

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

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ABSTRACT

Growing concerns about low awareness and take-up rates for government support programs like college financial aid have spurred calls to simplify the application process and enhance visibility. We present results from a randomized field experiment in which low-income individuals receiving tax preparation help were also offered immediate assistance and a streamlined process to complete the Free Application for Federal Student Aid (FAFSA) for themselves or their children. Treated participants were also provided with aid estimates that were 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, high school seniors whose parents received the treatment were 8 percentage points more likely to have completed two years of college, going from 28 to 36 percent, during the first three years following the experiment. Families who received aid information but no assistance with the FAFSA did not experience improved outcomes. The findings suggest many other opportunities for using personal assistance to increase participation in programs that require filling out forms to become eligible.

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I. INTRODUCTION

The college financial aid system for students in the United States is complex and difficult to navigate. All individuals seeking financial assistance must first access and complete a lengthy application known as the Free Application for Federal Student Aid (FAFSA). The FAFSA includes more than a hundred detailed questions on topics ranging from earnings, savings, the receipt of government benefits, parental education attainment, driver's license number, previous drug convictions, and intended college plans. Students who are still financially dependent on their parents must also include information about their parents' incomes, the year their parents were married or divorced, and their parents' social security numbers. Then, under the threat of fines and prison, applicants and the parents of dependents must formally attest that all responses are accurate. Since this process is required for accessing most government aid and many need-based institutional aid programs, researchers and policy makers have suggested that its complexity and inconvenience deters many from accessing higher education and contributes to the enrollment gap between high- and low-income students (Dynarski and Scott-Clayton 2006; ACSFA 2001, 2005).

Low take-up is a more general problem for social and employee benefit programs. Recent research in psychology and economics indicates that strategic but often modest and low-cost changes in choice architecture ("nudges" as termed by Thaler and Sunstein, 2008) can have significant and persistent effects on program participation. Changes in defaults as well as more transparent and personalized information have been found to have substantial impacts on take-up rates in several settings, including employee retirement savings plans (Beshears, *et al.* 2006a, 2006b), public school choice (Hastings and Weinstein, 2008), and Medicare Part D drug Plans (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 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 is available on tax forms. After transferring tax information to the appropriate FAFSA fields, 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 (with participant permission) or 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 the personalized aid estimates 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 increase a form's visibility and improve perceptions about the value in filling out the form. It may reduce procrastination by making the application process more convenient and more appealing. It may also help reduce anxiety about making a mistake and remove possible stigma from one's low-income status. Moreover, combining assistance with another activity (e.g., completing one's taxes at an H&R Block office) could minimize disruption and lower the opportunity costs of time. Personal assistance may even empower individuals to consider the possibility of change (e.g., helping their children get to college), and by streamlining the process, it may avoid the need for detailed instructions and facilitates to offer these services on a large scale.

We find significant impacts from this type of help. Treated participants who we 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.

Whether those nudged into college actually benefit depends on their lifetime returns. Those affected had to take at least some initiative to get to college because we did not help with the college application (another seemingly small obstacle that may inhibit individuals from enrolling). We cannot observe future earnings (nor do we have consent or access to do this), but we can examine college persistence to explore whether new students dropout within the first program year or stay on. We find that, by three years after the initial intervention, treated dependents are 8 percentage points more likely have been enrolled in college for at least two consecutive years, an increase from 28 percent in the control group. We also find a 1.2 percentage point increase (from 9.5 to 10.7 percent) in consecutive annual enrollment among treated independents with no prior college. These results suggest the treatment helped some individuals get to and persist in college.

Our findings have implications for a wide range of programs that require filling out forms to become eligible. We discuss some possibilities in the conclusion. 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 Klauw 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 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 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.

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).

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.

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. Georgia students completing the FAFSA online can also apply for the Hope Scholarship with no additional form requirements. 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, approximately 850,000 individuals, 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).

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.³ 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).⁴ After a person completed their taxes in an H&R Block

³ 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.

⁴ 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).

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 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.⁵ 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:

⁵ 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 could consent to participate for their dependent child.

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 answer 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 award letter appears in the online appendix).⁶ 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.⁷ 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

⁶ 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).

⁷ 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.

prepaid envelope or filed directly to the DOE when applicants agreed.⁸ In total, we completed the FAFSA for nearly seven out of ten treated participants, either in the office or using call center staff.⁹

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 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,

⁸ 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.

⁹ 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.

this group is our key comparison group for determining the effects of the other interventions. The brochure was also given to the treatment groups.

Target Sample Sizes and Outcomes of Interest

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); 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 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.

Recruitment and the Analytic Samples

Table 1 outlines our recruitment process including the consent rates for our respective treatment and control groups.¹⁰ 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).¹¹

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

¹⁰ 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).

¹¹ 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.

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 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.¹² Nevertheless, to address the issue of selective participation further, we present results below that treatment and control groups have similar means across a wide range of observable characteristics.¹³

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

¹² 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.

¹³ In the online appendix we also show similar results 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.

main 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.¹⁴

Data on FAFSA Filing Status and College Outcomes

To study the effects of FAFSA filing and college outcomes, we linked our final sample to data from three sources: the U.S. Department of Education (DOE), the Ohio Board of Regents (OBR), and the National Student Clearinghouse. (NSC) The DOE data cover the universe of all FAFSA filers, and we observe from it all participants' annual FAFSA submissions over the three years following intervention. Thus, the DOE data is the source of our analysis on the impact of the treatments on FAFSA submission rates. The DOE data also enable us to observe the federal financial aid received by all participants. Aid receipt requires confirmation of college enrollment, so this outcome provides us with a combined indicator of enrollment and aid receipt. Since our intervention may have affected aid receipt without affecting enrollment or vice versa, we also want to look at the enrollment effects separately and thus focus on our other two data sources when investigating the impact of the treatment on college enrollment alone.

Our main outcome variable is an indicator for whether participants enrolled in college as indicated from the NSC or OBR data.¹⁵ The NSC data allow us to observe college program registration across the United States but only at institutions covered by the Clearinghouse repository. The NSC is a non-profit organization that provides national student degree and enrollment verification for schools, colleges, and employers, and for our analysis, NSC covers 92.3 percent of all college institutions in Ohio and North Carolina. In comparing the NSC sample of schools to the universe of colleges in the Integrated Postsecondary Education Data System (IPEDS), the list of schools that participate in federal student financial aid programs, vocational

¹⁴ 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.

¹⁵ In an online appendix, we explore how the estimated effects differ according to which data sources are used to measure enrollment. This analysis shows similar results when using only NSC, NSC and OBR data together, and when adding the DOE data to measure the treatment effect of being enrolled and receiving aid.

colleges are the most under-reported. The OBR data, which provides registration information on all public vocational colleges in Ohio, is a nice compliment to the NSC by partially addressing this hole (but not North Carolina). While the nation's largest for-profit colleges are included in the NSC sample, smaller for-profit colleges, particularly private vocational colleges, are under-reported in our measure of college enrollment. As long as these missing schools are eligible for federal financial aid programs, they will be captured in the results. A complete list of colleges that are not covered by the NSC measure appears in Appendix Table 4. They constitute 7.7 percent of all enrollments in Ohio and North Carolina.

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.¹⁶ As expected, observable mean characteristics between the control and treatment groups are generally balanced, and 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

¹⁶ 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.

was lower, but these differences are not statistically significant at the 5 percent level. The average age of the dependent sample was about 17.7 at the time of the interview across all three groups.¹⁷

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

¹⁷ 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.

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 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.

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. These data from the DOE cover the universe of FAFSA filers. We regress filing status on indicators for whether the participant was exposed to simplification and information (the FAFSA treatment) or the information-only treatment using robust standard errors.¹⁸ 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

¹⁸ Our results are robust if we cluster our standard errors at the level of the tax professional or tax office.

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 customized 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 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. Enrollment is measured as a new college program registration between April 2008 and March 2009, either in the OBR or NSC data. The FAFSA treatment effect on dependent participants is substantial: 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 control group sample in Column 3 is slightly higher than in Column 2 because of measurement error or because of the increased coverage of institutions in the DOE data.¹⁹ The higher estimated impact ($p = 0.057$) in Column 3 suggests that FAFSA assistance helps students already intending to go to college to receive financial aid. The online appendix shows treatment effect estimates for subgroups. In general, effects are larger for those who say initially they are very interested in college, but not significantly different across race and sex. The sub-group estimates, however, are underpowered and imprecise.

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 they are 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

¹⁹ While the DOE data is more likely to cover for-profit and vocational colleges eligible for federal financial aid, it will miss students who did not submit the FAFSA and qualify for the Pell Grant.

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 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 (using NSC data only) 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 (e.g. Ohio University) which explain much of the treatment effect had rolling application deadlines. Individuals could apply to these colleges in the middle of tax season during 2008. 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 \$3,126) 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. 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 participants without prior college experience, those treated filed FAFSAs more than 2 months earlier than the control group.

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, 2012, 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 6A and 6B examine if the FAFSA treatment effects on enrollment in the first year following the experiment carry over into subsequent years using NSC and DOE data respectively. Our NSC data include college registration outcomes up until December 2010, almost three years after participants were invited to participate while visiting H&R Block (in January to April, 2008). The first row in Table 6A 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 (OBR data on enrollment after three years will not be available until 2013). Results are similar to Table 3, both for the dependent sample and independent sample without prior college experience.

The second and third rows of Table 6A look at whether FAFSA treatment sped up college entry. The rows show effects on college attendance beginning in the second or third year of the experiment respectively. The FAFSA treatment effect on whether dependents and independents first enter college in the second year following the experiment is negative and marginally significant for the dependent sample, suggesting that the program sped up college enrollment for some who would have entered later. The estimated effect on first entering college in the third year following the experiment is about zero for both groups. The effect on ever enrolling in college during the three years following the experiment is the sum of these three effects: a 4.8 percentage point increase for dependents and 0.9 percentage point increase for independents. The dependent effect is imprecisely estimated and no longer significant different from zero.

Given possible second year entry effects, our best measure of persistence is an indicator for whether an individual entered college in the first two years of the experiment and stayed in college for two consecutive years. Row 5 of Table 6A shows that dependents are 8 percentage points more likely persist compared to the control group's consecutive college enrollment rate of 28 percent. The higher persistent than initial enrollment effect seems odd, given that persistence

is conditional on enrollment. If dropout rates after first year for treated dependents were the same to what they were for controls, we would have expected the persistence effect to fall by 37 percent.

One explanation is that virtually every dependent induced to enter college in first year stayed on into second and virtually every dependent induced to enter college earlier would have dropped out otherwise (since the first year entry effect is the same as the persistence effect and the second year entry effect is negative). Research by Adelman (2006) supports this possibility in concluding that delayed entry into college reduces students' educational attainment. While we only examine enrollment patterns up for three years following the FAFSA intervention, our results are consistent with those of Adelman (2006) suggesting that delayed enrollment reduces attainment.

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. Some caution may be in order since the effects are observed after three years, and control group students may eventually catch up to their counterparts in the treatment. However, given the magnitude of the effect and the prior research, the results are certainly consistent that the intervention led to long-run effects on attainment.

Table 6B 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 6A, 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.

VI. CONCLUSION

The results of the H&R Block FAFSA experiment demonstrate 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. While there is some evidence that the enrollment effects came as a result of convincing students who would have delayed enrollment to enroll immediately, we find that both dependents and independents without prior college experience had higher educational attainment when measured three years after the intervention. 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).²⁰ These costs would likely fall significantly in a more automated and/or non-research setting. Even at \$87.50 per participant, this translates to a cost of about \$1,100 per dependent complier and \$5,833 per independent complier. We may also wish to count the additional cost from higher aid payments: \$375 on average per dependent or \$3,826 on average per dependent complier, and approximately \$100 on average per independent or \$4,157 on average per independent complier. Over two years of college, this amounts to a total cost of about \$8,750 and \$14,150 for dependent and independent complier respectively. Returns to college among compliers would have to be at least as large as this to consider the program cost effective.

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 transferring data directly from the tax return on 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

²⁰ 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).

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.

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.* (2006b) 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 transferring data directly from the tax return to the FAFSA, 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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Table 1. Consent, Exit, and Processing Rates by Treatment Status

	1	2	3	4	5	6	7
	Randomi- zation Rate	Initial Screening Qualification (<i>number</i>)	Expressed Interest	Final Qualification (<i>Fraction of Individuals who Initially Qualified</i>)	Accepted and Gave Consent	Finished Office Interview	Sample with Complete Data
A. DEPENDENT SAMPLE							
Control Group	46%	6438	0.532	0.413	0.403	0.4	0.302
FAFSA Treatment and Info-Only Group	54%	7510	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
	Randomi- zation Rate	Initial Screening Qualification (<i>number</i>)	Expressed Interest	Final Qualification (<i>Fraction of Individuals who Initially Qualified</i>)	Accepted and Gave Consent	Finished Office Interview	Sample with Complete Data
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%	4377	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. The last column of Panel B illustrates how the subsample without prior college experience varies across the treatment groups. 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), 9,237 independent students with no prior college experience (0.16769*55,083), and 6,637 independent students with prior college experience (0.12049*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	FAFSA Treatment	Info Treatment	Control	FAFSA Treatment	Info Treatment	Control	FAFSA Treatment	Info Treatment
Female	0.56	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]	0	0	0	1	1	1
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	4117	4389	722	3044	3085	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.

Table 3. Summary of the Results

	(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</u> (N=868)			
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]
<u>Independent Participants, No Prior College</u> (N=9232)			
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</u> (N=6647)			
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	(3) Control Mean	(4) FAFSA Treatment
Attended Public College	0.294	0.065 [0.033]*	0.07	0.011 [0.006]*
Attended Private College	0.04	0.019 [0.015]	0.023	0.001 [0.003]
Attended Four-year Campus	0.158	0.037 [0.027]	0.031	0.005 [0.004]
Attended Two-year Campus	0.176	0.047 [0.028]*	0.062	0.008 [0.005]
Attended Full-time	0.224	0.094 [0.032]***	0.049	0.008 [0.005]
Attended Part-time	0.111	-0.011 [0.022]	0.044	0.004 [0.005]
Attended In-State	0.302	0.081 [0.034]**	0.075	0.009 [0.006]
Attended 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 appear in parentheses. 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) Control Mean	(2) FAFSA Treatment	(3) Control Mean	(4) FAFSA Treatment
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 appear in parentheses. 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 6A. Treatment Effects on Enrollment and Retention During Three Years Following Experiment

	Dependent Participants (N = 788)		Independent Participants with No Prior College Experience (N = 8506)	
	(1) Control Mean	(2) FAFSA Treatment	(3) Control Mean	(4) FAFSA Treatment
Attended College within one year after experiment Apr 2008 - Mar2009	0.334	0.084 [0.034]**	0.093	0.012 [0.006]*
Attended College in second year after experiment Apr 2009 - Mar 2010	0.344	0.051 [0.034]	0.131	0.004 [0.007]
First Entered College in second year after experiment Apr 2009 - Mar 2010	0.11	-0.039 [0.021]*	0.074	-0.005 [0.006]
First Entered College in third year after experiment Apr 2010 - Dec 2010	0.038	0.003 [0.014]	0.032	0.001 [0.004]
Entered College in first, second, or third year after experiment Apr 2010 - Dec 2010	0.485	0.048 [0.036]	0.198	0.009 [0.009]
Enrolled in College for Two Consecutive Years, Apr 2008 - Dec 2011	0.28	0.08 [0.033]**	0.10	0.012 [0.007]*
Total Years in College, Apr 2008 - Dec 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. Single, double, and triple asterisks indicate statistical significance at the 10, 5, and 1 percent level, respectively

Table 6B. Treatment Effects on Pell Grant Receipt During Three Years Following Experiment

	Dependent Participants (N = 788)		Independent Participants with No Prior College Experience (N = 8506)	
	(1) Control Mean	(2) FAFSA Treatment	(3) Control Mean	(4) FAFSA Treatment
Received Pell within one year after experiment Apr 2008 - Mar2009	0.296	0.106 [0.034]***	0.111	0.03 [0.007]***
Received Pell in second year after experiment Apr 2009 - Mar 2010	0.362	0.056 [0.035]	0.167	0.002 [0.008]
First Received Pell in second year after experiment Apr 2009 - Mar 2010	0.15	-0.046 [0.024]*	0.1	-0.009 [0.006]
First Received Pell in third year after experiment Apr 2010 - Dec 2010	0.063	-0.012 [0.017]	0.06	0.002 [0.005]
Received Pell in first, second, or third year after experiment Apr 2010 - Dec 2010	0.513	0.049 [0.036]	0.271	0.022 [0.010]**
Received Pell for Two Consecutive Years, Apr 2008 - Dec 2011	0.28	0.101 [0.033]***	0.13	0.009 [0.007]
Total Years Received Pell Apr 2008 - Dec 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 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.