

The Role of Application Assistance and Information in College Decisions: Results from the H&R Block FAFSA Experiment*

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July 2011

ABSTRACT

Growing concerns about low awareness and take-up rates for government support programs like college financial aid have recently spurred calls to simplify the application process and enhance visibility. Offering personal assistance may address many potential barriers to program take-up even when options like changing the enrollment default or shortening the application further are not available. We present results from a randomized field experiment in which low-income individuals receiving tax preparation help were also offered immediate face-to-face assistance and a streamlined process to complete the Free Application for Federal Student Aid (FAFSA) for themselves or their children. Treated participants are also provided with aid estimates which are compared against tuition cost amounts for nearby colleges. The combined assistance and information treatment substantially increased FAFSA submissions and ultimately the likelihood college attendance, persistence, and aid receipt. In particular, college enrollment rates for high school seniors rose 8 percentage points, from 34 to 42 percent in the year following the experiment for those whose parents received the combined treatment. The information-only treatment, however, had no significant effect. The findings suggest many other opportunities for using personal assistance to increase participation in programs that require filling out forms to become eligible.

* This research was made possible with generous funding from The Bill and Melinda Gates Foundation; National Science Foundation (Award No. SES-0721158); Institute of Education Sciences, U.S. Department of Education (through the National Center for Postsecondary Research); Kauffman Foundation; Spencer Foundation; and the MacArthur Foundation. This research was conducted in collaboration with the LMI Policy Research Institute and H&R Block. The views expressed in this article are those of the authors and do not necessarily reflect the views of the LMI Policy Research Institute or H&R Block. All opinions and mistakes are our own. We are grateful to Santhi Hariprasad and Marquise McGraw for research assistance, and to Jennifer Douglas, Eric Smith, Andy Lechler, Jeffrey Kling, Jeremy White, Scott McBride, Helen Krosky, Mike Lammers, Paul Peterson, Bernie Wilson, Shatomi Luster, and the over 1,000 tax professionals at H&R Block for their help in organizing and implementing the experiment.

I. INTRODUCTION

Many individuals from low-income households appear unresponsive to financial aid (Manski and Wise 1983, Hansen 1983, Kane 1996). Researchers and policymakers have begun to question whether the process of learning about aid and applying for it actually deters individuals from going to college (Dynarski and Scott-Clayton 2006, ACSFA 2001, 2005). Even assuming individuals are aware of the application, they must still navigate through a virtual “gauntlet” (Advisory Committee on Student Financial Aid, 2005) of questions on topics including earnings, savings, assets, driver's license number, previous drug convictions, legal residency status, and parents’ earnings, welfare receipt, and assets. The form includes many more questions than the typical tax form faced by low-income families, and the submission process can be intimidating as the government threatens penalties such as fines up to \$20,000, prison, or both for giving false or misleading information.

Benefit programs like financial aid are often constructed as though their mere existence ensures take-up for everyone eligible and interested, yet many examples exist of missed opportunities (including financial) from failing to enroll (e.g., Currie 2004, King 2004). Recent research in economics and psychology demonstrates how seemingly small changes to sign-up procedures (“nudges” as Thaler and Sunstein (2008) call them) can lead to large changes in program participation. Defaulting employees into a corporate savings plan, for example, with the option to opt-out, dramatically increases participation compared to a policy of requiring them to opt-in (Beshears *et al.* 2006a). Shortening a savings plan application to one involving checking a box to enter a prevailing plan rather than having them choose a plan from a myriad of complicated options also increases participation (Beshears *et al.* 2006b). Summarizing information from multiple sources into a more easily-readable table increases take-up for parents deciding whether to transfer their children to better ranked schools (Hastings and Weinstein 2008), and mailing personalized information about alternative drug plans rather than relying on individuals to actively access it online leads to more take-up (Kling *et al.* 2011).

This paper introduces a different kind of nudge: personal assistance. We conducted a randomized field experiment in which low-income adults receiving tax preparation help were also offered immediate personal assistance to complete the Free Application for Federal Student Aid (FAFSA) for themselves or their children. Using the tax preparation process enabled quick and streamlined assistance because much of the information needed to complete the FAFSA

could be pre-populated using the already completed tax form. After pre-population, tax professionals guided treated participants through remaining questions, generally in less than 10 minutes. The form was then submitted electronically to the U.S. Department of Education and mailed to the applicant's household for signature. Treated participants were also provided with immediate personalized aid estimates that were compared with tuition costs for nearby colleges. We test both the impact of only providing this information and the impact of providing it combined with FAFSA assistance.

Streamlined personal assistance may address many potential barriers to program take-up, especially when options like changing the enrollment default or shortening the application further are not available. It may reduce procrastination by making the application process more convenient and more appealing. It may increase a form's visibility and improve perceptions about the value in filling out the form. It may help reduce anxiety about making a mistake or reduce stigma from one's low-income status. Moreover, offering help while already meeting (e.g., at an H&R Block office) could minimize disruption and lower opportunity costs of time. Personal assistance also avoids the need for detailed instructions and facilitates offering these services on a large scale. It may even empower individuals to consider more the possibility of change (e.g. helping their children get to college).

We find remarkable impacts from this type of help. Treated participants provided streamlined personal assistance to complete the FAFSA were not only more likely to apply for financial aid, they were significantly more likely to attend college and receive aid. College enrollment rates for high school seniors and recent high school graduates rose 8 percentage points, from 34 to 42 percent in the year following the experiment for those whose parents received the FAFSA help compared to those who did not. Offering FAFSA assistance also increased enrollment by 16 percent for adults out of high school with no prior college experience. Treated participants with prior college experience were more likely to receive Pell Grants, suggesting some foregoing of aid due to the application barrier. In addition, assistance with the form increased the likelihood of enrolling and staying in college for at least two years. The findings have implications for a wide range of programs that require filling out forms to become eligible.

The rest of the paper proceeds as follows. Section II provides a brief review of the literature on the complexity of the FAFSA. Section III details our experiment. Section IV describes our data sources. Section V presents results, and section VI concludes.

II. LITERATURE REVIEW

There is a long literature examining the effects of financial aid and price on college attendance (e.g., Kane 2003, Seftor and Turner 2002, Dynarski 2000 and 2003, Manski and Wise 1983), institutional choice (e.g., Long 2004; Avery and Hoxby 2004, Van der Klauuw 2002), and persistence (e.g., Bettinger 2004). While ability to pay influences decisions about college, many remain puzzled as to why some aid programs have not been more effective in spurring increased enrollment among targeted groups.¹ Some theorize this is due to low visibility and the complexity of the aid process, and in recent years, there has been increasing interest in understanding the role of the application process on student outcomes. For example, at the direction of Congress, the Advisory Committee on Student Financial Assistance (ACSFA) examined the federal aid system and concluded:

“Millions of students and adult learners who aspire to college are overwhelmed by the complexity of student aid. Uncertainty and confusion rob them of its significant benefits. Rather than promote access, student aid often creates a series of barriers – a gauntlet that the poorest students must run to get to college” (ACSFA, 2005, p. i).

The Free Application for Federal Student Aid (FAFSA) is at the center of policy discussions about reducing the complexity of the application process. The 2008 FAFSA was eight pages long and contained over 100 questions. To answer three of these questions, applicants had to complete three additional worksheets with nearly 40 additional questions. As shown by Dynarski and Scott-Clayton (2006), the FAFSA is four times longer than the simplest tax return (i.e., IRS Form 1040EZ), which is what most low-income families use, and longer than IRS Form 1040. Even the lowest-income individuals, who have already established their eligibility for other federal means-tested programs, must complete this long application to

¹ For example, researchers have not found large enrollment responses after the introduction of some financial aid programs, such as the Pell Grant in 1972 (Manski and Wise 1983, Hansen 1983, Kane 1996). See also GAO (2005).

receive aid for which they are almost certainly eligible.² In addition, the timing of the application process is troublesome. Individuals cannot submit the FAFSA until the January of the year of college entry. Therefore, they often must apply to college before even knowing with certainty whether they can afford it. Even after completing a FAFSA, applicants learn only what the government expects their family can pay (i.e. the Expected Family Contribution or EFC), and applicants hence cannot predict the exact amount of their potential aid package.

The complexity of the current federal financial aid system is even more apparent when comparing the existing application process to the processes of other financial aid programs shown to be effective. Administrators of the Social Security Student Benefit (SSSB) Program, for example, proactively mailed students approaching their eighteen birthday to inform them about available financial aid. Students only needed to return a short form to remain eligible for the benefit. Dynarski (2003) finds that the elimination of the program led to large reductions in college enrollment and eventual educational attainment. Similarly, the Georgia Hope Scholarship, which provides aid to students above a grade threshold, was heavily advertised and the application process was simplified.³ Researchers have found that Georgia's aid program had a substantial impact on college attendances rates (Dynarski, 2000; Cornwell, Mustard, and Sridhar, 2006).

Lack of information may also impede college aid receipt, since potential students and their parents must first know about the existence of aid in order to access it. A 2002 Harris Poll found that nearly two-thirds of all parents and young adults planning to go to college did not name grants as a possible source of funds when asked about types of financial aid. Moreover, low-income families often have less information than other families about how to pay for college (Sallie Mae Fund, 2003). Given these patterns, it is not surprising that many students eligible for aid do not apply for it. King (2004) estimates that over 10 percent of all college students in 2000 did not complete financial aid forms even though they would have been eligible for a Pell Grant had they done so. The same patterns can be found with state aid programs that also use the FAFSA. In California, as many as 19,000 students who would have qualified for a Cal Grant, a need-based aid program, failed to apply (Sturrock, 2002).

² Students who are already in college must also redo the FAFSA in a timely fashion each year to renew their aid, which may cause some students to lose their aid.

³ Interestingly, Georgia students completing the FAFSA online can also apply for the Hope Scholarship with no additional form requirements.

Lack of information about the true costs of college may pose an additional barrier to enrollment. ACSFA (2005) notes that students and families, as well as adult learners, are often intimidated by news reports about record increases in the college costs of the most selective universities and other impressions that college is unaffordable. These stories may contribute to the fact that individuals, particularly low-income individuals, often greatly overestimate the cost of higher education (Horn, Chen, and Chapman 2003). Among individuals participating in our study, we asked a subsample to report on the average costs of college and found that participants overestimated the costs by over 300 percent.⁴ Oreopoulos and Dunn (2009) find high school students are more likely to aspire going to college three weeks after being provided accurate information about costs and benefits.

Policymakers and researchers are increasingly aware that the design of a program can affect its take-up and effectiveness. Personal assistance in completing the FAFSA provides a simple method for making the process more visible, simple, informative, convenient, and encouraging. Offering this assistance immediately after offering tax-form assistance speeds up the process, makes it more convenient, and eliminates the need to ask many not-easily available questions. The extent to which this would actually affect college aid applications and enrollment, however, is unknown. Our project is designed to address this hole in the literature and demonstrate the potential benefits from form completion assistance.

III. THE FAFSA EXPERIMENT

We developed the FAFSA experiment in collaboration with H&R Block. On January 2, 2008, the program was implemented in most of Ohio and the Charlotte, North Carolina area (a total of 156 tax preparation offices).⁵ After a person completed their taxes in an H&R Block office, they were instantly screened for eligibility. Software we developed identified families with incomes less than \$45,000, as measured by the adjusted gross income reported on the tax

⁴ The average annual tuition at a two-year, public college in Ohio was \$3,099. In contrast, the median estimate among our participants was \$9,999. Dependents guessed \$8,500 at the median, and independents guessed \$10,000.

⁵ H&R Block invited proposals of interventions that would benefit low- and moderate-income families, have national scalability, and inform important and timely policy debates. After being selected through a competitive, peer-reviewed process, the team worked from spring 2006 to winter 2007 to develop the necessary procedures and software. Based on feedback from focus groups and analysis of the operational data from the pilot conducted January to April 2007, we finalized the procedures for the 2008 implementation. Charlotte offices were adopted in response to a request to include them from the Bill and Melinda Gates Foundation, although the final sample size was not large enough to separate effects between by region (only 12 percent of the final sample was from Charlotte).

return, who also had a family member between the ages of 15 and 30 who did not already have a bachelor's degree. These criteria map onto two samples of interest. The first is high school seniors and recent graduates who are typically dependent upon their parents financially.⁶ The second group is independent adults (often referred to as non-traditional students). These individuals are a growing segment of higher education, and research has shown that adult enrollments can respond strongly to financial aid availability (e.g., Seftor and Turner 2002). Because this group does not have access to high school counselors, the proposed interventions may provide information not readily available to this group. In cases where there was more than one eligible member in the household, we picked the independent adult in the office closest to age 18. After identifying an eligible participant, the H&R Block tax professional introduced the project explaining that we hoped “to learn how people make decisions about college and how to pay for it, as well as find out how H&R Block can best help its families navigate college finances.” The participant was also offered \$20 for their time. If interested, the tax professional then asked the individual to complete a statement of informed consent. Once individuals consented, we asked study participants general questions about their backgrounds and higher education perceptions. Then, we randomly assigned individuals to one of three groups:

1. *FAFSA Simplification and Assistance with Aid Eligibility Information (i.e., the FAFSA Treatment)*

For this group, we helped individuals complete the FAFSA. Our software first used individuals' tax returns to pre-populate about two-thirds of the questions on the FAFSA. Then, it led the H&R Block tax professional through an interview protocol to answer the remaining questions, which typically took less than 10 minutes. These questions mostly concerned relatively straightforward information such as the number of children in the household currently attending college. After the interview protocol, the software computed the amount of financial aid the client was eligible to receive from the federal and state governments and provided a written explanation of these numbers (a sample

⁶ In practice, most of our sample of younger students was age 17 at the time of the tax interview. This is because the FAFSA typically considers students under the age of 24 as dependent on their parents unless they are married, have a child, or are veterans. In these cases (in which the student is defined as a "dependent"), parental income is required for the FAFSA, and so we focused on cases where a parent was completing taxes and the student was declared a dependent on the tax forms. Our IRBs would require student consent if the student was over the age of 18, and almost universally, these students did not accompany their parents to the tax interview. Our IRBs allowed parents to grant consent on behalf of dependents under the age of 18. Individuals age 24 or older are automatically considered "independent" by FAFSA standards, and parental information is not needed for the FAFSA.

award letter appears in the Appendix Figure 1).⁷ The aid amounts reported to participants focused on need-based aid (e.g., the Pell Grant and the Ohio College Opportunity Grant) as well as federal loans. In reporting potential aid packages, we also presented the tuition prices of four nearby public four- and two-year colleges.⁸ If all of the information necessary to complete the FAFSA was obtained by the tax professional during this initial visit, we then offered to have H&R Block submit the FAFSA electronically to the DOE free of charge or send a completed paper FAFSA by mail so that the individual could submit it themselves using a prepaid envelope we provided. If not all information could be collected, an external call center contacted the household to collect answers to remaining questions. FAFSAs were completed as much as possible and mailed to households with a prepaid envelope or filed directly to the DOE when applicants agreed.⁹ In total, we completed the FAFSA for nearly seven out of ten treated participants, either in the office or using call center staff.¹⁰

2. *Aid Eligibility Information only (i.e., the Information-Only Treatment)*

For this group, we calculated individualized aid eligibility estimates using information from the tax return that the participant had just completed at the H&R Block office. We also gave individuals a written description of their aid eligibility and a list of the tuitions of four nearby colleges. To receive the aid amounts, the tax professional then encouraged individuals in this group to complete the FAFSA on their own (no help was given on the form as the emphasis for this group was only on providing information). This second treatment was added to contrast the estimated effects on FAFSA filing from information alone about financial aid eligibility (a separate and cheaper potential program) with the effect from providing both form completion assistance and information. A smaller

⁷ If we could not collect all the information needed for the office during the initial office visit, we still tried to compute the amount of aid students were eligible to receive. Typically we were only missing data that is irrelevant to the aid calculation (e.g. driver's license number). In other cases, we were missing information on specific income sources not listed on the tax return but needed for the FAFSA (e.g. SSI benefits).

⁸ For each region, we chose four plausible colleges based on enrollment patterns for that region. The schools were a mix of open admissions and large, slightly selective institutions.

⁹ Most often FAFSAs that were not completed in the office required additional information such as other sources of income (e.g., veteran's benefits) or the child's driver's license number.

¹⁰ Completion rates differed slightly by type of participant. Among independent students with no prior college experience, 54 percent completed their FAFSAs in the office and another 24 percent were completed with the help of the Call Center (for a total completion rate of 78 percent). Among dependent students, 11 percent completed the FAFSA by the end of the Call Center outreach process and another 66 percent nearly completed the form (having at least 91 of the 103 FAFSA items). FAFSAs with missing fields may still have been deemed complete enough to submit.

subsample was used to minimize power loss for the main treatment impact on college enrollment.

3. Control Group (no intervention)

For this group, we only provided a brochure with basic information about the importance of going to college and general information on costs and financial aid. We constructed the brochure using information readily accessible online and elsewhere with the goal that this information would not likely affect a participant's behavior. As such, this group is our key comparison group for determining the effects of the other interventions. The brochure was also given to the treatment groups.

Our target sample size for each FAFSA treatment and control group comparison was about 7,500 so that we might detect college enrollment effect sizes in the range of 1.5 to 2.0 percentage points. In order to consider subgroups, we aimed for a total sample size greater than 30,000. Prior to implementation, we outlined four subgroups based mainly by participant age and college experience: 1) High school sophomores and juniors aged 15 to 17 not yet eligible to apply for the FAFSA (to examine an early information treatment);¹¹ 2) High school seniors and recent graduates in the process of deciding whether to go to college and financially dependent on their parents; 3) Adults aged 24 to 30 with a high school degree or equivalent but with no prior college experience (potential non-traditional students with most currently working); 4) Adults aged 24 to 30 already with some college experience but without a college degree (more familiar with the college application process, but perhaps not with the financial aid process). We categorized our sample this way based on the likelihood that these subgroups would differ both in terms of FAFSA filing rates and treatment effects. Our proposed main outcome of interest for the first group was FAFSA filing, since the power from the information treatment may not be enough to detect subsequent enrollment effects. For those in groups 2 and 3, our proposed main outcome of interest was college enrollment. For group 4, we hypothesized our intervention would not increase enrollment, but would help increase aid receipt and, through this, retention (Bettinger, Long, and Oreopoulos, 2006). A pilot study in the previous year of the experiment helped predict the number of offices needed for the study. However, some uncertainty remained due to changes in operational details and year-to-year fluctuations in client base at H&R Block. The sample size for dependent students in particular was considerably smaller than desired due

¹¹ We expect to analyze these impacts on sophomores and juniors separately in a future study.

to an inability of obtaining consent from dependent students 18 years old not at the H&R Block office with their parents. Our main dependent sample therefore focused exclusively on 17 year olds.

Table 1 outlines our recruitment process including the consent rates for our respective treatment and control groups.¹² During the tax season, H&R Block met with 236,483 clients in the targeted offices. Of this group, 69,031 clients met the study's initial criteria (having an AGI less than \$45,000 and a family member age 15 to 30), 35,793 expressed interest in learning more about college (52 percent of clients meeting the study's criteria), and 26,401 qualified for the study after answering in the affirmative that the target participant did not already have a bachelor's degree (74 percent of those expressing interest).¹³

Nearly all of the individuals expressing interest verbally consented to participate in the project (26,168 individuals). Participants did not formally sign the consent form until the end of the interview, and a small number left before doing so. For the sample of dependent students, we found a significant difference in the fraction expressing interest to participate (more from the control group), but this occurred before even differentiating individuals by treatment status. For our final sample of dependents with received written consent, we found no statistically significant difference.

The last column in Table 1 reports the percentage of each group for whom we received a paper copy of the consent form. Some tax professionals mistakenly sent the signed copies of the consent forms home with the study participants, and we are prohibited from matching outcomes without proof of a signed consent form. As a result, we had to exclude some individuals who initially consented to participate. For our sample of independents, differences in consent rates by treatment status are marginally significant at the 10 percent level. This may be due to the large sample and the fact that more printed material was produced for treated participants, perhaps making it more likely that a few tax professionals came across treated consent forms more often

¹² The dependent sample figures include both high school seniors and recent graduates, who are examined in this paper, as well as participants who were high school sophomores and juniors, who will be examined in future, separate work. The independent sample figures include both those with no prior college and those with prior college but without a college degree (we examine these two groups separately since they differ substantially in experience with the college application process and in their predicted impacts).

¹³ The primary reasons why some individuals did not qualify for the study was that they already had college degrees, or were not considered independent by federal aid standards and so would need information from other family members not present in the office in order to complete the FAFSA. Among those who qualified, tax professionals during focus groups suggested that about half of those that expressed interest were initially attracted to the \$20 discount, and the other half were interested because they wanted more information about college.

than controls when reviewing what paper to keep and what paper to give to clients. Importantly, receiving a written consent depended on actions by the tax professional rather than the participant, and the reasons tax professionals and district managers gave for not submitting paper consent forms are not related to our outcomes of interest.¹⁴ Nevertheless, to address the issue of selective participation further, we present results in Section III that treatment and control groups have similar means across a wide range of observable characteristics. We also demonstrate in Appendix Table 1 that our results generally remain the same after dropping offices with significantly different treatment and control samples, after including office or tax professional fixed effects, and after excluding offices or tax professionals that filed more control consents than treated.

During the experiment, the software developed not only tracked completion of each question, but it also prompted and reminded tax professionals what questions they should ask at each point of the interview. H&R Block also monitored treatment fidelity through field visits. H&R Block received no reports of any serious deviation from the script from the field offices. If a problem arose, we immediately integrated new procedures and training modules to accommodate special circumstances.

We ended up with a total sample of 868 dependent, mostly high school, seniors, 9,228 independents with no prior college experience, and 6,646 independents with prior college experience. College experience was determined using information collected during the study's initial screening, prior to randomization. The information-only group is noticeably smaller as its only purpose was to detect differences in FAFSA submission rates compared to the Control and FAFSA assistance groups, not to detect small differences in college enrollment.¹⁵

To study the effects of FAFSA filing and college outcomes, we linked our final sample to data from the Department of Education (DOE), the Ohio Board of Regents (OBR), and the National Student Clearinghouse. (NSC) From the DOE, we observed whether an individual submitted a FAFSA since the intervention and the amount of financial aid paid. Payment

¹⁴ In focus groups with tax professionals, they identified two main reasons why H&R Block central processing unit did not receive a written copy of the consent form. First, many tax professionals accidentally sent all of the written copies of the consent form home with the client. Second, many tax professionals filed the consent form with the tax documentation rather than submitting the form to H&R Block's central processing center. In both cases, we had little recourse in retrieving the consent forms; however, we were able to identify which tax professionals made these mistakes and train them so that they did not repeat the mistakes.

¹⁵ With a control mean of 0.2, the sample size gives us about 80 percent statistical power to detect a 3 percentage point difference in FAFSA submission rates at the 5 percent significance level.

required confirmation of college enrollment. From the OBR, we observed college attendance for students at Ohio public colleges. And from the NSC, we observed college attendance across the United States for individuals enrolled in institutions covered by the Clearinghouse repository.¹⁶ Our main outcome of interest, college enrollment the year following the experiment (April 2008 - March 2009) was determined by combining OBR and NSC data to determine whether an individual registered for new classes in college at some point during this time. Appendix Table 2 shows similar results when using only OBR or only NSC outcomes instead.

IV. EMPIRICAL FRAMEWORK

Data – Descriptive Statistics

In Table 2, we report basic descriptive statistics for our three main samples of interest. For each group, we report the means for the control group and the differences (and their standard errors) with the treatment groups. Our algorithm for randomizing clients depended completely on the last two digits of the taxpayer's social security number, and the software automatically made the treatment assignment.¹⁷ As expected, observable mean characteristics between the control and treatment groups are generally balanced—we find no evidence of significant differences between the control and treatment groups.

Among the sample of dependent participants, over 56 percent of the sample is female. The racial distributions are also similar across treatment groups with comparable proportions of white, black, and Hispanic participants. In the control group, 55 percent of participants were white and about 38 percent of participants were black. Among the information-only treatment group, the proportion of white participants was higher while the proportion of black participants was lower, but these differences are not statistically significant at the 5 percent level. The

¹⁶ The NSC is a non-profit organization that provides national student degree and enrollment verification for schools, colleges, and employers. Founded in 1993, it currently serves as a central repository for the institutions of 92 percent of college students.

¹⁷ Tax professionals could not override the screen prompts that were dependent on treatment status, and did not know the nature of the treatment assignment algorithm. In focus groups, the tax professionals, confirmed that they did not know which group individuals had been assigned to until the software made the assignment, which occurred after the informed consent process.

average age of the dependent sample was about 17.7 at the time of the interview across all three groups.¹⁸

Across the groups, about 85 to 88 percent of the dependent sample were high school seniors according to parents. The others had either graduated from high school or had left high school and completed a GED. While most parents identified their children as being high school seniors, we searched the NSC records to see if any of these participants had a history of previously taking a college course. In our control and FAFSA treatment groups, nearly 6 percent of parents reported that their child had previously enrolled in college. These enrollments could represent a single course at a campus or being in a dual enrollment program. The percentage was higher for the information-only treatment group had previous enrollment. About 41 percent of parents reported that their children would be targeting a bachelor's degree while 35 percent of parents reported their children's target degree would be an associate's degree. The remaining parents indicated their child would be targeting a professional certificate or indicated that they did not know. Family's average incomes were about \$23,000 while their taxable incomes (not shown) were near \$6,000.

For the dependent participant sample, we find no statistically significant differences between the control group and the FAFSA treatment group or between the control group and the information-only treatment group. Because of our sample sizes, we have sufficient power to identify even small differences in the groups. Hence our failure to find differences is an affirmation of our randomization.

The rest of Table 2 shows the results for the independent adults with and without prior college experience. As with the dependent sample, there are very few differences comparing the control and treatment groups. There are, however, differences in means between independent groups, as is evident from comparing both control group means. About 64 percent of participants with prior college experience were female while about 58 percent of participants without prior college experience were female. Slightly more than 70 percent of independents without prior college experience were white, but for those with previous college experience the

¹⁸ In prior versions of the paper, we also included comparisons of parental education levels. For the dependent participant sample, about 58- 63 percent of participants in the respective treatment groups had fathers and/or mothers with a high school level of education. For mothers, 26-30 percent had completed some college while 16-19 percent of fathers had completed some college. The rest of the parents' education levels were either unknown or junior high. There were no significant differences in parental education levels across treatment groups.

proportion was about 64 percent. Participants with previous college experience also had incomes that were about \$1000 to \$1500 more than those with no previous college experience.

Empirical Strategy

Because the proposed treatment was administered using randomization, simple comparisons of participants in the various treatments can identify the relative effects of the interventions. Our control group (i.e. those receiving only a brochure of basic information) is compared to our treatment groups. The “intent-to-treat” (ITT) effect can be estimated with the following regression:

$$(1) \quad y_i = \delta_0 + \delta_1 * FAFSA_i + \delta_2 * INFO_i + \varepsilon_i$$

where y is an outcome for individual i , $FAFSA$ represents whether H&R Block offered individual i the first treatment – assistance with completing the FAFSA and a personalized aid estimate, and $INFO$ represents whether H&R Block offered individual i the second treatment – an estimate of the amount of financial aid he or she is eligible for at area colleges but no help with the FAFSA. Effectively, this analysis simply compares mean outcomes between treatment and control groups. Appendix Table 2 shows similar results when including additional background controls such as age, gender, race, parental education, and family income.

Treatment-on-the-treated (TOT) effects can be calculated by dividing ITT effects on college enrollment by the treatment effect on FAFSA filing. Interpretation of these effects, however, depends on the extent to which FAFSAs were filed electronically from H&R Block or whether FAFSAs were mailed first to participants and then to the DOE. In the first case, filing is automatic, regardless of initial college-going interest. In the second, only those interested enough to follow through after being mailed the application do so. Since dependents were not usually at H&R Block offices with parents, we could not obtain signatures for them and therefore these participants were required to sign the FAFSA we mailed or a signature page and send the form to the DOE. In contrast, for most independent participants we were able to collect all information required to complete the FAFSA and obtain consent to file the application electronically. About half agreed to have their application submitted electronically, while the other half opted to have their application sent to them first.

V. RESULTS

Program Effects on FAFSA Submission

Table 3 shows our main results. The first panel reports treatment effects on dependent participants (mostly high school seniors with parents offered assistance in completing the FAFSA at H&R Block). The second panel shows effects for independents with no prior college experience (mostly individuals in their twenties with a high school degree and working). The third shows effects for independents with prior college experience (those currently in college or who dropped out before graduating). Column 1 presents program impacts on the likelihood of submitting a FAFSA to the DOE for the school term immediately following the intervention. Filing status is regressed on indicators for whether the participant was exposed to simplification and information (the FAFSA treatment) or the information-only treatment using robust standard errors.¹⁹ Among dependents, 39.9 percent of the control group went on to file a FAFSA. In contrast, those who were offered help completing the form through our study were 15.7 percentage points more likely to file (column 1), which corresponds to a 40 percent increase (p-value<0.01). The requirement that both parent and dependent sign the FAFSA explains why the filing rate was not even higher among the treated. The application had to be first mailed to the dependent's household to be signed and then sent to the DOE. Likely those more interested in college actually followed through.

The information-only treatment did not have a substantial effect on aid application submission. Participants who received only information about their likely grant and loan eligibility relative to college costs were no more likely to file a FAFSA than the control group, though the small sample size of dependent children in this treatment group makes it difficult to rule out a possible effect for this group. However, we can rule out at the 5 percent significance level that the FAFSA assistance and information-only treatment effects are the same. There was a clear, large effect for those who received the FAFSA treatment.

For independent adults without prior college experience, the fraction who filed a FAFSA among the control group was, not surprisingly, smaller than that among dependents transitioning out of high school. 16.1 percent of the control group of independents without prior college experience filed the aid application. The FAFSA treatment effect on filing, however, was very large: a near tripling of the FAFSA submission rate to the DOE, from 13.8 percent to 39.5

¹⁹ Our results are robust if we cluster our standard errors at the level of the tax professional or tax office.

percent. Interestingly, filing rates were much higher for those who agreed to have H&R Block submit for them. For those who agreed, the filing rate was 87.2 percent (not 100 percent due to the need for additional information that our Call Center was unable to get). For those who opted instead to first have their FAFSA sent to them, the filing rate was 16.5 percent. Of course, independents choosing the electronic option may have been more interested. Conditioning on self-reported interest in college, filing rates for those opting to be mailed the paper FAFSA first submitted their application to the DOE at a rate of 26.9 percent while about the same for those choosing electronic (88.8 percent). Meanwhile, the information-only treatment had essentially no impact on filing.

The FAFSA filing rate for independents with prior college experience in the control group was 32.0 percent. This rate rose by 19.5 percentage points for the FAFSA treatment group, to 51.5 percent. The fraction filing among this treated group opting to file electronically was 84.2 percent compared to 15.8 percent for those opting to receive the application first before submitting to the DOE. As with the other samples, however, the information-only treatment appears to have had no effect on filing status.

Program Effects on College Enrollment and Pell Grant Receipt

Column 2 of Table 3 shows the estimated Intent-to-Treat (ITT) effects on college enrollment during the year immediately after participation in the program using data from the NSC and OBR. The FAFSA treatment effect on dependent participants is remarkable: College enrollment increased by 8.1 percentage points, from 34.2 percent among the control group to 42.3 percent among the treated, or a relative increase of about 23.7 percent ($p = 0.019$). Column 3 indicates an equally striking gain to the fraction of treated participants who received Pell Grants using DOE administrative data. Pell Grant receipt within a year of treatment rose by 10.6 percentage points, from 29.6 percent to 40.2 percent ($p = 0.002$). The higher estimated effect may imply that the program increased aid receipt among those who would have gone to college regardless of treatment, but the estimate is not precise enough to rule out only enrollment effects (to receive a Pell, a student must have his or her college registration verified). Note that these ITT effects suggest large TOT effects. If the program impact on college enrollment only occurred through FAFSA filing, the results suggest that more than half of the dependent sample induced to file ended up in college ($0.081/0.159$). An explanation for these large effects may be

that the FAFSA treatment for dependents involved mailing complete or near-complete FAFSAs only to households. FAFSAs were not actually filed unless applicants followed up by mailing these forms to the DOE.

Table 3 also indicates substantial treatment effects among independent participants with no prior college experience. Within a year after offering help to complete the FAFSA, college enrollment rose 1.5 percentage points, from 9.5 to 11.0 percent ($p = 0.026$). The fraction of college students who received Pell Grants rose even more, from 11.1 percent to 14.1 percent ($p < 0.001$). The higher estimated impact ($p = 0.057$) suggests that FAFSA assistance helps students already intending to go to college to receive financial aid.²⁰

We find no enrollment effects among independents with previous college experience, but we expected this, since many in this group are already enrolled or intending to complete an unfinished program. Our interest in looking at these individuals is primarily to explore whether the intervention increased financial aid receipt. The results provide some evidence in favor of this hypothesis and is consistent with the higher Pell Grant receipt effects estimated for the other samples. Overall, the fraction who received Pell Grants among FAFSA treatment participants with prior college experience is 1.7 percentage points higher than those in the control group ($p = 0.101$). Conditioning on going to college, FAFSA filing among this group rose from 79.7 percent to 86.1 percent ($p = 0.001$), and Pell Grant receipt rose from 59.4 percent to 64.0 percent ($p = 0.093$).

Our results hold up well against concerns about multiple testing. Our main analysis is limited to a very small number of pre-specified questions: 1) Does FAFSA filing increase with FAFSA or information treatments? 2) Does FAFSA treatment increase college enrollment for dependents or independents with no prior college, and 3) Does FAFSA treatment increase financial aid receipt for those already going to college? Virtually by definition, the FAFSA treatment increases the number of FAFSAs filed and therefore estimated effects on filing should not be included among the set of possible spurious effects. For participants agreeing to have H&R Block file the form on their behalf, this effect is virtually automatic. For other cases, greater fafsa filing merely confirms that participants mailed a complete or near-complete FAFSA with a pre-paid envelop were more likely to submit the application to the DOE than if left on

²⁰ The NSC data does not cover all schools, which likely explains why the control mean receiving financial aid in college using the comprehensive DOE data is slightly higher than the control mean in college regardless of aid using the NSC data.

their own to apply. This, of course, does not guarantee that the treatment impacts enrollment, but it is likely a necessary condition. The information-only treatment, on the other hand, shows no signs of successfully boosting applications. Because of this, we drop this sample from the rest of our analysis.

Turning to the second question of whether the FAFSA treatment increases college enrollment, the probability of finding at least one false positive effect is 9.8 percent (assuming a Type I error rate of 5 percent). With p-values for these estimates at 0.019 and 0.026 respectively, we easily reject the possibility that at least one of these effects is spurious using the Benjamin and Hochberg procedure and marginally reject the hypothesis under the more conservative Bonferroni procedure (Schochet, 2008). Turning to the third question of whether the FAFSA treatment increases Pell Grant aid receipt, we cannot answer the question directly using these two samples since aid receipt depends on college enrollment. We can, however, look at the sample of independents with some prior college since we estimate (and expect) no enrollment effects. This sample provides some marginal evidence of aid receipt among college-going. Since the remainder of the analysis focuses on more detailed enrollment outcomes and sub-populations, we end the analysis here for independents with prior college experience. As a further test on whether the FAFSA treatment increases aid receipt independently of enrollment, note that combining the entire sample to test whether the Pell Grant receipt effect is larger than the enrollment effect leads to a p-value of 0.018.

Program Effects on Type of College Enrollment

Table 4 focuses on NSC college enrollment outcomes to examine whether the FAFSA treatment increases particular types of attendance. For both dependents and independents without prior college, the treatment effect on enrollment occurred mostly from increases in public college enrollment. Public college enrollment rises 6.5 percentage points (p-value = 0.052) for the dependent sample, compared to 1.9 percentage points at private colleges (p-value = 0.226). Among independents without prior college, control and treated mean differences only arise when looking at public college attendance than private. Correspondingly, we find no treatment effects on going to for-profit colleges.

For dependent students, we also find a doubling in the rate of attendance at selective colleges for those who received the FAFSA treatment. Many selective colleges require

applications prior to the start of college; however, the particular selective colleges which explain much of the treatment effect had deadlines more in the middle of tax season during 2008. Ohio State University, is one of these colleges. It was often listed on a default list of schools for sending FAFSA results to when participants did not specify alternatives. Table 4 also shows that most of the increase in attendance rates comes from full-time attendance, especially for dependents. Effects appear to be spread similarly between two-year and four-year programs. Not surprisingly, they occur from higher in-state enrollment opposed to out-of-state enrollment.

Program Effects on Aid Amounts and Submission Date

In Table 5, we use the DOE administrative data to examine the effects of FAFSA treatment on the specific type and amount of financial aid received. The first row of outcomes replicates estimates from Table 3 showing substantial gains to Pell Grant receipt using DOE administrative data. These results translate into an average increase in Federal grant aid of \$766 for dependents (from \$2,360 to \$3126) and an average increase of \$173 among independents without prior college experience (from \$815 to \$988). These treated participants also experience an increase in federal loan receipt: dependents are 17.7 percent more likely to receive loan aid, and independents are 13.9 percent more likely, although these effects are somewhat imprecise.

For many states and institutions, there are binding deadlines for applying for financial aid.²¹ In Table 5, we also compare the timing of FAFSA applications among filers. Given that there was a treatment effect on FAFSA filing, it is somewhat difficult to interpret these results. The estimated difference in the time to file is a weighted average of the effect of the program on filing timing for participants who would have filed *regardless* of the experiment and the timing of participants who were newly *induced* to file because of the program and would not have filed otherwise. If the timing of new-filers is slower than the average participant, then the comparisons would be biased downward. However, the timing results reinforce the idea that the FAFSA intervention accelerated the aid application submission process. Among dependent students in the control group, the average filing date was around May 1. Participants in the treatment group filed their FAFSAs almost one month (29 days) earlier. For independent

²¹ The earliest deadline among states is March 1st. Arizona, Idaho, Maryland, Michigan, Montana, Rhode Island, Tennessee, and West Virginia all share this deadline.

participants without prior college experience, those treated filed FAFSAs more than 2 months earlier than the control group.

Heterogeneous Enrollment Effects

In Table 6, we explore whether the program had heterogeneous effects among participants. The first row again replicates estimated enrollment effects from Table 3 for both the dependent sample and independent sample with no college experience. The next rows present separate estimates by how participants responded to the pre-randomization survey about interest in college, reasons why some people don't go to college, race, and by gender and whether they chose a loan to receive an immediate tax rebate. Unfortunately, we lose considerable power when splitting our samples by these subgroups. The only two significant differences are for dependents responding to the college interest question (those very interested in college are more likely to enroll from receiving the FAFSA assistance) and for independents by race (African Americans are more likely to enroll than other treated participants). The point estimates are also larger for those who mention a need to work as the main reason why some do not go to college and for females.

Retention Effects

One concern with nudging individuals into benefit programs is that some may not actually benefit. A nudge that influences consequential long-term outcomes, positively or negatively, suggests the reverse possibility too: not nudging may make some worse off. An important consideration to note is that we helped with financial aid applications but not with actual college applications (another seemingly small obstacle that may inhibit individuals from enrolling). Compliers therefore had to take at least some initiative. Most North American papers suggest significantly positive and increasing returns to college for students at the margin of going (e.g. Hout, 2011, Card (1995), Card and Lemieux (2005), Carneiro and Lee (2011), Jepsen, Troske, and Coomes 2009, Oreopoulos and Salvanes, 2011).

While we have neither statistical power nor consent (nor data) to estimate FAFSA treatment effects on long term earnings, we can look at college persistence as evidence whether application assistance does more than nudge individuals into college who then quickly dropout within the first year. Tables 7A, 7B, and 7C examine if the FAFSA treatment effects on

enrollment in the first year following the experiment carry over into subsequent years. Our National Student Clearinghouse data include college registration outcomes up until December 2010, almost three years after participants were invited to participate while visiting H&R Block visits (in January to April, 2008). The first row in Table 7A shows similar estimates of FAFSA treatment effects on college enrollment in the first year of the experiment, but using only NSC data instead of NSC and OBR data combined. Results are similar to Table 3, both for the dependent sample and independent sample without prior college experience. The second row shows effects on college attendance in the second year following the experiment. The point estimates fall to 5.1 and 0.4 percentage points for dependents and independents, respectively, and are no longer significant, although in both cases we cannot reject that the true effects are the same as the first-year results. Note that the enrollment rate actually rises slightly for the control group. Between years, some participants drop out of college while others enter for the first time. To help separate these inflows and outflows, Rows three and four show effect estimates on first-time college enrollment in the second and third year following the start of the experiment. The FAFSA treatment effect on whether dependents first enter college between one to two years following FAFSA assistance is negative and marginally significant, suggesting that the program speeds up college enrollment for some who would have entered later. The effects on first entering college between the second and third year of the experiment are zero for both samples, although they are imprecise. The point estimates for FAFSA treatment effects on ever enrolling in college during the three years following the experiment are also imprecisely estimated at 4.8 percentage points for Dependents and 0.9 percentage points for Independents.

Despite the fact that some college entrants dropout, we find similar impacts on college enrollment whether looking at one or over two consecutive years. Dependents are 8 percentage points more likely to either enroll in college the first year after the experiment and enroll again in the second year, or enroll the second year after the experiment and enroll again the third year, compared to the control group's consecutive college enrollment rate of 28 percent (we cannot tell whether those in third year stay on into the fourth). Independents in the FAFSA treatment group are also more likely to remain enrolled in college over two years (11.2 percentage points compared to 10.0 percentage points for the control group). These results translate into increases in total years in college over the three years of data of 0.19 years and 0.03 years for the dependent and independent samples respectively. The higher point estimates on retention effects

compared to overall enrollment effects may suggest compliers of the FAFSA treatment are relatively more committed to completing a program once enrolled, or that the assistance help attendees receive aid over multiple years.

Table 7B presents similar results using DOE data on subsequent Pell Grant receipt instead of NSC data on college enrollment. Dependents assigned to the FAFSA treatment group were 10.6 percentage points more likely to receive a Pell in the first year following the experiment but 4.6 percentage points less likely to first receive one in the second year ($p = 0.057$). This pattern, also in Table 7A, suggests assistance in completing the FAFSA may speed up college-going outcomes. In addition, the fraction of participants that ever received a Pell over three years following the experiment is 4.9 percentage points higher for dependents in the main treatment group ($p = 0.168$) and 2.2 percentage points higher for independents in the treatment group ($p = 0.022$) compared to those among the controls. We also find significant FAFSA treatment effects on consecutive Pell Grant receipt. Dependents are 36 percent more likely to receive a Pell over two consecutive years from FAFSA assistance compared to those in the control group. The total number of Pell Grants received over the three year period is higher for both dependents and independents provided FAFSA assistance.

To increase precision, and in view of the similarity of the results using NSC and DOE data, we estimate FAFSA treatment effects after stacking enrollment and Pell Grant receipt outcomes and clustering standard errors by participant. These effects are similar to those reported in Tables 7A and 7B, though somewhat more precisely estimated. In particular, treatment effects on ever being enrolled or receiving a Pell Grant ($p = 0.139$) over the three year period are marginally significant for both dependent and independent samples ($p = 0.059$). Retention effects also remain large and significant.

VI. CONCLUSION

The results of the H&R Block FAFSA experiment demonstrate remarkably strong effects from providing information about aid eligibility and offering personal assistance to complete a more streamlined aid application. The FAFSA treatment substantially increased college financial aid applications, improved the timeliness of aid application submission, increased the receipt of need-based grant aid, and ultimately increased the likelihood of college attendance and

persistence. Students just graduating from high school whose parents received the assistance saw an 8 percentage point increase in college enrollment the following year. Independents without prior college experience saw a 2 percentage point increase, and both groups were significantly more likely to persist in their programs beyond the first year. Non-traditional students who had already spent time in college were not more likely to enroll in college, but were more likely to receive financial aid. Providing accurate grant and loan eligibility estimates, however, had no impact on FAFSA filing, and thus did not likely affect college enrollment.

The effects of the FAFSA treatment are large, especially relative to the intervention's low marginal cost. The treatment of providing FAFSA assistance took 8 minutes, on average, and cost about \$2.50 per participant for tax professional training and time. Software installation, maintenance and printing materials added roughly another \$15.00 per participant. The largest costs to the program were from call center support (\$30.00 per participant) and participation incentives (\$20.00 to participants and up to \$20.00 to tax professionals).²² These costs would likely fall significantly in a more automated and/or non-research setting. Still, even at \$87.50 per participant, the substantial impacts on actual college participation and retention are impressive. The only other comparable estimate of an enrollment effect stems from the introduction of the Georgia Hope Scholarship (Dynarski 2000). However, that effect was due to a \$3,000 scholarship. Moreover, that program and other interventions with large effects often go to students who would have enrolled anyway (Deming and Dynarski, 2009).

Personal assistance in completing the FAFSA makes the process more visible, simple, informative, and encouraging. Offering this assistance immediately after completing a tax form speeds up the process, makes it more convenient, and eliminates the need to ask many not easily-answerable questions. The FAFSA Experiment explores whether offering personal assistance while pre-populating many of the more difficult-to-get questions on the FAFSA makes the difference between some individuals going to college or not, but was not designed to distinguish which mechanisms played the greatest role. We hope future research can shed additional light on this issue. We do, at least, reject that information alone on aid eligibility increases FAFSA applications, though perhaps providing information earlier (i.e. when the student is a high school freshman or sophomore) would generate larger effects.

²² There is also the cost in aid to consider from a redistributive perspective: \$375 on average per dependent (\$3,826 on average for compliers), and approximately \$100 on average per independent (\$4,157 on average for compliers).

One key question of interest is whether our results would have occurred through form simplification alone, without face-to-face assistance. Venegas (2006) describes student frustration from having to pause and revisit the online FAFSA multiple times:

“...at first I had to go on-line to get a PIN [personal identification number] for myself. Then later I went back to fill out my FAFSA. When I was at the end of the form, I saw that I had to get a PIN for my parents... I got a PIN for my parents and then I went back to complete the form... then I had to go back again and look at my SAR [Student Aid Report]” (p. 9).

Treated participants of our study avoided the PIN process altogether from having H&R Block submit electronically or from submitting a paper application instead.

The Department of Education has made some headway into simplifying the existing online FAFSA, including introducing skip-logic to minimize the number of questions and allowing applicants and parents to import IRS income tax data (depending on some criteria). Is this enough to remove application barriers to college? Beshears *et al.* (2011) provides evidence that simplification on its own can increase program take-up (of corporate savings plans), but other evidence suggests a role for face-to-face communication. No amount of simplification will help if individuals do not actually access the form. Kinchelov and Brown (2005), for example, find that 49 percent of parents with eligible children for medical insurance (Medi-Cal) did not sign-up because they did not know about the program or because they believed their child was ineligible. Even those aware of a program must find time to complete the application. Koehler and Poon (2005) find that people regularly overestimate their likelihood of completing a task, and that the strength of one's intentions plays little role in actual completion. Similarly, Mullainathan and Shafir (2010) find that 90 percent of unbanked individuals provided a referral letter and instructions to open up a bank account reported thinking they would follow through, but only 50 percent actually did. Enrollment was 10 percentage points higher for a random subset of attendees given the opportunity to complete the application with personal assistance at the workshop location. The effects from face-to-face assistance and application simplification may interact, making it easier to offer to help complete an application “now.” Without pre-populating the FAFSA with information just collected from tax forms, for example, our treatment would have taken much longer.

Our findings suggest many other opportunities beyond the FAFSA for increasing participation in programs that require filling out forms to become eligible. Offering immediate personal assistance to complete a form quickly may help some obtain a bank account, become insured, receive unemployment insurance, set up an education savings account, register to vote, start a business, claim a patent, become a citizen, or get a job. As with the FAFSA, the eligibility processes associated with many of these outcomes cannot easily be simplified further. Personal assistance may provide a cost-effective way to further encourage individuals and increase participation.

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Appendix: Independent Sample Sensitivity Analysis

In section III we noted a small, but marginally significant difference in the number of consents received across treatment and control groups for the sample of independents (not the sample of dependents). This difference becomes more significant when focusing on our more key sample of independents with no prior college experience assigned to either the FAFSA treatment or control group. Among this sample, Appendix Table 1 shows in column 1, row 1, that we have consent to include in the analysis 51.6 percent from the FAFSA treatment compared to 48.4 percent from the controls. With this total sample of 8,506, we reject that the underlying distribution of this sample is 50/50 (as it should be). Even if we added back 306 control participants from the missing consent sample to balance the sample and all of them filed a FAFSA, we would still estimate significant FAFSA treatment effects on filing. The fraction enrolled in college among the 306 disproportionately missing controls would have to be 60 percent higher than that among the observed control group (15.8 percent versus 9.9 percent) to end up with an insignificant enrollment effect on the independent sample with no prior college. For Pell Grant receipt outcomes, the 306 missing controls would have to be more than 3 times as likely to receive a Pell than the observed controls (39.7 percent versus 12.9 percent).

As we noted in the main text of the paper, one explanation for this difference is that more printed material was produced for treated participants. This may have made it more likely that a few tax professionals came across treated consent forms more often than controls when reviewing what paper to keep and what paper to give to clients. Importantly, receiving a written consent depended on actions by the tax professional rather than the participant, and the reasons tax professionals and district managers gave for not submitting paper consent forms are not related to our outcomes of interest.

To further examine the sensitivity of our results from possible selective participation, rows 2 and 3 show estimated treatment effects after dropping offices with significantly different treatment and control samples at the 10 and 15 percent levels of statistical significance respectively. In both cases the remaining samples are balanced and the FAFSA treatment effects on college enrollment and Pell Grant receipt are similar to our main results. We also find similar results after dropping participants from all offices from which we received more consents from treated participants than controls and from including office fixed effects (rows 4 and 5). In row 6 we drop the sample connected with specific tax professions within offices who delivered more

treated consents than control. The enrollment effects are now imprecise, but the Pell Grant receipt effects are similar to the main results and significant. This is also the case when including fixed effects for tax professionals.

Appendix Figure 1. Information and Aid Calculation Worksheet

PERSONAL AID CALCULATION

The first step in applying for financial aid is to fill out a form called the Free Application for Federal Student Aid or FAFSA. The government uses this form to determine how much financial assistance you may be eligible for.

This letter provides you with an estimate of how much financial aid you may be eligible for if you were to attend college either part-time or full-time next year. This estimate can help you plan for your college education.

GRANT/SCHOLARSHIP AID

in college grants per year based on the information we have from helping you complete your taxes and the formula the Department of Education uses.

Even better, this financial aid would not need to be repaid.

The following is a list of local colleges, the full and part-time tuition expenses, and estimates of the financial assistance you could get from your state or the federal government.

College	Full-Time		Part-Time	
	Tuition	Estimated State and Federal Aid*	Tuition	Estimated State and Federal Aid*

*The estimate of financial aid is based on information reported to your H&R Block Tax Professional. If some of the information needed for the formula was not available, this may affect the reliability of the estimate. Local colleges' financial aid departments can arrange receipt of and provide the most accurate estimate of financial aid.

Please remember that eligibility for this type of financial aid is dependent upon the completion and submission of the Free Application for Federal Student Aid (FAFSA) form and selecting a college that is recognized by the federal government. Our estimates in this letter are based on the information we had available to us. The actual financial aid amount you receive could be higher or lower, depending on the rest of the information needed to complete the FAFSA.

LOAN AID

in Stafford student loans from the federal government.

You can use these loans to help pay for the costs of college that are not covered by other forms of aid. Although these loans must be repaid after graduating or leaving college, the government offers these at a very low interest rate making them especially helpful to many students. Given your information, it is likely that the federal government would even pay the interest on the loan for you while you are in college.

You may also be eligible for other aid in the form of grants, scholarships, and additional loans from some of the institutions that you may want to attend. Eligibility for these types of aid depends on various factors, which may include your financial need or your performance in high school. We encourage you to contact the admissions or financial aid offices of the schools that you may be interested in attending for further details.

We hope this information is helpful, and we thank you for your interest and participation in this program.

Table 1. Consent, Exit, and Processing Rates by Sample and Treatment Status

	1	2	3	4	5	6	7
	Random-ization Rate	Initial Screening Qualification (<i>number</i>)	Expressed Interest	Final Qualification	Accepted and Gave Consent	Finished Office Interview	Sample with Complete Data
			<i>(Fraction of Individuals who Initially Qualified)</i>				
A. DEPENDENT SAMPLE							
Control Group	46%	6,438	0.532	0.413	0.403	0.4	0.302
FAFSA Treatment and Info-Only Group	54%	7,510	0.512	0.404	0.395	0.392	0.298
Full Sample	100%	13,948	0.521	0.408	0.398	0.396	0.3
F-test p-value (Testing of Equality of Means)			0.023	0.284	0.327	0.334	0.31
B. INDEPENDENT SAMPLE							
Control Group	46%	25,215	0.515	0.374	0.372	0.369	0.284
FAFSA Treatment Group	46%	25,491	0.521	0.379	0.377	0.374	0.293
Information-Only Treatment Group	8%	4,377	0.511	0.367	0.365	0.361	0.283
Full Sample	100%	55,083	0.518	0.376	0.374	0.371	0.288
F-test p-value (Testing of Equality of Means)			0.274	0.287	0.216	0.144	0.06

Notes: The dependent sample figures includes both high school seniors and recent graduates, who are examined in this paper, as well as participants age 15-17, who will be examined in future work (they were not old enough to have enrolled in college yet). The independent sample analyzed in this paper includes both those with and without prior college experience. To initially qualify for this study, families had to have an AGI less than \$45,000 and a family member between the ages of 15 and 30 who did not already have a bachelor's degree. After asking whether these potentially eligible families were interested in learning more about college (the column labeled "Expressed Interest"), the tax professional posed additional questions to check for eligibility and determine final qualification (column labeled "Final Qualification"). Nearly all of these participants agreed to give consent (column labeled "Accepted and Gave Consent") and then completed the office interview (column labeled "Finished Office Interview"). The column labeled "Analysis Sample with Complete Data" reports the percentage of each group for whom we have complete survey, tax, and FAFSA filing data. In order for the data to be complete, a paper consent form had to be sent via snail mail to the central project office by the tax professional. The three analytic samples examined in this paper are: 868 the dependent sample in grade 12 or with completed high school (0.06224*13,946), 9,228 independent students with no prior college experience (0.16753*55,083), and 6,646 independent students with prior college experience (0.12065*55,083).

Table 2. Mean Characteristics of Control Group and Differences by Treatment Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent Participants			Independent Participants with No Prior College Experience			Independent Participants with Prior College Experience		
	Control Mean	FAFSA Treatment Difference	Info Treatment Difference	Control Mean	FAFSA Treatment Difference	Info Treatment Difference	Control Mean	FAFSA Treatment Difference	Info Treatment Difference
Female	0.560	0.019 [0.035]	0.015 [0.061]	0.575	-0.003 [0.011]	-0.031 [0.020]	0.638	-0.002 [0.012]	-0.028 [0.023]
White	0.553	0.004 [0.035]	0.097 [0.059]*	0.703	0.002 [0.010]	0.001 [0.018]	0.653	-0.014 [0.012]	0.005 [0.023]
Black	0.379	0.013 [0.035]	-0.079 [0.057]	0.246	0 [0.009]	-0.012 [0.017]	0.285	0.016 [0.012]	0.003 [0.022]
Hispanic	0.023	-0.005 [0.010]	0.002 [0.019]	0.024	-0.001 [0.003]	0 [0.006]	0.023	0 [0.004]	-0.003 [0.007]
Age	17.713	0.029 [0.035]	0.05 [0.051]	25.911	-0.034 [0.067]	-0.191 [0.124]	26.207	0.147 [0.072]**	-0.151 [0.132]
Never in College	0.965	0.015 [0.012]	-0.002 [0.023]	1	1	1	0	0	0
Married				0.131	-0.002 [0.007]	-0.021 [0.013]	0.128	-0.005 [0.008]	-0.022 [0.015]
Single				0.8	0.006 [0.009]	0.019 [0.016]	0.803	0 [0.010]	0.031 [0.018]*
Divorced or Separated				0.069	-0.005 [0.005]	0.002 [0.010]	0.069	0.005 [0.007]	-0.009 [0.011]
Target Degree Would Be Bachelor's	0.412	-0.015 [0.035]	-0.025 [0.060]	0.272	0.004 [0.010]	0.028 [0.018]	0.425	-0.009 [0.013]	0.012 [0.024]
Target Degree Would Be Associate	0.354	-0.018 [0.034]	-0.017 [0.058]	0.488	-0.002 [0.011]	-0.033 [0.020]	0.476	0.009 [0.013]	0.009 [0.024]
Target Degree Unsure	0.234	0.033 [0.031]	0.041 [0.054]	0.239	-0.001 [0.009]	0.004 [0.017]	0.099	0 [0.008]	-0.022 [0.013]*
Very interested in college	0.746	-0.013 [0.031]	0.029 [0.052]	0.51	0.006 [0.011]	0.03 [0.020]	0.655	0.004 [0.012]	0.004 [0.023]
Adjusted Gross Income	\$23,211	381 [816]	-702 [1402]	\$16,404	-226 [210]	-587 [379]	\$17,801	98 [254]	-343 [457]
Observations	398	390	80	4,117	4,389	722	3,044	3,085	517

Notes: Dependent students are typically under the age of 24 and financially dependent on their parents. Most dependent participants in this sample are high school seniors. Independent participants are over the age of 24 or married, had a child, a veteran, or an orphan. "Prior college experience" is defined from surveying participants. Standard errors of differences are in square brackets, estimated using Ordinary Least Squares. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.

Table3. Summary of Results During First Year Following Experiment

	(1)	(2)	(3)
	Outcome during first year following experiment		
	Filed FAFSA (Based on DOE data)	Attended College (Based on NSC and OBR data)	Attended College and Received Pell Grant (Based on DOE data)
<u>Dependent Participants (N=868)</u>			
Control Group Mean	0.399	0.342	0.296
FAFSA Treatment Effect	0.157 [0.035]***	0.081 [0.035]**	0.106 [0.034]***
Info Treatment Effect	-0.012 [0.060]	-0.004 [0.058]	0.004 [0.056]
	0.101123596		
<u>Independent Participants, No Prior College (N=9228)</u>			
Control Group Mean	0.161	0.095	0.111
FAFSA Treatment Effect	0.267 [0.009]***	0.015 [0.007]**	0.03 [0.007]***
Info Treatment Effect	-0.019 [0.014]	0.003 [0.012]	-0.016 [0.012]
<u>Independent Participants, Some Prior College (N=6646)</u>			
Control Group Mean	0.32	0.263	0.209
FAFSA Treatment Effect	0.195 [0.012]***	-0.003 [0.011]	0.017 [0.011]
Info Treatment Effect	0.027 [0.023]	0.013 [0.021]	0.015 [0.020]

Notes: Treatment effects are mean differences between treatment and control groups (estimated using OLS). Robust standard errors in square brackets. DOE = Department of Education. NSC = National Student Clearinghouse. OBR = Ohio Board of Regents. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.

Table 4. Treatment Effects on Patterns of Attendance, First Year Following Experiment

<i>Dependent Variable</i>	Dependent Participants (N = 788)		Independent Participants with No Prior College Experience (N = 8506)	
	(1) Control Mean	(2) FAFSA Treatment Effect	(3) Control Mean	(4) FAFSA Treatment Effect
Attended:				
Public College	0.294	0.065 [0.033]*	0.070	0.011 [0.006]*
Private College	0.04	0.019 [0.015]	0.023	0.001 [0.003]
Four-year Campus	0.158	0.037 [0.027]	0.031	0.005 [0.004]
Two-year Campus	0.176	0.047 [0.028]*	0.062	0.008 [0.005]
Full-time	0.224	0.094 [0.032]***	0.049	0.008 [0.005]
Part-time	0.111	-0.011 [0.022]	0.044	0.004 [0.005]
In-State	0.302	0.081 [0.034]**	0.075	0.009 [0.006]
Out-of-State	0.033	0.003 [0.013]	0.018	0.003 [0.003]

Notes: Treatment effect estimates are from OLS regressions of the outcome dummy variables on FAFSA assistance treatment status. Outcomes are determined using National Student Clearinghouse data only. Robust standard errors in square brackets. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.

Table 5. Treatment Effects on Aid Receipt and FAFSA Filing, First Year Following Experiment

<i>Dependent Variable</i>	Dependent Participants (N = 788)		Independent Participants with No Prior College Experience (N = 8506)	
	(1)	(2)	(3)	(4)
	Control Mean	FAFSA Treatment Effect	Control Mean	FAFSA Treatment Effect
Received Any Pell Grant (not conditional on enrollment)	0.296	0.106 [0.034]***	0.111	0.03 [0.007]***
Total Scheduled Amount of Federal Grants	2,360	766 [285.741]***	815	173 [53.915]***
Received Federal Student Loan	0.231	0.041 [0.031]	0.079	0.011 [0.006]*
Date of FAFSA Filing 2008 Conditional on Filing (in days)	May 1, 2008	-29.008 [11.228]**	July 5, 2008	-69.007 [7.480]***

Notes: Treatment effect estimates are from OLS regressions of the outcome dummy variables on FAFSA assistance treatment status. Outcomes are determined using U.S. Department of Education data only. Robust standard errors in square brackets. Total Scheduled Amount reflects the actual amount of money transferred to schools as of March 2009. This may differ from the actual payments if students withdraw from school or transfer or if payments for a spring term have not yet been transferred to the students' schools. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.

Table 6. Sub-group Analysis of Treatment Effects on College Enrollment during First Year Following Experiment

<i>Sub-group category</i>	Dependent Participants (N = 788)		Independent Participants with No Prior College Experience (N = 8506)	
	(1)	(2)	(3)	(4)
	Control Mean	FAFSA Treatment Effect	Control Mean	FAFSA Treatment Effect
Full-Sample	0.342	0.081 [0.035]**	0.095	0.015 [0.007]**
"Very Interested in College"	0.428	0.121 [0.041]***	0.140	0.018 [0.011]*
Not "Very Interested"	0.089	-0.012 [0.039]	0.048	0.01 [0.007]
Some Do not Go because Must Work	0.334	0.078 [0.039]**	0.094	0.019 [0.008]**
Other Reasons Why Some Do not Go	0.364	0.1 [0.074]	0.097	0.007 [0.011]
Took Loan to Receive Tax Rebate Immediately	0.316	0.093 [0.041]**	0.094	0.015 [0.008]*
Did not take loan	0.411	0.041 [0.065]	0.098	0.015 [0.013]
Female	0.381	0.11 [0.047]**	0.112	0.016 [0.009]*
Male	0.291	0.038 [0.050]	0.071	0.013 [0.009]
Black	0.377	0.06 [0.056]	0.116	0.037 [0.015]**
Not Black	0.320	0.094 [0.044]**	0.088	0.007 [0.007]

Notes: Treatment effect estimates are from OLS regressions of the outcome dummy variables on FAFSA assistance treatment status. Enrollment is determined using National Student Clearinghouse and Ohio Board of Regents data combined (similar to Table 3, column 4) . Sub-groups are classified using data collected at H&R Block offices. Robust standard errors in square brackets. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.

Table 7A. Treatment Effects on Enrollment and Retention During Three Years Following Experiment

<i>Dependent Variable</i>	Dependent Participants (N = 788)		Independent Participants with No Prior College Experience (N = 8506)	
	(1) Control Mean	(2) FAFSA Treatment Effect	(3) Control Mean	(4) FAFSA Treatment Effect
	Attended College within one year after experiment April 2008 - March 2009	0.334	0.084 [0.034]**	0.093
Attended College in second year after experiment April 2009 - March 2010	0.344	0.051 [0.034]	0.131	0.004 [0.007]
First Entered College in second year after experiment April 2009 - March 2010	0.113	-0.039 [0.021]*	0.074	-0.005 [0.006]
First Entered College in third year after experiment April 2010 - December 2010	0.038	0.003 [0.014]	0.032	0.001 [0.004]
Entered College in first, second, or third year after experiment April 2010 - December 2010	0.485	0.048 [0.036]	0.198	0.009 [0.009]
Enrolled in College for Two Consecutive Years, April 2008 - December 2011	0.281	0.08 [0.033]**	0.095	0.012 [0.007]*
Total Years in College, April 2008 - December 2011	0.947	0.191 [0.085]**	0.329	0.027 [0.016]*

Notes: Treatment effect estimates are from OLS regressions of the outcome dummy variables on FAFSA assistance treatment status. Enrollment is determined using National Student Clearinghouse data only. A student enrolled for two years entered either in the first year after the experiment and stayed into the second year, or entered during the second year after the experiment and stayed into the third year. A student enrolled only for one year either entered in the first year after the experiment but not the second year, or entered during the second year but not the third. Robust standard errors in square brackets. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.

Table 7B. Treatment Effects on Pell Grant Receipt During Three Years Following Experiment

<i>Dependent Variable</i>	Dependent Participants (N = 788)		Independent Participants with No Prior College Experience (N = 8506)	
	(1)	(2)	(3)	(4)
	Control Mean	FAFSA Treatment Effect	Control Mean	FAFSA Treatment Effect
Received Pell within one year after experiment April 2008 - March 2009	0.296	0.106 [0.034]***	0.111	0.03 [0.007]***
Received Pell in second year after experiment April 2009 - March 2010	0.362	0.056 [0.035]	0.167	0.002 [0.008]
First Received Pell in second year after experiment April 2009 - March 2010	0.153	-0.046 [0.024]*	0.100	-0.009 [0.006]
First Received Pell in third year after experiment April 2010 - December 2010	0.063	-0.012 [0.017]	0.060	0.002 [0.005]
Received Pell in first, second, or third year after experiment April 2010 - December 2010	0.513	0.049 [0.036]	0.271	0.022 [0.010]**
Received Pell for Two Consecutive Years, April 2008 - December 2011	0.281	0.101 [0.033]***	0.125	0.009 [0.007]
Total Years Received Pell April 2008 - December 2011	0.967	0.23 [0.083]***	0.443	0.047 [0.018]***

Notes: Treatment effect estimates are from OLS regressions of the outcome dummy variables on FAFSA assistance treatment status. Pell Grant receipt is determined using Department of Education data. A student receiving a Pell for two consecutive years received it over the first two years after the experiment, or over the second and third year after the experiment. Robust standard errors in square brackets. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.

Table 7C. Stacked Treatment Effects on Enrollment and/or Pell Grant Receipt During Three Years Following Experiment

<i>Dependent Variable</i>	Dependent Participants (N = 1,576)		Independent Participants with No Prior College Experience (N = 17,012)	
	(1)	(2)	(3)	(4)
	Control Mean	FAFSA Treatment Effect	Control Mean	FAFSA Treatment Effect
Attended College / Received Pell within one year after experiment April 2008 - March 2009	0.315	0.095 [0.032]***	0.102	0.021 [0.006]***
Attended College / Received Pell in second year after experiment April 2009 - March 2010	0.353	0.053 [0.032]*	0.149	0.003 [0.007]
First Attended College / Received Pell in second year after experiment April 2009 - March 2010	0.133	-0.042 [0.020]**	0.087	-0.007 [0.005]
First Attended College / Received Pell in third year after experiment April 2010 - December 2010	0.050	-0.004 [0.013]	0.046	0.001 [0.004]
Attended College / Received Pell in first, second, or third year after experiment April 2010 - December 2010	0.499	0.049 [0.033]	0.235	0.016 [0.008]*
Attended College / Received Pell for Two Consecutive Years, April 2008 - December 2011	0.281	0.09 [0.031]***	0.110	0.01 [0.006]*
Total Years Attended College / Received Pell April 2008 - December 2011	0.957	0.211 [0.079]***	0.386	0.037 [0.016]**

Notes: Treatment effect estimates are from OLS regressions of the outcome dummy variables on FAFSA assistance treatment status. Enrollment outcomes from the National Student Clearinghouse and Enrollment and Pell Grant Receipt outcomes from the Department of Education are stacked together to increase precision. Standard errors are in square brackets and clustered by participant. A student enrolled for two years entered either in the first year after the experiment and stayed into the second year, or entered during the second year after the experiment and stayed into the third year. A student enrolled only for one year either entered in the first year after the experiment but not the second year, or entered during the second year but not the third. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.

Appendix Table1: Sensitivity Analysis of Independent Sample Without Prior College

	(1)	(2)	(3)	(4)
	Outcome during first year following experiment			
	Fraction Treated (T) & Control (C) and p-value that samples are balanced	Filed FAFSA (Based on NSC and OBR data)	Attended College (Based on NSC and OBR data)	Attended College and Received Pell Grant (Based on DOE data)
Full Sample (N=8506)	0.516 / 0.484 p=0.003	0.267 [0.009]***	0.015 [0.007]**	0.03 [0.007]***
Offices without unbalanced T&C samples, p<0.10 (N=7269)	0.507 / 0.493 p=0.227	0.266 [0.010]***	0.014 [0.007]**	0.033 [0.008]***
Offices without unbalanced T&C samples, p<0.15 (N=6741)	0.505 / 0.495 p=0.387	0.27 [0.011]***	0.015 [0.007]**	0.034 [0.008]***
Offices with same or more control consents returned than treated (N=3619)	0.457 / 0.543 p=0.000	0.254 [0.015]***	0.018 [0.010]*	0.042 [0.011]***
Full Sample with office fixed effects (N=8506)	NA	0.265 [0.009]***	0.014 [0.007]**	0.03 [0.007]***
Taxpros with same or more control consents returned than treated (N=4403)	0.354 / 0.646 p=0.000	0.232 [0.014]***	0.010 [0.009]	0.022 [0.010]**
Full Sample with tax professional fixed effects (N=8506)	NA	0.254 [0.011]***	0.010 [0.007]	0.027 [0.008]***

Notes: Treatment effects are mean differences between treatment and control groups (estimated using OLS). Robust standard errors in square brackets. DOE = Department of Education. NSC = National Student Clearinghouse. OBR = Ohio Board of Regents. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.

Appendix Table 2: Sensitivity Analysis of Treatment Effects on First Year Enrollment with Alternative Data Sources and Additional Controls

	Dependent Participants				Independent Participants with No Prior College			
	1	2	3	4	5	6	7	8
	Enrolled in Yr 1 (NSC and OBR)		Enrolled in Yr 1 (NSC and OBR)	Enrolled in Yr 1 (NSC and OBR, with controls)	Enrolled in Yr 1 (NSC and OBR)		Enrolled in Yr 1 (NSC and OBR)	Enrolled in Yr 1 (NSC and OBR, with controls)
FAFSA Treatment	0.084 [0.034]**	0.036 [0.032]	0.081 [0.035]**	0.073 [0.032]**	0.012 [0.006]*	0.01 [0.005]**	0.015 [0.007]**	0.014 [0.006]**
Female				0.081 [0.032]**				0.027 [0.007]**
Black				0.021 [0.033]				0.021 [0.008]**
Hispanic				0.059 [0.128]				-0.007 [0.022]
Age (years)				0.158 [0.021]**				-0.007 [0.001]**
Dad's graduated from college				-0.001 [0.044]				0.006 [0.009]
Mom graduated from college				0.12 [0.040]**				0.011 [0.008]
Took out loan to receive tax rebate immediately				-0.022 [0.035]				-0.004 [0.008]
"Very Interested" in college				0.356 [0.030]**				0.084 [0.007]**
Adjusted Gross Income (000's)				0.003 [0.001]**				0.001 [0.000]
Constant	0.334	0.205	0.342	-2.85	0.093	0.044	0.095	0.204
Observations	788	696	788	788	8,506	7,778	8,506	8,506

Notes: Treatment effect estimates are from OLS regressions of the outcome dummy variables on FAFSA assistance treatment status, with and without indicated control variables. Robust standard errors in square brackets. NSC = National Student Clearinghouse. OBR = Ohio Board of Regents. Enrollment is determined using NSC data only, OBR data only, or both combined as indicated. A student enrolled only for one year either entered in the first year after the experiment but not the second year, or entered during the second year but not the third. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level respectively.