

THE ROLE OF APPLICATION ASSISTANCE AND INFORMATION IN COLLEGE DECISIONS: RESULTS FROM THE H&R BLOCK FAFSA EXPERIMENT*

ERIC P. BETTINGER
BRIDGET TERRY LONG
PHILIP OREOPOULOS
LISA SANBONMATSU

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 of 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%, 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. *JEL* Codes: I2, H4, J24.

I. INTRODUCTION

The college financial aid system for students in the United States is complex and difficult to navigate. Individuals seeking

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financial assistance must be aware of, access, and complete a lengthy federal application known as the Free Application for Federal Student Aid (FAFSA). The FAFSA includes more than 100 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. Because 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 (Advisory Committee on Student Financial Assistance 2001, 2005; Dynarski and Scott-Clayton 2006).

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. 2012).

This article 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 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 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 empower individuals to consider the possibility of change (e.g., helping their children get to college); streamlining the process may avoid the need for detailed instructions and facilities to offer these services on a large scale.

We find significant impacts from this type of help. Treated participants for whom we provided streamlined personal assistance to complete the FAFSA were not only more likely to apply for financial aid but 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% 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% 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 forgoing 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 drop out 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 to have been enrolled in college for at least two consecutive years, an increase from 28% in the control group. We also find a 1.2 percentage point increase (from 9.5% to 10.7%) 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 article 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., Manski and Wise 1983; Dynarski 2000, 2003; Seftor and Turner 2002; Kane 2003), institutional choice (e.g., van der Klauuw 2002; Avery and Hoxby 2004; Long 2004), and persistence (e.g., Bettinger 2004). Although 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

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 (Hansen 1983; Manski and Wise 1983; Kane 1996). See also General Accountability Office (2005).

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 3 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 January of the year of college entry. Therefore, they often must apply to college before 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 eighteenth 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

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.

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, because potential students and their parents must know about the existence of aid to access it. A 2002 Harris Poll (Sallie Mae Fund 2003) 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% 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 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 costs of the most selective universities and hold other impressions that college is unaffordable. These stories may contribute to the fact that people, 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 more than 300%.³ Oreopoulos and Dunn (2009) find high school students are more likely to aspire to go to college three weeks after being provided accurate information about costs and benefits.

Policy makers 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

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.

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 assess 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 people 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 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 financially dependent on their parents.⁵ The second group is independent adults (often referred to as nontraditional students). These individuals are a growing segment of higher education, and research has shown that adult enrollments can respond strongly to

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 by region (only 12% of the final sample was from Charlotte).

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, so we focused on cases where a parent was completing taxes and could consent to participate for a dependent child.

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.” Participants were offered \$20 for their time. If the person was interested, the tax professional then asked him or her to complete a statement of informed consent. Once individuals consented, we asked 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 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 consisted of 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 figure 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.

6. If we could not collect all the information needed for the FAFSA during the initial office visit, we still tried to compute the amount of aid students were eligible to receive. 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.

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 Department of Education (DOE) free of charge or send a completed paper FAFSA by mail so that individuals could submit it themselves. If not all information could not 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 with the DOE when applicants agreed. In total, we completed the FAFSA for nearly 7 out of every 10 treated participants, either in the office or using call center staff.⁸

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

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.

8. Completion rates differed slightly by type of participant. Among independent students with no prior college experience, 54% completed their FAFSAs in the office and another 24% were completed with the help of the call center (for a total completion rate of 78%). Among dependent students, 11% completed the FAFSA by the end of the call center outreach process and another 66% 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.

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 be likely to 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.

III.A. 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. 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 nontraditional students with most currently working); and (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, because 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 our inability to obtain consent from 18-year-old dependent students, who most times were not at the H&R Block office with their parents. Our main dependent sample therefore focused exclusively on 17-year-olds.

III.B. Recruitment and the Analytic Samples

Table I outlines our recruitment process, including the consent rates for our respective treatment and control groups.⁹ During the tax season, H&R Block met with 236,483 clients in the targeted offices. Of this group, 69,034 clients met the study's initial criteria (having an adjusted gross income [AGI] less than \$45,000 and a family member age 15 to 30), 35,778 expressed interest in learning more about college (52% of clients meeting the study's criteria), and 26,395 qualified for the study after answering in the affirmative that the target participant did not already have a bachelor's degree (74% of those expressing interest).¹⁰

Nearly all of the individuals verbally expressing interest consented to participate in the project (26,162 individuals). Only after verbal consent did tax professionals treat participants differently based on randomly assigned group. Participants did not formally sign the consent form until the end of the interview, but only a very small number left before doing so. We found no

9. The dependent sample figures include both high school seniors, who are examined in this article, and participants who were high school sophomores or juniors, who will be examined in future, separate work. We are unable to distinguish between these age groups until after the office interview is completed so they are grouped together in the Table I figures.

10. The primary reasons 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 to complete the FAFSA. Among those who qualified, tax professionals during focus groups suggested that about half of those who expressed interest were initially attracted to the \$20 discount, and the other half were interested because they wanted more information about college.

TABLE I
 CONSENT, EXIT, AND PROCESSING RATES BY TREATMENT STATUS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
	Selection rates based on last two digits of SSN (%)	Initial screening qualification (number)	Expressed interest	Final qualification <i>Sample size (fraction who initially qualified [column (2)])</i>	Accepted and gave consent <i>initially</i>	Assignment to treatment or control group occurs	Finished office interview <i>Fraction who initially consented (column (5))</i>	Analysis sample with complete data <i>initially</i>
A. Dependent sample								
Control group	46	6,438	3,422 [0.531]	2,659 [0.413]	2,593 [0.403]	0.993	0.751	
FAFSA treatment	54	7,510	3,846 [0.512]	3,034 [0.404]	2,963 [0.395]	0.993	0.756	
and information-only groups								
Full sample	100	13,948	7,268 [0.521]	5,693 [0.408]	5,556 [0.398]	0.993	0.754	
<i>F</i> -test <i>p</i> -value (attrition rates are the same across samples)						0.798	0.640	

TABLE I
(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Selection rates based on last two digits of SSN (%)	Initial screening qualification (number)	Expressed interest	Final qualification <i>Sample size (fraction who initially qualified [column (2)])</i>	Accepted and gave consent	Assignment to treatment or control group occurs	Finished office interview <i>Fraction who initially consented (column [5])</i>
B. Independent sample							
Control group	46	25,216	12,988 [0.515]	9,440 [0.374]	9,388 [0.372]	0.990	0.763
FAFSA treatment group	46	25,493	13,284 [0.521]	9,654 [0.379]	9,620 [0.377]	0.992	0.777
Information-only treatment group	8	4,377	2,238 [0.511]	1,608 [0.367]	1,598 [0.365]	0.988	0.775
Full sample	100	55,086	28,510 [0.518]	20,702 [0.756]	20,606 [0.374]	0.991	0.770
<i>F</i> -test <i>p</i> -value (attrition rates are the same across samples)						0.141	0.059

Notes. The dependent sample figures includes both high school seniors and recent graduates, who are examined in this article, 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 article includes both those with and without prior college experience. To initially qualify for this study, families had to have an adjusted gross income 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"). Among the group that gave consent (column [5]), individuals were randomized into treatment and control groups based on the last two digits of SSN 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. For the data to be complete, a paper consent form had to be sent via mail to the central project office by the tax professional. The three analytic samples examined in this article are: 868 dependent children in grade 12 (after dropping high school juniors and sophomores), 9,228 independent students with no prior college experience, and 6,646 independent students with prior college experience (for a total of 13,874 independents, which equals 20,606 [column (5)] * 0.7770358 [column (7)]). The *F*-test tests that the attrition rates are equal across the samples.

significant difference by treatment status in attrition rates for finishing the office interview.

The last column in Table I 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 dependents, differences in consents received are not significant. For our sample of independents, differences are marginally significant at the 10% 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.¹¹ Nevertheless, to address the issue of selective participation further, we present results that treatment and control groups have similar means across a wide range of observable characteristics. 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.

During the experiment, the software we developed not only tracked completion of each question, it also prompted and reminded tax professionals what questions they should ask at each point during 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

11. In focus groups with tax professionals, they identified two main reasons that H&R Block's central processing center 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.

problem arose, we immediately integrated new procedures and training modules to accommodate special circumstances.

Of the 4,187 individuals from the dependent sample with completed data (from Table I, 0.7536 * 5,556), 868 were seniors in high school, and this is our main dependent sample (the others were high school juniors and sophomores). Our independent sample of 15,874 individuals (from Table I, 0.770358 * 20,606) is further separated into those without prior college experience (9,228) and those with prior college experience (6,646). We examine these two groups separately because they differ substantially in experience with the college application process and in the predicted impact of the treatments. Information on prior college experience was collected during the study's initial screening, prior to randomization. The information-only group is noticeably smaller as its 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.¹²

III.C. 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 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 the intervention. Thus, the DOE data are 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. Because 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

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

data.¹³ The NSC data allow us to observe college program registration across the United States but only at institutions covered by the NSC repository. The NSC is a nonprofit organization that provides national student degree and enrollment verification for schools, colleges, and employers; for our analysis, NSC covers 92.3% of all enrollments 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 colleges are the most underreported. The OBR data, which provide registration information on all public vocational colleges in Ohio, is a nice complement to the NSC by partially addressing this gap (but not North Carolina). Although the nation's largest for-profit colleges are included in the NSC sample, smaller for-profit colleges, particularly private vocational colleges, are underreported 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 the Online Appendix. They constitute 7.7% of all enrollments in Ohio and North Carolina.

IV. EMPIRICAL FRAMEWORK

IV.A. Data: Descriptive Statistics

In Table II, 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.¹⁴ As expected, observable mean

13. In the 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.

14. Tax professionals could not override the screen prompts that were dependent on treatment status, and they 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.

TABLE II
 MEAN CHARACTERISTICS OF CONTROL GROUP AND DIFFERENCES BETWEEN TREATMENT AND CONTROL MEAN CHARACTERISTICS

	Dependent participants			Independent participants with no prior college experience			Independent participants with prior college experience		
	(1) Control mean	(2) FAFSA treatment difference	(3) Info treatment difference	(4) Control mean	(5) FAFSA treatment difference	(6) Info treatment difference	(7) Control mean	(8) FAFSA treatment difference	(9) 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)	0.050 (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	0	0
Married				0.131	-0.002 (0.007)	-0.021 (0.013)	0.128	-0.005 (0.008)	-0.022 (0.015)
Single				0.800	0.006 (0.009)	0.019 (0.016)	0.803	0 (0.010)	0.031 (0.018)*

TABLE II
(CONTINUED)

	Dependent participants			Independent participants with no prior college experience			Independent participants with prior college experience		
	(1) Control mean	(2) FAFSA treatment difference	(3) Info treatment difference	(4) Control mean	(5) FAFSA treatment difference	(6) Info treatment difference	(7) Control mean	(8) FAFSA treatment difference	(9) Info treatment difference
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)	0.510	0.006 (0.011)	0.030 (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, were a veteran, or were an orphan. "Prior college experience" is defined from surveying participants.

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, more than 56% 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% of participants were white and about 38% of participants were black. Among the information-only treatment group, the proportion of white participants was higher and the proportion of black participants was lower, but these differences are not statistically significant at the 5% level. The average age of the dependent sample was about 17.7 years at the time of the interview across all three groups.¹⁵

Across the groups, about 85% to 88% 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. Although 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 5% 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. About 41% of parents reported that their children would be targeting a bachelor's degree, and 35% 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. Families' average incomes were about \$23,000 and 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

15. For the dependent participant sample, about 58–63% of participants in the respective treatment groups had fathers and/or mothers with a high school level of education. For mothers, 26–30% had completed some college and 16–19% 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.

groups. Hence, our failure to find differences is an affirmation of our randomization.

The rest of Table II 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 independents with and without prior college experience, as is evident from comparing both control group means. About 64% of participants with prior college experience were female, and about 58% of participants without prior college experience were female. Slightly more than 70% of independents without prior college experience were white, but for those with previous college experience the proportion was about 64%. Participants with previous college experience also had incomes that were about \$1,000 to \$1,500 more than those with no previous college experience.

IV.B. Empirical Strategy

Because the proposed treatments were assigned using randomization, simple comparisons of participants in the various groups 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 form). Effectively, this analysis simply compares mean outcomes between treatment and control groups.¹⁶

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

16. We test the robustness of the main estimates by reestimating the models with the inclusion of baseline covariates. These results are shown in the Online Appendix, and the main estimates are robust to including these controls.

from H&R Block or whether they were first mailed 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. Because 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 then 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, and the other half opted to have their application sent to them first.

V. RESULTS

V.A. Program Effects on FAFSA Submission

Table III 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). The results represent the model in equation (1); as shown in the Online Appendix, the main estimates are robust to the inclusion of baseline covariates.

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.¹⁷ Among dependents, 39.9% 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

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

TABLE III
SUMMARY OF THE RESULTS

	Outcome during first year following experiment		
	(1) Filed FAFSA (based on DOE data)	(2) Attended college (based on NSC and OBR data)	(3) Attended college and received Pell Grant (based on DOE data)
Dependent participants ($N=868$)			
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)
Independent participants, no prior college ($N=9,228$)			
Control group mean	0.161	0.095	0.111
FAFSA treatment effect	0.267 (0.009)***	0.015 (0.007)**	0.030 (0.007)***
Info treatment effect	-0.019 (0.014)	0.003 (0.012)	-0.016 (0.012)
Independent participants, some prior college ($N=6,646$)			
Control group mean	0.320	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 ordinary least squares). Robust standard errors in parentheses. 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% levels, respectively. In the Online Appendix, we present the same models with the inclusion of baseline covariates, and the main estimates are robust to including these controls.

(column [1]), which corresponds to a 40% increase (p -value $< .01$). The requirement that both parent and dependent sign the FAFSA explains why the filing rate was not even higher among the treated. The application first had to be first mailed to the dependent's household to be signed and then sent to the DOE. It is likely those more interested in college actually followed through with the process.

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% 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. Of the control group of independents without prior college experience, 16.1% 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 16.1% to 42.8%. 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% (not 100% due to the need for additional information that our call center was unable to obtain). For those who opted instead to first have their FAFSA sent to them, the filing rate was 16.5%. Of course, independents choosing the electronic option may have been more interested. Conditioning on self-reported interest in college, those opting to be mailed the paper FAFSA submitted their application to the DOE at a rate of 26.9%, which was about the same for those choosing electronic submission. 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%. This rate rose by 19.5 percentage points for the FAFSA treatment group, to 51.5%. The fraction filing among this treated group opting to file electronically was 84.2% compared to 15.8% 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.

V.B. Program Effects on College Enrollment and Pell Grant Receipt

Column (2) of Table III shows the estimated 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% among the control group to 42.3% among the treated, or a relative increase of about 23.7% ($p = .019$). 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% to 40.2% ($p = .002$). 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 (0.081/0.157). An explanation for these large effects may be that the FAFSA treatment for dependents involved mailing only complete or nearly complete FAFSAs only to households. FAFSAs were not actually filed unless applicants followed up by mailing these forms to the DOE.

Table III 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% ($p = .026$). The fraction of college students who received Pell Grants rose even more, from 11.1% to 14.1% ($p < .001$). 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.¹⁸ The higher estimated impact ($p = .057$) 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 subgroup estimates, however, are underpowered and imprecise.

We find no enrollment effects among independents with previous college experience, but we expected this, because many in

18. Although the DOE data are more likely to cover for-profit and vocational colleges eligible for federal financial aid, they will miss students who did not submit the FAFSA and qualify for the Pell Grant.

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 ($p = .101$). Conditioning on going to college, FAFSA filing among this group rose from 79.7% to 86.1% ($p = .001$), and Pell Grant receipt rose from 59.4% to 64.0% ($p = .093$).

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? (3) Does FAFSA treatment increase financial aid receipt for those already going to college? To address multiple testing concerns, we adopt a sequential approach, with two treatments (information-only and FAFSA) and two samples (dependents and independents without prior college experience). In regard to the first question, the information-only treatment shows no signs of successfully boosting applications for either sample. Because of this, we drop the information treatment case from the rest of our analysis and focus only on the FAFSA treatment relative to the control group. Because the FAFSA treatment increases the number of FAFSAs filed almost by definition, it is not surprising that the estimated p -values for these effects are less than .001 for all samples. For participants agreeing to have H&R Block file the form on their behalf, the effect is virtually automatic. For other cases, greater FAFSA filing merely confirms that participants who were mailed a complete or nearly complete FAFSA with a prepaid 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. We therefore do not believe that the FAFSA filing outcome should be considered together with the college enrollment outcome when considering spurious effects.

Turning to the second question of whether the FAFSA treatment increases college enrollment, we test effects for two samples (dependents and independents without prior college experience). The probability of finding at least one significant effect when in

fact there are none (the familywise error rate) is 9.8%, assuming a Type I error rate for each test of 5% ($1 - [1 - 0.95^2]$). The (relatively low-powered) Holm-Bonferroni method controls the familywise error rate when both or either null hypotheses are true. Applying this method, we reject that the estimated college enrollment effects are spurious (the lowest p -value of .019 is less than $\frac{0.05}{2}$ and the second lowest p -value of .026 is less than $\frac{0.05}{1}$).

Looking at the third question of whether the FAFSA treatment increases Pell Grant aid receipt, we cannot answer the question directly using the samples of dependents or independents without prior college experience because aid receipt depends on college enrollment. We can, however, look at the sample of independents with some prior college since we estimate (and expected) no enrollment effects. This sample provides some marginal evidence of aid receipt among the college-going. 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 .018.

V.C. Program Effects on Type of College Enrollment

Table IV 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 = .052) for the dependent sample, compared to 1.9 percentage points at private colleges (p -value = .226). Among independents without prior college, differences in means between the controls and treated only arise when looking at public college attendance. 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 admissions applications by a specific date in the late fall or early winter; however, the particularly 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 IV also shows that

TABLE IV
TREATMENT EFFECTS ON PATTERNS OF ATTENDANCE, FIRST YEAR FOLLOWING
EXPERIMENT

Dependent variable	Dependent participants (N = 788)		Independent participants with no prior college experience (N = 8,506)	
	(1) Control mean	(2) FAFSA treatment	(3) Control mean	(4) FAFSA treatment
Attended public college	0.294	0.065 (0.033)*	0.070	0.011 (0.006)*
Attended private college	0.040	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 ordinary least squares 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% levels, respectively.

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 as opposed to out-of-state enrollment.

V.D. Program Effects on Aid Amounts and Submission Date

In Table V, 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 III 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

TABLE V
TREATMENT EFFECTS ON AID RECEIPT AND FAFSA FILING, FIRST YEAR FOLLOWING
EXPERIMENT

Dependent variable	Dependent participants ($N = 788$)		Independent participants with no prior college experience ($N = 8,506$)	
	(1) Control mean	(2) FAFSA treatment	(3) Control mean	(4) FAFSA treatment
Attended college and received Pell Grant (based on DOE data; sample is not conditional on enrollment)	0.296	0.106 (0.034)***	0.111	0.030 (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 ordinary least squares 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% levels, respectively.

(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% more likely to receive loan aid, and independents are 13.9% more likely, although these effects are somewhat imprecise.

For many states and institutions, there are binding deadlines for applying for financial aid. In Table V, 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 two months earlier than the control group.

V.E. 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., Card 1995; Card and Lemieux 2001; Jepsen, Troske, and Coomes 2009; Carneiro and Lee 2011; Oreopoulos and Salvanes 2011; Hout 2012).

Though 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 for whether application assistance does more than nudge individuals into college who then quickly dropout within the first year. Tables VI and VII 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.¹⁹ Our NSC data include college registration outcomes through November 2010, almost three years after participants were invited to participate in the study while visiting H&R Block (January to April 2008). The first row in Table VI 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

19. Models with the inclusion of baseline covariates are shown in the Online Appendix. The results do not change.

TABLE VI
TREATMENT EFFECTS ON ENROLLMENT AND RETENTION DURING THREE YEARS FOLLOWING
EXPERIMENT

	Dependent participants ($N = 788$)		Independent participants with no prior college experience ($N = 8,506$)	
	(1) Control mean	(2) FAFSA treatment	(3) Control mean	(4) FAFSA treatment
Attended college within one year after experiment Apr 2008–Mar 2009	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	0.110	-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	0.280	0.080 (0.033)**	0.100	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 ordinary least squares 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% levels, respectively. In the Online Appendix, we present the same models with the inclusion of baseline covariates, and the main estimates are robust to including these controls.

(OBR data on enrollment after three years will not be available until 2013). Results are similar to Table III, both for the dependent sample and independent sample without prior college experience.

The third and fourth rows of Table VI 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 entered 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 significantly 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. The sixth row of Table VI shows that dependents are 8 percentage points more likely to persist compared to the control group's consecutive college enrollment rate of 28%. The higher persistence than initial enrollment effect seems odd, given that persistence is conditional on enrollment. If drop-out rates after the 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%.

One explanation is that virtually every dependent induced to enter college in the first year stayed on into the second, and virtually every dependent induced to enter college earlier would have dropped out otherwise (because 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. Though we only examine enrollment patterns for three years following the FAFSA intervention, our results are consistent with those of Adelman (2006) and suggest 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 and 0.03 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 helped attendees receive aid over multiple years. Some caution may be in order because 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 VII 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 ($p = .057$). This pattern, also evident in Table VI, 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 ($p = .168$) and 2.2 percentage points higher for independents in the treatment group ($p = .022$) compared to those among the controls. We also find significant FAFSA treatment effects on consecutive Pell Grant receipt. Dependents are 36% 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 experienced an 8 percentage point increase in college enrollment the following year. Independents without prior college experience

TABLE VII
TREATMENT EFFECTS ON PELL GRANT RECEIPT DURING THREE YEARS FOLLOWING
EXPERIMENT

	Dependent Participants ($N = 788$)		Independent participants with no prior college experience ($N = 8,506$)	
	(1) Control mean	(2) FAFSA treatment	(3) Control mean	(4) FAFSA treatment
Received Pell within one year after experiment Apr 2008–Mar 2009	0.296	0.106 (0.034)***	0.111	0.030 (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	0.150	-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 experi- ment Apr 2010–Dec 2010	0.513	0.049 (0.036)	0.271	0.022 (0.010)**
Received Pell for two con- secutive years Apr 2008– Dec 2011	0.280	0.101 (0.033)***	0.130	0.009 (0.007)
Total years received Pell Apr 2008–Dec 2011	0.967	0.230 (0.083)***	0.443	0.047 (0.018)***

Notes. Treatment effect estimates are from ordinary least squares 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% levels, respectively. In the Online Appendix, we present the same models with the inclusion of baseline covariates, and the main estimates are robust to including these controls.

saw a 2 percentage point increase. Although 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. Nontraditional 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 eight minutes, on average, and cost about \$3 per participant for tax professional training and time. Software installation, maintenance, and printing materials added roughly another \$15 per participant. The largest costs to the program were from call center support (\$30 per participant) and participation incentives (\$20 to participants and up to \$20 to tax professionals). These costs would likely fall significantly in a more automated and/or nonresearch setting. Even at \$88 per participant, this translates to a cost of about \$1,100 per dependent induced to enroll in college and \$5,833 per independent induced to enroll in college in the first year following the experiment. 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 induced to attend college, and approximately \$100 on average per independent or \$4,157 on average per independent. Over two years of college, this amounts to a total cost of about \$8,750 and \$14,150 for dependents and independents, respectively. Returns to college among those who enrolled as a result of the treatment 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 and transferring data directly from the tax return to the FAFSA makes the difference between some individuals going to college or not; however, it 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.

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 by having H&R Block submit electronically or from submitting a paper application instead.

The DOE 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) provide 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. Kincheloe and Brown (2005), for example, find that 49% 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% of unbanked individuals provided a referral letter and instructions to open up a bank account reported thinking they would follow through, but only 50% 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.

STANFORD UNIVERSITY SCHOOL OF EDUCATION AND NATIONAL
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HARVARD GRADUATE SCHOOL OF EDUCATION, NATIONAL
BUREAU OF ECONOMIC RESEARCH, AND NATIONAL CENTER
FOR POSTSECONDARY RESEARCH
UNIVERSITY OF TORONTO, CANADIAN INSTITUTE FOR
ADVANCED RESEARCH, AND NATIONAL BUREAU OF ECONOMIC
RESEARCH
NATIONAL BUREAU OF ECONOMIC RESEARCH

SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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