Can Social Information Affect What Job You Choose and Keep?

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Abstract

We show that the provision of social information influences a high-stakes decision and this influence persists over time. In a field experiment involving thousands of admits to Teach For America, those told about the previous year’s matriculation rate are more likely to accept a teaching job, complete training, start, and return a second year. To show robustness, we develop a simple theory that identifies subgroups where we expect larger treatment effects and find our effect is always larger in those subgroups. That social information can have a powerful, persistent effect on high-stakes behavior broadens its relevance for policy and theory.

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1. Introduction

The idea that policy-makers can use cheap, subtle interventions to shape behavior has recently risen to prominence. Guiding this policy work is a wave of economic models that include psychological and social forces as motivators of decision-making. The question of which forces are sufficiently robust to be critical parts of such models is an important one. A natural criterion is that the force should affect behavior in high-stakes situations, and do so persistently. In this paper, we provide the first evidence that social information (i.e. providing information about the previous decisions of others) satisfies these conditions. Specifically, we show that when an established non-profit (Teach For America or for short, “TFA”) offers high-achieving college graduates modestly paying jobs teaching in underperforming schools, individuals are more likely to take the job when the offer letter includes information about the high percentage of people who accepted the job in the previous year. Moreover, the experimentally informed group is more likely to train for the job, begin the job, and return to the job the following year. To highlight the robustness of our finding, we develop a theory of which subgroups should be more affected by the treatment and show that the effect is, in fact, larger for several such subgroups.

The effect of social information on low-stakes decisions is well established. People are more likely to donate to charity when they learn a high percentage of others donate (Frey and Meier 2004, Martin and Randal 2008), and

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1 The Behavioral Insights Team (BIT) in the U.K. was explicitly formed to consider and implement such interventions. In the U.S., the Office of Regulatory Affairs and the Consumer Financial Protection Bureau have conducted similar work and plans to form an
2 Defaults have been shown to persistently affect high-stakes decisions like retirement savings (e.g. Carroll et al. 2009) and insurance plan choice (e.g. Ericson 2012). Field (2009) finds changing the framing of a financial aid offer affects job choice. Non-social information has also been shown to affect decisions; Jensen (2010) finds that providing high school students with information about wages can increase graduation rates two years later.
3 Background on the Teach For America organization is provided in Section 2.
they donate larger amounts when told of a large previous donation (Croson and Shang 2008, Shang and Croson 2009). They are also more likely to contribute to a movie rating website (Chen et al. 2010), take an environmentally friendly action, and contribute in a laboratory public goods game when told that others do so. Together, such studies show that social information works, and can persist, in low-stakes environments, but no previous work has shown that social information is effective, much less persists, for a high-stakes decision.

Consequently, it is an open question whether social information is sufficiently important to be a critical part of behavioral theories that aim to also explain high-stakes behavior. Our study addresses this question.

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5 See a vast experimental literature starting with Keser and van Winden (2000); Fischbacher, Gächter and Fehr (2001); and Potters, Sefton and Vesterlund (2005).

6 Shang and Croson (2009) show that information about another donor’s large gift makes subjects give more today and makes them more likely to give next year. Allcott and Rogers (2012) show the effects of social information on energy consumption decay gradually over time, suggesting evidence of persistence.

7 Stakes in previous studies range from donating a dollar to an art gallery, picking up a piece of litter, hanging and reusing a hotel towel, conserving a few dollars of energy per month, donating a few dollars to a student fund, and contributing a few dollars in a laboratory public good game. At the high end of the stakes spectrum, the literature has found that providing information about a large donation by a previous donor increased gifts by about $40 per person in a radio drive (Shang and Croson 2009). In research conducted concurrently with this paper, Hallsworth et al. (2013) find that reporting the high percentage of individuals who pay their taxes on time in reminder letters significantly increases taxes paid to the UK government within a 23-day period after the letters were mailed. With average debts of around £2,800 in the experiment, the retiming of tax payments may have had significant costs as well.

8 More generally, after observing a social or behavioral force influence choice in a low-stakes setting, we need to investigate whether such influence will be eclipsed by larger incentives or, alternatively, whether it will scale with the stakes and still affect choice (see Camerer and Hogarth 1999, List and Levitt 2007, and Camerer 2011). Similarly, we need to determine whether such effects will impact behavior only briefly, or whether they will persist over time (see Gneezy and List 2006). We do this for social information and think that it is a useful exercise for other social or behavioral forces that have been shown to impact behavior in low-stakes or static settings.
We show that social information can influence a high-stakes decision by adding one line to the end of randomly selected job offer emails sent by TFA: “Last year, more than 84% of admitted applicants made the decision to join the corps, and I sincerely hope you join them.”\footnote{Teach For America collectively refers to all of its teachers as the “corps”. The “and I sincerely hope you join them” is unlikely to be driving any effect we see, for reasons discussed near the end of Section 2.2.} We show that the effect can persist by following our subjects for two years after they receive the treatment. Those who received the social information are 1.8 percentage points more likely to accept the job, and the effect stays as large throughout the two-year follow-up.

We subsequently develop a theory (Section 4) showing that under natural assumptions we expect to see a larger effect of our treatment on matriculation in subgroups with a lower baseline matriculation rate. Intuitively, when the vast majority of individuals accept the TFA job, a subgroup with a lower baseline matriculation rate should have a higher proportion of marginal individuals (i.e. those “on the fence” about their decision to accept TFA’s offer).\footnote{Anecdotally, many applicants to TFA consider the program to be their top post-college job choice and are quite likely to accept the offer conditional on being admitted; we would not expect our treatment to affect their behavior. This approach is consistent with the approach in Frey and Meier (2004), which identifies marginal subgroups as those who have given to student funds in some, but not all, previous semesters.} The model helps to establish the robustness of our results, but it is also of independent interest for researchers investigating treatment effects on binary variables, as it provides natural conditions — not specific to this paper — for when we expect a larger treatment effect. Constraining ourselves to analyze subgroups as identified by our theory serves as a guard against data mining while allowing us to increase power and test the robustness of our effect.

When we look to the subgroups highlighted by the model — those with lower baseline matriculation rates — the effect size jumps to between 3 and 5 percentage points. We identify this result for numerous uncorrelated subgroups.
including one we identified (and stratified on) midway through the experiment. Furthermore, we follow individuals who accept the job to see who attends a mandatory training program, starts teaching in the first semester, returns to teach in the second semester, and returns again to teach in the second year.\textsuperscript{11} We find that the treatment effect persists at the same high levels even into the second year of teaching (by which time 25\% of those who accepted the job in the control group have left the program). Based on our results, TFA has started including a line of social information about their historical matriculation rate in all admissions letters.

There are a number of ways to benchmark the size of our effect. In the terminology of DellaVigna and Gentzkow’s (2009) survey of the empirical evidence on persuasion, our effect corresponds a persuasion rate of 8.4\%; that is, our treatment persuaded 8.4\% of the subjects who were not going to join TFA to do so.\textsuperscript{12} In subgroups with more marginal admits, our persuasion rate jumps to between 12\% and 14\%. It is perhaps surprising that our very subtle intervention has a persuasion rate that is comparable to more intensive interventions, such as the effect of door-to-door fund-raising on giving (DellaVigna, List, and Malmendier 2012, which has a persuasion rate of 11\%), the effect of reporting high levels of seed funding in a direct mail campaign on giving (List and Lucking-Reiley 2002, 8.2\%), and the effect of exposure to a media outlet that is either liberal (Gerber, Karlan, and Bergan 2009, 19.5\%) or conservative (DellaVigna and Kaplan 2007, 11.6\%) on voting behavior.

That one line of social information can significantly affect whether a person takes and keeps a teaching job speaks to the strength of the force and the efficacy of such an intervention — particularly since our intervention is so small

\textsuperscript{11} Previous data from TFA shows that 95\% of the individuals who fail to complete their two-year commitment to TFA have left by the start of the second teaching year.

\textsuperscript{12} In our setting, persuasion rate is \((y_T - y_C)/(1 - y_C)\), where \(y_T (y_C)\) is the fraction of subjects who join in the treatment (control) group.
relative to the amount of information admitted applicants receive about TFA during the application and interview process and after they have been admitted to the program. A follow-up survey confirms that though the intervention was subtle, the social information treatment measurably increased beliefs about the current year’s matriculation rate. The survey also suggests that our treatment’s effect on beliefs may wear off over time, even while the treatment’s effect on behavior persists. Finally, our survey does not find evidence for an information channel through which behavior is influenced, potentially lending credence to a story based on desire for conformity (e.g. Bernheim 1994).

Together our results present strong evidence for the importance of social information for theory as well as for policy. Our intervention was subtle, free, and occurred at a single point in time, but it had a persistent impact on a high-stakes decision. Social information has the potential be an effective lever to persistently affect behavior in important, high-stakes environments.

2. Background

Teach For America is a non-profit organization, founded in 1990, which recruits “committed recent college graduates and professionals of all backgrounds to teach for two years in urban and rural public schools.” Potential teachers, hoping to start the following fall, apply to TFA on a rolling basis between early September and late April. At four different times during the admission season,

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13 Admitted applicants report that they are well informed about TFA. In a TFA conducted survey, 67% of admitted applicants rate contact quality with TFA as excellent and 90% rate it as either excellent or good. In addition, TFA admits are well educated, a population that one might expect to be able to process information and make good decisions in a high-stakes environment.

TFA evaluates applicants and decides whether to admit or reject them. Each evaluation (called a “wave”) is a nine-week process of phone and in-person interviews (see Figure 1, described below) that is designed to both evaluate applicants and to give them information about TFA. Admitted applicants receive an offer letter via email and have approximately two weeks to decide whether to accept the TFA job offer.

If an admitted applicant accepts the offer, she has between 2 and 7 months (depending on her wave) before she must attend a one-month training program known as “Summer Institute” held in the July before teaching begins. TFA withdraws the acceptance of admits who fail to attend Summer Institute. Actual teaching begins in September and continues for two school years. The timing of the events surrounding our experiment is illustrated in Figure 1. The top timeline illustrates our intervention in the context of the other information TFA applicants receive during the admissions process, while the bottom timeline shows important training and teaching milestones.

3. Experimental Design

Our experiment was straightforward. Subjects randomly received either the standard TFA admissions letter (the Control condition, n = 3,337) or that same letter with an additional line at the end: “Last year more than 84% of admitted applicants made the decision to join the corps, and I sincerely hope you join them” (the Social Information condition n = 3,348). The 84% figure averages over all admits from the previous year. Figure A1 in the Appendix shows that adding the sentence is only a small change to the standard, full-page admissions letter.

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15 In the year of our experiment, there were roughly 50,000 applications to TFA of which 8,245 were accepted, making the acceptance rate approximately 17%.
16 Summer Institute teaches TFA members a variety of skills, including classroom management, lesson planning, and pedagogy. To meet certification standards of their districts, TFA teachers must also “complete coursework toward the next level of certification or licensure.” (TFA website)
We performed the randomization of admits into the treatment and control groups ourselves, and TFA staff members who were in contact with admits did not know who received each treatment. Demographics were balanced across treatments, for the full sample as well as within each subgroup that we consider below (see Table A1 in the Appendix for balance and Section 4 for definition of subgroups).

Although the experiment is relatively simple, several facets are worth pointing out. First, although we intended our added sentence to be purely informational, Teach for America felt that the sentence was too abrupt without the fragment “...and I sincerely hope you join them.” Although this part of the sentence could potentially be driving whatever effect we see, it is unlikely as very similar wording is contained earlier in the letter in both the Control and Social Information conditions (referencing TFA corps members: “…and we hope that you will join them in this important work”). Second, the fact that Teach For America admits applicants at four points in time essentially creates four small experiments (one for each admissions wave). We will deal with this using regression analysis in Section 5.1.

In our original design, we had a third condition meant to induce variation in the matriculation rate given to TFA admits. To achieve this without using deception, the condition used the same wording as the Social Information condition but provided the matriculation rate of a subset of last year’s admitted applicants (those who applied in the same wave in the previous year). However, TFA staff ultimately changed our wording so no admitted applicants would get the same exact letter with different numbers. Letters in this altered Wave-Specific Information condition read (boldface added to emphasize the difference from the Social Information condition): “At this deadline last year, more than 92% of admitted applicants made the decision to join the corps, and I sincerely hope you join them.” Unfortunately, this change in wording prevented us from identifying the effect of changing the matriculation rate (from 84% to 92%) separately from
the effect of changing the specificity of the wording (see Goldstein, Cialdini and Griskevicius 2008 about specificity of social information). As a result, we asked TFA to drop the *Wave-Specific Information* treatment after the second wave. The analysis in the body of this paper only includes data from the treatment that was run as we originally designed it, the *Social Information* treatment. The results do not qualitatively change if we include subjects who received the *Wave-Specific Information* treatment (see Table A3 in the Appendix).

4. Marginal Subgroups

Anecdotally, many admits consider joining TFA to be their best possible job outcome. For these admits, our social information intervention is unlikely to have a significant effect on behavior. As such, we will find it convenient to look for subgroups where we expect to find a larger fraction of admits who are “on the fence”, i.e. whose decision to join TFA can be changed by our treatment (see Frey and Meier 2004 for use of marginal subgroups in the context of a social information treatment). To discipline our identification of such *marginal subgroups*, we develop a simple theory that implies a data-based criterion under rather natural conditions: we expect a larger effect in subgroups that have a lower (higher) baseline matriculation rate when the modal utility gain from matriculating is positive. Finding more admits’ decisions affected in subgroups

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17 The second-wave matriculation rate in the previous year was slightly above 84% (the same as the rate averaged across all admits in the previous year). By keeping the *Wave-Specific Information* condition for the second wave, we attempted to identify the effect of changing the specificity of the language. As this was not part of our original design, we were underpowered to conclusively identify the effect, but we find that the “At this deadline last year” language is directionally less effective than the simpler “Last year” language. Results are consistent in the first wave. This contrasts somewhat with Goldstein, Cialdini and Griskevicius (2008), which finds that more specific social information is a more effective motivator, but again we are underpowered to say anything conclusively. Detailed analysis is available upon request.

18 If anything, adding the Wave-Specific treatment group attenuates a few significance levels, which again, provides some supporting evidence for the previous footnote.
predicted by theory provides additional evidence that our results are due to the impact of social information; that is, subgroup analysis provides a robustness check of the perhaps surprising main result of the paper. In addition, the subgroup analysis gives us a way to identify the magnitude of the treatment in relevant subpopulations. Finally, the theory is of independent interest, as it can guide any researcher seeking to test the robustness of a treatment on a binary outcome. The fact that the criterion for finding subgroups is data-based provides a guard against the data mining that can be an issue in such settings.

4.1. A Formal Model of Marginal Subgroups

Let an untreated (i.e. not exposed to social information) TFA admit have utility for joining TFA, $u$, and utility for a best outside alternative, $a$. Whether such an admit joins TFA is determined by whether $u - a \geq 0$. We call $e \equiv u - a$ the admit’s excess utility of TFA. The admit is also characterized by a vector of observable information, $x \in X$, which we will call his characteristics. Since we deal with a population of such admits, we consider their characteristics and excess utilities to be distributed according to some probability density function $f(e, x)$. The fraction of admits that join without being treated (the baseline) is

$$\int_{x} \int_{0}^{\infty} f(e, x) \cdot de \cdot dx.$$  

To model treatment, assume that when admits with characteristics $x$ are exposed to social information, those with $e \in [-\varepsilon(x), 0]$ join TFA, even though they would not have done so without treatment. Note that this does not assume that treatment increases the excess utility of all admits with the same characteristics by
the same amount.\footnote{To see this, consider a more general model where an admit with characteristics \((e, x)\) has her excess utility increased by \(\delta(e, x) > 0\). The set of admits pushed into joining TFA by the treatment for some characteristic \(x\) is then \(\{e : -\delta(e, x) \leq e \leq 0\}\). We are assuming that this set is a simple closed interval with upper bound of 0, which intuitively means that, holding \(x\) constant, if the treatment doesn’t change the behavior of someone with \(e’\), then it also doesn’t for someone else with \(e’’ < e’\).} Call \(\varepsilon(x)\) the marginal range for the treatment. The extra fraction of admits that join when treated, which we call the matriculation effect, is

\[
\int_x \int_{-\varepsilon(x)}^0 f(e, x) \cdot de \cdot dx.
\]

Two forces drive the matriculation effect. The first, codified by \(\varepsilon(x)\), is how much treatment affects the excess utility of the individual. In our setting, we have little intuition for how this varies with characteristics. The second, codified by \(f(e, x)\), is how many admits are close enough to \(e = 0\) for the treatment to change their choice behavior. We call such admits marginal, and we feel that we have a better intuition for which characteristic subgroups will have more marginal admits.

To formalize this intuition, consider two groups of characteristics, \(A\) and \(B\). Randomly choosing such groups is unlikely to identify more marginal admits, so we need to add some structure by restricting the conditional distributions \(f(e \mid x \in A)\) and \(f(e \mid x \in B)\). For simplicity, we will assume that they are single-peaked with distinct modes \(e^*_A \neq e^*_B\). To give intuition to our further restrictions, we consider a thought experiment: given a value of \(e\) drawn from either \(A\) or \(B\) with equal likelihood, if the distributions conditional on \(A\) and \(B\) are known to an observer, which subgroup does she think is more likely to have produced \(e\)? Our first restriction, the modal inference property states that if the observer sees \(e^*_A\) or \(e^*_B\), then she will think it more likely that the draw was from \(A\) or \(B\), respectively; formally \(f(e^*_A \mid x \in A) > f(e^*_A \mid x \in B)\), with a similar condition for \(e^*_B\). The second restriction, the one-switch property, means that as
the observed $e$ increases, the observer will only switch the subgroup she thinks is more likely once, codifying the idea that we think of one subgroup as having “higher $e$”. Formally, the property is equivalent to single-crossing of the two distributions, that is, there is a unique value $\hat{e}$ such that $f(\hat{e}|x \in A) = f(\hat{e}|x \in B)$. If two single-peaked distributions obey the properties laid out in this paragraph, we call them **simple shifts** of each other.\(^{20}\)

With two more assumptions, we can present a sufficient condition for one subgroup to have a bigger matriculation effect than another. First, we assume positive modes, which is natural if the baseline within both subgroups is large.\(^{21}\) Second, we assume that we do not have much intuition for how the marginal range depends on characteristics; formally, our prior over the size of the marginal range, $g(e)$, does not depend on characteristics. We call such a prior **characteristic-agnostic** — it serves to prevent us from singling out subgroups based on assumptions for which we have little intuition. Now we can state our result. The intuition behind the proof is illustrated in Figure 2.

**Proposition 1:** If the distributions of $e$ conditional on characteristic subgroups $A$ and $B$ are simple shifts of each other, and these distributions have strictly positive modes, then with a characteristic-agnostic prior, a larger baseline matriculation rate in $B$ than in $A$ implies a larger expected matriculation effect in $A$ than in $B$.

**Proof:** First, note that if we are considering an expectation over some characteristic-agnostic prior about marginal ranges, $g(e)$, we are essentially just adding one more integral to our expression for treatment effect:

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\(^{20}\) The terminology is motivated by the fact that these properties are also met by translations of distributions, i.e. $g(e) = f(e - \Delta)$ for some $\Delta > 0$, although we do not require this much structure to prove our result.

\(^{21}\) Note that the logic can be reversed if the baseline is low, which would then require an assumption that both modes are negative.
\[ \int_0^\infty \int_x f(e, x) \cdot de \cdot dx \cdot g(\epsilon) \cdot d\epsilon. \] From this expression, it is clear that if we can show our result for any constant marginal range \( \epsilon \), then we will also have shown it for the expectation. Hence, we assume a constant \( \epsilon \). Now, assume that the baseline acceptance rate is larger in \( B \), that is, \( 1 - F(0|x \in B) > 1 - F(0|x \in A) \). The one-switch property implies that there is a unique point \( \hat{\epsilon} \) where \( f(\hat{\epsilon}|x \in A) = f(\hat{\epsilon}|x \in B) \). Below \( \hat{\epsilon} \), the density is bigger in one of the subgroups, and above \( \hat{\epsilon} \), in the other. By the modal inference property, it must be that \( \hat{\epsilon} \) is between the two modes, \( e^*_A \) and \( e^*_B \). It cannot be that \( e^*_B < \hat{\epsilon} < e^*_A \), because if that were the case, then \( f(e|x \in A) < f(e|x \in B) \) for all \( e \leq \hat{\epsilon} \), which would mean that \( F(0|x \in A) < F(0|x \in B) \), contradicting that the baseline is larger in \( B \). Therefore, \( e^*_A < \hat{\epsilon} < e^*_B \), and hence \( f(e|x \in A) > f(e|x \in B) \) for \( e < \hat{\epsilon} \). So, for any constant \( \epsilon \), the definition of the treatment effect yields the result. And as discussed at the beginning of the proof, this implies the result for the expectation relative to a characteristics-agnostic prior. \( \blacksquare \)

Hence, under the assumptions above, subgroups with a lower baseline matriculation rate are where we expect to see bigger matriculation effects.\(^\text{22}\)

4.2. Subgroups in the Context of TFA

Bringing this theory to the context of our experiment, we will now introduce the marginal subgroups that form an important part of our analysis. Before doing so, however, we should note that even restricting ourselves to groups with lower baseline matriculation as suggested by the theory, there remains some flexibility in how we define the subgroups, which might lead to concerns about data mining or multiple hypothesis testing. We aim to assuage these concerns in Section 5.3 where we analyze the robustness of subgroup

\(^\text{22}\) The same logic suggests that if a researcher has a setting where baselines are low, she should find more marginal subjects in subgroups that have larger baseline.
definitions and show that the particulars of the definitions do not change the results.

The first subgroup we look at takes advantage of the fact that TFA assigns each admitted applicant to a teaching position using that applicant’s rank-order preferences over subjects to teach and geographic regions in which to teach (see Featherstone 2013 for a more detailed description of the matching mechanism). Admits submit these rankings with their initial applications (i.e. before being treated). Unsurprisingly, those who do not receive their first choice for either region or subject are substantially less likely to join TFA than those who do. We call this subgroup the Disappointing Assignment subgroup; its complement is the Pleasing Assignment subgroup.\textsuperscript{23} Table 1 lists the size of the subgroups and their baseline matriculation rates (i.e. the percentage of subjects who say yes to the TFA job in the control condition for each subgroup). The difference in baseline matriculation rates between the Disappointing Assignment and the Pleasing Assignment subgroup is significant (test of proportions $p < 0.001$), and the difference between the subgroups holds as applicants step through the milestones to teaching for TFA in the second semester of the second year (i.e. January 2013). After the first two admission waves, we stratified our randomization by Disappointing Assignment.\textsuperscript{24}

Our second marginal subgroup takes advantage of the subjective “fit score” assigned by TFA staff and alumni during the interview process. “Fit” is

\textsuperscript{23} Assignment provides a clean classification but is not random. TFA attempts to match everyone to their most preferred regions and subjects. While on many observables there are no differences between those who get their first choices and those who do not (gender, rank of undergraduate university, race) there are differences on other dimensions. In particular, individuals coming from large metropolitan centers are less likely to get their first choices, which is likely a result of applicants from metropolitan areas preferring to teach in metropolitan areas.

\textsuperscript{24} See Table A3 in the Appendix for number of subjects in each treatment in each wave, including how many were in the Disappointing Assignment and Pleasing Assignment groups.
meant to be an assessment of how well an applicant aligns with TFA’s organizational objectives. We define the *Moderately Aligned* subgroup as those with a fit score below the median for admitted applicants; its complement is the *Highly Aligned* subgroup. Unsurprisingly, those in the *Moderately Aligned* subgroup are less likely to matriculate than those in the *Highly Aligned* subgroup (see Table 1). Again, this difference is significant (test of proportions $p < 0.001$) and persists as admits step through the milestones.

The final subgroup we consider takes advantage of the fact that TFA asks admits where they were in their decision process (i.e. how certain they were that they were going to join TFA) when they received their admissions letter. This question is on a 7-point Likert scale ranging from “I was certain I would join” to “I was certain I would not join”. It makes sense that our treatment would not have a large effect on those who were certain they would join, so we define the *Not Certain to Join* subgroup as those who answered anything else; the complement to this group is the *Certain to Join* subgroup. Those in the *Not Certain to Join* subgroup are much less likely to matriculate (see Table 1, test of proportions $p < 0.001$) and this difference persists as admits proceed through the milestones.

An important caveat for the *Not Certain* subgroup is that it is based on a response to a survey question asked at the time of initial commitment, that is, after treatment. Fortunately, the question essentially asks subjects to retrospectively assess whether they were marginal at the time of treatment:

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25 The fit score is a composite of scores received at the application, phone, and in person interviews. We learned about the fit score measure only after the experiment was run.

26 Note that we name the group “moderately aligned” because those with low alignment are very rarely admitted to the TFA program.

27 The discreteness of fit allows more than 50% of applicants to be at or above the median.

28 The two marginal subgroups are almost completely uncorrelated: the correlation coefficient between being *Moderately Aligned* and receiving a *Disappointing Assignment* is $\rho = -0.003$ (with $p = 0.80$) and is not significantly different from 0. Hence, performing the same analysis for both subgroups is not redundant.
“Please indicate where you were in your decision-making process when you found out you had been accepted and received your regional assignment”. Since subjects make a decision about joining TFA (and are asked to answer this question) within two weeks of receiving their acceptance letter, one might think it is possible that subjects can accurately report what their likelihood of joining was before they were treated. In fact, even though our social information treatment affects matriculation decisions, it does not influence self-reported certainty \((p > 0.985 \text{ for OLS and Ordered Probit specifications})\). Thus, although the Not Certain subgroup is formed based on information gathered after treatment, the information is meant to reflect likelihood before the treatment, and we do not find any evidence this measure was affected by our treatment. We include it because it so closely aligns with the intuition in the theory.

In addition to the three subgroups, we can also show that the matriculation effect is larger when demographics predict a lower baseline matriculation rate. Due to our data agreement with TFA we cannot disclose how individuals with particular demographics respond to the treatment; however, we can show the effect by analyzing how treatment interacts with a “propensity to join TFA” measure estimated based on demographics. The methodology is described in Section 5.3.

Finally, we should note that none of our subgroups are strongly correlated (see Table 1), so running a separate subgroup analysis for each is not redundant; rather, doing so provides several independent demonstrations that the effect of social information is larger in all the subgroups suggested by the theory.

5. Results

As we will show, social information has a significant positive effect on the decision to commit two years to TFA. Depending on the specification, the effect is significant or marginally significant across the entire population. It is larger in
magnitude and more statistically significant when we consider our marginal subgroups. In addition, the effect of the social information treatment persists over time — from initial acceptance to teaching in the fall of the second year — even though nearly 25% of those who initially accept the job offer leave the program during this period.

5.1 Overall Effect of Social Information on Matriculation

Although it does not account for the wave structure of the admissions cycle, the easiest way to visualize our results is to graph the average likelihood of working for TFA over time across subjects in all waves. Figure 3, Panel A shows this likelihood for the entire set of admits, without subgrouping. The downward slope of the solid line shows people leaving the program in the control group and demonstrates the importance of following our treatment effect over time.

We confirm the patterns in Figure 3 by running OLS regressions with fixed effects for the wave in which an applicant was admitted (see Table 2). As can be seen in regressions (1) through (10), when looking across the entire population, the social information treatment increases the likelihood that admitted applicants are still in TFA at a milestone by between 1.5 and 3.1 percentage points. The even-numbered regressions show that estimates of the treatment effect are stable and become more significant when we soak up extra variation by controlling for gender, race, ethnicity, socioeconomic status (based on whether the admit had a full, partial or no Pell grant during college), whether the admit was a math/science major, and whether the admit is coming straight out of college. Without demographic controls, the effect is significant for Initial Commitment ($p < 0.1$) and Teaching Fall 2013 ($p < 0.05$). Once demographic controls are added, these increases become significant at the 10% level or 5% level for all milestones.
5.2 Effect on Matriculation in Marginal Subgroups

In Panel B of Figure 3 and regressions (11) through (20) of Table 2, we show the effect of the social information treatment on both the Disappointing Assignment subgroup and the Pleasing Assignment subgroup. For the Disappointing Assignment subgroup, where we expect to see larger effects, the likelihood of working for TFA increases by between 3.2 and 4.5 percentage points across milestones and specifications, always significantly at the 10% or 5% level. The Pleasing Assignment subgroup, however, is not measurably affected by the treatment.

In Panel C of Figure 3 and regressions (21) through (30) of Table 2 we show the effect of the social information treatment on both the Moderately Aligned and Highly Aligned subgroups. For the Moderately Aligned subgroup, where we expect to see larger effects, the likelihood of working for TFA increases between 3.6 and 5.2 percentage points across milestones and specifications, always significantly at the 5% or 1% level. The Highly Aligned subgroup, however, was not measurably affected by the treatment.

Finally, in Panel D of Figure 3 and regressions (31) through (40) of Table 2 we show an effect of the social information treatment on both the Not Certain to Join and Certain to Join subgroups. For the Not Certain to Join subgroup, where we expect to see larger effects, the likelihood of working for TFA increases between 2.7 and 3.8 percentage points across milestones and specifications, always significantly at the 5% or 1% level save one (Teaching Spring 2013 without demographic controls is significant at the 10% level). The Certain to Join subgroup, however, was not measurably affected by the treatment.

For all of the subgroups, adding one sentence of social information to the offer letter significantly increases the likelihood that an individual joins TFA, and this effect persists over a year later. To provide some context on the size of these effects, note that failing to be assigned to a favorite region and subject (i.e. being
in the *Disappointing Assignment* subgroup) decreases the likelihood of working for TFA at each milestone by between 7.8 and 9.3 percentage points. The one sentence of social information mitigates between 40% and 55% of that decrease for this subgroup, depending on the milestone. Similarly, being in the *Moderately Aligned* subgroup decreases the likelihood of working at TFA by between 5.8 and 8.6 percentage points. The social information mitigates between 50% and 70% of that decrease.

### 5.3 Robustness Checks

In this section, we address whether the results in 5.2 are robust to alternative inclusion rules for the subgroups. First, note that for each of our subgroups, there is a richer set of information that is used to construct them. For the *Disappointing Assignment* subgroup, we used the ranking of regions and subjects submitted by admits along with their applications; for the *Moderately Aligned* subgroup, we used the fit score assigned by the admit’s TFA interviewer; and for the *Not Certain to Join* subgroup, we used a survey response on a 7-point Likert scale. Although our subgroup definitions are specific, they are meant to capture the broader idea that these sets of information somehow shift the distributions of excess utility, as described in Section 4.1.

A more general (although less transparent) way to capture the same idea is to estimate a propensity for initial commitment, \( \hat{p}_S \), based on all possible interactions of a group of variables, \( S \). Per Proposition 1, then, we would expect that the matriculation effect for a subgroup defined by a certain value of \( \hat{p}_S \) would be decreasing in that value. So, if we estimate a \( \hat{p}_S \) based on the information, \( S \), used to construct a given subgroup and then regress our milestone dummies on the interactions between the treatment dummy and \( \hat{p}_S \), we would expect a positive

---

29 By using all possible interactions of a set of dummy variables, we are non-parametrically estimating the probability that an admit joins, conditional on that subgroup information (Angrist and Pischke 2009, Theorem 3.1.4).
coefficient on $p_s$ and the treatment dummy, and a negative coefficient on the interaction. Note that none of this depends on a particular definition of a subgroup, it only depends on what group of variables, $S$, is used to construct $p_s$. Table 3 summarizes the results of this exercise for the information used to construct each of our three subgroups. For all subgroups and at all milestones, we find that the interaction of each $p_s$ with the treatment is negative and always statistically significant.\footnote{\textsuperscript{30} It is worth noting that since $p_s$ is estimated, using it in a regression introduces measurement error. Such error actually serves to bias our results towards zero, that is, with a perfect measured $p_s$, the results in Table 3 would be even stronger (see Sullivan 2001).}

The strategy we have just discussed also allows us to show that demographics can change the size of the matriculation effect in the way described by the theory in Section 4.1. We estimate a propensity to join from our demographic variables, and use the strategy of this subsection (see Table 3).\footnote{\textsuperscript{31} We are not able to use a saturated regression to estimate a propensity from demographics, as there are too many interactions for that to be meaningful. Instead, we estimate propensity without any interactions.} The regressions line up with theory at all milestones at the 1\% level of significance, demonstrating that demographic groups whose baseline matriculation rates are lower have larger matriculation effects, as predicted by theory.

### 5.4 Matriculation Beliefs

Our interpretation of the results assumes that admits who received the line of social information would believe the matriculation rate was higher than those who did not, and that this increase in beliefs would make admits more likely to join TFA. This requires that admits read and processed the social information, that they updated their beliefs, and that the information increased their beliefs.

To test these criteria, we conducted our own survey of the admitted applicants in our experiment. In June 2012, a TFA staff member emailed our
online survey to all admitted applicants, both those who had accepted their offer and those who had not. The survey was not incentivized, but of the 6,685 applicants we analyze, 2,970 filled out the survey — a 44% response rate. While this response rate is high for a non-incentivized survey, we must recognize two limitations of the survey data. First, selection into the survey is not random; specifically, the response rate was higher for admits who accepted their TFA offer than those who did not (48% versus 33%). Second, the survey responses were collected in June 2012 — after all applicants had decided whether or not to accept the TFA offer. It is possible that admits’ beliefs changed to align more closely with the decisions they had made. Hence we interpret the results with reasoned caution.

The first question in the survey directly measured subjects’ beliefs of TFA matriculation rates: “Out of every 100 admitted applicants this year, how many do you think accepted their offer to join Teach for America?” Figure 4 shows the median of survey respondents’ reported beliefs concerning the probability a TFA offer is accepted. First, note that in the control condition, the median belief is consistently 71%, well below 84% (the number provided in the treatment); in fact, 84 is the 81st percentile of responses in the control group, suggesting that if admits

32 The survey was conducted before they had gone to summer institute or started teaching, both of which may change their sentiments toward TFA (Dobbie and Fryer 2011).
33 Standard methods for dealing with differential survey response do not work here. The worst-case bounds of Horowitz and Manski (2000) are non-informative, while the worst-case approach of Lee (2009) requires that whether an admit accepts TFA’s offer be independent of whether they respond to the survey, which is not true. Adjusting for non-response with inverse probability weighting (cf. Wooldridge 2007 for a survey) or propensity score matching (cf. Heckman, Ichimura, and Todd 1997 and Dehejia and Wahba 1999) requires a reasonably predictive propensity score for survey response, which our available covariates do not provide.
34 Note that the survey question asks beliefs about the current year, while the social information was about the previous year and subjects were informed the matriculation rate was “more than 84%”.

20
treated last year’s matriculation rate as their estimate for this year’s matriculation rate. 80 percent of admits were potentially treatable in the expected direction. Second, the treatment significantly increased beliefs overall. The median increased by 3 percentage points, and the difference is statistically significant (rank-sum $p < 0.01$).\(^{35,36}\)

The effect on beliefs is much larger for those who received the treatment more recently. Our survey was conducted in June 2012; subjects received the treatment months earlier. Survey respondents treated less than 14 weeks before the survey (i.e. Waves 3 and 4) show substantial and significant differences in beliefs between the Control and Social Information conditions, while those treated five or seven months before the survey (i.e. Waves 1 and 2) do not. This pattern of results is consistent with, although does not conclusively prove, the idea that beliefs decay over time; that is, admits forget the information they received in the treatment. However assignment to wave is not random (it is a function of when subjects choose to apply to TFA), and there is selection into completing the survey, so other differences in the groups could drive the patterns observed in Figure 4. Nevertheless, the control group reports very similar beliefs across waves ($F$-test $p = 0.52$), lending credence to the “forgetting” explanation in which subjects forget the social information even while its effect on behavior persists.

\(^{35}\) The mean increase was 1.3 percentage points (t-test $p < 0.1$).

\(^{36}\) To address the potential issues of selection and cognitive dissonance, we run the same analysis of the treatment on beliefs separately for those who said yes to the TFA job and those who said no to the TFA job. We find that both groups display the same increase in beliefs in response to the treatment. Mean (median) responses among those who said yes are 67.7% (71%) in control vs. 68.9% (73%) in social information treatment (2,488 observations, rank-sum $p = 0.042$); of those who said no, they are 69.1% (71%) in control vs. 71.4% (74%) in social information treatment (482 observations, rank-sum $p = 0.025$). That those who said no had a higher average belief about current year’s matriculation is a likely indication of selection bias into the survey. Only a third of those who declined the job took the survey, the people are likely particularly favorable towards the organization such that they would take a TFA survey even though they declined the job.
5.5 Comments on Mechanism

Our social information treatment increased beliefs about the matriculation rate. There are two main channels through which this effect on beliefs might influence behavior. First, the social information might provide information about the value of the TFA experience (e.g. learning that most other people take a TFA job might lead admits to believe that TFA is particularly good for their resumes or that the program is particularly effective at achieving its goals of improving student outcomes). Second, the social information might trigger a desire to conform to the actions of others, absent any transmission of information about the quality of TFA.

If the former channel is paramount (i.e. if information about TFA quality drives behavior), then we should be able to detect a treatment effect on beliefs about TFA value. In an attempt to find evidence of the information channel, we used Likert-scale questions to elicit beliefs about some important dimensions of TFA’s value to its corps members: how much does TFA help employment prospects, how much does TFA help graduate school admissions prospects, and how much does TFA impact its students. Our measures are significantly correlated with the decision to join TFA, indicating that they are picking up meaningful variance (see Table A4 in the Appendix). However, while the social information treatment directionally increases beliefs on all three of these dimensions, we find no statistically significant results (see Table A5 in the Appendix). We interpret these results as rather precise zeros: ex post, we are powered to identify an effect equal to 0.11 standard deviations of the control.

37 Vesterlund (2003) presents a model of how sequential fundraising can allow potential donors to provide information to one another about the quality of a charity.
38 The full wording of these questions can be found in Figure A2 in the Appendix.
39 One way in which TFA is considered to be a positive signal for employers and graduate schools is that it has a significant pro-social component, as many TFA corps members work at a wage well below their outside option (see Ariely, Bracha and Meier 2009).
group (i.e. we can identify an effect of 0.095 points, or about one-ninth of a standard deviation, on a 7-point Likert scale). That our treatment did not statistically significantly affect beliefs of TFA value on our three measured dimensions is consistent with a conformity mechanism. That said, the value of the TFA experience is multidimensional and information about the decisions of others could lead admitted applicants to update on any number of dimensions, and different applicants may update on different dimensions. Our social information treatment may have moved beliefs about a dimension of TFA value that we either imperfectly measured with our questions or missed altogether. Nevertheless, the results suggest the potential importance of a conformity mechanism in this setting.

6. Conclusion

Social information can have powerful and persistent effects on high-stakes behavior. Adding one line of social information to a TFA admissions letter increases the likelihood that admitted applicants accept the offer to spend two years working as a teacher in an underperforming public school. In addition, the effects we observe persist. Those who received the social information are more likely to train for the teaching job, show up to teach, and return to it the following fall, 17 to 21 months after they were treated.

Consistent with theory, the effect of our treatment was particularly large in subgroups of subjects where we expected to find a larger mass at the margin. All such subgroups met the data-based criterion highlighted by the theory: that the baseline matriculation rate was lower than the overall average matriculation rate.

40 More precisely, for each of the three questions, if we average across all four waves, a two-sided t-test with 0.05 significance level is 80% powered to find an effect size of at least 0.11. Standard deviations within control and treatment groups are around 0.9 Likert points.
Such data-based subgroup identification provides a framework for analyzing the effects of a treatment on a binary choice variable and provides a guard against data mining.

That our subtle intervention had a pronounced and persistent effect on a high-stakes decision like job choice suggests the power of social information\(^{41}\) and emphasizes the importance of including such a motivator in models of decision-making. The results also highlight the potential use of social information as a policy tool, even in domains where the stakes are high and decision makers have sufficient time, information, and incentive to carefully consider their choice.

\(^{41}\) Kessler (2013) shows that social information is powerful on another dimension: even soft information in the form of announcements of support for a public good can induce others to contribute to it.
References


Tables and Figures

Figure 1: Timing of events: admissions (top) and training and teaching (bottom)

Table 1: Overview of subgroups
Panels show the average rate of being committed to TFA at the five milestones; standard error bars are shown around each mean.

Panel A aggregates over all admits; Panels B, C, and D split the data into subgroups.

In all panels:

Social Information is dashed ( ),

Control is solid ( ).

Abbreviations:

IC: Initial Commitment
SI: Showed to Institute
TF `12: Teaching Fall 2011
TS `13: Teaching Spring 2013
TF `13: Teaching Fall 2013

Panel A: No subgrouping

Panel B: Disappointing assignment
(Top 2 are Pleasing Assignment; Bottom 2 are Disappointing Assignment)

Panel C: Moderately aligned
(Top 2 are Highly Aligned; Bottom 2 are Moderately Aligned)

Panel D: Not certain to join
(Top 2 are Certain to Join; Bottom 2 are Not Certain to Join)

Figure 3: Working for TFA over Time
<table>
<thead>
<tr>
<th>Subgrouping by Disappointing Assignment (N = 6685)</th>
<th>No subgrouping (N = 6685)</th>
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</tr>
<tr>
<td>Table 2: Regression of Treatment on Working for TFA over Time, Subgroup analysis</td>
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Table shows Linear Probability Model (OLS) regression results of whether the individual was working for TFA at each of the five milestones. All regressions include dummy variables for wave during which the applicant was admitted and a dummy for displeasing assignment, which was a stratifying variable (Bruhn and McKenzie 2009). The even columns control for demographic characteristics: gender, race, ethnicity, socioeconomic status (based on whether an accepted applicant had a full, partial or no Pell grant during college), whether they were a math/science major, and their student status or profession before applying to TFA. There are fewer observations in Not Certain to Join subgroup regressions because about 5% of admits did not respond to the survey question used to construct the subgroup. Robust standard errors are reported in parentheses. *, **, *** indicate significance at 0.1, 0.05, and 0.01 respectively. The omitted group’s mean likelihood of working for TFA at that milestone is reported.
Table 3: Regression of Propensity to Join and Treatment on Working for TFA over Time

Table shows Linear Probability Model (OLS) regressions of a propensity to join measure regressed on whether the admit is still with TFA at each of the five milestones. The propensity to join measures vary in what groups of variables were regressed on the initial commitment dummy per the description in Section 5.2. The Disappointing Assignment, Moderately Aligned, and Not Certain to Join propensities were estimated non-parametrically with a saturated regression, while the Demographic Propensity had too many regressors for a saturated regression to be possible; instead, a regression with no interactions was estimated. All propensities included interactions for wave dummies. There are fewer observations for the Not Certain to Join propensity because of imperfect survey response. Robust standard errors are reported in parentheses. *, **, *** indicate significance at 0.1, 0.05, and 0.01 respectively.
Figure 4: Reported Median Matriculation Beliefs by Treatment (and Wave)

Figure shows the median survey response (overall and among those who were accepted in each of the four waves) to the question asking the number of admitted applicants out of 100 who joined TFA in the current year. Standard error bars are shown around each median; *** indicates significance at the 0.01 level.
Appendix

Appendix

Dear Lauren,

I am pleased to extend you an offer to join the Teach For America 2012 corps! Your acceptance into Teach For America reflects your outstanding accomplishments, leadership potential, and commitment to expanding educational opportunity for children in low-income communities. In order to secure your place in the 2012 corps, you must complete matriculation forms on the Applicant Center on or before Monday, November 21 at 6 p.m. ET.

Changing children’s life trajectories by effecting meaningful gains in their academic achievement is an incredibly challenging pursuit. You have demonstrated great potential to excel as a classroom leader who will work in partnership with families, schools, and communities to offer your students the educational opportunities they deserve, and we are excited to welcome you to this effort. More than 28,000 Teach For America corps members and alumni are using their unique talents, skills, and perspective to help transform education for children in low-income communities and address the factors that contribute to educational inequity, and we hope that you will join them in this important work.

On the Applicant Center, you can view a special welcome video from our Chief Executive Officer and Founder Wendy Kopp, as well as access information that will help you make an informed decision about this significant commitment, including:

- **Regional and grade/subject assignment:** In determining your assignment, we made the best possible match between your regional, grade-level, and subject-area preferences, the projected needs of the school districts with which we work, and the requirements necessary to teach in those districts. Since we carefully consider each applicant’s qualifications and preferences when determining his or her assignment, we rarely reassign an applicant to a new region.
- **Information about your region:** The staff in your assigned region has posted important resources on the Applicant Center that will provide: details about living and teaching in your region; information about the summer institute; and the phone numbers and/or e-mail addresses for corps members and alumni who would be happy to answer any questions you have.

Last year, more than 84% of admitted applicants made the decision to join the corps, and I sincerely hope you are one. If we can be of any assistance, please contact us at admissions@teachforamerica.org.

Congratulations again, and welcome to Teach For America!

Sincerely,

Sean Waldheim
Vice President, Admissions

Figure A1: Admissions letter (dashed box added to highlight location of social information line)
Overall | Disappointing Assignment | Moderately Aligned | Not Certain to Join
--- | --- | --- | ---
Disappointing Assignment | 34.3% | 34.7% | 100% | 100% | 34.3% | 34.3% | 25.8% | 28.7%
Moderately Aligned | 38.9% | 38.7% | 39.0% | 38.2% | 100% | 100% | 32.2% | 32.7%
Not Certain to Join | 74.8% | 74.9% | 80.7% | 78.9% | 79.2% | 78.7% | 100% | 100%
Male | 28.7% | 28.7% | 30.4% | 31.1% | 31.4% | 29.2% | 26.5% | 25.9%
Non-white | 36.1% | 34.7% | 36.7% | 36.0% | 29.3% | 26.7% | 41.6% | 41.9%
Math, Sci. or Eng. Major | 18.0% | 16.7% | 14.6% | 14.0% | 21.1% | 19.7% | 13.5% | 14.2%
Current college senior | 75.2%* | 77.1%* | 71.5% | 73.3% | 78.8% | 78.5% | 75.2% | 75.2%
Gets max. Pell Grant | 15.1% | 16.3% | 15.7% | 17.9% | 14.1% | 15.7% | 17.0% | 19.4%

Table A1: Balance across Treatments and by Subgroups (Means)

Only one variable (marked with a *) is significantly different ($p = 0.06$) comparing Social Information to Control within each panel ($\chi^2$-test). The $F$-test $p$-value is calculated by jointly testing that all coefficients are equal zero from an OLS regression predicting treatment assignment using all variables in the table (for the subgroup in that panel).

<table>
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<th>Wave</th>
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<tr>
<td>1</td>
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<tr>
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<td>3</td>
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</tr>
<tr>
<td>4</td>
<td></td>
</tr>
<tr>
<td>Overall</td>
<td></td>
</tr>
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</table>

Table A2: Number of admitted applicants, broken down by treatment and wave

Balance and stratification were not perfect because the TFA data we randomized included a few “repeat” applicants who were sent multiple acceptance letters. These subjects have been dropped from all analysis.
### Table A3: Regression of Propensity to Join and Treatment on Working for TFA over Time (including Wave-Specific treatment)

Table shows Linear Probability Model (OLS) regression results of whether the individual was working for TFA at each of the five milestones. All regressions include dummy variables for wave during which the applicant was admitted and a dummy for displeasing assignment, which was a stratifying variable (Bruhn and McKenzie 2009). The even columns control for demographic characteristics: gender, race, ethnicity, socioeconomic status (based on whether an accepted applicant had a full, partial or no Pell grant during college), whether they were a math/science major, and their student status or profession before applying to TFA. There are fewer observations in Not Certain to Join subgroup regressions because about 5% of admits did not respond to the survey question used to construct the subgroup. Robust standard errors are reported in parentheses. *, **, *** indicate significance at 0.1, 0.05, and 0.01 respectively. The omitted group’s mean likelihood of working for TFA at that milestone is reported.
**Survey questions**

All three questions measured on a scale from 1, “very unlikely”, to 7, “very likely”.

- **Employment Prospects** - “In two years’ time, how much more or less likely would an employer be to hire you if that employer knew you had participated in Teach for America?”

- **Graduate School Prospects** - “In two years’ time, how much more or less likely to be admitted to a graduate program (e.g. medical school, law school, master’s degree) if that school knew you had participated in Teach for America?”

- **TFA Impact on Students** - “Consider two otherwise identical students, one of whom has a TFA teacher for one year, and one of whom does not. How much more or less likely is the student with the TFA teacher to succeed?”

---

<table>
<thead>
<tr>
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**Table A4: Predicting Initial Commitment to TFA with Survey Responses**

Table shows Linear Probability Model (OLS) regression results showing how responses to our survey questions about the value of TFA (each measured on a 7-point Likert scale) predict the decision to accept the offer to join TFA. Controls included: treatment and wave fixed effects. Robust standard errors are in parentheses. *, **, *** indicate significance at 0.1, 0.05, and 0.01 respectively.
### Table A5: Effect of Treatment on Other Survey Questions

Table shows Linear Probability Model (OLS) regression results showing how responses to our survey questions about the value of TFA (each measured on a 7-point Likert scale) predict the decision to accept the offer to join TFA. Controls included: treatment and wave fixed effects. Robust standard errors are in parentheses. *, **, *** indicate significance at 0.1, 0.05, and 0.01 respectively.