Liquidity and Job Choice*

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November 14, 2018

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 randomize incremental increases in packages offered to nearly 7,300 potential teachers who apply for funding to support their transitions into teaching. The first two years of the experiment reveal that most applicants do not respond to a marginal \$600 of grants or loans, but the highest need applicants—those in the worst financial position—become teachers at much higher rates when provided with additional funding. We continue the experiment into the third year and self-replicate our results, generating point estimates nearly identical to the results from the first two years. The effects are large. 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. That additional grant and loan dollars are equally effective suggests a liquidity mechanism, a hypothesis that is bolstered by follow-up survey evidence. Survey results also show that individuals pulled into teaching by the additional funding would have otherwise worked in private sector firms. Providing liquidity to finance a transition into teaching could be a powerful tool to address the U.S. teacher shortage.

*The authors thank their many partners at Teach for America, as well as Katherine B. Coffman, Tatiana Homonoff, Simon Jäger, Lisa Kahn, Charles Sprenger, and seminar participants at the Advances in Field Experiments conference.

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 facing additional expenses associated with moving and getting ready to teach.[2] 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.[3]

In this paper, we investigate the role of liquidity on 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).[4] TFA draws many of its potential teachers from highly selective colleges and universities. Given the competitiveness of those admitted to TFA, one might expect that they are not subject to liquidity constraints; consequently, finding these constraints

[1] For example, the transition into teaching—the focus of this paper—is unlikely to be supported by such benefits. A summary of 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] Liquidity might be needed to: secure a new residence (possibly requiring multiple months of rent and a security deposit), move, buy or lease a car, pay for preparatory coursework, pay for state licensing tests, buy a new wardrobe, and buy classroom materials not provided by the school.

[3] See Brown, Scholz, and Seshadri (2011), Gross and Souleles (2002), and Johnson, Parker, and Souleles (2006). 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. In addition, 10% (and 7% of college graduates) either reported that in the past year they had a loan application rejected or were "discouraged borrowers," meaning they did not apply for a loan they needed because they believed such an application would be rejected. Other studies, albeit not focusing on college graduates, have found that 20% of Americans are credit constrained and that those who are credit constrained are poorer and younger than those who are not (e.g., Hayashi 1985; Jappelli 1990; Zeldes 1989).

[4] Our TFA partners describe TFA as focused on "creating systemic change to ensure educational equity in communities where opportunities are limited" and describe the TFA model as finding "promising leaders that commit to teach for at least two years to advance the academic and personal growth of students and help strengthen schools" with the goal that TFA teachers will "cultivate leadership skills and foster a community" that will help them make "systemic change throughout their career".

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 for TFA.[5] Applicants who accept the funds from TFA receive them in late May or June to cover costs faced during the summer before they begin teaching, during which they participate in a TFA-administered training program.[6] 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.

Our experiment randomly varies the grant and loan packages offered to TGL program applicants. In our main treatments, applicants either receive a control package or a package that randomly includes an additional $600 in loans or an additional $600 in grants. These "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% of applicants who are 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. As described in detail in Section 3, halfway through the second year of our experiment, TFA increased the TGL budget, allowing us to add a treatment that provided an additional $1,200 in grants. Again, this treatment had no effect on the ma-

[5]Grants do not need to be repaid if the applicant joins TFA and is teaching on October 1 of the first year of the program. Loans are scheduled to be paid off in 18 equal payments starting 6 months into the two-year teaching commitment with TFA.

[6]The existence of the TGL program reflects TFA leadership's belief that such a program is valuable. However, the existence of their program does not guarantee that providing liquidity through such a program is necessary. TFA has both reputational and altruistic reasons to offer a TGL program to support its teachers, even if its teachers could access credit markets on their own. The composition of TGL awards suggests that TFA leadership prefers their teachers have less debt. In particular, TFA puts a cap on the loans that it provides as part of the TGL program and provides all additional funding with grants. Conversations with TFA leadership suggest they believe lower monthly payments will make TFA more "financially sustainable" for teachers, keeping teachers happy and engaged during and after the program.

jority of applicants, but had a large positive effect on the highest need applicants. These large treatment effects arise even though the highest need applicants are offered substantial grant and loan packages (averaging around $5,000 per highest need applicant) in the control group.

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 an opportunity to "self-replicate" our results from the first two years.[7] This self-replication succeeded, generating results nearly identical to results from the first two years.

Across the three years of the experiment, we estimate that providing an extra $600 in loans, $600 in grants, or $1,200 in grants increase the likelihood the highest need applicants become teachers for TFA by 12.2, 11.4, or 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.

The results strongly suggest that our treatments work by providing liquidity to the highest need applicants. Providing additional funding in loans is just as effective as providing funding in grants, even though loans need to repaid over the course of the TFA program and grants do not. This result suggests that applicants respond to the liquidity provided by our treatments rather than to the implicitly higher compensation associated with the grant funding, which would predict the effect of grants to be larger than that of loans. As discussed in Section 4, null results from other applicants in our experiment—including applicants in the third year of the experiment randomly offered an additional $1,800 in grants—suggest that less needy applicants also do not respond to the effectively higher compensation in our grant treatments.

For liquidity to be the mechanism driving our treatment effects, the highest need applicants must have unmet liquidity need after receiving the TGL award, exhausting their cash-on-hand, and accessing credit markets; while the less needy applicants must be able to fund their transitions from these sources. Evidence suggests these conditions are met. First, as described in Section 2, a kink in TFA's formula to determine TGL funding creates a wedge between the estimated need of the highest need applicants and what the TGL program provides, which could cause the highest need applicants to have larger residual liquidity needs than the less needy

[7] Existing methods to address heterogeneous treatment effects include committing to a pre-analysis plan ex ante and correcting for multiple hypothesis testing (see, e.g., Katz, Kling, and Liebman 2001) ex post. We take a different approach and self-replicate our field experiment. As described in detail in Section 3, in the third year, we also introduced treatment variation of $1,800 of grants and $1,800 of loans for the less-needy applicants in order to stress test the null result observed in that group in the first two years.

applicants. Second, in a post-experiment survey of the applicants in our experiment, 60.8% of the highest need applicants randomized to our control group report having unmet need after exhausting cash-on-hand and their TGL award. This group additionally reports having limited access to formal and informal sources of credit. For example, among the applicants who said they had unmet need, 88% reported applying for a credit card (or credit limit increase), applying for a bank loan (or increase in a bank loan), or asking friends or family for a personal loan or gift; of those who applied or made such requests, 27.3% report being denied. The high rate of applying for credit among the highest need group suggests that applicants are aware of credit opportunities and not averse to taking on debt, but are instead credit constrained.[8] In contrast, less needy applicants appear to have more access to credit markets.

The post-experiment survey also reveals that applicants induced to become teachers for TFA by our treatments would have otherwise ended up in private sector jobs (i.e., not in other teaching positions or in other public service jobs). This finding reveals that additional funding not only generated more teachers for TFA, but also more teachers overall. It also suggests that liquidity needs may be a barrier that prevents workers from choosing jobs in public service.[9]

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).[10] 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. In addition, given the importance of teacher quality to student outcomes, policy makers are increasingly concerned about attracting high quality teachers into the profession.[11] Our setting allows us to observe that liquidity constraints bind for even the high-ability

[8]Consistent with awareness of credit opportunities among this group, 87.0% of the highest need applicants have credit card debt and average credit card debt is $6,500. Indeed, this relationship is somewhat mechanical since credit card debt contributes to TFA's estimate of financial need.

[9]While we find that liquidity needs prevent people from taking public service jobs, liquidity needs might also prevent workers from taking certain jobs in the private sector, such as jobs that begin as unpaid internships. These jobs have additional transition costs, as they require workers to fund living expenses until they are hired for pay.

[10]The last presidential administration was particularly concerned with identifying more teachers and more teachers of color. In 2010, the Obama administration launched the TEACH initiative, whose mission, according to then-Secretary of Education Arne Duncan, was "to increase the number, quality, and diversity of teachers in the classroom as we see the baby boomers retiring over the next ten years" (Duncan 2011). In 2016, then-Secretary of Education John B. King Jr. said, "[W]hen the majority of students in public schools are students of color and only 18 percent of our teachers are teachers of color, we have an urgent need to act." (Hull 2017).

[11]For evidence that teacher quality improves student outcomes, see Chetty, Friedman, and Rockoff (2014), Lankford, Loeb, and Wyckoff (2002), Aaronson, Barrow, and Sander (2007), Rivkin, Hanushek, and Kain (2005), and

individuals who are accepted into the competitive TFA program. Our estimates suggest that increasing no-interest loans to well-targeted applicants (i.e., applicants in the highest need group) can cost TFA as little as $186 per teacher in expectation.[12] 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.[13] By mitigating an existing market friction, such a program could simultaneously help both firms and potential workers in these industries.

Along with their policy implications, our results also make contributions to three related literatures. First, we add to an existing literature on how liquidity constraints affect important life decisions, such as consumption choices and educational investments.[14] In particular, we provide the first experimental evidence on the effect of liquidity constraints on job choice, especially with respect to public service.[15] We find that liquidity constraints can have a big impact on job choice, even for high ability college graduates. That we find our results among recent college graduates means that such liquidity constraints could have life-long consequences. Oreopoulos, Von Wachter, and Heisz (2012) and Kahn (2010) both show that first jobs are quite important for future outcomes: graduating in a weak economy can have substantial short-run effects and smaller, but persistent, long-run effects on careers and earnings. Altonji, Kahn, and Speer (2016) suggests that these lingering effects can be due to initial job mismatch.[16] If liquidity constraints also lead to initial job mismatch, they may have long-term impacts. The closest work related to how finances affect job choice is work on unemployment insurance (UI), which necessarily focuses on older workers whose decisions are on the margins of both unemployment duration and

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Rockoff (2004) among others.

[12]This estimate assumes full payback and a 3% interest rate. Additional details of this calculation are presented in Section 7, after we report on the relevant empirical results.

[13]Existing programs (and programs provided in recent years) designed to attract and retain teachers—such as the TEACH Grant Program, California's Governor's Teaching Fellowship (Steele, Murnane, and Willett 2010), the Massachusetts Signing Bonus Program (Liu, Johnson, and Peske 2004), Florida's Critical Teacher Shortage Program (Feng and Sass 2018), and the North Carolina Bonus Program (Clotfelter et al. 2008)—focus on providing loan forgiveness or salary support (usually targeted towards high-performing teachers, teachers who teach certain subjects, or teachers who work in high-needs schools), rather than solving the specific need of providing short-term liquidity for individuals considering joining the teaching profession.

[14]For evidence on the impact of liquidity constraints on consumption, see Agarwal, Liu, and Souleles (2007) and Johnson, Parker, and Souleles (2006). For a review of the role of liquidity constraints in decisions about education, see Lochner and Monge-Naranjo (2012).

[15]As discussed below, Field (2009) uses random variation in order to investigate the importance of debt aversion—rather than liquidity concerns—on job choice.

[16]Students who are forced to take first jobs that are mismatched on training or interest are estimated to suffer longer-term consequences than those who take jobs that are a better match.

job choice. This work finds evidence that liquidity can indeed affect unemployment duration.[17] However, there does not seem to be consensus on whether the liquidity provided by UI affects post-unemployment earnings or job match quality.[18]

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. Rothstein and Rouse (2011) shows that a policy change that replaces loans with grants for students at a selective university leads more students to take lower-paying jobs in the "public interest." Similarly, Zhang (2013) shows that loan burden among recent college graduates predicts lower rates of taking low-paying jobs and matriculating into graduate school. As Rothstein and Rouse (2011) notes, there are two leading explanations for these findings. First, students may be debt averse and take higher-paying jobs to avoid taking on further debt or to pay off their existing debt as quickly as possible.[19] Second, loans may affect job choice because some students are credit constrained and take jobs that give them liquidity immediately (e.g., that provide a more immediate first paycheck or offer a signing bonus) in order to make ends meet while they are making payments against their student loans.[20] While both of these channels can be active simultaneously, our results provide evidence that liquidity constraints are a first-order concern for at least a subset of job seekers.[21] While we speak to this literature, we cannot directly assess the effect of student loans on job choice since all TFA teachers are eligible to put federal loans into forbearance while teaching for TFA.

Third, we add to a literature on the efficacy of programs designed to encourage individuals to join or remain in the teaching profession. Previous work has focused on the effect of increas-

[17] Chetty (2008) finds that both larger UI and severance pay increase unemployment duration, with much larger effects for households that are particularly liquidity and credit constrained.

[18] 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, based on measures of match quality and salary; 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 than those with less access. See also Centeno and Novo (2006), Ours and Vodopivec (2008), and Addison and Blackburn (2000), which includes a helpful review of earlier studies.

[19] Field (2009) provides evidence of this channel: using a randomized experiment at NYU law school that framed otherwise identical financial packages as either tuition assistance or a loan, students in the loan treatment (i.e., perceived to be "in debt") were less likely to take low-paying, public-sector jobs. Further, Burdman (2005) and Callender and Jackson (2005) provide anecdotal evidence of debt aversion, particularly among the most financially disadvantaged.

[20] Rothstein and Rouse (2011) finds that debt decreases graduates' donations to their university and argues that this finding is more consistent with credit constraints than debt aversion.

[21] As presented in Section 5.2, the results from our experiment and our post-experiment survey are inconsistent both with debt aversion and with a lack of awareness of credit opportunities.

ing teacher compensation in various ways, including analyzing the effect of providing retention bonuses (Clotfelter et al. 2008), of offering incentives for teaching in under-performing schools (Steele, Murnane, and Willett 2010), and of providing signing bonuses to high-performing college students (Liu, Johnson, and Peske 2004). Our results suggest that part of the efficacy of providing funds to prospective teachers may work through a liquidity channel, helping individuals finance their transitions into the profession. Our results provide evidence that such policies could be made more effective by targeting them towards teachers with credit constraints and suggest timing the provision of these funds to when transition costs are incurred.

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. 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.

2 Setting: TFA and the TGL Program

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 in 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") held in a city near the school district where they have been assigned to teach. The school year begins around the start of September, and TFA teachers are meant to remain in the program for two school years. Roughly half of TFA alumni continue in the profession, teaching at least three years in addition to their initial two-year commitment. Admission to TFA is very selective; TFA recruits its teachers from highly ranked colleges and universities across the United States.[22]

[22] 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 transitioning into teaching—particularly those costs that teachers must incur before receiving their first paycheck—TFA offers a Transitional Grants and Loans program to which prospective TFA teachers can apply.[23]

TGL applicants must complete an extensive application, which requires them to provide portions of their federal tax returns or portions of their Free Application for Federal Student Aid (FAFSA) forms, pay stubs if they are working, information about any dependents, and documentation of their checking and savings accounts and any debts they have. Applications to the TGL program are submitted on a rolling basis. Award offers are calculated, and information about award offers is dispersed to applicants, in approximately weekly batches.

The TGL program offers a personalized package of grants and loans to each applicant. The package depends on two key variables. The first is the applicant's "expected expenses," which is a function of the cost of living in the city or region where the applicant has been assigned to teach, the location of the Summer Institute they have been assigned to attend, and whether the applicant must move to a new city to begin the assigned teaching job. The second is the applicant's "expected contribution" (EC), which is a function of an applicant's cash-on-hand (i.e., funds in checking and savings accounts); credit card and other debts (excluding federal student loans); income (if working); the amount of financial support the applicant received from parents for educational expenses; the applicant's number of dependents; and whether the applicant is about to graduate college or is changing careers. While we are not permitted to share the specific function that is used to calculate EC, Online Appendix Table A1 reports on how much variation in EC each component listed above can explain. The amount in checking and savings accounts (i.e., cash-on-hand) is by far the most important factor.

The size of the TGL package offered to applicants—the sum of grants and loans, called the "total award"—is equal to expected expenses minus expected contribution, with a caveat that introduces a kink in the award schedule. TFA's algorithm sets a maximum award at expected expenses, which binds whenever EC ≤ \$0. In particular, having negative EC does not generate higher awards than having EC = \$0; instead, all applicants with EC ≤ \$0 are offered their expected expenses. Roughly 10% of TGL applicants included in our experiment in each year had EC ≤ \$0. This kink in the award schedule gives us a reason to believe applicants in this "1st

[23] These costs include: living expenses during the summer, travel and other costs associated with attending Summer Institute, moving expenses, and the costs of taking mandatory state certification exams.

decile" of EC will have more unmet liquidity need than others in the experiment. We pay special attention to this decile in the analysis that follows.[24]

A package offered to a TGL applicant was offered as a specific combination of grants and loans. Grants did not need to be repaid if participants were teaching on October 1st of the year they joined TFA; otherwise, they needed to be repaid in full. Loans were offered at a 0% interest rate and were expected to be repaid in 18 equal monthly payments starting six months after an applicant began teaching for TFA.[25] The amount of the total award that was comprised of loan funding was determined by a personalized loan cap based on financial need and was always at or below a maximum loan amount set by TFA.[26] TFA would offer as much as possible in loans up to this personalized cap. If total award exceeded the personalized cap, TFA would offer the balance of the award as grants. During the three years of our experiment, described below, TFA offered its TGL applicants an average of $5.5 million a year in grants and $6.2 million a year in loans.

3 Experimental Design

Our experiment was embedded into the TGL program for three years and included 7,295 TGL applicants who were eligible to begin teaching in the fall of 2015, 2016, or 2017. For the years of our experiment, we used TFA's algorithm to construct a base award for each applicant.[27] This base award is the award that would be offered to an applicant if she were randomized into our control group.

Figure 1 summarizes base awards by showing the distribution of grants, loans, and total awards across the three years of our experiment. These base awards are often quite substantial:

[24]TFA deemed any applicant with EC $\geq$ 80% of expected expenses "grant ineligible" and offered the applicant an award, comprised entirely of loans, equal to 20% of expected expenses. Roughly 15% of TGL applicants in each year meet this criteria. Given that they could not receive grants, they were excluded from our experiment. As discussed in the "Grant-Ineligible Applicants" section of the Online Appendix, during the first two years of our experiment, we ran a separate mini-experiment on these applicants, which varied the size of the loans this group was offered. Additional loan funding did not affect whether these grant ineligible applicants joined TFA; in response, during the third year of our experiment, TFA offered all "grant ineligible" applicants $500 in loans, regardless of expected expenses.

[25]Applicants who failed to make on-time payments were put on adjusted, personalized repayment plans.

[26]This maximum, which varied across years of the experiment, reflected TFA leadership's concern about overburdening applicants with debt.

[27]In the years of the experiment, total awards (calculated in the manner described above) were 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.

the mean of grants offered and the mean of loans offered are each roughly $2,000. Everyone in the experiment is offered at least $500 in loans in their base award, and the total award offered 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 base awards offered by decile of EC. Applicants with lower EC receive substantially larger offers—and grant money comprises a larger proportion of their offers—than those with higher EC. For example, applicants in the 1st decile of EC (i.e., those with the lowest EC and the highest estimated need) are offered base awards of almost $5,000 on average, while applicants in the 10th decile of EC (i.e., those with the highest EC and the lowest estimated need) are offered base 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 offered the base award. Applicants in the two treatment groups received a total award that was $600 more than the base award. In the *$600 Grant* treatment, this additional $600 came in the form of grants, while in the *$600 Loan* treatment, this additional $600 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, 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 base 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 assuage concerns that typically accompany the reporting of heterogeneous treatment effects, we ran the experiment for a third year to self-replicate our positive treatment effects and to stress test our null results.

In particular, in the third year of the experiment, we left the treatments unchanged for applicants in the 1st and 2nd deciles of expected contribution. While our results from the first two years only appeared in the 1st decile of expected contribution, we chose to continue the experiment with both the 1st and 2nd deciles to see if we could replicate the differential treatment effects across those deciles. In addition, we designed a stress test of the null results found for the rest of the experimental population by increasing the experimental variation. In particular, in the third year, 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 base award—an *\$1800 Grant* treatment or an *\$1800 Loan* treatment—each with one-third probability. This variation was quite large, even relative to the control packages offered; the \$1,800 treatments increased the average total award offer by 59%.[28]

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 batch of applicants for TGL award processing. Since the point of randomization is the batch, all analysis conducted in Section 4 includes fixed effects for processing batch. These fixed effects also control for any potential differences in the applicant pool that might arise across years of the experiment or over time within each year 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.[29] However, the award offer

[28]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, EC = \$220, to sort applicants into the two versions of the experiment in the third year.

[29]Empirically, most applicants who join TFA accept the entire award offered. Nearly all (98%) of applicants accept the entire grant offered to them and over 80% of applicants accept the entire loan offered to them. Those who choose not to accept the entire loan or grant almost always accept none of it (only 0.5% take a partial grant and only 3.2% take a partial loan).

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 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 grants and loan treatments—and of offering $600 in higher effective earnings—provided by the grant treatment only.

If applicants do not need access to this $600 of liquidity from TFA, and if $600 of extra earnings is not enough to induce applicants to join TFA, then neither of the treatments will have an effect. If applicants respond to the $600 of extra earnings but do not need the $600 of liquidity provided by TFA, then the grant treatment will have an effect but the loan treatment will not. If applicants need the $600 in liquidity but do not respond to the $600 of extra earnings, then the loan and grant treatments will have similar effects. Finally, if applicants need the $600 in liquidity and respond to the $600 of extra earnings, then both the grant and loan treatments will have an effect, but the effect of grants will be larger. These hypotheses are summarized in Table 2.

TABLE 2 ABOUT HERE

In the next section, we evaluate the effect of $600 in grants and $600 in loans to assess which of these hypotheses best fit our data and use our additional experimental treatments to further explore these hypotheses.

4 Results

4.1 Summary Statistics and Balance

Our experimental sample consists of 7,295 admitted applicants to Teach for America who also applied to the Transitional Grants and Loans program in anticipation of beginning teaching in the fall of 2015, the fall of 2016, and the fall of 2017.

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.[30] Randomization was successful overall and in relevant deciles of expected contribution: Online Appendix Table A2 reports p-values of balance tests on our demographic characteristics without 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."[31]

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 and then present the results from the third year in Sections 4.3 and 4.4.

How did the treatments affect the likelihood that applicants began teaching for TFA in the first two years of our experiment? Figure 3 shows the treatment effects on joining TFA, first across all applicants and then by decile of expected contribution.[32] 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

[30] Roughly 6,000–7,000 applicants were admitted to TFA in each of the years of our experiment, of which approximately 40% apply to the TGL program. In addition, our experiment excludes the least needy 15% of TGL applicants in each year that TFA deems grant ineligible (see footnote 24). Consequently, our applicants are high need relative to the population of individuals admitted to TFA.

[31] We only include an applicant in our experiment the first time the applicant applies for TGL funding during the years of our experiment. An applicant who applies for TGL in one year but then defers and re-applies for TGL funding in a later year is counted as not becoming a teacher. Only 2.0% of applicants are deferrals who reapply for TGL funding in a subsequent year of our experiment. Because so few applicants defer, our results are virtually unchanged when we included deferrals who re-apply (and are thus re-randomized) in multiple years of the experiment.

[32] 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.

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 ($p = 0.015$), and the effect of the *\$600 Grant* treatment is 9.7 percentage points ($p = 0.047$).[33] No other deciles show significant effects of either the *\$600 Loan* treatment or the *\$600 Grant* treatment.

These results suggest that additional grants and loans have an effect on applicants in the 1st decile of expected contribution but nowhere else in the distribution of EC. As discussed in Section 2, the formula that TFA uses to generate award packages introduces a kink when EC = \$0, such that the awards TFA offers do not get more generous as EC decreases from \$0, even as TFA estimates that an applicant's need is larger. As shown in Online Appendix Figure A1, the 1st decile of expected contribution is almost exactly the group with EC ≤ \$0 that is affected by this kink (the 10th percentile of EC is \$4.20). Consequently, we might expect results to differ for the 1st decile of expected contribution since—if TFA's estimate of EC is indeed a good estimate of an applicant's available resources to fund her transition into teaching—applicants in that decile may have a particular need for funds, even after receiving their (often substantial) control award. Consistent with this explanation, Figure 4 shows the percentage of applicants in the control group who join TFA in the first two years of the experiment, first overall and then by decile of expected contribution. Only 61.5 percent of applicants in the control group of the 1st decile join TFA, a rate that is substantially lower than the 74.3 percent of applicants in the pooled control groups of the 2nd–10th deciles who join TFA in the same two years ($p = 0.002$).

FIGURE 3 ABOUT HERE

FIGURE 4 ABOUT HERE

Table 4 reports estimates of the effects of grant or loan money during the first two years of the

[33] The small difference between the loan treatment and the grant treatment in the 1st decile is not statistically significant ($p = 0.613$).

experiment, including data from all treatments, from regression specifications (1a) and (1b).

$$Join\,TFA_i = \beta_G \cdot Extra\,Grants_i + \beta_L \cdot Extra\,Loans_i + \sum_j \gamma^j \cdot Batch_i^j + \boldsymbol{\delta} \cdot \mathbf{X}_i + \varepsilon_i, \tag{1a}$$

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

In those specifications, $Join\,TFA_i$ is a dummy for whether applicant i is teaching for TFA on the first day of school, $Extra\,Grants_i$ 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, or 12), 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 or 6). Each $Batch^j$ denotes a batch of applicants in the TGL program, which is the level at which randomization into treatment occurred, and so $Batch_i^j$ is a dummy for applicant i being in $Batch^j$. In some specifications, we include a vector of demographic controls, $\mathbf{X}_i$.[34] In regression specification (1b), $Decile^d$ are deciles of expected contribution, and so $Decile_i^d$ is a dummy for applicant i being in $Decile^d$.

In regression specification (1a), the coefficients of interest are β_G and β_L. In regression specification (1b), the coefficients of interest are those same coefficients for each decile, β_G^d and β_L^d, where d indicates decile. Each β_G is the estimated treatment effect of offering an additional \$100 in grants, and each β_L is the estimated treatment effect of offering an additional \$100 in loans. These coefficients are estimated under a linearity assumption that each additional \$100 of grants is equally effective and, similarly, each additional \$100 of loans is equally effective. This linearity assumption allows us to combine variation across treatments of various levels.

Column 1 of Table 4 shows results from regression (1a) and column 3 shows results from regression (1b), without the vector of demographic controls (i.e., without $\mathbf{X}_i$), reporting coeffi-

[34] 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, 103 obs.; race, 10 obs.; and fit, 2 obs.).

cients for the 1st decile and suppressing the rest. Columns 2 and 4 report the results of these regression specifications when the demographic controls are included.[35]

TABLE 4 ABOUT HERE

Looking at the full sample 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 estimated effects for both grants and loans are directionally larger (with $p = 0.002$ 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 for either grants or loans, regardless of whether demographic controls are included.[36]

Reporting subgroup treatment effects comes with reasonable concerns, as the dimension and definition of subgroups provides many degrees of freedom for the researcher. As discussed in Section 3, to rule out the possibility of spurious results, we used the third year of the study to replicate our experiment for the 1st and 2nd deciles.[37] For the 3rd–10th deciles, we increased the experimental variation to \$1,800 to stress test our null results from those deciles. The results from the third year of the experiment are presented in Sections 4.3 and 4.4 and are followed by an analysis of the pooled results in Section 4.5.

4.3 Joining Teach for America: Replication (2017)

Results from the third year experiment replicate the results from the first two years. Figure 5 compares the estimated treatment effects (including the \$1200 Grants treatment) among applicants in the 1st and 2nd deciles of EC in the first two years of the experiment (2015–2016) to

[35]Regressions that report the full set of coefficient estimates from regression equation (1b), both with and without controls, are in Online Appendix Table A3.

[36]Additional regressions which pool the 2nd–10th deciles (not reported), reveal that the treatment effect for grants and the treatment effect 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).

[37]While our results from the first two years only appeared in the 1st decile of expected contribution, we continued the experiment with both the 1st and 2nd deciles to see if we could replicate the differential treatment effects across those deciles.

the estimated treatment effects among applicants in those deciles during the third year of the experiment (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$).[38]

FIGURE 5 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 6 shows the results by treatment and decile of EC. Looking across the deciles, we see no systematic pattern with respect to treatment and the percentage of applicants who join TFA.[39] 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.

FIGURE 6 ABOUT HERE

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. Table 5 reports the results of regressions estimated using the specifications in (1a) and (1b)—reporting coefficients for the 1st decile and

[38] The latter estimate is about twice as large as those estimated for the two $600 treatments, although we cannot reject that it is equal to either group.

[39] The *$1800 Loan* treatment is statistically significant for the 8th decile ($p = 0.049$); the *$1800 Grant* treatment is never statistically significant ($p > 0.1$ for all tests).

suppressing the rest.[40] Columns 1 and 2 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 3 and 4, however, show that if we interact additional grants and loans with decile of expected contribution, there are large, statistically significant effects of both grants and loans in the 1st decile. The most precise estimates (from column 4, 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.[41]

TABLE 5 ABOUT HERE

While the regression specifications make linearity assumptions with regard to the effect of additional grant and loan funding, results are consistent in the absence of that assumption. Figure 7 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. 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.

FIGURE 7 ABOUT HERE

5 Liquidity Mechanism

Results from Section 4 reveal that the applicants in the 1st decile of expected contribution respond dramatically to additional grant and loan funding while applicants in the other deciles

[40] In the pooled data, $ExtraGrants_i$ and $ExtraLoans_i$ can both take the value of 18 (i.e., $ExtraGrants_i$ is either 0, 6, 12, or 18, and $ExtraLoans_i$ is either 0, 6, or 18). Full results of the specification in equation (1b), with and without controls, are shown in Online Appendix Table A3.

[41] As in the first two years, additional regression results reveal that the treatment effects for grants and for loans among applicants in the 1st decile are statistically significantly larger than the (null) effects observed in the 2nd–10th deciles, both with and without controls ($p < 0.01$ for all tests). As shown towards the bottom of Table 5, including all three years and looking across all other 9 deciles—eighteen hypothesis tests—only one treatment effect is statistically significant (the effect of grants for the 6th decile of expected contribution).

do not. This pattern of results is best explained by a liquidity mechanism. In this section, we first describe the results that lead us to that conclusion and then address—and rule out—other potential hypotheses.

First, our experiment finds that grants and loans are equally effective in inducing applicants in the 1st decile of EC to join TFA, even though loans need to be repaid over two years and grants do not. As outlined in Section 3.1, this pattern of results suggests a liquidity mechanism. It also rules out the other alternative hypotheses that will be discussed in Section 5.3.

For liquidity to be the mechanism driving the grant and loan treatment effects in the first decile, however, it must be the case that applicants in the 1st decile have unmet liquidity needs after exhausting their cash-on-hand, accessing credit markets, and receiving the TGL control award. In addition, it must be the case that the additional funding provided by the treatments meets that need. For the same liquidity mechanism to explain the null results in the other deciles, it must also be the case that applicants in the 2nd–10th deciles do not have liquidity need in excess of exhausting their cash-on-hand, accessing credit markets, and receiving the TGL control award. In the following subsections, we present evidence that these conditions are met. First, in Section 5.1, we present a visual framework for demonstrating the liquidity mechanism and consider the role of the kink in the TGL algorithm (see Section 2) in driving differences between the 1st decile of EC and the 2nd–10th deciles. Second, in Section 5.2, we report on survey responses about financial need and credit access that we collected in a post-experiment survey in May 2018. Results from this survey also support the liquidity mechanism.[42]

5.1 Visual Framework

In Section 2, we described the algorithm that TFA uses to determine total award and highlighted that it introduces a kink when expected contribution drops below zero. We now use that award schedule to help articulate how the liquidity mechanism could explain our pattern of treatment effects.

FIGURE 8 ABOUT HERE

Figure 8 plots how total award (on the vertical axis) responds to expected contribution (on the horizontal axis). For simplicity, we have drawn the picture for only one level of expected expenses

[42]Survey design and implementation are described in the appendix. The appendix also assesses—and addresses—potential concerns about sample selection in the survey responses.

so we can draw a one-to-one relationship between expected contribution and total award.[43] The "Control award" solid line shows the total award level that applicants would be offered if randomized into the control group, the "$600 Treatment award" dashed line shows the total award level in the $600 treatment groups and the "$1200 Treatment award" dotted line shows the total award level in the $1200 treatment group.[44] Notice that, looking from right to left, the award lines increase one-for-one as expected contribution decreases, until expected contribution reaches zero, and then the award lines become flat.

To show the predictions of a liquidity mechanism in this graph, we plot the amount of residual liquidity need that hypothetical TGL applicants require to make the transition into teaching. In particular, we plot need twice, once (marked with a circle and labeled "Pre-credit liquidity need") indicating liquidity need before the applicant accesses credit markets and so reflecting need after exhausting cash-on-hand (i.e., funds in checking and savings accounts), and once (marked with a dot and labeled "Post-credit liquidity need") indicating liquidity need after the applicant also accesses formal and informal credit markets (i.e., additional liquidity from credit cards, bank loans, friends or family, etc.). Since the amount of liquidity that can be accessed in credit markets is always weakly positive, the dot is always weakly below the circle, indicating less liquidity need. A hypothetical applicant who is fully credit constrained (i.e., who cannot access any liquidity on credit markets) will have the dot inside the circle and will thus be marked with a $\odot$.

What does it mean for our treatment effect to be positive in the 1st decile and zero in the 2nd–10th deciles? First, it means that for the 1st decile (i.e., roughly those with EC $\leq$ \$0), there are dots between the control and treatment award lines. That is, there are applicants who—after exhausting their cash-on-hand and accessing credit markets—do not have enough liquidity to make the transition into teaching when they are offered the control award but do have enough when they are offered the treatment award. Such a pattern requires at least some applicants in the 1st decile to have pre-credit liquidity need circles that are above the control award line (and not enough access to credit for their post-credit liquidity dots to fall below the control award line). Second, it means that there are very few dots between the control and treatment award lines for the 2nd–10th deciles of EC, which requires applicants in the 2nd–10th deciles to either

[43] The graph excludes the range of expected contribution for which applicants are "grant ineligible" and thus do not appear in our experiment (see discussion in Section 2 and in the "Grant-Ineligible Applicants" section of the Online Appendix).

[44] Since this picture describes a liquidity mechanism, it does not distinguish between additional grants and loans. While not shown, the $1,800 treatments would simply be shifted up further.

have pre-credit liquidity need circles below the control award line or to have enough access to credit that their post-credit liquidity need dots fall below the control award line.[45]

Figure 8 also provides a visual intuition for why we might expect a larger fraction of applicants in the 1st decile than in the 2nd–10th deciles to have pre-credit liquidity need above the control award line. That is, assuming pre-credit liquidity need continues to (at least somewhat) increase through EC = \$0, while award remains flat, more circles will be above the control award line when EC ≤ \$0 than when EC > \$0.[46]

While we do not know the true levels of residual liquidity need—either before or after applicants access credit markets—we have drawn the circles and dots in Figure 8 in a way that is consistent with the findings from our financial need and credit access survey, described in the next section.

5.2 Liquidity Need and Credit Access

We begin by evaluating where the pre-credit liquidity need circles are likely to fall in Figure 8. In our post-experiment survey (see details in the appendix), we asked respondents whether they needed funds to make the transition into TFA beyond what was available in checking and savings accounts, and whether the total award offered by the TGL program fully covered that need.[47] As shown in Table 6, among the respondents in the 1st decile (response rate 52.5%), 60.8% of applicants in the control group reported having financial need that was not covered by their TGL award. This suggests that for the 1st decile, roughly 61% of the pre-credit liquidity need circles will be above the control award line.

As expected, the rate at which survey respondents reported that they needed additional funding was lower when they were randomized to a treatment group and thus were offered a

[45] While roughly 25% of TGL applicants in the 2nd–10th deciles choose not to join TFA in the control group (see Figure 4), this framework assumes these applicants do so for reasons unrelated to TGL funding. This is consistent with similar rates of joining TFA among non-TGL applicants in the years of our experiment.

[46] Whether this is true in practice is an empirical question—the TGL award algorithm is designed assuming that true initial liquidity need is expected expenses (i.e., TFA's prediction about the amount needed to finance a transition into becoming a TFA teacher in that school district). If this assumption is true, we might not expect pre-credit liquidity need to continue to increase once EC falls below \$0.

[47] The full question read: "After you were admitted to Teach For America for the 20— school year, did you need financial assistance to accept your TFA offer, make the summer transition, and start teaching? For example, did you need more money than you already had on hand (e.g., in checking or savings accounts) to cover the costs of attending Summer Institute and to secure housing?" Respondents could choose between "No", "Yes, but TFA's Transitional Grants and Loans program fully covered my needs", and "Yes, and TFA's Transitional Grants and Loans program **did not** fully cover my needs".

larger TGL award. Combining the grant and loan treatments for statistical power, respondents are 1.26 percentage points less likely to report having unmet liquidity need for every \$100 of additional funding they were provided by the experiment ($p = 0.048$, regression not reported).[48] These estimates suggest that for the 1st decile, approximately 53% ($60.8 - 1.26 \times 6$) of the pre-credit liquidity need circles are above the \$600 treatment award line and that roughly 8% (1.29×6) of the circles are between the control and \$600 treatment lines (i.e., the pre-credit liquidity need was above the control award but below the \$600 treatment award).

What about applicants in the other deciles? Across the 2nd–10th deciles in the control group, 56.1% said that they had financial need that was not fully covered by their TGL award. Table 7 reports regressions of this binary measure of liquidity need from the survey on the expected contribution of the survey respondents in the 2nd–10th deciles. The relationship is rather flat, suggesting that across the graph as EC rises from 0, roughly 56% of the pre-credit need circles remain above the control award line.[49]

What about the location of the post-credit liquidity need dots, which determine whether we see a treatment effect under the liquidity mechanism? If respondents to our post-experiment survey indicated that their TGL award did not fully cover their liquidity need, they were asked about their attempts at accessing credit. In particular, the survey asked about applying for a new credit card (or an increase in the limit of an existing credit card); applying for a new loan from a bank (or an increase in the limit of an existing loan); or seeking an informal loan or gift from friends or family. For each type of funding sought, respondents were asked whether the request was successful. When respondents answered that they did not apply for a specific type of credit, the survey asked why they did not.[50] Table 6 summarizes responses to these questions. Among applicants in the 1st decile who indicated that they needed additional funds, 87% reported applying for some form of credit.[51]

While applicants in the 1st decile attempted to access credit markets, they had difficulty

[48] This estimate is comparable to the estimated effect of additional funding on joining TFA among the same group of survey respondents, which is 1.57 percentage points for every \$100 of additional funding ($p = 0.006$). See Table 8.

[49] For these deciles, respondents are -0.73 percentage points less likely to report having unmet liquidity need for every \$100 of additional funding they were provided by the experiment ($p < 0.001$). This pattern is roughly constant across the distribution of EC as well, suggesting that as EC rises from \$0, roughly 52% of the pre-credit liquidity need circles are above the \$600 treatment line and 4% are between the control and \$600 treatment lines.

[50] Online Appendix Table A4 shows the reasons respondents provided for why they did not apply for particular sources of credit.

[51] This rate is comparable to the 88.2% who reported applying for some form of credit among those respondents in the 2nd–10th deciles who needed additional funds.

doing so successfully. As shown in Table 6, among applicants in the 1st decile who needed funds, 26.5% were denied in at least one of their attempts to access credit.[52] In addition, many applicants in the 1st decile were discouraged borrowers (i.e., those who fail to apply for credit due to a belief that they will be rejected). As shown in Online Appendix Table A4, almost a third of the survey respondents in the 1st decile who did not apply for a new credit card (or an increase in the borrowing limit for a current card) cited their belief that such a request would be denied. The rates are similar for bank loans. Overall, 29.5% of those in the 1st decile cited anticipated rejection as a reason they did not apply for at least one type of loan (see the variable "Any discouragement" in Table 6). As noted above, those in the 1st decile of EC face relatively high rates of rejection in the credit market, so these beliefs may be justified. Taken together, almost half (47%) of those in the 1st decile were denied or discouraged for at least one type of borrowing opportunity (see the variable "Any discouragement or denial" in Table 6), and nearly one in six (15.5%) were denied or discouraged at least once and failed to get a loan of any kind (see the variable "No credit access" in Table 6). These responses suggest that applicants in the 1st decile had restricted access to credit markets, and we may expect many of their post-credit liquidity need dots to fall inside or only slightly below their pre-credit liquidity need circles.

To explore these credit access variables in the 2nd–10th deciles, Table 7 reports regressions of these variables on expected contribution for the 2nd–10th deciles.[53] The likelihood an applicant was denied or discouraged from accessing credit—conditional on reporting that they need funds—is decreasing in expected contribution. These results suggest that the post-credit liquidity need dots are likely to be further from the pre-credit liquidity need circles as expected contribution increases.

How do credit constraints differ between the 1st decile and the the 2nd–10th deciles, and do they differ discontinuously? The second to last row of Table 7 projects the estimated relationship between expected contribution and these measures of credit constraints onto the expected contributions of the respondents in the 1st decile. The last row of Table 7 shows the actual responses among applicants in this decile. For nearly all of the measures of credit constraints, respondents from the 1st decile of EC reported outcomes that are as bad or worse than projected, suggesting

[52] Online Appendix Table A5 breaks down acceptance rates by credit type.

[53] As described in detail in the appendix, applicants in the 2nd–10th deciles were recruited under different incentives than the 1st decile. We consequently estimate our regressions using respondents from the 2nd–10th deciles. We then project our estimates onto the 1st decile and compare this projection to the survey responses we actually received from the 1st decile (see further discussion in footnote 54).

a particularly tough credit market for respondents in the 1st decile.[54]

TABLE 6 ABOUT HERE

TABLE 7 ABOUT HERE

Looking back at Figure 8, we see that our survey results are consistent with the way the pre-credit liquidity need circles and post-credit liquidity need dots were drawn. These survey results support a liquidity mechanism driving our positive treatment effects of additional funding among applicants in the 1st decile of EC and our null results among applicants in the 2nd–10th deciles.

Finally, we highlight that the credit constraints faced by applicants in the 1st decile documented in this section arise because of applicants' poor financial situations rather than because of a lack of awareness of credit markets or because of debt aversion. First, note that applicants' high rates of applying for credit rule out a lack of awareness of credit markets. In particular, as reported in Table 6, among respondents in the 1st decile who report needing additional funds, 58.0% apply for (additional) credit card debt, 19.5% apply for (additional) bank loans, and 68.0% seek informal loans of gifts from friends or family. In addition, as reported in Online Appendix Table A4, very few applicants in the 1st decile who did not seek each source of credit listed "I did not know how to go about doing this" or "It did not occur to me" as a reason for not doing so.[55] In addition, the data applicants submit to the TGL program reveals their awareness of credit markets. Among applicants in the 1st decile of EC, 87.0% have credit card debt, with an average credit card debt of $6,500. Second, our evidence suggests that debt aversion is unlikely to be relevant here. Our applicants report actively seeking out debt to finance their transitions into teaching. In addition, as reported above, grants and loans are equally effective in our experiment. If applicants were debt averse, we would expect the grant treatments to be more effective than the loan treatments.

[54]As noted in footnote 53 and described in detail in the appendix, applicants in the 1st decile were provided with larger incentives to complete the survey, and subsequently had a higher response rate (53%) than applicants in the 2nd–10th deciles (37%). While we are generally not concerned about selection into the survey (see discussion in the appendix), the projection of the estimates from the 2nd–10th deciles onto the 1st decile addresses any potential concerns surrounding differential selection into the survey based on expected contribution.

[55]Rates of reporting these excuses were 3.6% and 6.0% for credit cards and 8.7% and 8.7% for bank loans, respectively. Only 1.6% of respondents said the informal gift or loan did not occur to them (we did not ask the "did not know how" question about asking friends and family for money). These rates are all directionally or significantly lower for respondents in the 1st decile than for respondents in the 2nd–10th deciles who also chose not to seek each form of credit.

5.3 Ruling Out Alternative Mechanisms

In this subsection, we discuss non-liquidity mechanisms that might potentially be relevant in our experiment and discuss evidence that speaks to each of them.

5.3.1 Awards Effectively Increase Earnings

Our treatments, in particular our grant treatments, could make joining TFA more attractive by effectively increasing earnings for those who join TFA. This mechanism would predict larger treatment effects from grant funding than from loan funding, however, which is inconsistent with our data.[56] In addition, there is no particular reason to believe that a response to an effective increase in earnings would be isolated in the 1st decile. In particular, if applicants responded to the effectively higher earnings in the grant treatments, we would expect to see an increase in joining TFA in response to the *$1800 Grant* treatment.[57] That we do not see an effect of this treatment suggests applicants are not responding to the effectively higher earnings associated with additional funding.

5.3.2 Gift Exchange

A gift exchange hypothesis would predict positive reciprocity by applicants, in the form of joining TFA, in response to increased TGL award funding. Without more structure, a gift exchange motive could make many different predictions (e.g., depending on whether reciprocity is triggered by the monetary value of the gift, the proportional increase in the gift size, the cost of the gift to TFA, or the benefit to the applicant). Nevertheless, most models of gift exchange are ruled out by our data. Grants have more monetary value and are more costly than zero-interest loans and yet our treatment effects are comparable for grant and loan funding. The monetary value and the cost of the gift are also constant across deciles of EC and yet we only see treatment effects in the 1st decile. The proportional increase in gift size induced by the treatment is *smallest* in the 1st decile given their larger baseline awards. This evidence all points against gift exchange as a driver of our results.

[56] While one might be concerned that the lack of a difference in treatments is due to applicants acting as if loans are like grants (i.e., acting as if neither have to be repaid), this concern is not borne out by repayment data provided to us by TFA. For example, applicants from the 1st decile of EC in the first two years of the experiment are directionally more likely to be paid in full (as of September 2017) than applicants from the 2nd–5th deciles from those years.

[57] Self-reported salary data from TFA teachers who responded to our survey suggests an average of $43,020, so $1,800 represents roughly 4.2% of annual salary.

Nevertheless, a version of gift exchange in which reciprocity is triggered by benefit to the recipient is harder to rule out. It could be that additional funds induced reciprocity from applicants in the 1st decile because they were facing binding liquidity constraints and the additional funding (of either grants and loans) eased these constraints. Note, however, that this story presupposes that liquidity constraints explain part of the effects we find. A gift exchange motive would then amplify the effects of the liquidity mechanism. While we cannot rule out this possibility, it does not change our main results on the importance of liquidity concerns or the policy relevance of providing transitional funding as a way of inducing people to become teachers or take other jobs in the public interest.[58]

5.3.3 Other Characteristics of the 1st Decile of EC

A final alternative hypothesis is that the applicants in the 1st decile of EC differ from applicants in the 2nd–10th deciles along a dimension beyond need for liquidity, and that this difference drives the differential results across deciles. The data do not provide evidence of any such confounding factor. Online Appendix Table A6 tests whether any of our demographic variables interact with treatment. No interaction removes the explanatory power of the interaction of either experimental treatment with the 1st decile of EC, and none substantially reduces its magnitude.

6 Occupations Outside of TFA

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

To answer these questions, we report on responses to questions that we asked in our post-experiment survey (see details in the appendix). In particular, we asked respondents their occupation 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.[59] The sur-

[58]If gift exchange were driving a sizable fraction of our effect, however, this could have implication for policy, since one might imagine the source of transitional funding could matter. For example, if gift exchange were operative, we might expect transitional funding from a potential employer to have a larger effect on behavior than transitional funding from a third party (such as the government), unless the third party indicated that working for particular types of employers would be a way for recipients to reciprocate the gift.

[59]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, less

vey then asked follow-up questions about respondents' jobs (e.g., about industry and salary) and educational pursuits (e.g., about degree sought).

The survey results suggest that applicants induced to become TFA teachers by our treatments were pulled out of private sector jobs.[60] Table 8 shows the effect of additional funding provided by the experiment, combining the grant and loan treatments to maximize power, on occupation for respondents in the 1st decile of EC. The first column replicates the main finding of the paper for survey respondents: among survey respondents in the 1st decile of EC, extra funding has a large and statistically significant effect on the likelihood of joining TFA. Column 2 shows the treatment also increased the likelihood that respondents are teaching at any school, through TFA or otherwise. Thus, our treatments created additional teachers overall, not just more teachers for TFA. Column 3 shows that the effect also persists in the medium-term, with extra funding increasing the likelihood that applicants in the 1st decile are or are planning to be teaching two years later.

Column 4 reports on whether the applicant is in a private sector job and shows a statistically significant negative coefficient on extra funding, suggesting 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.[61] Column 5 shows that the effect on private sector jobs is gone two years later. 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 two years later.

TABLE 8 ABOUT HERE

7 Discussion

In this paper, we investigate whether liquidity constraints affect job choice. In particular, we explore a setting in which individuals face transition costs to become a teacher, and we find that

than two years had elapsed since the fall after they applied to the TGL program, so they were asked about their expected occupation. The survey also asked about aspirational career goals by asking about plans 10 years later. As shown in Online Appendix Table A7, we find no significant differences on these outcomes.

[60]Private sector jobs are jobs that respondents categorized as either: Banking/Finance, Consulting, Publishing/Journalism/Media, Law, Engineering/Technology, or Other Business (e.g., Marketing or Real Estate).

[61]While not higher paying, however, these private sector jobs may have smaller transition costs or start earlier than TFA, which might be a draw for applicants with liquidity constraints.

liquidity constraints prevent a subset of individuals from joining the profession. We randomly increase the size of grant and loan packages offered to potential Teach for America teachers who apply for transitional grant and loan funding 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 our 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, so the applicants in our experiment are talented college graduates. One might think they all 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 our treatments, suggesting they are able to finance any unmet liquidity 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 they receive as part of our experiment, suggesting liquidity is a first-order concern for their job choice.

From a policy maker's perspective, additional loans provided a low-cost way to generate more TFA teachers, and more teachers overall. Given our estimates, and assuming marginal loans are paid back on schedule (as the vast majority are), it only costs TFA $186 in additional interest payments to attract one additional teacher (in expectation) from the 1st decile of EC into TFA using transitional loans.[62] While the cost of inducing a new teacher to join the profession is likely to be context-specific, our results suggest that providing liquidity could be an effective and low-cost means of increasing the pool of teachers and other public servants.

[62]This number is calculated using the estimate from column 4 of Table 5 that each additional $100 in loans increases the rate at which 1st 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.

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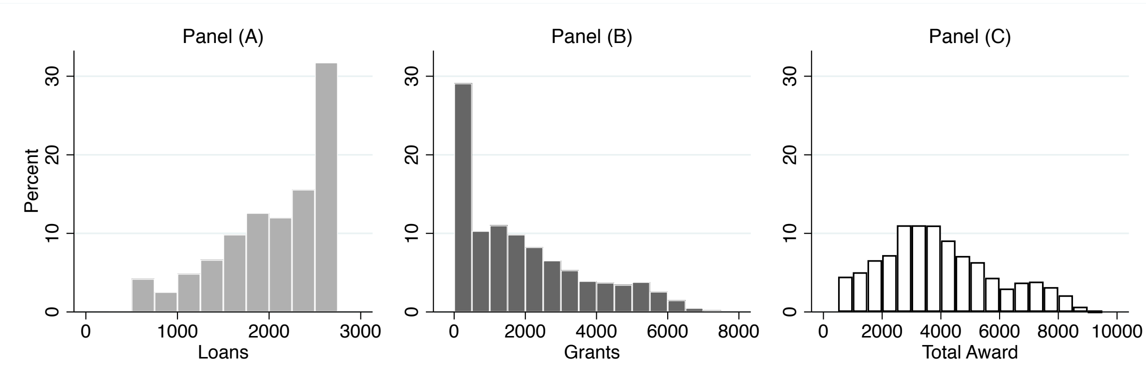
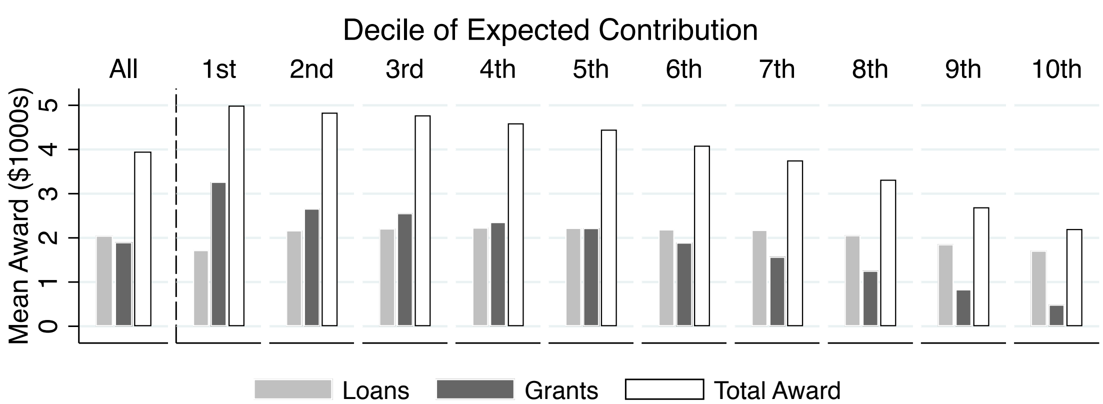
Figures and Tables

Figure 1: Base Award Offers (2015–2017)



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

Figure 2: Base Award Offers, by Decile of Expected Contribution (2015–2017)



Base Award Offers are the award that would be offered to an applicant randomized into our control group and the base onto which additional funding from our experimental treatments was added. Figure shows the mean loan, mean grant, and mean total base awards offered (leftmost group of bars) and by decile of expected contribution (all other groups of bars).

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

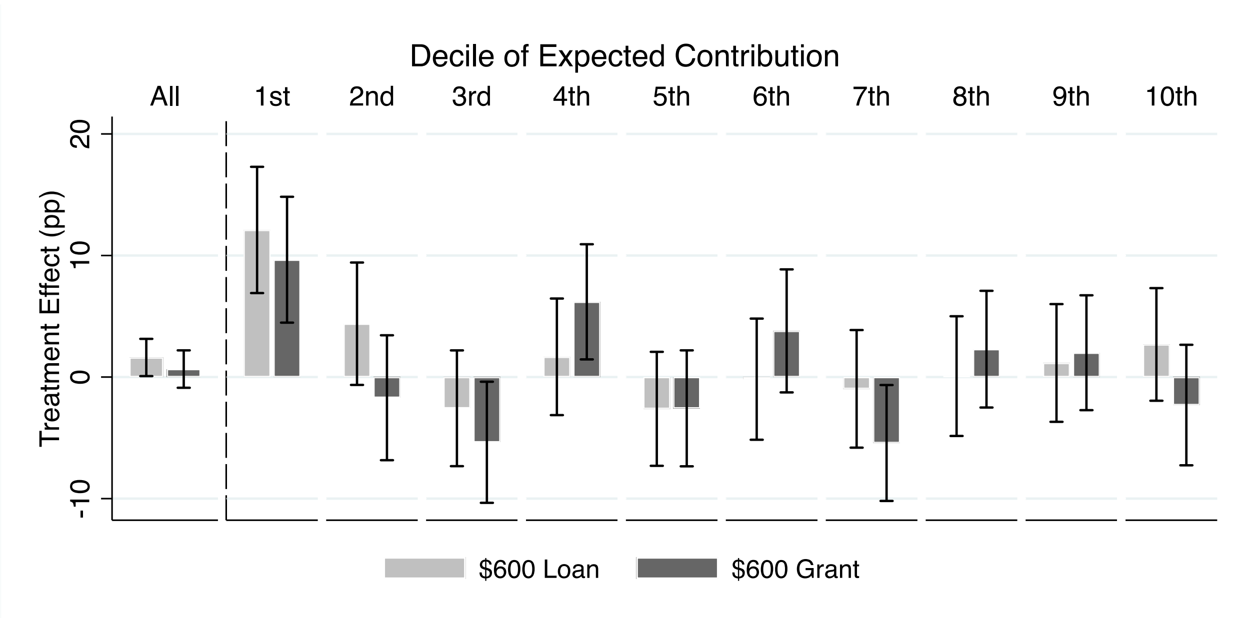


Figure shows treatment effects of offering $600 in additional loans or $600 in additional grants on whether applicants join TFA. The two leftmost bars show the effect pooled across all applicants. The other pairs of bars show the effect by decile of expected contribution. Error bars show robust standard errors. The full regression specification generating these estimates is reported in 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 32.

Figure 4: Percentage of Applicants Joining TFA in the Control Group (2015–2016)

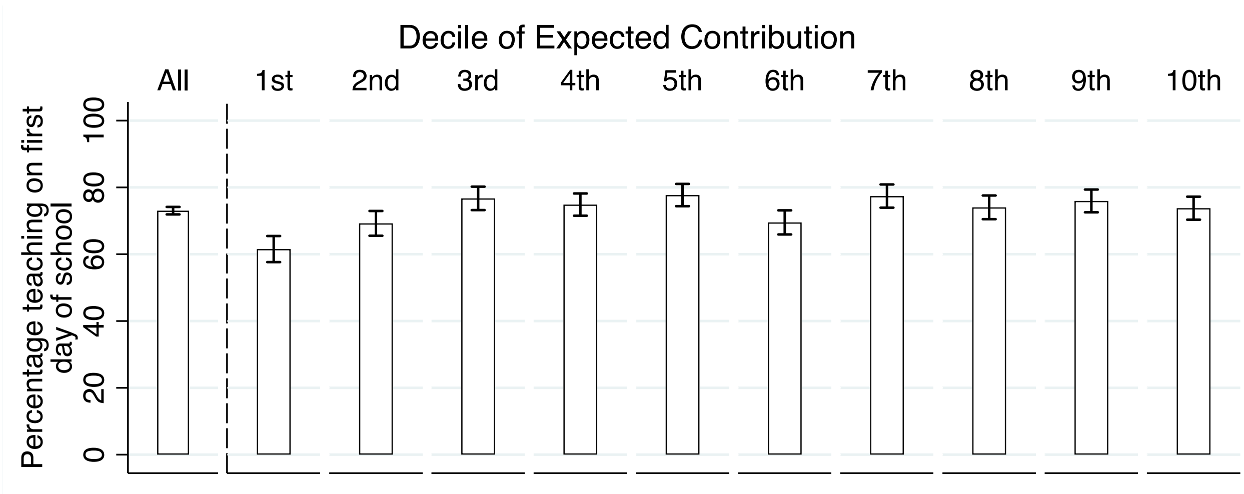


Figure shows the percentage of applicants in the control group of our experiment who are teaching for TFA on the first day of school in the first two years of our experiment. The leftmost bar shows the overall percentage. The other bars report the percentage by decile of expected contribution. Error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

Figure 5: Replication of Treatment Effects (2015–2016 vs. 2017)

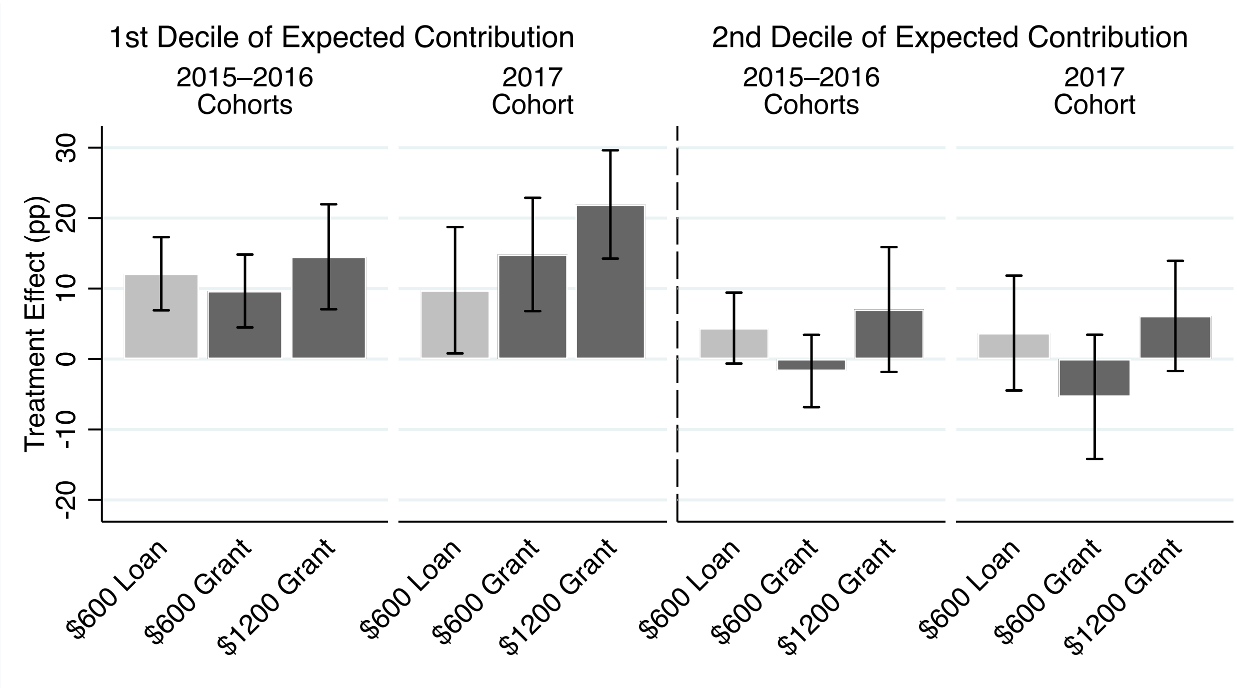


Figure compares treatment effects observed in the first two years of the experiment to treatment effects observed in the third year. The left panel shows the treatment effects estimated for the 1st decile of expected contribution and the right panel shows the treatment effects estimated for the 2nd decile 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 robust standard errors. The full regression specification generating these estimates is reported in the Online Appendix.

Figure 6: Stress Test of Null Results (2017)

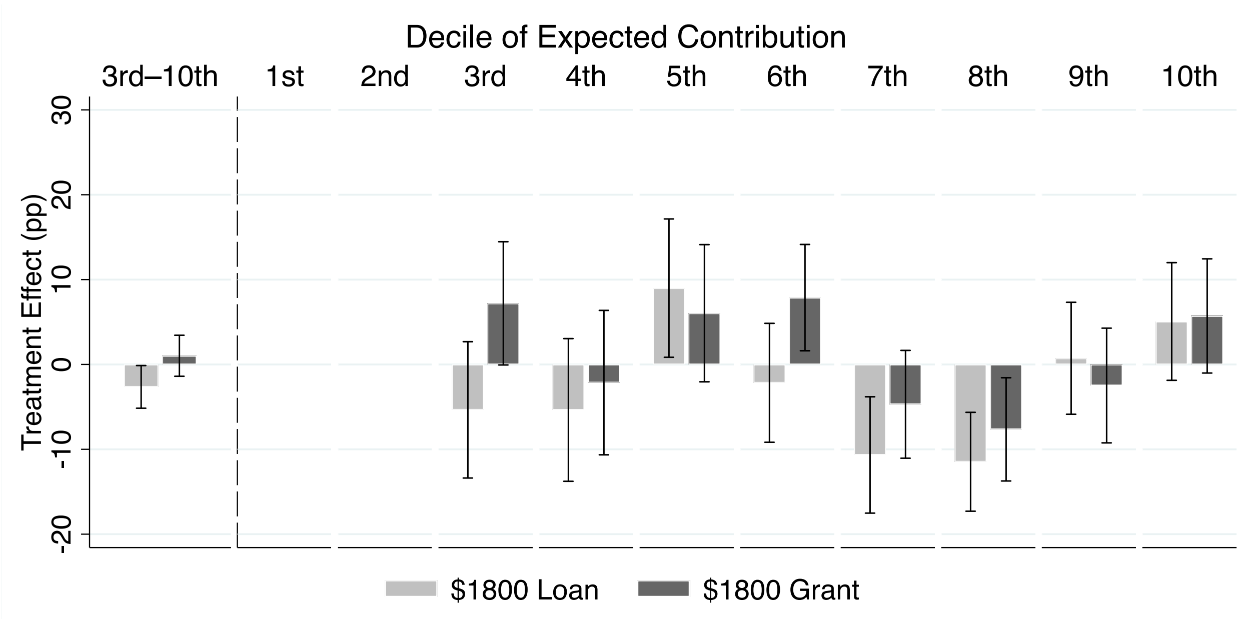


Figure shows treatment effects of offering $1,800 in additional loans or $1,800 in additional grants on whether applicants join TFA. The two leftmost bars show the effect pooled across all applicants in the 3rd–10th deciles of expected contribution. The other pairs of bars show the effect by decile of expected contribution. Error bars show robust standard errors. The full regression specification generating these estimates is reported in the Online Appendix. 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 5.

Figure 7: Treatment Effects in 1st Decile and in 2nd–10th Deciles (2015–2017)

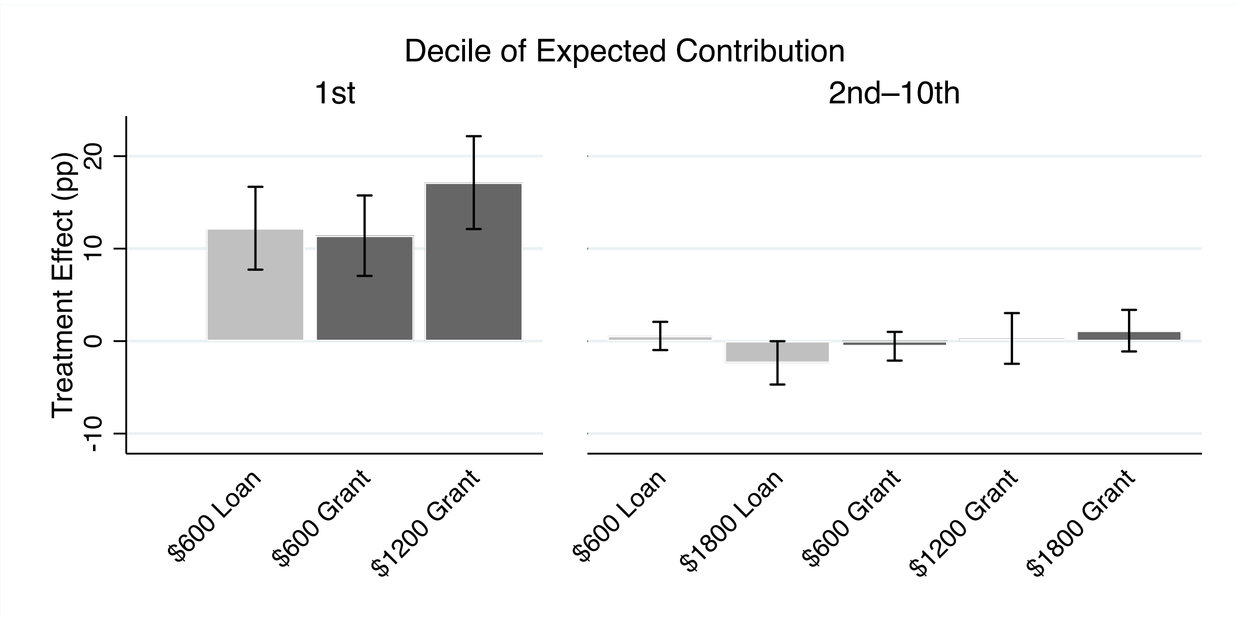


Figure shows treatment effects pooled across all years of the experiment. The left set of three bars show the treatment effects observed among applicants in the 1st decile of expected contribution. The right set of bars show the treatment effects observed among applicants in the 2nd–10th deciles of expected contribution. The sample includes applicants from all three years of our experiment (2015–2017). Error bars show robust standard errors. The full regression specification generating these estimates is reported in the Online Appendix.

Figure 8: Visual Framework

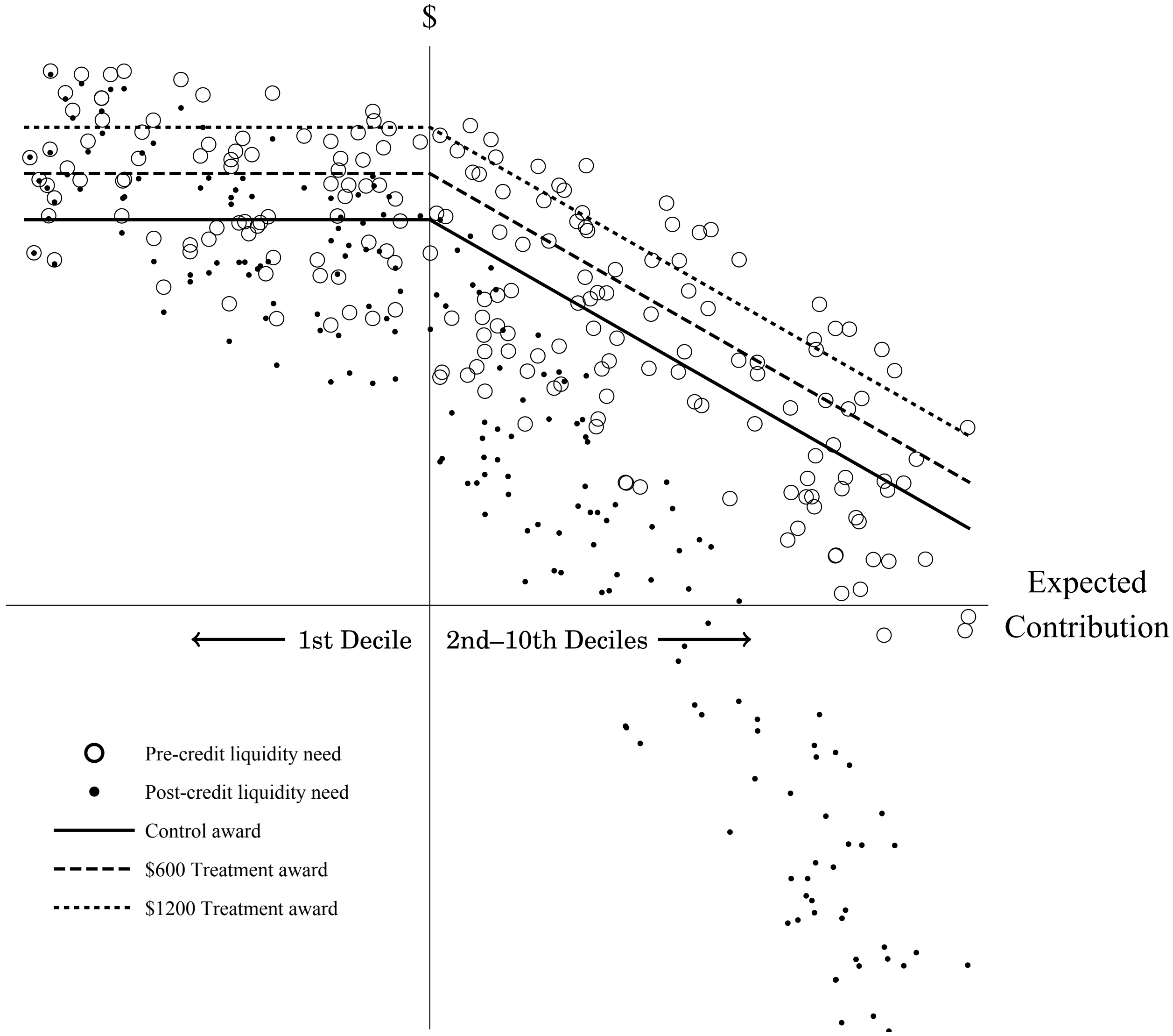


Figure shows a visual framework for understanding our liquidity mechanism. The circles are liquidity need beyond cash-on-hand (i.e., cash in checking and savings accounts) for hypothetical applicants. Dots are liquidity need beyond cash-on-hand and credit accessed from formal and informal credit markets for those same hypothetical applicants. Solid line shows the level of the Control award. Dashed line shows the level of the $600 Treatment award. Dotted line shows the level of the $1200 Treatment award.

Table 1: Treatment Assignments (2015–2017)

	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 of EC					
Control	84	35	37	45	201
$600 Loan	104	32	34	53	223
$600 Grant	113	31	28	50	222
$1200 Grant			25	55	80
3rd–10th Deciles of EC					
Control	732	286	242	545	1805
$600 Loan	798	318	252		1368
$600 Grant	795	289	243		1327
$1200 Grant			231		231
$1800 Loan				524	524
$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 (likewise 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 in 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.

Table 3: Summary Statistics

	Decile of Expected Contribution			
	All	1st	2nd	3rd–10th
Female (%)	75.8	75.7	76.1	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	35.5	36.4
Subject Not First Choice (%)	29.8	30.8	31.9	29.4
Expected Contribution ($)	1,157	-484	126	1,503
Checking and Savings ($)	1,071	241	174	1,293
Parental Contribution ($)	6,525	1,136	1,226	7,901
Income ($)	38,034	18,134	15,547	43,483
Credit Card Debt ($)	1,684	6,490	1,655	1,052
Private Student Loans ($)	5,100	19,693	3,814	3,331
Graduating Senior (%)	46.8	26.2	37.8	50.6
Number of Dependents	0.61	0.56	0.59	0.62
Local (%)	39.1	43.8	41.0	38.2
Regional Cost ($)	6,057	5,974	5,905	6,087
Federal Loans ($)	27,822	45,060	29,411	25,343
N	7,295	767	727	5,801

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 34. "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 expenses 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)

	(1)	(2)	(3)	(4)
Extra Grants ($100s)	0.06	0.11		
	(0.20)	(0.20)		
Extra Loans ($100s)	0.25	0.26		
	(0.25)	(0.25)		
Extra Grants × 1st Decile EC			1.35**	1.81***
			(0.59)	(0.61)
Extra Loans × 1st Decile EC			1.93**	2.16***
			(0.83)	(0.83)
Other Deciles Included	—	—	Yes	Yes
Demographics	No	Yes	No	Yes
Batch FEs	Yes	Yes	Yes	Yes
N	5233	5233	5233	5233
R^2	0.04	0.09	0.05	0.10
Control Group Mean	73.03	73.03	73.03	73.03
1st Decile Control Group Mean	61.54	61.54	61.54	61.54
Number of interactions with other deciles...				
that are positive, $p < 0.10$			0	0
that are negative, $p < 0.10$			0	0

Table shows Linear Probability Model (OLS) regressions of whether an applicant joins TFA from equations (1a) and (1b), 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 $p < 0.1$, 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. Demographics includes a linear age term, a linear term for the applicant's "fit" with TFA (described in footnote 34), and dummies for race, gender, assigned region, whether the applicant was assigned to his or 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"). The coefficient estimates for all deciles of expected contribution from the specifications reported in columns 3 and 4 can be found in the Online Appendix Table A3. The bottom two rows report how many treatment effect estimates from the 2nd–10th deciles of expected contribution are significant at $p < 0.1$ for each specification.

Table 5: Treatment Effects of Additional Grants and Loans (2015–2017)

	(1)	(2)	(3)	(4)
Extra Grants ($100s)	0.11	0.15		
	(0.11)	(0.10)		
Extra Loans ($100s)	-0.01	0.02		
	(0.12)	(0.11)		
Extra Grants × 1st Decile EC			1.51***	1.77***
			(0.42)	(0.41)
Extra Loans × 1st Decile EC			1.90***	2.06***
			(0.71)	(0.69)
Other Deciles Included	—	—	Yes	Yes
Demographics	No	Yes	No	Yes
Batch FEs	Yes	Yes	Yes	Yes
N	7295	7295	7295	7295
R^2	0.04	0.12	0.05	0.12
Control Group Mean	74.33	74.33	74.33	74.33
1st Decile Control Group Mean	61.06	61.06	61.06	61.06
Number of interactions with other deciles...				
that are positive, $p < 0.10$			1	1
that are negative, $p < 0.10$			0	0

Table shows Linear Probability Model (OLS) regressions of whether an applicant joins TFA from equations (1a), with decile dummies added to columns 1 and 2, and (1b). These specifications are described in Section 4.2. Sample includes applicants from all years of the experiment. Robust standard errors are reported in parentheses. *, **, *** 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. Demographics includes a linear age term, a linear term for the applicant's "fit" with TFA (described in footnote 34), and dummies for race, gender, assigned region, whether the applicant was assigned to his or 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"). The coefficient estimates for all deciles of expected contribution from the specifications reported in columns 3 and 4 can be found in Online Appendix Table A3. The bottom two rows report how many treatment effect estimates from the 2nd–10th deciles of expected contribution are significant at $p < 0.1$ for each specification.

Table 6: Liquidity Need and Credit Access

	Decile of Expected Contribution			
	1st		2nd–10th	
	Control	All	Control	All
Needed additional funds	60.8%	51.0%	56.1%	51.7%
N	125	394	706	2329
Conditional on needing additional funds				
Sought any funding	88.0%	87.0%	88.3%	88.2%
Applied for credit card	61.3%	58.0%	54.1%	57.4%
Applied for bank loan	17.3%	19.5%	18.5%	19.3%
Sought informal loan or gift	68.0%	68.0%	71.6%	71.1%
Received any funding	77.3%	72.0%	75.9%	76.4%
Any denial	24.0%	26.5%	14.0%	15.1%
Any discouragement	25.3%	29.5%	16.0%	16.5%
Any discouragement or denial	40.0%	47.0%	26.6%	27.5%
No credit access	13.3%	15.5%	7.9%	8.1%
N	75	200	394	1197

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 of expected contribution. Within each panel, table reports the values for the control group only and for all respondents in that decile. "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" is equal to 1 and the respondent did not receive credit from any source. For details about the survey, see the appendix.

Table 7: How Expected Contribution Affects Liquidity Need and Credit Access

	Needed Additional Funds (1)	Any Denial (2)	Any Discouragement (3)	Any Discouragement or Denial (4)	No Credit Acess (5)
			Control Group		
Expected Contribution ($1,000s)	0.01	-2.50*	-3.40**	-4.65***	-1.86*
	(0.02)	(1.37)	(1.45)	(1.74)	(1.07)
N	706	394	394	394	394
R^2	0.00	0.01	0.01	0.02	0.01
Predicted 1st Decile Mean	0.54	18.70	22.43	35.47	11.40
Actual 1st Decile Mean	0.61	24.00	25.33	40.00	13.33
			Entire Sample		
Expected Contribution ($1,000s)	0.01	-2.10***	-3.41***	-3.93***	-1.87***
	(0.01)	(0.80)	(0.82)	(0.99)	(0.61)
N	2329	1197	1197	1197	1197
R^2	0.00	0.01	0.01	0.01	0.01
Predicted 1st Decile Mean	0.50	19.15	23.01	35.04	11.69
Actual 1st Decile Mean	0.51	26.50	29.50	47.00	15.50

Table reports OLS estimates from a regression of the listed variable on expected contribution (in $1,000s) among survey respondents in the 2nd–10th deciles of expected contribution. The top panel shows data from control group respondents only, the bottom panel shows data from all respondents. Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.1$, 0.05, and 0.01, respectively. The full regression specification generating these estimates is

$$Y_i = \beta \cdot EC_i + \sum_j \gamma^j \cdot Batch_i^j + \varepsilon_i, \quad (2)$$

where EC_i is expected contribution in thousands, and where Y_i is one of the following five variables. "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. "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" is equal to 1 and the respondent did not receive credit from any source. "Predicted 1st Decile Mean" projects our estimates from these regressions (estimated from respondents in the 2nd–10th deciles) onto the respondents in our 1st decile based on expected contribution and reports the mean. "Actual 1st Decile Mean" reports the true mean from respondents in our 1st decile. For details about the survey, see the appendix.

Table 8: Treatment Effects on Actual and Expected Occupations

	Joined TFA	Teaching First year	Teaching 2 years out	Private sector First year	Private sector 2 years out	Grad student First year	Grad student 2 years out
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Extra Funding ($100s)	1.57***	1.11*	1.16*	-1.13***	0.21	0.29	-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	1
negative, $p < 0.10$	0	0	1	1	0	0	0

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 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); and 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). Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.1$, 0.05, and 0.01, respectively. The full regression specification generating these estimates is reported in the Online Appendix. “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 34). 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 and 4 can be found in Online Appendix Table A7. The bottom two rows report how many treatment effect estimates from the 2nd–10th deciles of expected contribution are significant at $p < 0.1$ for each specification.

Appendix: Follow-up Survey

Design and Implementation

In May 2018, TFA emailed a survey invitation on our behalf to all 7,295 applicants from the three years of our experiment.[63] One part of the survey concerned liquidity needs. It began by asking each applicant whether, before receiving the TGL award, she had needed extra liquidity (in excess of any funds in her checking and savings accounts) to make the transition into TFA. If so, it also asked whether her TGL offer had fully covered that need. If she responded in the negative, the survey asked whether she attempted 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 types, the survey asked whether the request was successful or why she chose not to make it.

The other part of the survey concerned employment. It asked each applicant what she was doing, or planned to be doing, in the fall immediately after, two years after, and ten years after she applied for TGL funding. The immediate-horizon question allows us to measure career and educational outcomes for applicants who did not join TFA (those who joined TFA simply answered they were teaching as part of the TFA program). The two-year and ten-year horizon questions were designed to give us some insight into whether those who become teachers because of TGL funding actually plan to stay in the teaching profession long term.[64]

As with any survey, selection into response can lead to bias. To mitigate this problem, we offered financial rewards for survey completion, since higher response rates leave less room for selection bias. 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 allowed us to more effectively limit potential selection bias for the group in which we found a treatment effect, and hence in which we are most interested (see Section 4). Second, we randomly chose whether applicants were offered a larger or a smaller reward for survey completion. Such variation allows us to directly gauge potential selection bias on answers to

[63] We are able to link individual survey responses to our experimental data, so we can match survey responses to financial data, experimental treatment, and TGL award.

[64] For applicants in the 2015 cohort, more than two years had elapsed since the fall after they applied for TGL funding, so the two-year horizon question concerned actual, not planned, employment.

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 is 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).[65]

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 a small and a large reward that lead to response rates of r_S and r_L, respectively, where $r_L > r_S$. Further, assume that the mean answers to some survey item are y_S and y_L under the small and large 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 fraction 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

$$y_{marg} = \frac{r_L}{r_L - r_S}\cdot y_L - \frac{r_S}{r_L - r_S}\cdot y_S. \tag{3}$$

Using this equation, we can effectively partition respondent types into always-responders and marginal-responders.[66] 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

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

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

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}.[67]

Returning to the details of our survey, the variation in response incentives that we used is summarized in Table 9. 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.[68]

Table 9: 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 prepaid debit cards.

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 across the two groups.[69] Table 10 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, allowing more room for selection bias to arise. The statistically significant differences do not seem to be economically large.

[67] This follows from applying the delta method to equation 3.

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

[69] 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, which is the relevant breakdown for the results reported in the main text.

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 9, 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 11).

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.

Table 10: Selection into Survey

	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 $p < 0.1$, 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 34. "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.

Table 11: Comparing Survey Incentive Groups

	EC decile 1			EC deciles 2–10		
	Low Incentive	High Incentive	Difference	Low Incentive	High Incentive	Difference
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)
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)
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	
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 $p < 0.1$, 0.05, and 0.01, respectively. The bottom panel includes only respondents who answered that they needed additional funds.

EVERYTHING THAT FOLLOWS IS FOR ONLINE PUBLICATION ONLY

(unless requested otherwise)

Online Appendix

Additional Figures and Tables

Figure A1: Applicants' Expected Contributions (2015–2017)

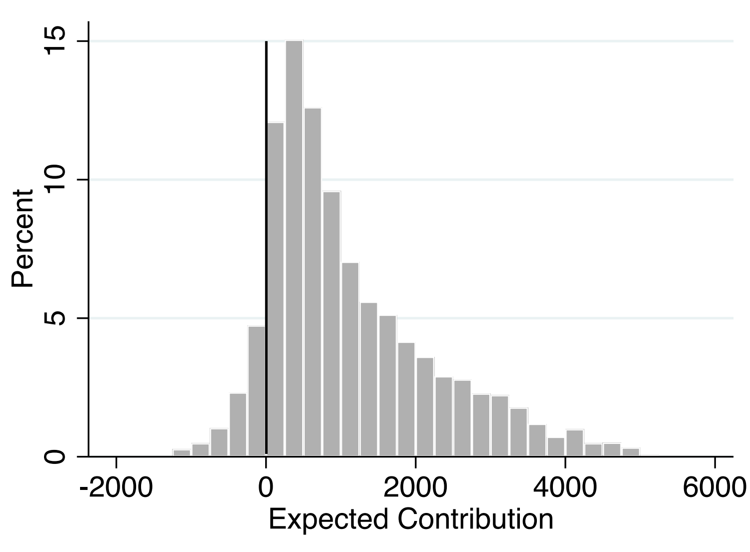


Figure is a histogram of expected contribution of applicants in the experiment. The vertical line represents the 10th percentile of expected contribution, equal to $4.20.

Table A1: 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 A2: p-values of Balance Tests

	Decile of Expected Contribution			
	All	1st	2nd	3rd–10th
Female	0.869	0.434	0.200	0.547
White	0.069	0.850	0.616	0.230
Age	0.229	0.470	0.655	0.902
"Fit" Score	0.208	0.668	0.416	0.449
Region Not First Choice	0.483	0.188	0.098	0.919
Subject Not First Choice	0.985	0.106	0.567	0.371
Expected Contribution	0.445	0.372	0.467	0.618

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 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. The full regression specification generating these estimates is reported in the Online Appendix. "Fit Score" is a measure of an applicant's fit with the organizational objectives of TFA, as defined in footnote 34. "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.

Table A3: Treatment Effects of Additional Grants or Loans, All Coefficients (2015–2017)

	2015–2016		2017		2015–2017	
	(1)	(2)	(3)	(4)	(5)	(6)
Extra Grants ($100s)	1.35**	1.81***	1.84***	1.75***	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	0.20	0.50	0.34
× 2nd Decile EC	(0.64)	(0.64)	(0.63)	(0.57)	(0.44)	(0.42)
Extra Loans	0.90	0.70	1.18	0.75	1.06	0.82
× 2nd Decile EC	(0.81)	(0.81)	(1.08)	(0.97)	(0.65)	(0.63)
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	-0.33	0.13	-0.30	-0.24	-0.44	-0.32
× 3rd Decile EC	(0.77)	(0.77)	(0.45)	(0.41)	(0.36)	(0.35)
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	-0.34	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	-0.42	-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**	-10.98**	-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	6.82	-4.14	-1.38
	(4.84)	(4.93)	(7.41)	(6.98)	(3.71)	(3.66)
3rd Decile EC	2.81	2.65	1.46	4.42	3.06	4.11
	(4.74)	(4.80)	(7.25)	(6.89)	(3.32)	(3.31)
4th Decile EC	-1.09	-0.41	5.54	6.17	2.84	4.07
	(4.61)	(4.68)	(7.55)	(7.21)	(3.31)	(3.27)
5th Decile EC	1.95	1.51	-0.24	-1.68	1.58	1.80
	(4.58)	(4.67)	(8.26)	(8.10)	(3.29)	(3.28)
6th Decile EC	-6.12	-4.80	3.73	4.14	-3.49	-2.45
	(4.72)	(4.81)	(6.96)	(6.61)	(3.28)	(3.24)
7th Decile EC	1.63	4.52	9.41	11.51*	4.61	5.99*
	(4.67)	(4.66)	(6.68)	(6.17)	(3.22)	(3.15)
8th Decile EC	1.08	2.98	14.47**	14.94***	3.89	5.19*
	(4.77)	(4.77)	(6.23)	(5.74)	(3.20)	(3.12)
9th Decile EC	1.23	0.85	5.01	4.92	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 from equation (1b), described in Section 4.2. Columns denote sample of applicants included. Robust standard errors are reported in parentheses. *, **, *** 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; 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 34), and dummies for race, gender, assigned region, whether the applicant was assigned to his or 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 A4: 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 main appendix.

Table A5: 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 the main appendix.

Table A6: Interacting Treatment with Other Demographics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Extra Grants ($100s)	1.77***	1.65***	1.72***	1.62***	1.77***	1.79***	1.73***	1.77***	2.08***	2.00***	1.82**
× 1st Decile EC	(0.41)	(0.44)	(0.42)	(0.56)	(0.59)	(0.41)	(0.42)	(0.42)	(0.50)	(0.42)	(0.82)
Extra Loans ($100s)	2.06***	1.83**	2.03***	1.86**	2.77***	2.13***	2.04***	1.84***	1.89**	2.20***	2.28**
× 1st Decile EC	(0.69)	(0.72)	(0.70)	(0.81)	(0.82)	(0.70)	(0.70)	(0.70)	(0.75)	(0.70)	(1.00)
Extra Grants		0.14									0.16
× Female		(0.21)									(0.21)
Extra Loans		0.30									0.32
× Female		(0.25)									(0.25)
Extra Grants			0.15								0.14
× White			(0.19)								(0.20)
Extra Loans			0.13								0.13
× White			(0.21)								(0.22)
Extra Grants				0.01							0.01
× Age				(0.01)							(0.01)
Extra Loans				0.01							0.01
× Age				(0.02)							(0.02)
Extra Grants					-0.00						0.03
× “Fit” Score					(0.11)						(0.11)
Extra Loans					-0.18						-0.19*
× “Fit” Score					(0.11)						(0.11)
Extra Grants						-0.10					-0.14
× Region Not First Choice						(0.19)					(0.21)
Extra Loans						-0.22					-0.16
× Region Not First Choice						(0.22)					(0.23)
Extra Grants							0.09				0.11
× Subject Not First Choice							(0.20)				(0.21)
Extra Loans							0.07				0.02
× Subject Not First Choice							(0.23)				(0.23)
Extra Grants								-0.03			-0.03
× Local								(0.19)			(0.20)
Extra Loans								0.44**			0.42*
× Local								(0.21)			(0.22)
Extra Grants									-0.06		-0.06
× Regional Cost ($1,000s)									(0.05)		(0.05)
Extra Loans									0.03		0.00
× Regional Cost									(0.05)		(0.05)
Extra Grants										-0.06**	-0.06**
× Federal Loans ($10,000s)										(0.03)	(0.03)
Extra Loans										-0.03	-0.03
× Federal Loans										(0.03)	(0.03)
N	7295	7295	7295	7295	7295	7295	7295	7295	7295	7295	7295
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Mean of Dep. Var.	75.17	75.17	75.17	75.17	75.17	75.17	75.17	75.17	75.17	75.17	75.17

Table shows Linear Probability Model (OLS) regressions of whether an applicant joins TFA. Column (1) shows estimates from equation (1b), described in Section 4.2. The other columns show this specification but add interactions of the treatment variables with a demographic variable (one in each column). Robust standard errors are reported in parentheses. *, **, *** 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. Every regression includes: a linear age term, a linear term for the applicant's “fit” with TFA (described in footnote 34), and dummies for race, gender, assigned region, whether the applicant was assigned to his or 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). When a demographic variable is interacted with treatment, so is missing data dummy for that variable. All regressions include fixed effects for the batches in which applicants' TGL awards were processed, the point at which randomization occurred (“Batch FEs”).

Table A7: Treatment Effects on Actual and Expected Occupations, All Coefficients

	Joined TFA	Teaching			Private sector			Grad student	
		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	0.29	-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	0.58	0.54	-0.16	-0.52	-0.74	-0.04	-0.41	-0.53
× 2nd Decile EC	(0.65)	(0.70)	(0.89)	(0.93)	(0.51)	(0.60)	(0.73)	(0.29)	(0.63)
Extra Funding	-0.21	0.20	-1.13**	-0.62	-0.07	0.34	0.59	0.12	0.62*
× 3rd Decile EC	(0.37)	(0.36)	(0.54)	(0.55)	(0.19)	(0.30)	(0.43)	(0.26)	(0.38)
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	0.27	-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	0.02	-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	-0.11	-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	-0.16
× 10th Decile EC	(0.31)	(0.30)	(0.44)	(0.43)	(0.19)	(0.26)	(0.41)	(0.19)	(0.33)
1st Decile EC	-12.14**	-12.53***	-1.86	-3.37	5.13	0.30	0.18	4.32*	-0.90
	(5.29)	(4.73)	(6.04)	(6.14)	(3.26)	(3.59)	(5.35)	(2.44)	(4.07)
2nd Decile EC	-0.49	-7.50	0.01	1.32	2.87	9.32*	-4.92	4.79	-3.16
	(6.12)	(5.83)	(7.67)	(7.80)	(4.33)	(5.10)	(6.53)	(2.93)	(5.26)
3rd Decile EC	7.59	-3.89	19.06***	11.17*	-2.61	-3.09	-12.12**	4.55	-9.52**
	(5.14)	(4.71)	(6.27)	(6.69)	(2.80)	(3.53)	(5.35)	(2.86)	(4.09)
4th Decile EC	4.33	-2.49	3.18	0.94	-3.81	-0.20	1.39	2.94	-1.53
	(5.02)	(4.38)	(6.32)	(6.49)	(2.46)	(3.86)	(6.00)	(2.48)	(4.36)
5th Decile EC	3.90	-1.54	5.46	5.68	-2.81	1.32	-5.01	0.78	-1.83
	(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	9.71	-1.26	-2.27	-2.69	1.79	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	-2.27	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*	-4.27
	(5.05)	(4.53)	(6.16)	(6.46)	(2.79)	(3.72)	(5.56)	(2.87)	(4.19)
9th Decile EC	-1.93	-4.56	3.21	1.98	-0.42	-1.39	1.54	4.74*	-2.75
	(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). Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.1$, 0.05, and 0.01, respectively. The full regression specification generating these estimates is reported in the Online Appendix. "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 34). All regressions include fixed effects for the batches in which applicants' TGL awards were processed, the point at which randomization occurred ("Batch FEs").

Grant-Ineligible Applicants

As described in Section 2, 15% of TGL applicants received the minimum award, comprised entirely of loans. While they are not included in our experiment, in the first two years we worked with TFA on our experiment, we randomized them 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 A2 shows the distribution of base awards for these grant-ineligible applicants. Table A8 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 A2: Base Award Offers, Grant-Ineligible Applicants (2015–2016)

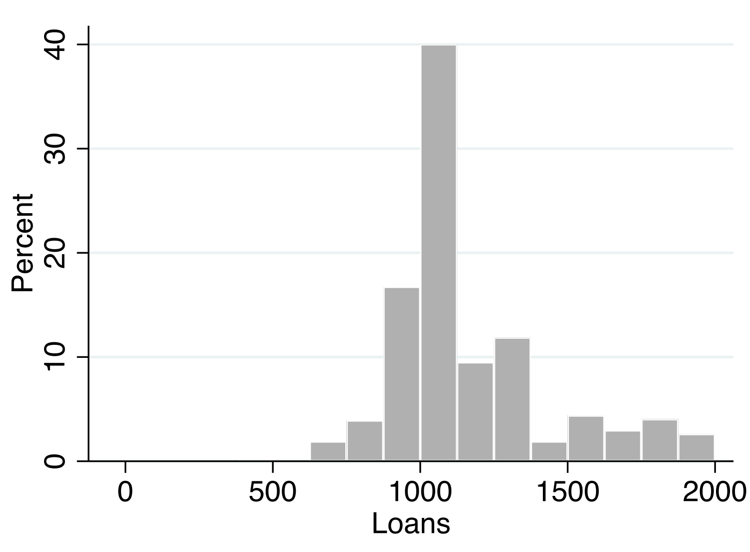


Figure shows a histogram of base award loan offers to grant-ineligible applicants in the 2015 and 2016 cohorts. Bin width is $125. TGL grant offers were always zero for these applicants.

Table A8: 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. Sample is restricted to grant-ineligible applicants in the first two years of the experiment. Robust standard errors are reported in parentheses. *, **, *** denote $p < 0.1$, 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 34), and dummies for race, gender, assigned region, whether the applicant was assigned to his or 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").

Regression Specifications for Figures and Tables

This section clarifies the regression specifications used to create our figures and tables. Each row of Table A9 lists a table or figure along with the specification number(s) that we used to construct it. The specifications not mentioned in the main text are listed below, along with brief notes on their use. Table A10 defines the variables used in the specifications that were not defined in the main text. Note that, depending on the years of data to which the specification is applied, some terms may drop out (e.g., terms pertaining to the *$1800 Grant* treatment will not appear in regressions that only include data from 2015–2016). Finally, in all specifications, we omit demographics (i.e., $\boldsymbol{\delta} \cdot \mathbf{X_i}$), with the understanding that this term is included when explicitly mentioned in the table notes.

Table A9: Regression Specifications Used to Produce Figures and Tables

Figure or Table	Specification number(s)
Figure 3	A1a, A1b
Figure 5	A1a, A1b
Figure 6	A1a, A1b
Figure 7	A1c
Table 4	1a, 1b (main text)
Table 5	1a, 1b (main text, decile dummies added to 1a)
Table 7	2 (main text, notes for Table 7)
Table 8	A2
Table A2	A1a (with a different dependent variable)
Table A3	1b (main text)
Table A6	1b (main text, with additional interactions)
Table A7	A2
Table A8	1a (main text)

For each figure and table that uses a regression, this table lists the specification(s) used to construct it.

Specification (A1a) determines the effects of each of our treatments pooled across all deciles, while specification (A1b) breaks the effects down by decile, and specification (A1c) compares the effect on the 1st decile to the effect on the pooled 2nd–10th deciles. In applying specification (A1a) to the "All" column of Table (A2), decile dummies like those in specification (A1b) must be added.

Table A10: Variable Definitions

Variable	Definition
G600	Dummy for *$600 Grant* treatment
L600	Dummy for *$600 Loan* treatment
G1200	Dummy for *$1200 Grant* treatment
G1800	Dummy for *$1800 Grant* treatment
L1800	Dummy for *$1800 Loan* treatment
ExtraFunds	Sum of *ExtraGrants* and *ExtraLoans*

This table defines the variables used in the regression specification appendix that are not defined in the main text (see Section 4.2).

$$\begin{aligned} JoinTFA_i = {} & \beta_{L600} \cdot L600_i + \beta_{G600} \cdot G600_i + \beta_{G1200} \cdot G1200_i + \\ & \beta_{G1800} \cdot G1800_i + \beta_{L1800} \cdot L1800_i + \sum_j \gamma^j \cdot Batch_i^j + \varepsilon_i \end{aligned} \tag{A1a}$$

$$\begin{aligned} JoinTFA_i = {} & \sum_{d=1}^{10} \beta_{L600}^d \cdot L600_i \cdot Decile_i^d + \sum_{d=1}^{10} \beta_{G600}^d \cdot G600_i \cdot Decile_i^d + \\ & \sum_{d=1}^{10} \beta_{G1200}^d \cdot G1200_i \cdot Decile_i^d + \sum_{d=3}^{10} \beta_{G1800}^d \cdot G1800_i \cdot Decile_i^d + \\ & \sum_{d=1}^{10} \beta_{L1800}^d \cdot L1800_i \cdot Decile_i^d + \sum_{d=1}^{9} \varphi^d \cdot Decile_i^d + \sum_j \gamma^j \cdot Batch_i^j + \varepsilon_i \end{aligned} \tag{A1b}$$

$$\begin{aligned} JoinTFA_i = {} & \beta_{L600}^1 \cdot L600_i \cdot Decile_i^1 + \beta_{L600}^{2-10} \cdot L600_i \cdot (1 - Decile_i^1) + \\ & \beta_{G600}^1 \cdot G600_i \cdot Decile_i^1 + \beta_{G600}^{2-10} \cdot G600_i \cdot (1 - Decile_i^1) + \\ & \beta_{G1200}^1 \cdot G1200_i \cdot Decile_i^1 + \beta_{G1200}^{2-10} \cdot G1200_i \cdot (1 - Decile_i^1) + \\ & \beta_{L1800}^1 \cdot L1800_i \cdot Decile_i^1 + \beta_{L1800}^{2-10} \cdot L1800_i \cdot (1 - Decile_i^1) + \\ & \beta_{G1800}^1 \cdot G1800_i \cdot Decile_i^1 + \beta_{G1800}^{2-10} \cdot G1800_i \cdot (1 - Decile_i^1) + \\ & \sum_{d=1}^{9} \varphi^d \cdot Decile_i^d + \sum_j \gamma^j \cdot Batch_i^j + \varepsilon_i \end{aligned} \tag{A1c}$$

In the following specification, the variable Y_i represents an actual or expected occupational outcome, as defined in the notes of Tables 8 and A7.

$$Y_i = \sum_{d=1}^{10} \beta^d \cdot ExtraFunds_i \cdot Decile_i^d + \sum_{d=1}^{9} \varphi^d \cdot Decile_i^d + \sum_j \gamma^j \cdot Batch_i^j + \varepsilon_i \qquad \text{(A2)}$$

In the following specification, $Demographic_i$ represents one of the demographics listed in the notes of Table A2.