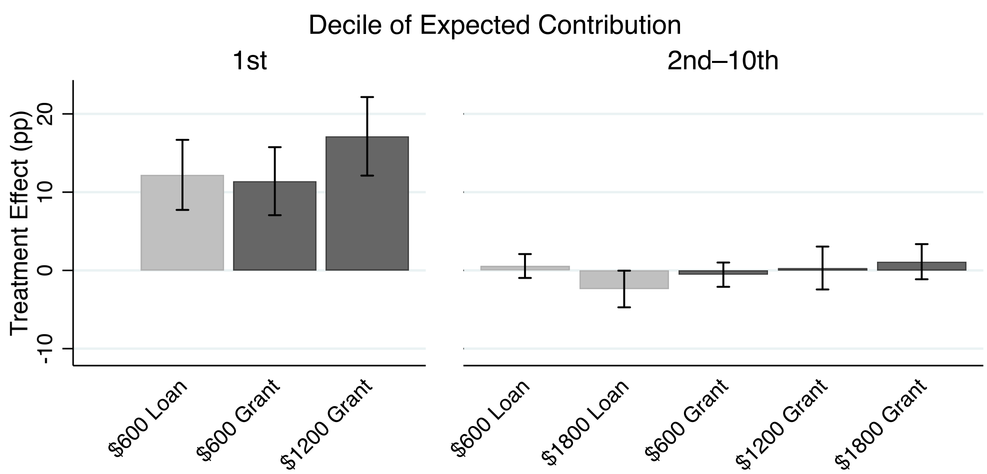


Figure shows treatment effects pooled across all years of the experiment, estimated with a variant of specification 1b (see Section 4.2) in which there is one dummy for being in deciles 2–10 instead of one dummy for each of those deciles. The left set of three bars show the treatment effects observed among applicants in the 1st decile of expected contribution. The right set of bars show the treatment effects observed among applicants in the 2nd–10th deciles of expected contribution. The sample includes applicants from all three years of our experiment (2015–2017). **Error bars show standard errors.** All estimates from the regression underlying the figure are reported in Table A.7 of the Online Appendix.

Table 1: Treatment Assignments

	2015	2016 (1st half)	2016 (2nd half)	2017	Total
1ST DECILE OF EC					
Control	86	38	32	70	226
$600 Loan	85	36	36	47	204
$600 Grant	85	41	46	63	235
$1200 Grant			39	63	102
2ND DECILE					
Control	84	35	37	45	201
$600 Loan	104	31	34	53	222
$600 Grant	113	31	28	50	222
$1200 Grant			25	55	80
3RD–10TH DECILES					
Control	732	286	242	545	1805
$600 Loan	798	319	252		1369
$600 Grant	795	289	243		1327
$1200 Grant			231		231
$1800 Loan				525	525
$1800 Grant				546	546

Table shows the number of applicants randomly assigned to each treatment by year of the experiment and decile of expected contribution. 2015 refers to applicants scheduled to begin teaching in fall 2015 (*mutatis mutandis* for 2016 and 2017). Halfway through 2016, the *$1200 Grant* treatment was added to the experiment. Starting in 2017, the experimental design was different for the 1st–2nd and 3rd–10th deciles of expected contribution. Cutoffs for deciles are based on 2015–2016 levels of expected contribution, which allows deciles to vary slightly in size for any given year.

Table 2: Effect of Marginal Grants and Loans: Theoretical Predictions

		EARNINGS CHANNEL	
		Does not affect behavior	Affects behavior
LIQUIDITY CHANNEL	Does not affect behavior	$Grants = Loans = 0$	$Grants > Loans = 0$
	Affects behavior	$Grants = Loans > 0$	$Grants > Loans > 0$

The earnings channel is present only in grants, while the liquidity channel is present in both grants and loans. This table shows the predicted magnitudes of marginal grants and marginal loans when the two channels either affect behavior or fail to do so. Ultimately, the experimental results will match the lower left cell for those in the bottom decile of EC and the upper left cell for everyone else.

Table 3: Summary Statistics

	Full sample	By Decile of Expected Contribution		
		1st	2nd	3rd–10th
Female (%)	75.8	75.7	76.0	75.8
White (%)	33.7	27.7	18.9	36.3
Age	26.2	28.4	26.0	25.9
"Fit" Score	3.89	3.97	4.11	3.85
Region Not First Choice (%)	35.9	32.9	35.6	36.4
Subject Not First Choice (%)	29.8	30.8	32.0	29.4
Expected Contribution ($)	1,157	-484	126	1,503
Checking and Savings ($)	1,071	241	174	1,293
Parental Contribution ($)	6,525	1,136	1,221	7,900
Income ($)	38,034	18,134	15,570	43,471
Credit Card Debt ($)	1,684	6,490	1,657	1,052
Private Student Loans ($)	5,100	19,693	3,824	3,330
Graduating Senior (%)	46.8	26.2	37.9	50.6
Number of Dependents	0.61	0.56	0.59	0.62
Local (%)	39.1	43.8	40.8	38.2
Regional Cost ($)	6,057	5,974	5,910	6,086
Federal Loans ($)	27,822	45,060	29,469	25,338
N	7,295	767	725	5,803

Table reports means for applicants in our experiment, overall and by deciles of expected contribution. "Fit Score" is a measure of an applicant's fit with the organizational objectives of TFA, as defined in footnote 12. "Region Not First Choice" is a dummy equal to 1 if the applicant was not assigned to teach in her most preferred geographic region. "Subject Not First Choice" is a dummy equal to 1 if the applicant was not assigned to teach in her most preferred subject. Expected contribution is as defined in the text in Section 2 and is comprised of the variables indented below it. "Checking and Savings" is the sum of funds in checking and savings accounts, "Parental Contribution" is the amount applicants' parents contributed to their undergraduate or graduate educational costs. "Income" is the income of applicants who were working before applying to TFA, "Credit Card Debit" is the amount of money owed on credit cards at the time of application. "Private Student Loans" are educational loans, excluding federal loans (federal loans can be put into forbearance during TFA and are not used to calculate expected contribution). "Graduating Senior" is a dummy equal to 1 if the applicant applied to TFA while a college senior. "Local" is a dummy equal to 1 if the applicant is assigned to teach in a region close to the applicant's current residence. "Regional Cost" is an estimate of how much money TFA expects local applicants will spend on attending Summer Institute and making the transition into teaching in a given region. "Regional Cost" is the primary component of expected expense as defined in Section 2. "Federal Loans" are federal student loans. Given how we define decile cutoffs, deciles need not contain exactly the same number of observations. See notes for Table 1.

Table 4: Treatment Effects of Additional Grants and Loans

	2015–2016				2017				2015–2017			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Extra Grants ($100s)	0.06	0.11			0.20	0.18			0.15	0.16		
	(0.20)	(0.20)			(0.13)	(0.12)			(0.11)	(0.10)		
Extra Loans ($100s)	0.25	0.26			-0.01	-0.03			0.05	0.05		
	(0.25)	(0.25)			(0.13)	(0.13)			(0.12)	(0.11)		
Extra Grants ($100s)			1.35**	1.81***			1.84***	1.76***			1.51***	1.77***
× 1st Decile EC			(0.59)	(0.61)			(0.64)	(0.60)			(0.42)	(0.41)
Extra Loans ($100s)			1.93**	2.16***			1.44	1.84			1.90***	2.06***
× 1st Decile EC			(0.83)	(0.83)			(1.42)	(1.34)			(0.71)	(0.69)
Demographics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Batch FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5233	5233	5233	5233	2062	2062	2062	2062	7295	7295	7295	7295
R^2	0.04	0.09	0.05	0.10	0.04	0.22	0.06	0.24	0.04	0.12	0.05	0.12
Control Mean	73.03	73.03	73.03	73.03	77.42	77.42	77.42	77.42	74.33	74.33	74.33	74.33
1st Decile Control Mean	61.54	61.54	61.54	61.54	60.00	60.00	60.00	60.00	61.06	61.06	61.06	61.06
Number of interactions with other deciles...												
...that are positive, $p < 0.10$			0	0			0	1			1	1
...that are negative, $p < 0.10$			0	0			1	3			0	0

Table shows Linear Probability Model (OLS) regressions of whether an applicant joins TFA from specifications 2a and 2b, described in Section 4.2. Sample includes only applicants from the first two years of the experiment. Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.10$, 0.05, and 0.01, respectively. "1st Decile EC" is a dummy equal to 1 if the applicant's expected contribution is in the lowest 10% of applicants' expected contributions. Demographics includes a linear age term, a linear term for the applicant's "fit" with TFA (described in footnote 12), and dummies for race, gender, assigned region, whether the applicant was assigned to her most preferred region, and whether the applicant was assigned to her most preferred subject. We also include a missing data dummy for each demographic variable that is sometimes missing (age, race, and fit). All regressions include fixed effects for the batches in which applicants' TGL awards were processed, the point at which randomization occurred ("Batch FEs"). The coefficient estimates for all deciles of expected contribution from the specifications reported in columns 3–4, 7–8, and 11–12 can be found in the Online Appendix Table A.8. The bottom two rows report how many treatment effect estimates from the 2nd–10th deciles of expected contribution are significant at $p < 0.10$ for each specification.

Table 5: Liquidity Need and Credit Access

	Decile of Expected Contribution			
	1st		2nd–10th	
	Control	Treatment	Control	Treatment
Needed additional funds	60.8%	46.5%	56.1%	49.7%
N	125	269	706	1623
CONDITIONAL ON NEEDING ADDITIONAL FUNDS				
Sought any funding	88.0%	86.4%	88.3%	88.2%
Applied for credit card	61.3%	56.0%	54.1%	59.0%
Applied for bank loan	17.3%	20.8%	18.5%	19.7%
Sought informal loan or gift	68.0%	68.0%	71.6%	70.9%
Received any funding	77.3%	68.8%	75.9%	76.6%
Any denial	24.0%	28.0%	14.0%	15.7%
Any discouragement	25.3%	32.0%	16.0%	16.7%
Any discouragement or denial	40.0%	51.2%	26.6%	27.9%
No credit access	13.3%	16.8%	7.9%	8.2%
N	75	125	394	803

Table shows liquidity need and credit outcomes of survey respondents, for respondents in the 1st decile of expected contribution in the left panel and the 2nd–10th deciles in the right panel. Within each panel, table reports the values for the control group only ("Control") and for those in any treatment group ("Treatment"). "Needed additional funds" is a dummy equal to 1 if the respondent said they needed funds in addition to the TGL award to make the transition into TFA. "Sought any funding" is a dummy equal to 1 if the respondent said they sought funding from any of the three sources listed. "Received any funding" is a dummy equal to 1 if the respondent said at least one attempt at accessing credit was successful. "Any denial" is a dummy equal to 1 if the respondent was denied in at least one attempt to access credit. "Any discouragement" is a dummy equal to 1 if the respondent at least once reported not seeking access to a source of credit because of a belief that the request would be denied. "Any discouragement or denial" is a dummy equal to 1 if either "Any denial" or "Any discouragement" is equal to 1. "No credit access" is a dummy equal to 1 if "Any discouragement or denial" is equal to 1 and the respondent did not receive credit from any source. For details about the survey, see Section 3.1 and Online Appendix Section A.2.

Table 6: Treatment Effects on Actual and Expected Occupations

	Joined TFA	Teaching		Private sector		Grad student	
		First year	2 years out	First year	2 years out	First year	2 years out
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Extra Funding ($100s)	1.57***	1.11*	1.16*	-1.13***	0.21	0.30	-0.85**
× 1st Decile EC	(0.57)	(0.58)	(0.64)	(0.36)	(0.42)	(0.30)	(0.41)
N	2718	2718	2718	2718	2718	2718	2718
R^2	0.18	0.12	0.11	0.12	0.12	0.10	0.11
Mean of Dependent Variable	79.54	83.55	67.66	4.82	7.91	5.30	11.52
Number of interactions with other deciles that are...							
... positive, $p < 0.10$	1	0	0	0	0	0	0
... negative, $p < 0.10$	0	0	1	1	0	0	0

Table reports how additional funds affect occupational choices of survey respondents, using the specification

$$Y_i = \sum_{d=1}^{10} \beta^d \cdot Extra\,Funds_i \cdot Decile_i^d + \sum_{d=1}^{9} \varphi^d \cdot Decile_i^d + \sum_j \gamma^j \cdot Batch_i^j + \boldsymbol{\delta} \cdot \boldsymbol{X}_i + \varepsilon_i.$$

The main independent variable, $Extra\,Funds_i$, is the combined extra grant and loan received by individual i. The dependent variable, Y_i, represents whether the respondent joined TFA, as defined in Section 4.2 (column 1); whether the respondent was teaching in the fall when they would have joined TFA and 2 years later (columns 2 and 3, respectively); whether the respondent was working in the private sector in the fall when they would have joined TFA and 2 years later (columns 4 and 5, respectively); or whether the respondent was a graduate student in the fall when they would have joined TFA and 2 years later (columns 6 and 7, respectively). The variables $Decile_i^d$, $Batch_i^j$, $\boldsymbol{X}_i$, and ε_i are the same as in the specifications discussed in Section 4.2. Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.10$, 0.05, and 0.01, respectively. "Private sector" jobs are those categorized on the survey as "Banking/Finance," "Consulting," "Publishing/Journalism/Media," "Law, Engineering/Technology," or "Other Business (e.g., Marketing or Real Estate)." The estimates for all deciles of expected contribution can be found in Online Appendix Table A.11. The bottom two rows report how many treatment effect estimates from the 2nd–10th deciles of expected contribution are significant at $p < 0.10$ for each specification.

ONLINE APPENDIX

Liquidity Affects Job Choice: Evidence from Teach For America

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A.1 Grant-Ineligible Applicants

As described in footnote 7 of the main text, 15% of TGL applicants had an expected contribution that was greater than 80% of their expected expense, and hence were deemed grant ineligible by TFA. These applicants received an award comprised entirely of loans, whose size was a function of expected expense, with a floor of $500. While they are not included in the experiment reported in the main text, in the first two years, we randomized these applicants either to receive their control loan award with 1/3 chance or to receive a treatment award that included $600 more in loans with 2/3 chance. Figure A.1 shows the distribution of control awards for these grant-ineligible applicants. Table A.1 shows that those in the treatment group were no more likely to be teaching through TFA the fall after they were admitted (indeed, they are directionally less likely to be doing so).

Figure A.1: Control Awards, Grant-Ineligible Applicants (2015–2016)

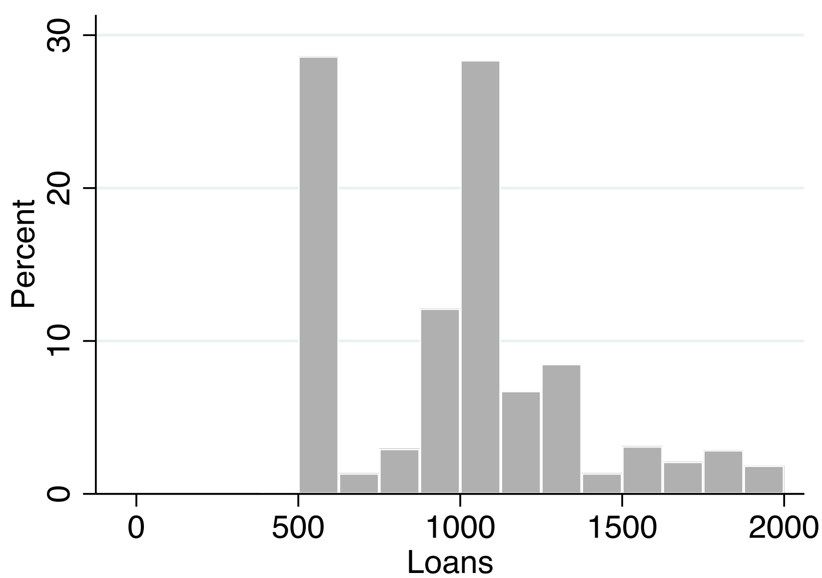

Figure shows a histogram of control award loan offers to grant-ineligible applicants in the 2015 and 2016 cohorts. Bin width is $125. TGL grant offers were always zero for these applicants.

Table A.1: Treatment Effects of Additional Loans, Grant-Ineligible Applicants (2015–2016)

	(1)	(2)
Extra Loans ($100s)	-0.24	-0.63
	(0.52)	(0.55)
Demographics	No	Yes
Batch FEs	Yes	Yes
N	842	842
R^2	0.14	0.25
Mean of Dependent Variable	78.86	78.86

Table shows Linear Probability Model (OLS) regressions of whether an applicant joins TFA, using specification 1a in the main text. Sample is restricted to grant-ineligible applicants in the first two years of the experiment. Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.10$, 0.05, and 0.01, respectively. Demographics includes a linear age term, a linear term for the applicant's "fit" with TFA (described in footnote 12 of the main text), and dummies for race, gender, assigned region, whether the applicant was assigned to her most preferred region, and whether the applicant was assigned to his or her most preferred subject. We also include a missing data dummy for each demographic variable that is sometimes missing (age, race, and fit). All regressions include fixed effects for the batches in which applicants' TGL awards were processed, the point at which randomization occurred ("Batch FEs").

A.2 Post-Experiment Survey: Methodology

A.2.1 Incentives

As with any survey, selection into response can lead to bias. To mitigate this problem, we offered financial rewards for survey completion. We further improved our understanding of any potential selection bias in our survey by varying the rewards across applicants in two ways. First, we offered larger rewards to applicants in the 1st decile of EC than to applicants in the 2nd–10th deciles of EC. For a given budget, this allows us to more effectively increase the response rate for the group in which we found a treatment effect, and hence in which we are most interested (see Section 4 of the main text). Second, we randomly chose whether applicants were offered a larger or a smaller reward for survey completion. In theory, such variation allows us to directly gauge potential selection bias on answers to specific survey items.

To understand this approach, first consider data that are available for both respondents and non-respondents (usually demographics). With such data, the standard approach is to compare the respondent means to the non-respondent means. If there are no differences, then there is no selection *on observables*. If there are differences, then we can try to correct for selection using methods like propensity score matching or inverse probability weighting (Horvitz and Thompson 1952; Rosenbaum and Rubin 1983; Hirano, Imbens, and Ridder 2003; Angrist and Pischke 2009).[1a]

Of course, such an approach is useless when considering selection on data that is only available for respondents (usually answers to survey items). For instance, in our context, one might worry that those with liquidity need are more likely to respond. Since liquidity need is only gauged for respondents, the rate of liquidity need among non-respondents is unknown, and hence cannot be used for comparison. But, if there were a group of non-respondents for whom liquidity need were gauged, we would be able to use the old approach. One way to create such a setup is to randomize high and low completion incentives for the surveyed population.

Consider large and small rewards that lead to response rates of r_L and r_S, respectively, where $r_L > r_S$. Further, assume that the mean answers to some survey item are y_L and y_S under the large and small rewards, respectively. If we assume that response to incentive is monotonic, then of those that respond to the large reward, a fraction r_S/r_L are *always-responders* (i.e., those that respond to low and high incentives), who are identical in type to those that respond to the small reward. The remaining frac-

[1a]Such methods require the further assumption that response is independent of unobservables, *conditional on observables*.

tion are *marginal-responders* (i.e., those that respond to high but not low incentives). Under these assumptions, if y_{marg} represents the mean answer among the marginal-responders, then simple accounting dictates that $(r_S/r_L) \cdot y_S + (1 - r_S/r_L) \cdot y_{marg} = y_L$. Solving, we find that the mean answer among marginal respondents is

$$y_{marg} = \frac{r_L}{r_L - r_S} \cdot y_L - \frac{r_S}{r_L - r_S} \cdot y_S. \tag{A.1}$$

Using this equation, we can effectively partition respondent types into always-responders and marginal-responders.[2a] By comparing the mean answer among marginal-responders, y_{marg}, to the mean answer among always-responders, y_S, we can directly gauge selection on the answer to that survey item. Note that since $r_L - r_S$ is in the denominator, for this approach to work well, we need the difference between r_L and r_S to be relatively large. Otherwise, we would expect even small errors in y_L and y_S to translate into large errors in y_{marg}.[3a]

Returning to the details of our survey, the variation in response incentives that we used is summarized in Table A.2. Given that the survey was advertised to take 5 minutes, the rewards were quite generous, with an implied expected hourly rate of at least \$30/hr and up to \$480/hr.[4a]

Table A.2: Differential Financial Incentives for Survey Completion

Reward offered	EC Decile 1		EC Deciles 2–10	
	# receiving offer	Response rate	# receiving offer	Response rate
Certain \$20	381	48.3%	0	—
Certain \$40	386	56.7%	0	—
0.5% chance at \$500	0	—	3265	36.6%
1% chance at \$500	0	—	3263	37.0%

The \$20 and \$40 rewards were issued as Amazon gift cards, while the \$500 rewards were disbursed using pre-paid debit cards.

A.2.2 Analysis of Potential Selection Bias

As described in the previous section, when considering potential selection bias in data that we have for both respondents and non-respondents, we simply compare means

[2a]The monotonicity assumption described above rules out respondents who respond to the low incentive but not to the high.

[3a]This follows from applying the delta method to equation A.1.

[4a]The advertised completion time was accurate: median survey response time was 4 minutes and 23 seconds.

across the two groups.[5a] Table A.3 shows these means, broken down by responses and whether the applicant is in the 1st decile of EC. In the 1st decile, we find little evidence of selection, save for a moderately significant difference in "fit" score. Further, the mean of the most important variable in our analysis, expected contribution, only differs by $29 across respondents and non-respondents. In the 2nd–10th deciles, we see more significant differences, consistent with the fact that a lower response incentive led to a lower response rate. The statistically significant differences do not seem to be economically large.

When considering selection on answers to survey items, we are limited by the difference in survey response rate that we can elicit through differential incentives (see the discussion in the previous section). Looking to Table A.2, we see that in the 2nd–10th deciles, larger incentives induced only an additional 0.4 percentage points of survey completion, while in the 1st decile, the difference was larger, but still relatively small, at 8.4 percentage points. Unsurprisingly, these small differences in completion rate did not yield large differences in average responses (see Table A.4).

Although the lack of a large response to incentives prevents us from directly applying the lessons of the previous section, it does provide some reassurance that there is not much room for selection on unobservables having to do with the time value of money. Doubling the $20 incentive to $40 (for 5 minutes work) only increased the completion rate by 17%—an implied elasticity of 0.17. *A priori*, it was not clear that doubling an already generous incentive would have such a modest effect.

In short, on observables, we have little indication of selection bias in the 1st decile of EC, and some slight indication in the 2nd–10th deciles. On answers to survey items, we see no strong differences across the high and low incentive groups, but our large variation in financial incentive did not produce commensurately large variation in the survey response rate. This provides some reassurance that selection on the time value of money is limited in our sample. As such, in the main text, we report raw results without attempting to correct for selection bias.

[5a] Essentially, we are treating respondents as a unified group, combining applicants that received different response incentives. We can think of the effective incentive for this group as a random offer of either the high or low incentive. In the language of the previous section, half of marginal-responders are grouped with the respondents and half with the non-respondents (since half receive the larger incentive and half do not), which is the relevant breakdown for the results reported in the main text.

Table A.3: Selection into Survey on Demographics

	Respondents	Non-respondents	Difference
1ST DECILE OF EXPECTED CONTRIBUTION			
Female (%)	76.7	74.7	1.9
	(2.1)	(2.3)	(3.1)
White (%)	30.1	25.0	5.1
	(2.3)	(2.3)	(3.2)
Age	28.4	28.3	0.1
	(0.4)	(0.4)	(0.5)
"Fit" Score	3.9	4.0	-0.1**
	(0.0)	(0.0)	(0.1)
Region Not First Choice (%)	35.4	32.6	4.0
	(2.4)	(2.5)	(3.5)
Subject Not First Choice (%)	30.9	31.3	-1.0
	(2.3)	(2.4)	(3.4)
Expected Contribution ($)	-470	-499	28.6
	(33)	(48)	(57.7)
N	403	364	
2ND–10TH DECILE OF EXPECTED CONTRIBUTION			
Female (%)	75.9	75.8	0.1
	(0.9)	(0.7)	(1.1)
White (%)	37.8	32.4	5.4***
	(1.0)	(0.7)	(1.2)
Age	25.8	26.0	-0.2
	(0.1)	(0.1)	(0.1)
"Fit" Score	3.9	3.9	-0.0**
	(0.0)	(0.0)	(0.0)
Region Not First Choice (%)	36.0	37.9	-1.7
	(1.0)	(0.8)	(1.3)
Subject Not First Choice (%)	31.1	29.2	2.0*
	(0.9)	(0.7)	(1.2)
Expected Contribution ($)	1,410	1,315	94.7***
	(26)	(19)	(31.9)
N	2,403	4,125	

Table shows summary statistics of our demographic variables and expected contribution, comparing survey respondents to non-respondents. The top panel includes only applicants in the 1st decile of expected contribution. The bottom panel includes only applicants in the 2nd–10th deciles. The column on the right reports the difference between respondents and non-respondents. Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.10$, 0.05, and 0.01, respectively. "Fit Score" is a measure of an applicant's fit with the organizational objectives of TFA, as defined in footnote 12 of the main text. "Region Not First Choice" is a dummy equal to 1 if the applicant was not assigned to teach in her most preferred geographic region. "Subject Not First Choice" is a dummy equal to 1 if the applicant was not assigned to teach in her most preferred subject. "Expected Contribution" is as defined in Section 2 of the main text.

Table A.4: Comparing Survey Incentive Groups

	EC Decile 1			EC Deciles 2–10		
	Low Incentive	High Incentive	Difference	Low Incentive	High Incentive	Difference
MODE OF EMPLOYMENT QUESTIONS						
Teaching 0 years out (%)	77.5	81.6	4.0	85.4	83.0	-2.5
	(3.1)	(2.6)	(4.1)	(1.0)	(1.1)	(1.5)
Teaching 2 years out (%)	67.4	68.2	0.8	67.6	67.6	0.0
	(3.5)	(3.2)	(4.7)	(1.4)	(1.4)	(1.9)
Private sector 0 years out (%)	6.2	6.9	0.7	4.6	4.5	-0.1
	(1.8)	(1.7)	(2.5)	(0.6)	(0.6)	(0.9)
Private sector 2 years out (%)	6.7	10.6	3.9	7.6	7.9	0.3
	(1.9)	(2.1)	(2.8)	(0.8)	(0.8)	(1.1)
Graduate student 0 years out (%)	7.3	5.5	-1.8	4.6	5.7	1.1
	(2.0)	(1.6)	(2.5)	(0.6)	(0.7)	(0.9)
Graduate student 2 years out (%)	10.7	6.9	-3.8	12.3	11.7	-0.6
	(2.3)	(1.7)	(2.9)	(1.0)	(0.9)	(1.3)
Needed additional funds (%)	52.2	50.0	-2.2	49.8	53.5	3.7*
	(3.7)	(3.4)	(5.1)	(1.5)	(1.5)	(2.1)
N	184	219		1196	1207	
CREDIT ACCESS QUESTIONS						
Sought any loan (%)	86.0	87.9	1.8	89.2	87.3	-1.9
	(3.6)	(3.2)	(4.8)	(1.3)	(1.3)	(1.9)
Received any loan (%)	72.0	72.0	-0.1	78.1	74.8	-3.3
	(4.7)	(4.4)	(6.4)	(1.7)	(1.7)	(2.5)
Any denial (%)	23.7	29.0	5.3	14.8	15.4	0.7
	(4.4)	(4.4)	(6.2)	(1.5)	(1.4)	(2.1)
Any discouragement (%)	30.1	29.0	-1.1	16.0	16.9	0.9
	(4.8)	(4.4)	(6.5)	(1.5)	(1.5)	(2.1)
Any discouragement or denial (%)	45.2	48.6	3.4	27.5	27.5	0.0
	(5.2)	(4.9)	(7.1)	(1.9)	(1.8)	(2.6)
No credit access (%)	12.9	17.8	4.9	7.3	8.8	1.5
	(3.5)	(3.7)	(5.1)	(1.1)	(1.1)	(1.6)
N	93	107		575	622	

Table shows answers to the follow-up survey by decile of expected contribution and incentive group. The left panel includes only respondents in the 1st decile of expected contribution. The right panel includes only respondents in the 2nd–10th deciles. The column on the right of each panel reports the difference between high-incentive and low-incentive respondents. Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.10$, 0.05, and 0.01, respectively. The bottom panel includes only respondents who answered that they needed additional funds (see Section 5 in the main text).

A.3 Additional Figures and Tables

Figure A.2: Percentage of Applicants Joining TFA in the Control Group (2015–2016)

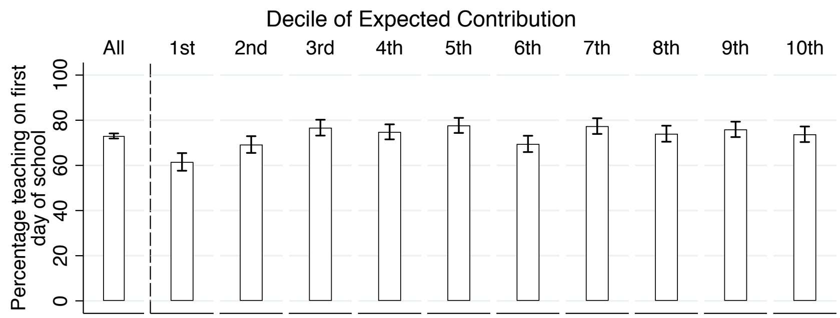

Figure shows the percentage of applicants in the control group of our experiment who are teaching for TFA on the first day of school in the first two years of our experiment. The leftmost bar shows the overall percentage. The other bars report the percentage by decile of expected contribution. **Error bars show standard errors.**

Figure A.3: Applicants' Expected Contributions (2015–2017)

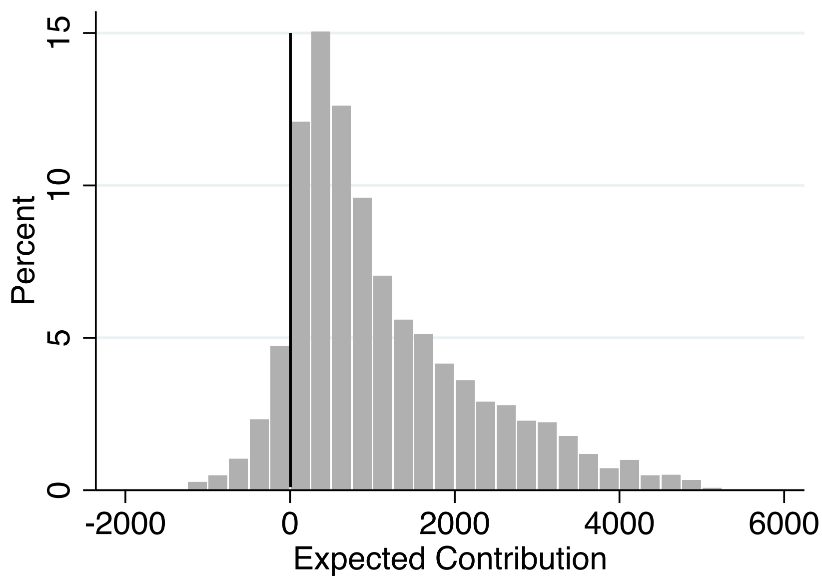

Figure is a histogram of expected contribution of applicants in the experiment. The vertical line represents the 10th percentile of expected contribution, equal to $4.20.

Figure A.4: Stress Test of Null Results (2017)

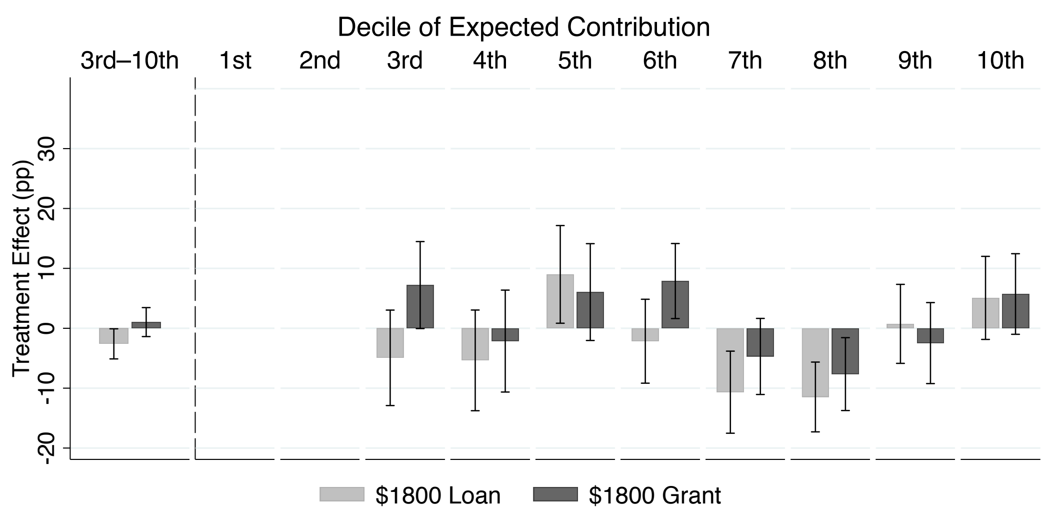

Figure shows treatment effects of offering $1,800 in additional loans or grants on whether applicants join TFA, using specifications 1a and 1b from the main text. Full results of these regressions are reported in Figure A.7 of this Online Appendix. The two leftmost bars show the effect pooled across all applicants in the 3rd–10th deciles of expected contribution. The other pairs of bars show the effect by decile of expected contribution. **Error bars show standard errors.** Figure only includes applicants from the 3rd–10th deciles from the third year of the experiment, since they were the only ones randomized to these treatments. Treatment effects observed from applicants in the 1st–2nd deciles in the third year of the experiment are shown in Figure 4 of the main text.

Table A.5: Components of Expected Contribution

	Squared Semipartial Correlation	Shapley Value
Checking and Savings	52.8%	55.7%
Parental Contribution	9.6%	15.9%
Income	8.7%	13.7%
Credit Card Debt	6.7%	8.9%
Private Student Loans	3.8%	3.3%
Graduating Senior	0.0%	2.2%
Number of Dependents	0.3%	0.4%

This table shows how important each member of a set of regressors is in explaining the variation of expected contribution. The squared semipartial correlation of a regressor, as noted in Abdi (2007), is simply its marginal explanatory power, that is, the amount by which R^2 drops upon removing it from the regression. To shed light on a regressor's inframarginal explanatory power, we also look at its Shapley value in the cooperative game whose "players" are regressors and whose coalitional value function is the regression's R^2. "Checking and Savings" is the sum of funds in checking and savings accounts, "Parental Contribution" is the amount applicants' parents contributed to their undergraduate or graduate educational costs. "Income" is the income of applicants who were working before applying to TFA, "Credit Card Debt" is the amount of money owed on credit cards at the time of application. "Private Student Loans" are educational loans, excluding federal loans (federal loans can be put into forbearance during TFA and are not used to calculate expected contribution). "Graduating Senior" is a dummy equal to 1 if the applicant applied to TFA while a college senior.

Table A.6: p-values of Balance Tests

	Decile of Expected Contribution			
	All	1st	2nd	3rd–10th
Female	0.866	0.434	0.193	0.549
White	0.070	0.850	0.602	0.235
Age	0.229	0.470	0.647	0.904
"Fit" Score	0.207	0.668	0.410	0.450
Region Not First Choice	0.488	0.188	0.105	0.922
Subject Not First Choice	0.986	0.106	0.585	0.379
Expected Contribution	0.446	0.372	0.497	0.636

Each cell reports p-values from F-tests of the null hypothesis that the coefficients on the treatment groups are jointly zero in separate OLS regressions (following a variant of specification 1a in the main text in which the left-hand side is a demographic variable instead of a dummy for whether the applicant joined TFA) of each demographic variable on dummies for the treatment groups and batch fixed effects. The columns indicate which deciles of expected contribution are included in the regression sample. The regressions reported in the first column also include dummies for decile of expected contribution. "Fit Score" is a measure of an applicant's fit with the organizational objectives of TFA, as defined in footnote 12 of the main text. "Region Not First Choice" is a dummy equal to 1 if the applicant was not assigned to teach in her most preferred geographic region. "Subject Not First Choice" is a dummy equal to 1 if the applicant was not assigned to teach in her most preferred subject. "Expected Contribution" is as defined in Section 2 of the main text.

Table A.7: Treatment Effects of Additional Grants or Loans, Coefficients from Figures

	2015–2016		2017		2015–2017
				3rd–10th Deciles	
	(1)	(2)	(3)	(4)	(5)
	FIG. 3	FIGS. 3 & 4	FIGURE 4 & A.4	FIGURE A.4	FIGURE 5
$600 Grant	0.66				
	(1.55)				
$1200 Grant	0.20				
	(2.94)				
$600 Loan	1.61				
	(1.54)				
$1800 Grant				1.03	
				(2.42)	
$1800 Loan				-2.60	
				(2.51)	
$600 Grant		9.65*	14.84*		11.39***
× 1st Decile EC		(5.18)	(8.05)		(4.34)
$1200 Grant		14.51*	21.94***		17.24***
× 1st Decile EC		(7.45)	(7.68)		(5.02)
$600 Loan		12.10**	9.77		12.17***
× 1st Decile EC		(5.19)	(8.97)		(4.47)
$600 Grant					-0.69
× 2nd–10th Decile EC					(1.55)
$1200 Grant					-0.08
× 2nd–10th Decile EC					(2.70)
$600 Loan					0.36
× 2nd–10th Decile EC					(1.52)
$1800 Grant					1.62
× 2nd–10th Decile EC					(2.21)
$1800 Loan					-1.87
× 2nd–10th Decile EC					(2.31)
$600 Grant		-1.70	-5.01		
× 2nd Decile EC		(5.14)	(8.91)		
$1200 Grant		7.03	6.48		
× 2nd Decile EC		(8.86)	(7.92)		
$600 Loan		4.22	4.05		
× 2nd Decile EC		(5.04)	(8.25)		
$600 Grant		-5.36			
× 3rd Decile EC		(4.98)			
$1200 Grant		-4.89			
× 3rd Decile EC		(8.86)			
$600 Loan		-2.42			
× 3rd Decile EC		(4.75)			
$1800 Grant			7.21		
× 3rd Decile EC			(7.26)		
$1800 Loan			-4.93		
× 3rd Decile EC			(7.97)		
$600 Grant		6.19			
× 4th Decile EC		(4.74)			
$1200 Grant		7.17			
× 4th Decile EC		(7.40)			
$600 Loan		1.66			
× 4th Decile EC		(4.80)			
$1800 Grant			-2.14		
× 4th Decile EC			(8.51)		
$1800 Loan			-5.36		
× 4th Decile EC			(8.41)		
$600 Grant		-2.57			
× 5th Decile EC		(4.77)			
$1200 Grant		-3.99			
× 5th Decile EC		(8.08)			
$600 Loan		-2.61			
× 5th Decile EC		(4.69)			
$1800 Grant			6.04		
× 5th Decile EC			(8.08)		
$1800 Loan			8.99		
× 5th Decile EC			(8.15)		
$600 Grant		3.80			
× 6th Decile EC		(5.06)			
$1200 Grant		7.43			
× 6th Decile EC		(8.63)			
$600 Loan		-0.17			
× 6th Decile EC		(4.98)			
$1800 Grant			7.88		
× 6th Decile EC			(6.26)		

$1800 Loan			-2.16		
× 6th Decile EC			(7.01)		
$600 Grant		-5.42			
× 7th Decile EC		(4.77)			
$1200 Grant		3.81			
× 7th Decile EC		(7.96)			
$600 Loan		-0.97			
× 7th Decile EC		(4.84)			
$1800 Grant			-4.70		
× 7th Decile EC			(6.35)		
$1800 Loan			-10.67		
× 7th Decile EC			(6.85)		
$600 Grant		2.30			
× 8th Decile EC		(4.80)			
$1200 Grant		-13.37			
× 8th Decile EC		(10.04)			
$600 Loan		0.08			
× 8th Decile EC		(4.92)			
$1800 Grant			-7.65		
× 8th Decile EC			(6.08)		
$1800 Loan			-11.47**		
× 8th Decile EC			(5.82)		
$600 Grant		2.00			
× 9th Decile EC		(4.73)			
$1200 Grant		-3.69			
× 9th Decile EC		(7.97)			
$600 Loan		1.16			
× 9th Decile EC		(4.84)			
$1800 Grant			-2.47		
× 9th Decile EC			(6.76)		
$1800 Loan			0.74		
× 9th Decile EC			(6.60)		
$600 Grant		-2.30			
× 10th Decile EC		(4.96)			
$1200 Grant		-16.37			
× 10th Decile EC		(10.81)			
$600 Loan		2.68			
× 10th Decile EC		(4.63)			
$1800 Grant			5.72		
× 10th Decile EC			(6.73)		
$1800 Loan			5.07		
× 10th Decile EC			(6.94)		
Demographics	No	No	No	No	No
Batch FEs	Yes	Yes	Yes	Yes	Yes
N	5233	5233	2062	1616	7295
R^2	0.04	0.05	0.06	0.05	0.05
Mean of Dep. Var.	73.88	73.88	78.47	79.27	75.17

Table shows Linear Probability Model (OLS) regressions (specifications 1a and 1b in the main text) of whether an applicant joins TFA. Columns 1 and 2 include only applicants from the first two years of the experiment. Robust standard errors are reported in parentheses. Columns 3 and 4 include only applicants from the final year of the experiment, and column 4 further restricts the sample to those in the third through 10th deciles of expected contribution. Column 5 include applicants from all years of the experiment. *, **, *** denote $p < 0.1$, 0.05, and 0.01, respectively. "1st Decile EC" is a dummy equal to 1 if the applicant's expected contribution is in the lowest 10% of applicants' expected contributions (and similarly for other deciles). All regressions include fixed effects for the batches in which applicants' TGL awards were processed, the point at which randomization occurred ("Batch FEs").

Table A.8: Treatment Effects of Additional Grants or Loans, All Coefficients

	2015–2016		2017		2015–2017	
	(1)	(2)	(3)	(4)	(5)	(6)
Extra Grants ($100s)	1.35**	1.81***	1.84***	1.76***	1.51***	1.77***
× 1st Decile EC	(0.59)	(0.61)	(0.64)	(0.60)	(0.42)	(0.41)
Extra Loans ($100s)	1.93**	2.16***	1.44	1.84	1.90***	2.06***
× 1st Decile EC	(0.83)	(0.83)	(1.42)	(1.34)	(0.71)	(0.69)
Extra Grants	0.18	0.16	0.59	0.22	0.49	0.33
× 2nd Decile EC	(0.64)	(0.64)	(0.66)	(0.59)	(0.44)	(0.42)
Extra Loans	0.87	0.68	1.19	0.83	1.02	0.77
× 2nd Decile EC	(0.82)	(0.81)	(1.30)	(1.15)	(0.69)	(0.67)
Extra Grants	-0.61	-0.40	0.40	0.40	-0.05	0.04
× 3rd Decile EC	(0.62)	(0.63)	(0.40)	(0.38)	(0.31)	(0.29)
Extra Loans	-0.30	0.15	-0.28	-0.22	-0.42	-0.30
× 3rd Decile EC	(0.77)	(0.77)	(0.44)	(0.40)	(0.36)	(0.34)
Extra Grants	0.78	0.69	-0.12	-0.19	0.12	-0.03
× 4th Decile EC	(0.56)	(0.57)	(0.47)	(0.45)	(0.34)	(0.33)
Extra Loans	0.20	0.22	-0.29	-0.15	-0.27	-0.24
× 4th Decile EC	(0.78)	(0.77)	(0.47)	(0.45)	(0.36)	(0.35)
Extra Grants	-0.36	-0.17	0.34	0.53	0.02	0.13
× 5th Decile EC	(0.58)	(0.58)	(0.45)	(0.44)	(0.29)	(0.28)
Extra Loans	-0.41	-0.33	0.50	0.56	0.16	0.13
× 5th Decile EC	(0.76)	(0.75)	(0.45)	(0.45)	(0.31)	(0.31)
Extra Grants	0.63	0.61	0.44	0.62**	0.62**	0.71***
× 6th Decile EC	(0.62)	(0.61)	(0.35)	(0.31)	(0.27)	(0.25)
Extra Loans	-0.03	-0.07	-0.12	0.08	0.04	0.16
× 6th Decile EC	(0.81)	(0.81)	(0.39)	(0.37)	(0.32)	(0.30)
Extra Grants	-0.31	-0.67	-0.26	-0.34	-0.26	-0.36
× 7th Decile EC	(0.60)	(0.58)	(0.35)	(0.33)	(0.28)	(0.27)
Extra Loans	0.08	-0.45	-0.59	-0.74**	-0.42	-0.44
× 7th Decile EC	(0.78)	(0.76)	(0.38)	(0.36)	(0.32)	(0.31)
Extra Grants	-0.40	-0.48	-0.43	-0.55*	-0.17	-0.27
× 8th Decile EC	(0.66)	(0.64)	(0.34)	(0.31)	(0.29)	(0.28)
Extra Loans	-0.28	-0.39	-0.64**	-0.57*	-0.27	-0.33
× 8th Decile EC	(0.80)	(0.79)	(0.32)	(0.29)	(0.29)	(0.28)
Extra Grants	-0.06	-0.10	-0.14	0.01	-0.17	-0.01
× 9th Decile EC	(0.58)	(0.56)	(0.38)	(0.35)	(0.29)	(0.26)
Extra Loans	0.06	0.07	0.04	0.26	-0.03	0.18
× 9th Decile EC	(0.78)	(0.78)	(0.37)	(0.34)	(0.30)	(0.26)
Extra Grants	-0.88	-0.79	0.32	0.34	-0.05	0.03
× 10th Decile EC	(0.68)	(0.68)	(0.37)	(0.35)	(0.28)	(0.26)
Extra Loans	0.29	0.41	0.28	0.07	0.20	0.10
× 10th Decile EC	(0.76)	(0.76)	(0.39)	(0.37)	(0.29)	(0.28)
1st Decile EC	-11.92**	-10.97**	-12.74*	-8.58	-11.81***	-9.90***
	(4.89)	(5.02)	(7.46)	(7.20)	(3.76)	(3.74)
2nd Decile EC	-6.16	-4.63	1.47	6.66	-4.10	-1.31
	(4.84)	(4.93)	(7.62)	(7.13)	(3.74)	(3.69)
3rd Decile EC	2.81	2.65	1.45	4.42	3.07	4.10
	(4.74)	(4.80)	(7.25)	(6.89)	(3.32)	(3.31)
4th Decile EC	-1.09	-0.41	5.55	6.20	2.85	4.07
	(4.61)	(4.68)	(7.55)	(7.21)	(3.31)	(3.27)
5th Decile EC	1.95	1.51	-0.26	-1.68	1.59	1.80
	(4.57)	(4.67)	(8.26)	(8.10)	(3.29)	(3.28)
6th Decile EC	-6.12	-4.80	3.73	4.15	-3.49	-2.45
	(4.72)	(4.81)	(6.96)	(6.61)	(3.28)	(3.24)
7th Decile EC	1.63	4.52	9.43	11.52*	4.61	5.99*
	(4.67)	(4.66)	(6.68)	(6.17)	(3.22)	(3.15)
8th Decile EC	1.08	2.98	14.49**	14.96***	3.89	5.19*
	(4.77)	(4.77)	(6.23)	(5.74)	(3.20)	(3.12)
9th Decile EC	1.23	0.85	4.99	4.93	3.27	2.06
	(4.65)	(4.65)	(6.92)	(6.64)	(3.22)	(3.17)
Demographics	No	Yes	No	Yes	No	Yes
Batch FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	5233	5233	2062	2062	7295	7295
R^2	0.05	0.10	0.06	0.24	0.05	0.12
Mean of Dep. Var.	73.88	73.88	78.47	78.47	75.17	75.17

Table shows Linear Probability Model (OLS) regressions of whether an applicant joins TFA using specification 2b, described in Section 4.2 of the main text. Columns denote sample of applicants included. Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.10$, 0.05, and 0.01, respectively. "1st Decile EC" is a dummy equal to 1 if the applicant's expected contribution is in the lowest 10% of applicants' expected contributions; other decile dummies are defined accordingly. "10th Decile EC" is the excluded group. Demographics includes a linear age term, a linear term for the applicant's "fit" with TFA (described in footnote 12 of the main text), and dummies for race, gender, assigned region, whether the applicant was assigned to her most preferred region, and whether the applicant was assigned to his or her most preferred subject. We also include a missing data dummy for each demographic variable that is sometimes missing (age, race, and fit). All regressions include fixed effects for the batches in which applicants' TGL awards were processed, the point at which randomization occurred ("Batch FEs").

Table A.9: Reasons Respondents Did Not Seek Various Sources of Credit

	Credit Card (by decile of EC)		Bank Loan (by decile of EC)		Informal Loan/Gift (by decile of EC)	
	1st	2nd–10th	1st	2nd–10th	1st	2nd–10th
Too time consuming	0.0%	2.5%	2.5%	4.9%		
Borrowing rates too high	25.0%	19.2%	25.5%	23.6%		
Did not know how	3.6%	6.1%	8.7%	10.4%		
Did not occur to me	6.0%	10.2%	8.7%	12.3%	1.6%	4.9%
Thought request would be denied	32.1%	14.9%	28.0%	13.8%	10.9%	10.1%
Covered need another way	27.4%	46.9%	29.8%	39.6%	12.5%	30.3%
Was not willing	59.5%	61.8%	51.6%	59.6%	43.8%	47.7%
Too much strain on relationships					29.7%	33.5%
No one had enough money to ask					48.4%	42.5%
Other	6.0%	5.1%	3.1%	2.9%	9.4%	7.2%
N	84	510	161	966	64	346

Table shows the percent of respondents who listed each item as a reason they did not apply for each type of credit. For details about the survey, see the Section 3.1 in the main text and Section A.2 in this Online Appendix.

Table A.10: Credit Request Outcomes

	Credit Card (by decile of EC)		Bank Loan (by decile of EC)		Informal Loan/Gift (by decile of EC)	
	1st	2nd–10th	1st	2nd–10th	1st	2nd–10th
Prefer not to answer	8.6%	12.5%	15.4%	18.7%	10.3%	10.7%
Rejected	19.8%	9.8%	41.0%	21.3%	14.0%	11.5%
Partially granted	22.4%	18.7%	28.2%	22.6%	49.3%	47.3%
Fully granted	49.1%	59.0%	15.4%	37.4%	26.5%	30.5%
N	116	686	39	230	136	850

Table shows the outcome of credit requests by respondents. For details about the survey, see Section 3.1 in the main text and Section A.2 in this Online Appendix.

Table A.11: Treatment Effects on Actual and Expected Occupations, All Coefficients

	Joined	Teaching			Private sector			Grad student	
	TFA	First year	2 years out	10 years out	First year	2 years out	10 years out	First year	2 years out
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Extra Funding ($100s)	1.57***	1.11*	1.16*	0.67	-1.13***	0.21	-0.04	0.30	-0.85**
× 1st Decile EC	(0.57)	(0.58)	(0.64)	(0.66)	(0.36)	(0.42)	(0.55)	(0.30)	(0.41)
Extra Funding	0.74	0.62	0.43	-0.39	-0.55	-0.73	-0.01	-0.40	-0.51
× 2nd Decile EC	(0.67)	(0.72)	(0.92)	(0.94)	(0.53)	(0.62)	(0.76)	(0.30)	(0.66)
Extra Funding	-0.20	0.19	-1.09**	-0.55	-0.07	0.33	0.58	0.10	0.60
× 3rd Decile EC	(0.37)	(0.36)	(0.54)	(0.55)	(0.19)	(0.29)	(0.43)	(0.26)	(0.37)
Extra Funding	-0.04	0.16	0.57	0.01	0.02	-0.15	-0.35	0.07	-0.40
× 4th Decile EC	(0.39)	(0.35)	(0.48)	(0.55)	(0.20)	(0.28)	(0.49)	(0.20)	(0.34)
Extra Funding	-0.11	-0.01	-0.26	-0.24	-0.23	-0.33	0.82	-0.00	0.28
× 5th Decile EC	(0.41)	(0.40)	(0.53)	(0.56)	(0.16)	(0.26)	(0.50)	(0.20)	(0.42)
Extra Funding	0.69*	-0.06	-0.36	0.49	0.05	0.19	-0.27	-0.20	0.10
× 6th Decile EC	(0.37)	(0.40)	(0.48)	(0.48)	(0.24)	(0.24)	(0.43)	(0.21)	(0.38)
Extra Funding	-0.14	0.03	-0.01	0.26	-0.07	0.13	-0.25	-0.02	-0.40
× 7th Decile EC	(0.35)	(0.34)	(0.48)	(0.49)	(0.21)	(0.26)	(0.40)	(0.16)	(0.33)
Extra Funding	-0.26	0.18	0.71	0.01	-0.08	-0.15	-0.38	0.02	0.25
× 8th Decile EC	(0.38)	(0.35)	(0.44)	(0.52)	(0.16)	(0.25)	(0.38)	(0.26)	(0.35)
Extra Funding	0.46	-0.05	0.13	-0.12	-0.30*	-0.20	-0.41	0.22	-0.05
× 9th Decile EC	(0.33)	(0.38)	(0.44)	(0.47)	(0.16)	(0.22)	(0.38)	(0.28)	(0.34)
Extra Funding	0.41	-0.08	0.34	-0.04	-0.22	0.17	0.01	0.27	-0.17
× 10th Decile EC	(0.31)	(0.30)	(0.44)	(0.43)	(0.19)	(0.26)	(0.41)	(0.19)	(0.33)
1st Decile EC	-12.13**	-12.53***	-1.85	-3.38	5.13	0.30	0.18	4.31*	-0.91
	(5.29)	(4.73)	(6.04)	(6.14)	(3.26)	(3.59)	(5.35)	(2.44)	(4.07)
2nd Decile EC	-0.48	-7.66	0.50	2.33	3.01	9.29*	-5.06	4.73	-3.23
	(6.16)	(5.89)	(7.74)	(7.84)	(4.38)	(5.16)	(6.57)	(2.93)	(5.32)
3rd Decile EC	7.56	-3.86	18.90***	10.93	-2.63	-3.06	-12.08**	4.59	-9.46**
	(5.14)	(4.70)	(6.27)	(6.69)	(2.80)	(3.53)	(5.35)	(2.85)	(4.09)
4th Decile EC	4.32	-2.49	3.16	0.91	-3.81	-0.20	1.40	2.94	-1.52
	(5.02)	(4.38)	(6.32)	(6.48)	(2.46)	(3.86)	(6.00)	(2.48)	(4.36)
5th Decile EC	3.92	-1.53	5.47	5.65	-2.82	1.31	-5.01	0.77	-1.85
	(5.20)	(4.43)	(6.46)	(6.67)	(2.69)	(3.69)	(5.78)	(2.11)	(4.56)
6th Decile EC	-4.71	-5.12	9.70	-1.28	-2.27	-2.69	1.80	4.29*	-2.61
	(5.34)	(4.49)	(6.12)	(6.16)	(2.85)	(3.42)	(5.65)	(2.52)	(4.24)
7th Decile EC	2.85	-3.92	4.47	-2.28	0.01	-0.80	-1.71	1.84	-0.19
	(5.06)	(4.51)	(6.27)	(6.30)	(3.06)	(3.58)	(5.66)	(2.30)	(4.38)
8th Decile EC	2.82	-6.01	1.46	4.91	-1.15	0.64	-6.35	4.86*	-4.28
	(5.05)	(4.53)	(6.16)	(6.46)	(2.79)	(3.72)	(5.56)	(2.87)	(4.19)
9th Decile EC	-1.93	-4.57	3.20	1.97	-0.41	-1.38	1.55	4.75*	-2.74
	(4.98)	(4.37)	(6.12)	(6.23)	(2.79)	(3.55)	(5.52)	(2.77)	(4.25)
N	2718	2718	2718	2718	2718	2718	2718	2718	2718
R^2	0.18	0.12	0.11	0.12	0.12	0.12	0.13	0.10	0.11
Mean of Dependent Variable	79.54	83.55	67.66	48.31	4.82	7.91	22.70	5.30	11.52

Table reports how additional funds affect occupational choices of survey respondents. The dependent variables are whether the respondent joined TFA (column 1); whether the respondent was teaching in the fall when they would have joined TFA, 2 years later, and 10 years later (columns 2, 3, and 4, respectively); whether the respondent was working in the private sector in the fall when they would have joined TFA, 2 years later, and 10 years later (columns 5, 6, and 7, respectively); and whether the respondent was a graduate student in the fall when they would have joined TFA and 2 years later (columns 8 and 9, respectively). The "10 years later" responses are prospective, as are the "2 years later" responses for the 2016 and 2017 cohorts. See footnote 19 in the main text. Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.10$, 0.05, and 0.01, respectively. The estimates come from regression specification 2b in the main text. "Private Sector" occupations include Banking/Finance, Consulting, Publishing/Journalism/Media, Law, Engineering/Technology, or Other Business (e.g., Marketing or Real Estate). All regressions include demographic controls: a linear age term, dummies for race, gender, assigned region, whether the applicant was assigned to his or her most preferred region, whether the applicant was assigned to his or her most preferred subject, and a linear term for the applicant's "fit" with TFA (described in footnote 12 of the main text). All regressions include fixed effects for the batches in which applicants' TGL awards were processed, the point at which randomization occurred ("Batch FEs").

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