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Incentivizing STEM participation: Evidence from the SMART Grant Program

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Abstract

The U.S. National Science and Mathematics Access to Retain Talent (SMART) Grant program provided up to $8000 to high-achieving, low-income undergraduates majoring in STEM fields. We evaluate the effects of this financial incentive on college graduates' major fields and subsequent STEM workforce retention using nationally-representative survey data and a difference-in-differences quasi-experimental approach. The SMART Grant program significantly increased the probability that first-generation college graduates majored in STEM, by about 7 percentage points. However, this increase is almost entirely offset by affected STEM graduates' significantly lower STEM workforce retention. These program effects also appear to be concentrated among students whose parents had some college experience rather than those who were first in their families to attend college.

KEYWORDS

financial aid, higher education, post-college employment, STEM degrees, workforce

JEL CLASSIFICATION

I22, I28, J24, H52

1 | INTRODUCTION

Policymakers in the United States have sought to increase the number and diversity of graduates with degrees in science, technology, engineering, and mathematics (STEM) fields, to maintain national competitiveness and economic growth (U.S. House of Representatives, 2010; National Science Board, 2010). In principle, following the pipeline analogy for STEM education and workforce training, a supply-side subsidy that increases the number of STEM graduates should also increase the size of the STEM workforce. In practice, the influence of financial incentives in higher education decision-making is complex, with heterogeneous effects across demographic groups. Some undergraduates might respond to financial incentives by changing their major to a STEM field. More likely, these incentives would simply cause intended STEM majors to persist in STEM instead of switching into non-STEM fields. In any case, it is unclear whether such financially-incentivized STEM majors would enter STEM occupations at the same rate after graduation as their perhaps more intrinsically-motivated classmates.

This paper evaluates the impact of the U.S. Department of Education's Science and Mathematics Access to Retain Talent (SMART) Grant program, which provided Pell Grant-eligible undergraduates up to $2000 per semester for their junior and senior years, conditional on their maintaining a 3.0 GPA and majoring in an eligible field. We first demonstrate the impacts of the program on the probability that first-generation college graduates majored in a STEM field, then provide novel evidence of changes in affected STEM graduates' probability of working in STEM occupations.

To date, studies of the effectiveness of the SMART Grant program have found mixed results. Both Evans (2017) and Denning and Turley (2017) employ regression discontinuity (RD) designs using university administrative data. For Ohio's public university system, Evans (2017) finds no significant impact of the SMART Grant program on undergraduates' probability of selecting a SMART-Grant-eligible STEM major at the beginning of their junior year, even among the undergraduates who said at initial enrollment that they intended to major in STEM fields. For public universities in Texas, Denning and Turley (2017) do find a small but significant 3.08 percentage point ($p < .05$) increase in juniors' probability of majoring in SMART-eligible STEM fields, but no overall impact on students' probability of graduating with a SMART-eligible degree. On the other hand, for the one private institution that Denning and Turley (2017) study—Brigham Young University—the authors find a 10.2 percentage point increase in STEM majors, and a “marginally significant” increase of 6.6 percentage points on SMART-eligible degree completions.

These mixed results raise the questions: Why are students' responses different across these universities, and what can we conclude about the overall impact of this national policy, given these disparate local outcomes?

This paper extends the existing literature in four important ways. First, we examine national data rather than individual institutions' data to evaluate the SMART Grant program's effect on the probability that college graduates majored in a STEM field. Second, we address the program's longer-term employment outcomes. Specifically, we examine whether students thus incentivized to major in STEM fields are equally likely to enter and remain in the STEM workforce after graduation. Since broadening STEM workforce participation was the policy's ultimate goal, this gap in the extant literature is important to remedy. Third, we employ a different identification strategy than the prior literature, allowing for spillover effects of the program on major choices among students who might reasonably have expected to be eligible, even if they did not ultimately receive a SMART Grant. Finally, we focus on the program's impact among first-generation college graduates, to better evaluate the program's impact within the demographic groups that policymakers arguably intended to treat. The Great Recession changed the demographic composition of Pell Grant-eligible students while the SMART Grant program was in effect. Compared to Pell-eligible students in the pre-policy period, students who received SMART Grants tended to be from higher-income families with higher parental educational attainment. Thus, measuring the effects of the program among all students who were eligible for Pell Grants during this period may not provide representative estimates of the effects for the underrepresented populations that policymakers intended to treat.

We find that first-generation college graduates who received grant support were 7 percentage points more likely to have majored in STEM fields while the SMART Grant program was in effect. This increase is concentrated among first-generation college graduates who had at least one parent with some college experience, rather than among those who were first in their families to attend college. However, this increase is almost entirely offset by the affected STEM graduates' significantly lower STEM workforce retention.

The remainder of this paper proceeds as follows: Section 2 provides background on the SMART Grant program and further discusses the prior literature and evidence; Section 3 describes our data source, key variables, and controls; Section 4 explains our empirical strategy and presents evidence supporting the validity of the difference-in-difference model's identifying assumptions; Section 5 presents our results including sensitivity and robustness checks; and Section 6 concludes with a discussion of our results, limitations, and their broader policy implications.

2 | BACKGROUND AND PRIOR LITERATURE

The U.S. Department of Education's SMART Grant program ran from Fall 2006 through Summer 2011, providing eligible undergraduate students with up to \$2000 per semester, up to a maximum of \$8000 in total. Eligibility requirements evolved somewhat over time, but the core requirements were: (a) U.S. citizenship, (b) Pell Grant eligibility, (c) having declared a major in a designated STEM field or a critical foreign language, (d) full-time enrollment, (e) at least 3.0 cumulative GPA, (f) junior or senior standing, and (g) no prior bachelor's degree.

For a student to be designated Pell Grant eligible, they must first have chosen to apply for financial aid using the Free Application for Federal Student Aid (FAFSA). These students typically come from lower-income families. For example, throughout the SMART Grant program's term, Pell-eligible dependent students' median family income was under \$30,000. In academic year (AY) 2006–07, a student whose expected family contribution (EFC) based on FAFSA information was \$4050 or lower would have received a Pell Grant of up to \$4050 to offset their cost of attendance (U.S. Department of Education, 2008). Unfortunately, completing the FAFSA itself can be a considerable barrier, especially for dependent students who are first in their families to attend college (Bettinger et al., 2012; Dynarski & Scott-Clayton, 2006).

Relatedly, one advantage of the SMART Grant program's implementation was that students did not need to apply separately or otherwise opt-in. Any eligible student would receive the SMART award automatically in the semester(s) they were eligible, conditional on the requirements listed above. If students were unaware of the program before (or even after) receiving the benefit, the SMART Grant program would not have influenced their major choice per se. Instead, any real effects of the program we observe would be due to its subsidizing students' full-time enrollment, perhaps reducing students' need for outside work per Carruthers and

Ozek (2016). Thus, the program's main effect could be an increase in STEM-interested students' persistence in STEM majors and their likelihood of completing their bachelor's degrees.

Supporting this increased-persistence effect, Choy et al. (2010) find almost 10 percentage points higher persistence from junior to senior year for SMART Grant recipients versus other Pell Grant recipients (78% vs. 69%) in the early years of the program. Recent studies of other need-based scholarship programs *without* degree-field restrictions also support a possible STEM persistence effect. For example, Anderson et al. (2020) observe a modest increase in STEM degree completions and a reduction in time-to-degree for students in the private, need-based Wisconsin Scholars grant program. In addition, Castleman et al. (2018) demonstrate that Florida's earlier need-based grant program substantially increased eligible students' credit hours in STEM fields, despite that grant program having no explicit STEM major eligibility restriction. On the other hand, like Anderson et al. (2020), Sjoquist and Winters (2015b) find no evidence that state merit scholarships—awards with no major or income restrictions but with some minimum GPA requirement—increase bachelor's degree completions, overall. These latter results suggest we should anticipate that the SMART Grant program might have had little effect on graduation rates, instead resulting in a higher *share* of college graduates who completed STEM coursework and STEM majors. If this is the case, the full effects of the SMART Grant program may be adequately captured by estimating the change in the probability that a college graduate majored in STEM.

Beyond the persistence effects described above, it is not clear a priori whether the SMART Grant program should have substantially changed otherwise-eligible students' major declarations. Students from lower-income and first-generation-to-college families are more likely to enter college with the intent to major in pre-professional fields like engineering that they expect will lead to higher-earning, stable jobs after graduation (Trejo, 2016). On the other hand, if these students attended lower-performing high schools, they may be inadequately prepared for college-level math and science courses when they arrive on campus. Poor performance in the initial STEM gateway courses can be especially discouraging to first-generation, female, and racial or ethnic minority students, for various reasons including stereotype vulnerability (Astorne-Figari & Speer, 2019; Ost, 2010). Furthermore, at many institutions, the grade distributions in STEM courses—particularly in the 100- and 200-level gateway classes—are often substantially lower than for humanities and less-quantitative social science courses (Ahn et al., 2019; Rask, 2010). Since state and institution-based merit scholarships typically require students to maintain some minimum GPA but carry no major field restrictions, financially vulnerable students concerned with losing their *other* scholarship support may be more likely to switch out of STEM fields, consistent with Sjoquist and Winters' (2015a) finding.

From a methodological perspective, focusing solely on outcomes among students who appear to be eligible for SMART Grant awards based on their family income and cumulative undergraduate GPA is insufficient to assess the program's full and intended impacts. For example, both prior studies of the SMART Grant's impact mentioned above use the EFC as the running variable in an RD design, which necessarily narrows their studies' analytic window to students from families with incomes near the cutoff, and well above the median for Pell-eligible dependent students overall (Denning & Turley, 2017; Evans, 2017). In addition, Pell-eligible students are disproportionately female, historically underrepresented racial/ethnic minorities, and first-generation college students. As such, these are the demographic groups that policymakers arguably intended the SMART Grant program to treat. However, during the Great Recession (which occurred while the SMART Grant program was in effect), the demographic composition of Pell-eligible students substantially changed. In AY 2007–08, roughly 1 in 4 (27.6%) bachelor's degree-seeking undergraduates were Pell-eligible, but this share rose to 40% after the Great Recession (Ifill & Hufford, 2015). Many of these newly Pell-eligible students were continuing-generation students from families likely to have greater "college knowledge," including the ability to identify and avail themselves of new funding opportunities. Including students who experienced only temporary family income shocks in the "treated" group thus may misrepresent the effects of the program for the population that policymakers intended to treat.

Identifying treated and control groups based on students' cumulative GPA is similarly fraught. On the one hand, students who can maintain a 3.0 in non-STEM courses may not be able to maintain a 3.0 in STEM coursework, so including these near-eligible students in the treated group could contribute to measurement error. At the same time—as any teacher or advisor of undergraduates is well aware!—undergraduates cannot perfectly forecast their future course performance. Entering students may be overly optimistic about their ability to maintain a 3.0 in their junior and senior years, and thus might have been encouraged by the SMART Grant program to pursue STEM coursework, even if they ultimately end up below the GPA cut-off in their junior or senior year. For example, over 1 in 5 Pell Grant recipients who received SMART awards in their third year no longer met the eligibility requirements for a SMART Grant award in their fourth year, for reasons including not meeting the GPA requirement, dropping below full-time status, changing their major to an ineligible one, or not taking at least one course fulfilling the requirements of their major (Choy et al., 2011).

To address the limitations noted above, we take an intent-to-treat perspective that focuses on program effects among all first-generation college graduates who received grant or scholarship support. As we show in the next section, these students closely resemble the original Pell-eligible students on gender, institutional control, historically underrepresented racial/ethnic minority (URM) share, and STEM degree completions.

3 | DATA

We use data from the 2015 National Survey of College Graduates (NSCG). The NSCG is a nationally-representative survey of U.S. residents under age 76 who hold bachelor's or higher degrees, conducted for the National Science Foundation by the U.S. Census Bureau. In earlier years, the NSCG sampling frame focused on those who either (a) worked in STEM occupations, or (b) held a bachelor's or higher degree in a STEM or STEM-related field. Since 2010, however, each round of the NSCG adds new respondents with bachelor's or higher degrees drawn from the previous year's American Community Survey, thus providing a more representative sample of the U.S. resident college-educated workforce.

The NSCG data include detailed demographic and educational information for each respondent, including the type of institution they attended, their major(s), and their high school and college graduation years. In addition, since 2015, the NSCG has asked a series of detailed, binary (yes/no) questions regarding the respondent's sources of financial support while in school, and whether they attended community college en route to earning their bachelor's degree. The 2015 NSCG also captures cumulative student debt load at graduation, which allows us to descriptively compare whether SMART-eligible students who majored in STEM benefitted from reduced debt load. Finally, the NSCG includes a series of questions asking respondents to evaluate the relative importance of various job characteristics, including opportunities for advancement, job security, fringe benefits, geographic location, level of responsibility, degree of independence, intellectual challenge, and contribution to society. The question posed is, "When thinking about a job, how important is each of the following factors to you?" Because personal preferences and attitudes correlate with preferences over fields of study as well as occupations (Burn & Martell, 2020; Chen & Simpson, 2015; Humburg, 2017), we include indicators for extreme responses (i.e., "Very Important" or "Not Important at All") to each of these questions as covariates in our final variants of the model predicting STEM occupations.

3.1 | Analytic sample

We restrict the NSCG sample in several ways before conducting our analysis. First, because students needed to be enrolled at U.S. colleges or universities to be eligible for SMART Grants, we exclude respondents who earned their bachelor's degrees abroad or for whom institution classification information (Carnegie classification, and public or private control) is missing. Second, for several reasons including NSCG data limitations, the program's evolving eligibility requirements, and assuring validity of the difference-in-difference model's identifying assumptions, we include only native U.S. citizens in our sample. The NSCG does not ask naturalized U.S. citizens in what year they became citizens or legal permanent residents, so we cannot reliably infer whether these respondents were already citizens while enrolled in their bachelor's degree program. In addition, in the fourth and final years of the program, AY2009-10 and AY2010-11, eligibility was expanded to include legal permanent residents. This compositional change in the treated population (like the change in Pell-eligible students' backgrounds during the Great Recession) raises concern about validity of the difference-in-difference identification strategy if we include immigrants in the analysis. Specifically, individuals who immigrated to the U.S. as children may differ from native U.S. citizens on a variety of other important but unobservable characteristics that affect their probability of STEM degree completion and STEM workforce participation, such as hyper-selectivity and parental academic support or expectations (Thomas & Lonobile, 2021; Tran et al., 2019).

Respondents would generally only be eligible for SMART Grant awards if they were third- or fourth-year students while the program was in effect.[1] Because the NSCG data do not include the student's date of first enrollment in college, we impute time-period eligibility based on the respondent's high school and college graduation years. Benchmarking with the 2008 National Postsecondary Student Aid Study (NPSAS) data, we find that almost 80% of Pell-eligible graduating seniors who received SMART Grant awards in the previous academic year were under age 25. Furthermore, 96% of Pell-eligible graduating seniors under age 25 who had received SMART Grant awards in the previous academic year graduated from high school three to 6 years earlier. Similarly, in our NSCG data, 96.5% of graduates in the analytic sample described above graduated college three to 6 years after graduating from high school. So, to ensure greatest comparability between the treated and control groups, we further restrict our analytic sample to respondents who were under age 25 when they graduated from college, and who graduated high school three to 6 years prior to graduating from college. We also exclude a small number of respondents whose reported high school graduation year was less than 3 years prior to their reported college graduation year, including those with a high school graduation year later than their college graduation year, as these discrepancies raised concerns about data accuracy. The SMART Grant program ended Spring 2011, so students graduating after 2011 would not have

[1]Students in eligible five-year degree programs, for example in some fields of engineering, could also receive a SMART Grant award for the fifth year of their program.

benefitted from the program in their final year (and may not have received any support at all, if they were not yet juniors in AY2010-11). To avoid "forbidden comparisons" due to first-generation grant recipients switching out of treatment after the program ended, we exclude respondents who graduated after 2011.

Finally, as noted above, the SMART Grant program also supported "critical foreign language" majors, nominally for studying languages rarely studied by U.S. students. In fact, by the final year of the program, all foreign language and literature majors were eligible for SMART awards. Since our intent in this paper is explicitly to evaluate the impact of the SMART Grant program on STEM degrees and STEM workforce retention, to avoid confounding we exclude about 120 remaining graduates from the sample whose only major was in foreign languages and literatures.

3.2 | Outcome variables

We seek to identify whether the SMART Grant program successfully encouraged first-generation college students to pursue bachelor's degrees (and then choose jobs) in natural sciences, math and computational sciences, or engineering fields. Our first outcome variable is thus a binary indicator that takes on value 1 if the respondent majored in a SMART Grant-eligible STEM field, and zero otherwise. To identify whether each respondent's bachelor's degree field is SMART-eligible, we crosswalked each numeric major code in the NSCG data to its corresponding U.S. Department of Education Classification of Instructional Programs (CIP) code, then compared those CIP codes against those in the original list of SMART-eligible STEM fields. Appendix A lists the SMART-eligible STEM majors coded in our dataset.

Our second outcome variable is a binary indicator for employment in a STEM occupation. Our STEM occupations list follows the job titles provided in the U.S. Bureau of Labor Statistics' (BLS') *Detailed 2018 SOC Occupations Included in STEM*. In some cases, the NSCG defines occupation codes more narrowly than in the Standard Occupation Classification (SOC) codes. In other cases, where NSCG codes cover multiple SOCs, we use other NSCG variables to disambiguate the NSCG codes and identify STEM workers. For example, the BLS STEM SOCs identify medical scientists as STEM workers but exclude health practitioners from the primary STEM fields. NSCG variables capturing the respondent's primary work activities allow us to disambiguate respondents who self-identify as physicians, but whose primary work activities include basic or applied medical sciences research. Similarly, while the BLS STEM SOCs include foresters, in the NSCG data this occupation is grouped with farmers and fishers. To address this discrepancy, we code respondents in the NSCG "Farmers, Foresters, and Fishermen" occupation group as STEM workers only if the respondent also indicates that their job requires the "technical expertise of a bachelor's degree or higher in engineering, computer science, math, or the natural sciences," in addition to their naming basic or applied research among their primary work activities.

We evaluate the sensitivity of our results to this STEM occupation definition using several alternative outcome variables, for example: including individuals who, at the time of the survey, were full-time graduate students earning degrees in STEM fields; including respondents enrolled in medical school or employed in medical or allied health occupations; and including quantitative social scientists, secondary school teachers, and others who hold STEM degrees and indicate their job required bachelor's-level STEM expertise per above, while excluding a small subset of computing and information technology workers like web content developers and help desk workers who assert their work does *not* require such technical expertise, and who furthermore have earned no STEM degree. Since per Choy et al. (2011) the life sciences were the most common major fields for SMART Grant awards and many life sciences graduates pursue health-related work after graduation, an increase in these majors would not necessarily translate to an increase in STEM workforce participation per the BLS definition.

3.3 | Explanatory variables

For this analysis, we identify respondents in the intended-treated demographic group by combining information on parents' highest educational attainment and the respondent's sources of financial support for college. Because natal family income, Pell Grant eligibility, and cumulative GPA are not in the NSCG data, we cannot directly identify graduates who received SMART Grant awards. Instead, we cast a wider net, evaluating whether the probability of majoring in a STEM field and working in a STEM occupation changed among first-generation graduates who received grant support, overall. Though first-generation status is an imperfect proxy for Pell Grant eligibility, low income and first-generation status do substantially overlap (Redford & Hoyer, 2017). For example, using data from the 2008 National Postsecondary Student Aid Study (NPSAS), we find that 78% of native U.S. citizen Pell Grant recipients are first-generation college students. Furthermore, among native U.S. citizen first-generation grant recipients in the 2008 NPSAS, almost two-thirds (65%) had Pell Grants. In contrast, less than 15% of continuing-generation students received Pell Grant awards.

The NSCG asks separately for the highest educational attainment of the respondent's father or male guardian and their mother or female guardian. For both questions, one option is "not applicable." We define our first-generation indicator to take on value 1 if the respondent has no parent that is known to have earned a bachelor's or higher degree, and 0 if at least one parent is noted to have earned a bachelor's or higher degree. Following prior literature, we refer to this latter group as "continuing-generation" graduates. For some models, we further divide first-generation college graduates into two groups: first-to-attend, and first-to-complete. We classify respondents as first-to-attend if the highest educational attainment reported for their parents or guardians is a high school diploma or less than high school. We classify respondents as first-to-complete if the highest educational attainment reported for their parents or guardians is "some college, vocational, or trade school (including 2-year degrees)."

We also require that the individual report using "tuition waivers, fellowships, grants, or scholarships" to finance their education, as those who did *not* receive such support could not have received a SMART Grant. Using the 2008 NPSAS data, we confirmed that this combination—first generation college graduate and grant recipient—is highly predictive of Pell Grant eligibility. For greatest comparability, we first restricted the 2008 NPSAS sample to native U.S. citizen graduating seniors under age 24 as of December 31, 2007. Then, we estimated a logistic regression model predicting receipt of a Pell Grant solely as a function of binary indicators for first-generation status and total grants of \$400 or more. The test statistic for this model's overall fit is 58,555 ($p < .0001$). Furthermore, controlling for having received any grants totaling \$400 or more, the odds of receiving a Pell Grant are 3.8 times higher ($p < .0001$) for first-generation college students than for continuing-generation students.

Because the NSCG data do not include the student's date of first enrollment in college, we impute each respondent's eligibility cohort based on their high school and college graduation years. For example, respondents who graduated high school in 2004 and entered college immediately the following fall would have been third-year students in Fall 2006, when the SMART program began. If those same respondents graduated college in Spring 2008 or later, they would have been exposed to the program in both their third and fourth years.

There is good reason to expect heterogeneity in the program's impacts for respondents who were in their fourth year (or in their final year, if graduating within 3 years) when it came into effect, compared to those who could have received support in both their third and fourth years. We therefore split the treated cohort into two groups: partially-exposed respondents who were already fourth-year students or in their third but final year in AY2006-07 when the program began, and fully-exposed respondents who could have received support in both their third and their fourth years. These cohort indicators are distinct from the graduation-year calendar-time fixed effects, which we also include in our model.

Figure 1 compares race and gender, institutional control, and proportion majoring in SMART-eligible STEM fields for native U.S. citizen Pell Grant recipients from the 2008 NPSAS who were graduating seniors in AY2007-08 and under age 24 as of December 31, 2007, versus first-generation grant recipients from our 2015 NSCG analytic sample who graduated while the SMART Grant program was in effect. We observe that the shares of female students and URM males, the shares who attended public institutions, and the shares graduating with SMART-eligible STEM majors closely match across these two datasets. However, we do note a discrepancy between the two datasets for URM females. This discrepancy appears to be driven by the relatively lower representation of Black women in our 2015 NSCG analytic sample versus the 2008 NPSAS.

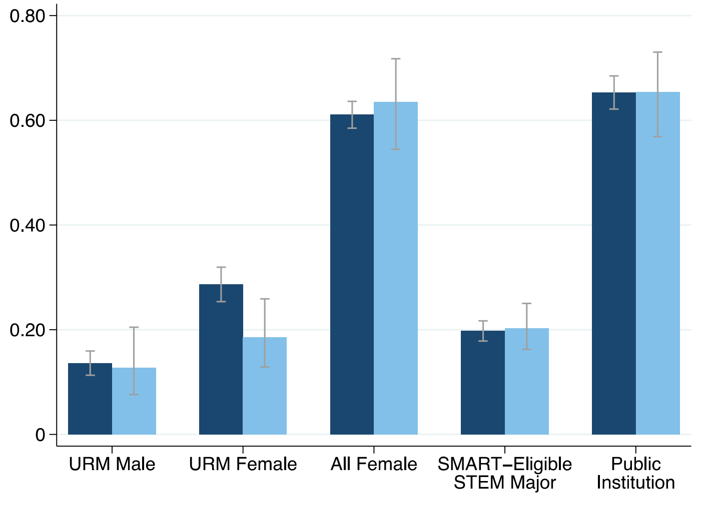

FIGURE 1 Comparison of selected characteristics of Pell Grant recipients in the 2008 NPSAS with first-generation grant recipients in the 2015 NSCG. This Figure benchmarks shares of historically underrepresented minority (URM) males and females, all females, SMART Grant eligible STEM majors, and those attending public institutions, for native U.S. citizen respondents under age 25 at graduation. The navy bar on the left of each pair depicts the share among Pell Grant recipients who were graduating seniors in AY2007-08, from the 2008 NPSAS data, estimated using the U.S. Department of Education National Center for Education Statistics' PowerStats DataLab. The light blue bar on the right of each pair depicts the share among first-generation grant recipients in our 2015 NSCG analytic sample who graduated while the SMART Grant program was in full effect, 2008–2011. The gray cap markers indicate the 95% confidence interval for each estimate. Survey weights are applied in both cases to retrieve nationally-representative estimates. [Color figure can be viewed at wileyonlinelibrary.com]

3.4 | Descriptive analysis

As a preliminary test of the difference-in-difference model's parallel-trends assumption, we begin by using just the pre-policy subset of our sample—respondents who graduated between 2002 and 2006—to estimate a probit model predicting our SMART Grant eligible STEM major outcome variable. For this descriptive exercise, we include only our binary indicators for first-generation status, grant receipt, and their interaction, and a separate time trend for each of the four groups: first-generation grant recipients, first-generation non-grant recipients, continuing-generation grant recipients, and continuing-generation non-grant recipients. We then estimate the derivatives with respect to graduation year separately for each group. Finally, we test for significant differences between our "intended-eligible" first-generation grant recipients' pre-policy trend and the trends for each of the other three groups. For first-generation non-grant recipients and continuing-generation grant recipients, we find no significant difference in the pre-policy time trends: $-.002$, $p = .73$, and $-.005$, $p = .527$, respectively. However, for continuing-generation non-grant recipients, the difference is both larger in magnitude and highly significant: $-.011$, $p = .004$. Due to this descriptive evidence of violation of the parallel-trends assumption, we exclude continuing-generation non-grant recipients from our analytic sample.

Table 1 presents descriptive statistics for variables of interest from the NSCG, including a comparison of the full analytic sample with those identified as part of the "intended-eligible" demographic based on their first-generation status and receipt of grant support and the control group, both before and after the program's implementation. These statistics reveal three key points. First, among the intended-eligibles, demographic and educational history characteristics are quite similar before and after the SMART Grant program came into effect. The only statistically significant differences ($p < .10$) we observe over time are an increase in the share of Black, non-Hispanic males from 1.6% to 6.6%, and a decrease in the share of multiracial and other URM females from 2.6% to 0.5%. Evidence also suggests that the share of STEM graduates employed in STEM occupations after graduation fell by almost 18% (that is, by roughly 10 percentage points, $p = .136$), and the share of Hispanic males of Mexican origin may have increased (point estimates 1.5% vs. 3.6%, $p = .146$). Cumulative student debt load at graduation significantly increased for both the intended-eligible and control populations, by roughly 26%–29% for both groups, with no significant difference over time across the two groups in their growth in debt load.

Both the intended-eligible and control group's shares of first-to-complete graduates fell, but this difference over time was only statistically significant ($p < .05$) in the control group. As with cumulative debt load changes, we find no significant difference in this change over time across groups. That is, the time-variation in both debt load and first-to-complete status appears to be group-invariant.

Although the decreases in shares of Black, non-Hispanic males and Hispanic males of Mexican origin over time in the control group are not statistically significant, juxtaposed with the apparent increases in these groups' respective shares among intended-eligibles we find a significant difference in the differences over time for each of these demographic groups. We observe a similar significant difference for multiracial and other URM females, where shares appear to increase in the control group, but significantly decrease in the intended-eligible group. In addition, the share of the control group that attended community college before completing their bachelor's degree fell from 19.0% to 8.5% ($p < .01$), even while the share of intended-eligibles attending community college appeared to increase. None of the differences in share attending public universities is statistically significant at conventional levels. Finally, while the control group's probability of employment in a STEM occupation conditional on earning a STEM degree remained roughly constant over time (point estimate increased by less than 10%, from

TABLE 1 Descriptive statistics for National Survey of College Graduates: analytic sample

		Intended-eligibles			Control group			
Variables	**Analytic sample**	**Enrolled pre-policy period**	**SMART program in full effect**	**Sig. Diff.?**	**Enrolled pre-policy period**	**SMART program in full effect**	**Sig. Diff.?**	**Sig. Diff. In diff.?**
First generation: first-to-complete (≥1 parent w/some college)	31.4%	64.1%	58.2%		20.9%	15.5%	**	
First generation: first-to-attend (no parent w/ college experience)	21.0%	35.9%	41.8%		14.2%	11.8%		
Graduated with SMART-eligible STEM major	20.7%	18.2%	20.3%		22.0%	20.9%		
Employed in STEM occupation after graduation | STEM degree	48.2%	54.2%	44.6%	$^{\Psi}$.136	44.5%	48.8%		*
Female, all races and ethnicities	60.6%	67.1%	63.5%		57.7%	60.8%		
White, non-Hispanic male	30.6%	23.7%	22.2%		34.5%	32.1%		
White, non-Hispanic female	44.4%	41.5%	41.9%		46.1%	45.4%		
Black, non-Hispanic male	2.4%	1.6%	6.6%	*	2.4%	1.5%		**
Black, non-Hispanic female	4.9%	7.8%	6.6%		3.7%	4.0%		
Asian, non-Hispanic male	1.8%	1.6%	1.5%		1.7%	1.7%		
Asian, non-Hispanic female	3.1%	2.1%	3.1%		2.5%	3.8%		
Hispanic, Mexican origin male	1.8%	1.5%	3.6%	$^{\Psi}$.146	1.8%	1.1%		*
Hispanic, Mexican origin female	2.6%	3.7%	3.2%		1.5%	3.2%		
Hispanic, non-Mexican origin male	1.7%	2.5%	1.1%		1.6%	1.3%		
Hispanic, non-Mexican origin female	2.6%	9.4%	8.2%		3.1%	3.1%		
Multiracial or other historically URM male	1.1%	2.1%	1.5%		0.3%	1.4%		

(Continues)

TABLE 1 (Continued)

Variables	Analytic sample	Intended-eligibles: Enrolled pre-policy period	Intended-eligibles: SMART program in full effect	Intended-eligibles: Sig. Diff.?	Control group: Enrolled pre-policy period	Control group: SMART program in full effect	Control group: Sig. Diff.?	Sig. Diff. In diff.?
Multiracial or other historically URM female	1.2%	2.6%	0.5%	**	0.7%	1.3%		**
Attended a public university	65.6%	70.8%	65.4%		63.0%	66.4%		$^{\Psi}$.130
Cumulative student debt load at graduation (est. mean)	$21,676	$21,270	$27,664	**	$18,701	$23,657	***	
Attended community college prior to bachelor's degree	15.5%	16.5%	22.7%		19.0%	8.5%	***	***
Observations	15,992	2219	1846		5060	4462		

Note: All observations come from the 2015 National Survey of College Graduates, including U.S. citizens who graduated between 2002 and 2011 at age 25 or younger, after 3 or more years in college. The analytic sample removes continuing-generation graduates who received no grants or scholarships to finance their undergraduate education. To be classified as "intended-eligible" a respondent must have reported both that they were a first-generation college graduate (that is, no parent had earned a bachelor's or higher degree), and that they received a grant or scholarship to finance their undergraduate education. Respondents who were seniors/4th-year students when the SMART Program began are included in the full analytic sample, but excluded from the treated vs. control breakdown.

$^{\Psi}p < .15$.

$*p < .10$. $**p < .05$. $***p < .01$.

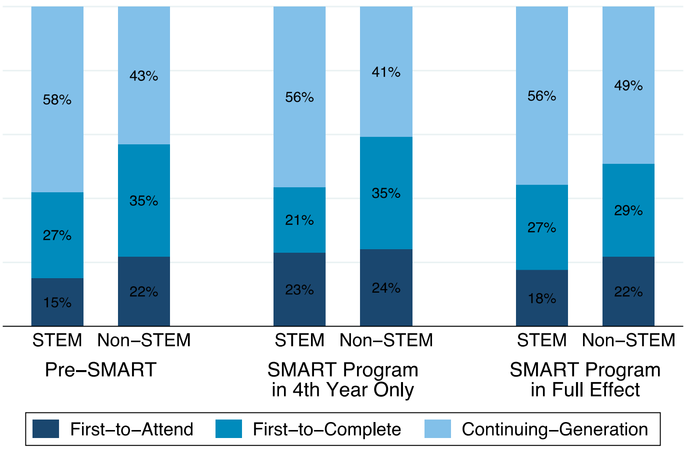

FIGURE 2 Distribution of first-to-attend, first-to-complete, and continuing-generation students by degree and exposure to SMART Grant program. This Figure shows, for each cohort in the analytic sample, the shares of SMART-eligible STEM majors and non-STEM majors who were, respectively, first-to-attend, first-to-complete, or continuing-generation. Note that continuing-generation graduates are only included in the analytic sample if they received grant support. [Color figure can be viewed at wileyonlinelibrary.com]

44.5 to 48.8 percentage points), this juxtaposed with the apparent decline for intended-eligibles noted above yields a statistically significant ($p < .10$) relative decline in STEM workforce retention among intended-eligibles.

Figure 2 compares the shares of graduates by first-generation status who majored in a SMART-eligible STEM field versus in a non-STEM field, by exposure cohort. In the pre-SMART period, the shares of first-to-attend and first-to-complete students are significantly higher ($p < .01$) among non-STEM graduates than among STEM graduates. For graduates with only 1 year of exposure to the SMART Grant program, the share of STEM graduates who were first-to-attend increased, suggesting the windfall gain may have increased STEM persistence for this cohort. For the later cohort, however, we see no evidence of a difference in the shares of STEM and non-STEM graduates who were first-to-attend, compared to the pre-SMART period. The share of non-STEM graduates who were first-to-complete remained constant at 35% for the first cohort in the program. The first-to-complete group's smaller share among STEM graduates in this cohort is due to the relatively larger number of first-to-attend graduates in this cohort who completed their degrees. Finally, recall that Table 1 showed a statistically significant decrease overall in the share of graduates who were first-to-complete when the SMART Grant program was in full effect. Thus, while the share of STEM majors who were first-to-complete college graduates appears unchanged relative to the pre-policy period, the share of first-to-complete college graduates who majored in STEM fields increased.

Finally, we must note some key limitations of the NSCG data. These data provide a rich source for understanding college graduates' labor market choices. However, these data do not include students' cumulative GPA, SAT/ACT scores, or any other direct measure of academic performance in college or earlier. Moreover, even in the previous years of NSCG data available under restricted-use license, detailed geographic information including the respondent's high school state and time-of-survey state are suppressed. In the 2015 NSCG, our only geographic indicator is Census region. Thus, while we can and do include region-by-year fixed effects to control for regional and national economic forces, we cannot control for state or local differences in economic conditions or states' concurrent merit scholarship programs. Nonetheless, because this is a nationally representative sample, any changes over time in state-level scholarship programs would only bias our impact of the effect if the program changes (a) coincide with the SMART Grant program's dates, and (b) occurred across multiple states, particularly those with disproportionately larger populations of first-generation college students. In fact, the strong state merit programs that existed during our study period were already well-established prior to the SMART Grant program's beginning (Sjoquist & Winters, 2015b).

4 | IDENTIFICATION AND EMPIRICAL STRATEGY

To evaluate the effect of the SMART Grant program, we employ probit estimation and a difference-in-difference modeling approach. This section introduces our empirical model and provides evidence to support the difference-in-difference model's identifying assumptions.

4.1 | Econometric model

We estimate:

$$\Pr(STEM_i = 1) = \Phi\Big(\alpha_0 + \alpha_1 Grant + \varphi_f FirstGen_f + \delta_c SMART_c + \theta_c Grant{*}SMART_c + \rho_{f,c} FirstGen_f{*}Grant{*}SMART_c + \kappa_{k,t} + \tau_{r,t} + \gamma_g + \lambda_g \mathrm{t} + \mathbf{X}_{\boldsymbol{i}}\beta + u_i\Big) \quad (1)$$

In this equation, *STEM* equals 1 if individual i majored in a SMART-eligible STEM field, or alternatively—for our second outcome studied—if the graduate is working in a STEM occupation, and 0 otherwise. *Grant* is a binary indicator that equals 1 if the individual reports financing their undergraduate education, wholly or in part, with a grant or scholarship. *SMART* is a two-element vector, where $c = 1$ indicates the individual is in the cohort for whom the SMART program was in effect in both their third and fourth years ("SMART Program in Full Effect"), and $c = 2$ indicates the individual's cohort was partially treated, with the SMART Program in effect in only their fourth/senior year. $FirstGen_f$ is either a binary indicator variable for first-generation college graduate, or a vector with two dummy variables representing, respectively, first-to-attend ($f = 1$) and first-to-complete ($f = 2$) status. The $Grant * SMART_c$ interactions allow for the possibility that all students who received grants might have had different rates of STEM degree completion while the SMART Grant program was in effect, for reasons unrelated to the program. The main policy effect is then given by $\rho_{f,c}$, the coefficient or coefficients on the three-term interaction of first-generation status, grant receipt, and cohort. Note that because continuing-generation non-grant-recipients are excluded from the sample, we cannot include an additional interaction term for $FirstGen_f * Grant$ when only one first-generation indicator is included in the model. However, when we split first-generation status into two groups—first-to-attend and first-to-complete—we do include one additional interaction term to allow for baseline differences in outcomes among first-generation grant recipients who are first-to-attend versus those who are first-to-complete.

Graduation year by region fixed effects (shown as the vector $\tau_{r,t}$ in Equation (1)) control for historical differences in job market prospects, differences in regional economic opportunities, and the changing opportunity cost of attending college, for example during the Great Recession.

The $\kappa_{c,t}$ vector of institution-type-by-year fixed effects controls for the respondent's bachelor's-degree institution type (as per Carnegie Classification) as a measure of institutional prestige and research focus, while also accounting for differences in the impacts of the Great Recession on funding at public versus private institutions.

The γ_g vector contains indicators for female gender, race and ethnicity (including: Asian and biracial Asian/white; Black, non-Hispanic; Hispanic, Mexican origin; Hispanic, non-Mexican origin; Other multiracial/multiethnic or other; and white, non-Hispanic), as well as their interactions. In models predicting SMART-eligible STEM major, we also include time trends $\lambda_g t$ for each race/ethnicity-by-gender group. These time trends account for possible secular differences in enrollment trends by gender and race/ethnicity over time, for example differences in enrollment trends for Black women versus for white non-Hispanic men.

Additional controls (included in X) include a binary indicator for whether the individual attended community college prior to attending a 4-year institution, and in models predicting STEM occupation, X also includes indicators for marital status, whether (if married) the respondent's spouse works, presence of a child under age 6 in the home, and interactions of these three indicators with female gender. Finally, some model variants predicting STEM occupation include indicators for job-related preferences (as described in section 3), and models predicting STEM occupation among STEM graduates include STEM major-by-year fixed effects which control for differences in baseline attrition rates across majors, and differences over time in employment opportunities across major fields.

4.2 | Validating the difference-in-difference modeling assumptions

As Wing et al. (2018) discuss, for estimates from difference-in-difference models to be valid, two key assumptions must hold. First, *within* each group (respectively, the treated group and the control group), any unmeasured factors besides the treatment that affect the outcome variable should be time-invariant. If one or both groups experience statistically and practically significant changes in composition after the policy goes into effect, we must consider whether that change in composition may be responsible for the observed changes in outcome. Second, in the counterfactual case (if no policy change was implemented), both groups' outcomes must share a common time trend. So, if there is a significant change in composition *within* a group that could impact that group's propensity towards majoring in STEM fields—for example, if we see a higher proportion of male students graduating while the STEM program is in effect—there should still be no significant difference *between* groups in that rate of change over time. Thus it is important to evaluate whether the differences-in-differences we observed in Table 1 are likely to impact our model's results.

Table 2 explores the relationships between the observed differences in Table 1 and our first outcome, graduating with a STEM major, before and after the SMART program came into effect. For example, in Table 1 we see the intended-eligibles' shares of Black, non-Hispanic and Hispanic of Mexican origin males increased while the SMART grant program was in effect, so we first investigate whether URM male graduates tend disproportionately to major in STEM fields. In Table 2, we see that in the pre-policy period, URM male first-generation grant recipients were about 2.7 times more likely to have majored in STEM than in a non-STEM field. However, while the SMART grant program was in effect, even while URM males' share among intended-eligible graduates increased, their probability of graduating with a STEM major declined to about parity. Thus, any increase we observe in intended-eligibles' STEM majors is

TABLE 2 Demographic characteristics of first-generation grant recipients

	Non-STEM majors			STEM majors			
Variables	**Enrolled pre-policy period**	**SMART program in full effect**	**Sig. Diff.?**	**Enrolled pre-policy period**	**SMART program in full effect**	**Sig. Diff.?**	**Sig. Diff. STEM vs. non-STEM, pre-SMART?**
Historically underrepresented minority (URM) male	5.8%	13.0%	*	15.9%	11.5%		***
Historically underrepresented minority (URM) female	25.7%	20.4%		13.1%	11.1%		**
Attended a public university	65.7%	66.1%		63.4%	66.0%		
Attended community college prior to bachelor's degree	19.3%	13.1%	**	14.2%	12.1%		*
Cumulative student debt load at graduation (est. mean)	$19,571	$25,872	***	$18,942	$21,092		
Observations	1228	1018		991	828		

Note: All observations come from the 2015 National Survey of College Graduates, including U.S. citizens who graduated between 2002 and 2011 at age 25 or younger, after 3–6 years in college. The analytic sample removes continuing-generation graduates who received no grants or scholarships to finance their undergraduate education. To be classified as "intended- eligible" a respondent must have reported both that they were a first-generation college graduate (that is, no parent had earned a bachelor's or higher degree), and that they received a grant or scholarship to finance their undergraduate education.

*$p < .10$. **$p < .05$. ***$p < .01$.

unlikely to be due to the increasing share of URM male graduates, overall. The other key difference observed in Table 1—a relative increase in community college attendance among intended-eligibles while the SMART grant program was in effect—would normally be associated with lower (rather than higher) probability that a graduate majored in STEM, so unobserved attributes related to community college attendance also are unlikely to explain any difference in our outcome variable.

To further test the identifying parallel-trends assumption, we estimated a model allowing different pre-policy trends in STEM degree completion rates for all groups by *Grant* and $FirstGen_f$ status, including continuing-generation non-grant recipients. Recognizing concerns raised by Roth et al. (2022) and others, we first evaluated whether the parallel trends assumption holds without conditioning on any observed covariates. Through this process, we found no significant differences across groups' pre-trends among respondents graduating before the SMART Grant program came into effect, except among continuing-generation students who did *not* receive grant or scholarship aid. The difference in pre-trends for continuing-generation non-grant recipients versus first-generation grant recipients, however, was highly significant ($p < .004$), and intuitively continuing-generation non-grant recipients are least relevant as a counterfactual or control. Therefore, we omit continuing-generation grant recipients from the analyses that follow. With continuing-generation non-grant-recipients excluded, we find no evidence of any pretrend in the sample: a test of joint significance for the groups' trend terms fails to reject the null ($p = .48$), and the statistical tests of differences in the trend coefficients for first-generation grant recipients versus first-generation non-grant recipients and continuing-generation grant recipients respectively are both very small and statistically insignificant (less than .003 in absolute value, and $p > .44$ in both cases).

While the unconditional test above avoids some problems, it may still be underpowered, especially when the time series is short and there is substantial year-to-year volatility (Roth et al., 2022; Wing et al., 2018). We therefore also provide graphical evidence to assess the parallel trends assumption conditioning on covariates and to provide a "placebo" test for significant differences between first-generation grant recipients and the combined control group before the policy came into effect. Figure 3 shows two event-study plots, each showing the estimated difference, by graduation year, between first-generation grant recipients and the control group in their probability of having majored in a SMART-eligible STEM field, controlling for baseline (pre-policy) differences in the average rates across groups. The estimates depicted in this Figure are from probit estimation of the full model shown in Equation (1), but instead of including one or two post-policy indicators ($SMART_c$) in the model, we interact our treatment indicators, $FirstGen_f$*Grant*, with individual graduation-year fixed effects. The results depicted in Figure 3 are average marginal effects (with their 90% confidence intervals) for these interaction terms. If first-generation grant recipients and our control group share a common pre-policy trend, then the <u>non</u>-interacted year fixed effects will be sufficient to describe <u>both</u> groups' year-on-year changes in the probability of earning a STEM major in the pre-policy period. On the other hand, if the $FirstGen_f$*Grant*-interacted year fixed effects became statistically significant prior to the policy or demonstrate a clear trend away from zero in the pre-policy period, this would suggest the parallel-trends assumption does not hold.

For first-to-complete graduates (top panel of Figure 3), we observe little deviation from zero through 2007, supporting the common-trend assumption. We also observe a strong break in trend for respondents who graduated in 2008, which includes the first cohort with full 2 years' exposure to the SMART Grant program. This higher probability of STEM degrees remains essentially constant through the end of the program in 2011. For graduates who were first-to-

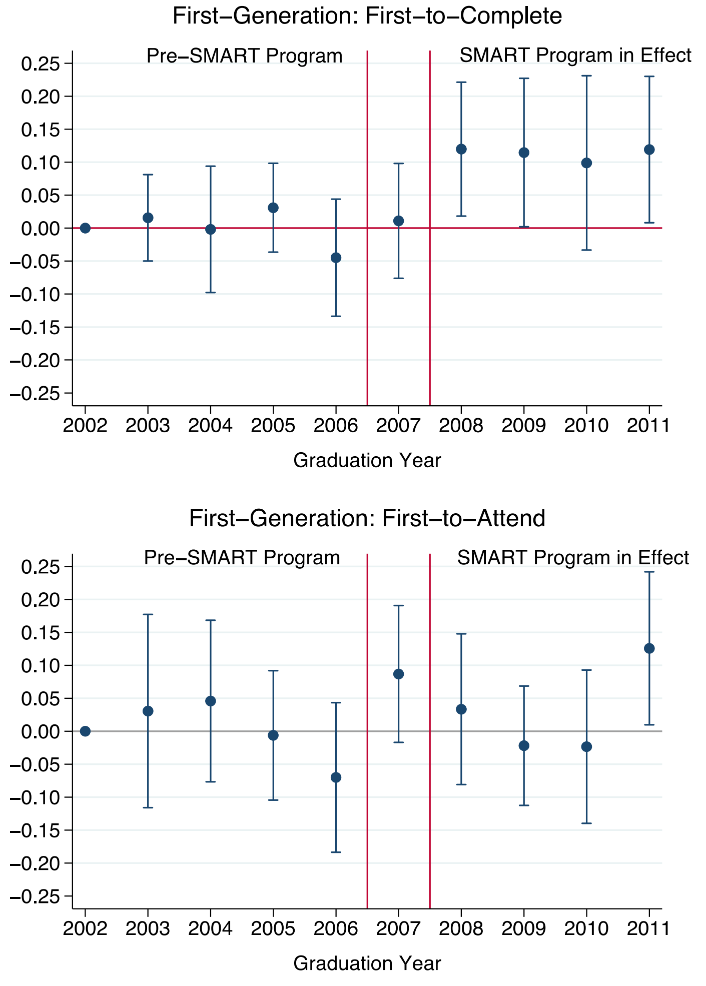

FIGURE 3 Event study for probability of STEM major, by first-generation status [Color figure can be viewed at wileyonlinelibrary.com]

attend (bottom panel of Figure 3) there is similarly no statistically significant variation prior to the policy period, and there is also no evidence that first-to-attend grant recipients' probability of STEM degree trended upward before the policy came into effect. We note that the first-to-attend grant recipients' estimates are generally much less precise than those for first-to-complete grant recipients, especially during the pre-policy period, which suggests some caution should be taken in interpreting results for the first-to-attend group.

5 | RESULTS

Tables 3 and 4 display average marginal effects from probit estimation of the difference-in-differences models predicting, respectively, STEM degree completion and STEM workforce participation. For the latter outcome, we first examine the SMART Grant program's overall impact on college graduates' STEM workforce participation, then evaluate whether the apparent lack of impact on STEM workforce participation we observe is attributable to higher attrition rates among STEM graduates exposed to the program. Recognizing the issues described by Ai and Norton (2003), we use Stata's margins command to obtain correctly calculated average marginal effects, including for all of the interaction terms.

5.1 | STEM major completion

Table 3 Model 1 reports results for all first-generation grant recipients combined. The SMART Grant program is associated with a 7 percentage point ($p = .014$) increase in the probability that a first-generation grant recipient majored in a STEM field. However, we find no significant impact overall when the program first came into effect, for graduates who were in their fourth (or third and final) year. This result is robust to our choice of estimation approach and inclusion of covariates. Linear regression estimation with the same covariates as in Table 3 column 1 results in a coefficient of .070, that is, 7.0 percentage points ($p = .013$, see Appendix C). Alternative specifications, for example including only year fixed effects and the difference-in-difference terms or removing the gender-by-race/ethnicity time trends, result in similar and significant point estimates (see Appendix B). These checks reassure us that our result is not due to model overparameterization.

The second column of Table 3 splits first-generation grant recipients into two groups, first-to-attend and first-to-complete. Here, we estimate that first-to-complete graduates had 8.8 percentage points ($p < .004$) higher probability of completing a STEM degree than would otherwise be expected, due to the SMART Grant program. However, we observe no significant effect ($p = .97$) of the program for students who were first in their family to attend college. As previously noted, this heterogeneity in response may be attributable to differences in students' "college knowledge"—students with a parent who attended college, even without graduating, may be better able to avail themselves of financial aid opportunities.

Finally, given that life sciences were the most popular field for SMART Grant awardees, we estimated a supplemental multinomial logistic regression predicting graduates' broad field—computer science or engineering, life sciences, and mathematics or physical sciences—with non-STEM fields as the reference group. Results from this regression are presented in Appendix D. In brief, controlling for demographics and other covariates as in Table 3, we find first-to-complete grant recipients' odds of having majored in life sciences versus non-STEM fields more than doubled while the SMART Grant program was in full effect, and their odds of having majored in computer science and engineering fields were 1.5 times higher as well. We observe no significant change in major choices among first-to-attend college graduates.

5.2 | STEM workforce participation

Table 4 shows results from probit models predicting our second outcome, STEM occupation. The model specification and covariates are very similar to those in Table 3 column (1), with the following exceptions: we remove the community college indicator and demographic (race/ethnicity-by-gender) time trends, and we include indicators for marital status (and, if married, whether the respondent's spouse works), as well as an indicator for the presence of a child under age 6 in the home, and the interactions of each of these with female gender.

TABLE 3 Effects of the SMART Grant program on probability that a graduate majored in SMART-eligible STEM field

	(1)	(2)
First-generation	0.0698**	
* Grant * SMART program in effect	(0.0293)	
First-generation: parent w/ some college		0.0883***
* Grant * SMART program in effect		(0.0303)
First-generation: first to attend college		0.0014
* Grant * SMART program in effect		(0.0455)
First-generation	0.0041	
* Grant * SMART program in effect 4th year only	(0.0467)	
First-generation: parent w/ some college		−0.0115
* Grant * SMART program in effect 4th year only		(0.0438)
First-generation: First to attend college		0.0054
* Grant * SMART program in effect 4th year only		(0.0719)
First-generation	−0.0328**	
	(0.0155)	
First-generation: parent w/some college		−0.0852**
		(0.0357)
First-generation: first to attend college		−0.1242***
		(0.0337)
Received grant support	0.0656***	0.0307**
	(0.0209)	(0.0141)
Graduation year X institution type fixed effects	Y	Y
Graduation year X geographic region fixed effects	Y	Y
Demographic characteristics	Y	Y
Race-ethnicity X gender time trends	Y	Y

Note: Average marginal effects from survey-weighted probit estimation predicting probability of a college graduate having majored in a SMART-eligible STEM field. Heteroskedasticity-robust standard errors clustered on first-generation-statusXgrantXgraduation-year are presented in parentheses below each estimate. Data are from the 2015 National Survey of College Graduates, with analytic panel restricted to 15,992 U.S. citizens who graduated from college between 2002 and 2011, who graduated at age 25 or younger, and whose college graduation year was at least 3 years after their high school graduation year. Sample excludes continuing-generation non-grant recipients. Models also include race/ethnicity-by-gender fixed effects per demographic groups shown in Table 1 and an indicator for attending community college prior to bachelor's degree program, indicators for bachelor's institution type (Carnegie Classification and public versus private control) and geographic region each interacted with graduation year, and separate time trends for each race/ethnicity-by-gender group.
***$p < 0.01$. **$p < 0.05$.

Among respondents in the full analytic sample who are in the labor force or attending graduate school, in Table 4 column 1 we see no evidence that the SMART program increased the probability that first-generation grant recipients worked in STEM occupations or enrolled in STEM graduate programs, overall. The model in Table 4 column 2 restricts the sample to respondents who were employed at the time of the survey and predicts STEM occupation per

TABLE 4 Effects of SMART Grant program on probability of scientific workforce participation

	(1)	(2)	(3)	(4)	(5)	(6)
First-generation	0.0037	0.0070	−0.0749*	−0.0887*	−0.0972**	−0.0706*
* Grant * SMART program in effect	(0.0303)	(0.0315)	(0.0419)	(0.0469)	(0.0454)	(0.0424)
First-generation	0.0225	0.0300	0.0594	0.0424	0.0498	0.1097**
* Grant * SMART program 4th year only	(0.0260)	(0.0274)	(0.0413)	(0.0527)	(0.0531)	(0.0467)
Observations	15,357	14,830	7851	7365	7365	7365
Preferences over job characteristics	N	N	N	N	Y	Y
STEM Major X graduation year fixed effects	N	N	Y	Y	Y	Y
Geographic region X grad. year fixed effects	Y	Y	Y	Y	Y	Y
Institution type fixed effects	Y	Y	Y	Y	Y	Y
Demographic characteristics	Y	Y	Y	Y	Y	Y

Note: Average marginal effects from survey-weighted probit estimation predicting working in a STEM occupation. Heteroskedasticity-robust standard errors clustered on first-generation-statusXgrantXgraduation-year groups are presented in parentheses below each estimate. Data are from the 2015 National Survey of College Graduates; model (1) restricts the analytic sample described in the Table 2 notes to respondents who are currently in graduate school or in the labor force and predicts either STEM occupation or current enrollment in a STEM graduate program. Model (2) includes only those respondents who were employed at the time of the survey. Models (3)–(6) restrict the sample to respondents who graduated with a SMART Grant eligible STEM major, with model (3) otherwise the same as model (1), and models (4)–(6) including only those respondents who were employed at the time of the survey. Model (6) expands the STEM occupation outcome to include health professions.

$^{**}p < 0.05$. $^{*}p < 0.1$.

the BLS' definition, and again we find no significant impact of the program overall on graduates' STEM workforce participation.

Table 4 columns (3)–(6) restrict the sample to respondents who graduated with SMART-eligible STEM degrees, to examine their occupation choices. These models include major field by graduation year fixed effects, to account for baseline differences in STEM attrition rates across major fields and year-to-year differences in the employment opportunities new graduates faced in each field. As in column 1, the model in column 3 includes individuals in the labor force or in graduate school, and predicts STEM workforce participation including attending graduate school in a STEM field. We see that the probability of STEM workforce participation by this measure fell by 7.5 percentage points ($p = .074$) in the cohort with 2 years (full) exposure to the program. Restricting the sample and outcome further (as in column 2), in column 4 we find that the SMART Grant program is associated with an 8.9 percentage point decrease ($p = .059$) in probability that a STEM major worked in a STEM occupation after graduation. These results indicate that, on the margin, individuals who were incentivized to complete STEM majors due to the SMART Grant program were less likely to enter or remain in STEM occupations after graduation. Thus, the lack of any evidence of the SMART Grant program on STEM workforce participation we note in the first two models cannot be simply attributed to lack of power to detect an effect. Rather, graduates in the likely-eligible group seem to have had lower STEM workforce attachment.

The model in Table 4 column (5) considers one hypothesis to explain this greater attrition among STEM graduates. If individuals incentivized by the SMART grant program to earn STEM degrees during this time had job-related preferences inconsistent either with the characteristics of STEM occupations or the attitudes of STEM occupations' incumbents, accounting for these differences in preferences should reduce the magnitude of the effect on STEM workforce participation. Of these job characteristics, only a few are significant predictors of STEM workforce participation after conditioning on degree field. All else equal, STEM graduates are more likely to work in STEM occupations if they view their job's intellectual challenge as very important (13.5 p.p., $p < .0001$), and if they view job security or the job's location as unimportant (29.0 p.p., $p < .0001$, and 31.0 p.p., $p < .0001$). On the other hand, STEM graduates are much less likely to work in STEM occupations if they view their job's contribution to society as very important (−10.6 p.p., $p < .0001$). However, after including these controls for job-related preferences, we find the point estimate only grows larger: the SMART Grant program is associated with a 9.7 percentage point decline ($p < .033$) in STEM workforce participation among STEM graduates. Thus, differences in job-related preferences do not present a plausible explanation for the observed gap in workforce retention, except to the extent that they influence which STEM major an individual chooses.

This brings us to another possible explanation for the higher attrition we observe: the high share of SMART grant awards that supported life sciences majors. All else equal, graduates who majored in computer science and engineering have the highest probability of working in STEM occupations after graduation: about 3 out of 4 remain in STEM. Similarly, over half of chemistry and physical sciences graduates remain in STEM. However, only 36%–42% of life sciences graduates (along with graduates in mathematics and statistics) work in STEM occupations after graduation, and women and URMs have higher propensity than white non-Hispanic males towards life sciences majors. As Jiang (2021) notes, gendered segregation across STEM majors and a definition of STEM occupations that excludes health sciences together account for a significant share of the gender gap in STEM workforce attrition. However, because our models predicting STEM occupations among STEM graduates already control for gender, race and ethnicity, the graduate's major field, and major-by-graduation-year fixed effects, for this explanation to hold there would need to be a significant increase in the STEM workforce attrition rate among intended-eligible life sciences majors while the program is in effect, above and beyond any change in attrition that control-group life sciences majors may have experienced.

To investigate this possibility, the model presented in Table 3 column (6) uses an expanded outcome variable that includes individuals working in medical and allied health (STEM-H) occupations. We continue to restrict the sample to respondents who graduated with SMART-eligible STEM degrees and who are currently employed. Here, we see STEM-H workforce attrition is only 7.1 p.p. ($p = .096$) for individuals exposed to the full effects of the program. Second, among individuals who only had an unexpected windfall gain in their fourth year, it appears the probability of STEM-H workforce participation significantly increased. That is, to the extent that unexpected grant support in AY 2006–07 increased STEM degree completion in among first-generation college graduates, these individuals were more likely to proceed into health professions than into other STEM occupations. Re-estimating that model allowing different effects for first-to-attend versus first-to-complete (results in Appendix C), we find positive point estimates for both groups, but the larger and statistically significant impact is among graduates who were first-to-attend: an 11.6 p.p. ($p = .029$) increase in probability of STEM-H workforce participation.

Our final model (results shown in Appendix E) is almost identical to that presented in Table 4 column (4), except that it allows all of our key terms' coefficients to differ across three broadly-defined major fields: computer science and engineering, life sciences, or physical sciences and mathematics. Among STEM graduates with majors in life sciences, at baseline we find that first-generation graduates are more likely than continuing-generation graduates to work in STEM occupations, by about 13 percentage points ($p < .05$). However, while the SMART Grant program was in full effect, the probability of a STEM occupation among first-generation life sciences graduates fell by 22 percentage points ($p < .001$). On the other hand, first-generation grant recipients who majored in computer science or engineering and who benefitted from a windfall gain in their senior year were significantly *more* likely to work in STEM occupations.

5.3 | Sensitivity and robustness checks

To test robustness of the results to our model specification and estimation approach, we began by re-estimating our original probit models from Table 3, excluding all covariates and including only the difference-in-difference terms and graduation-year fixed effects. Results for this and subsequent models described in this section are provided in Appendices B and C. For the base model, we estimate a 6.1 percentage point ($p < .05$) increase in probability of having majored in a STEM field. Broken out by first-to-attend and first-to-complete status, we estimate a 9.6 percentage point ($p < .001$) increase in probability of having majored in a STEM field for first-to-complete graduates, with no significant effect (point estimate 0.0042) for those first-to-attend. Subsequent models adding additional covariates yield estimates ranging from 5.9 to 7.0 percentage points for the overall effect, and 8.4 to 9.6 percentage points (with $p < .01$ in nearly all cases) for first-to-complete graduates. The point estimates for first-to-attend graduates range from 0.4 to −0.6 percentage points, and none are statistically significant.

We also investigated the sensitivity of our Table 3 results to our broader analytic decisions regarding choice of control group and use of probit versus linear regression estimation. We obtain very similar overall results when using only continuing-generation grant recipients as the control group (excluding first-generation non-grant recipients) and when using a linear probability model instead of probit. Furthermore, inclusion of individual trend terms for each FirstGen X Grant group simply drives our point estimates for the overall and first-to-complete effects higher.

We also re-estimated all the models shown in Table 4 as linear probability models with the same covariates and we find quantitatively similar and qualitatively identical results in all cases. We also find very similar results when we expand the definition of a STEM occupation to include respondents in secondary school teaching, managerial, or other professions who said their work required technical expertise at a bachelor's or higher level in a STEM field, and excluding computing help desk and web content developers who said their work required no such expertise (see Appendix C, column 5). However, when we exclude first-generation non-grant recipients from the control group (Appendix C, column 6), this causes our estimate of first-to-attend graduates' attrition rate to increase. This seems to be driven by the fact that, prior to the SMART Grant program, first-to-attend grant-recipient STEM graduates had relatively higher probability of STEM workforce retention, overall, compared to all other groups in our analytic sample. During the SMART Grant program, attrition rose among first-to-attend graduates regardless of grant receipt. Comparing first-to-attend grant recipients to continuing-generation grant recipients thus may overstate the change that occurred within this group during the SMART Grant program.

6 | DISCUSSION

Using a difference-in-difference quasi-experimental approach and nationally representative data, we find that the SMART Grant program significantly increased the probability that first-generation native U.S. citizen college graduates majored in STEM, but only among those with at least one parent who had some college experience. For these first-to-complete graduates, all else equal, the probability of having majored in a STEM degree increased by 8.8 percentage points, a 52% increase over baseline. Most notably, the odds of a first-generation college graduate majoring in life sciences instead of a non-STEM field more than doubled while the SMART Grant program was in full effect. Along these lines, Choy et al. (2011) report that life sciences were the most common field for SMART Grant awards. However, this increase in STEM degree completion was significantly offset by new STEM graduates' occupational choices, such that we find no net effect of the program on STEM workforce participation. Life sciences majors tend to have a higher attrition rate than other majors in any case, partly due to their choosing medical and health professions not traditionally included in STEM. However, among life sciences graduates who experienced the full effect of the SMART Grant program—i.e., those for whom the SMART Grant was in effect in both their third and fourth years—we find that STEM workforce attrition significantly increased, and only a small fraction of that change is explained by graduates choosing medical and health professions.

On the other hand, we also find evidence that the unexpected windfall gain for fourth-year/senior students in the first year of the SMART Grant program significantly increased the probability that first-generation college graduates majored in computer science and engineering. Furthermore, these graduates had higher probability of working in a STEM occupation after graduation than would otherwise be expected. This suggests that, for students with intrinsic interest in STEM fields, financial support can play an important role in broadening participation. Unfortunately, it also appears that simply offering financial incentives to students for majoring in STEM fields may not yield the intended outcome of increased STEM workforce participation. Students thus incentivized may declare STEM majors and complete STEM coursework, and may even complete STEM degrees, but still have little interest in working in a STEM occupation after graduation.

Prior to the SMART program, only 18% of graduates who would have been eligible for a SMART grant based on their demographic characteristics were graduating with STEM majors. If the program's true effect was (per our initial estimate) a 7 percentage point increase in STEM majors, that would imply a 38% increase versus expectation in STEM degree completions for the eligible population, conditional on completing a degree. This effect may seem extraordinarily large compared to those reported in previous literature, but the magnitudes are not directly comparable. For example, Denning and Turley (2017)'s outcome variable was coded 1 if the student completed any SMART-eligible degree, and 0 if the student completed a non-SMART-eligible degree *or no degree at all*. Since their denominator is larger, their estimated effect size is necessarily smaller. Considering that less than half of Pell-eligible first-time freshmen complete a bachelor's degree within 6 years of matriculating, a 7 percentage point increase in STEM majors among eligible *graduates* would correspond to a roughly 3.5 percentage point increase in STEM degree completions among eligible *students*, overall. This back-of-the-envelope calculation suggests our national results for all institutions are indeed quite similar to (and split the difference between) estimates for the private and public institutions that Denning and Turley (2017) reported.

Our study does have limitations. First, though cumulative GPA was an eligibility requirement for the program, it is unobserved in our national data. Some students included in the intended-eligible group for this analysis may not have been close enough to the GPA cutoff to perceive themselves as potentially eligible. Second, our analysis relies on imputed time-based eligibility, which contributes to measurement error. Due to the strict eligibility constraints we imposed, this misclassification would primarily cause eligible (treated) students to be counted as ineligible (controls), increasing the noise around our estimates. Finally, our analysis is conditional on degree completion: to be in the NSCG dataset, an individual must have completed a bachelor's or higher degree. This conditionality obscures any increase in probability of degree completion, overall.

Despite these limitations and caveats, we note that these results fit comfortably in and reinforce a growing body of research on the financial aid process as it impacts lower-income and first-generation college students. Studies reveal that the financial aid process is opaque and confusing for students and families, especially for low income and first generation students who often lack "college knowledge" (Avery & Kane, 2004; Engle, 2007). Recent work by Toutkoushian et al. (2021) further emphasizes the differences in outcomes for first-generation students, depending on whether they are first-to-attend or first-to-complete college. Providing information and guidance is especially critical for these students; research has shown that such information and guidance alters students' behavior, making them, for example, more likely to know about and take advantage of grant programs (Bettinger et al., 2012; Engle, 2007; Toutkoushian et al., 2021). Along these lines, prior evidence also indicates that students whose parents did have college experience were also more likely to know about the SMART Grant program (Choy et al., 2010).

This paper presents evidence that direct financial incentives to low income and other underrepresented groups *can* be an effective method of increasing STEM majors, but our results also support the U.S. Government Accountability Office's assessment, which argued that future programs must be more widely advertised and economically-disadvantaged students and families must have sufficient support to navigate the federal financial aid process, for such programs to achieve greater success. Finally, incentivizing students to major in STEM fields may not, on its own, achieve policymakers' aim of increasing STEM workforce participation among individuals from underrepresented groups. More research is needed to better understand why STEM graduates opt out of working in STEM occupations, and what can be done to improve their retention.

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APPENDIX A: SMART eligible majors in the national survey of college graduates data

Aerospace/aeronautical/astronautical engineering	Geology
Agricultural engineering	Geophysical and geological engineering
Animal sciences	Industrial and manufacturing engineering
Applied mathematics	Industrial production technologies
Architectural engineering	Information services and systems
Astronomy and astrophysics	Materials engineering, incl. Ceramics and textiles
Atmospheric sciences and meteorology	Mathematics, general
Biochemistry and biophysics	Mechanical engineering
Bioengineering and biomedical engineering	Mechanical engineering-related technologies
Biology, general	Metallurgical engineering
Botany	Microbiological sciences and immunology
Cell and molecular biology	Mining and minerals engineering
Chemical engineering	Naval architecture and marine engineering
Chemistry, except biochemistry	Nuclear engineering
Civil engineering	Oceanography
Computer and information sciences	Operations research
Computer and systems engineering	OTHER agricultural sciences
Computer programming	OTHER biological sciences
Computer science	OTHER computer and information sciences
Computer systems analysis	OTHER engineering
Data processing	OTHER engineering-related technologies
Earth sciences	OTHER mathematics
Ecology	OTHER physical sciences
Electrical and electronic technologies	Petroleum engineering
Electrical and communications engineering	Pharmacology, human and animal
Engineering sciences, mechanics and physics	Physics
Engineering, general	Physiology and pathology, human and animal
Environmental engineering	Plant sciences
Genetics, animal and plant	Statistics
Geological sciences, other	Zoology, general

Note: This table contains all STEM majors considered by the Department of Education as SMART-eligible in 2006 that are also found in our data set, the 2015 National Survey of College Graduates. For a full list of eligible majors, including those not included in our data set, see Choy et al., 2010.

APPENDIX B: Tests for sensitivity to model saturation

	(1)	(2)	(3)	(4)	(5)	(6)
First-generation	0.0614**	0.0619*	0.0593**	0.0619**	0.0678**	0.0698**
* Grant * SMART program in effect	(0.0308)	(0.0329)	(0.0300)	(0.0290)	(0.0295)	(0.0293)
Model AIC	6,146,020	5,849,500	5,611,841	5,416,920	5,393,259	5,391,397
First-generation: parent w/ some college	0.0958***	0.0945**	0.0851***	0.0843***	0.0855***	0.0883***
* Grant * SMART program in effect	(0.0305)	(0.0317)	(0.0307)	(0.0291)	(0.0306)	(0.0303)
First-generation: no parent attended college	0.0042	−0.0058	−0.0064	−0.0039	−0.0012	0.0014
* Grant * SMART program in effect	(0.0450)	(0.0494)	(0.0473)	(0.0459)	(0.0458)	(0.0455)
Model AIC	6,135,392	5,835,876	5,598,403	5,404,782	5,380,583	5,378,820
Year fixed effects	Y	Y	Y	Y	Y	Y
Demographic characteristics	N	Y	Y	Y	Y	Y
Graduation year X institution type fixed effects	N	N	Y	Y	Y	Y
Graduation year X geographic region fixed effects	N	N	N	Y	Y	Y
Gender X race-ethnicity time trends	N	N	N	N	Y	Y
Attended community college	N	N	N	N	N	Y

Note: Average marginal effects from survey-weighted probit estimation predicting STEM major completion among U.S. citizen graduates. Heteroskedasticity-robust standard errors clustered on first-generation-statusXgrantXgraduation-year groups are presented in parentheses below each estimate. Data are from the 2015 National Survey of College Graduates, with 15,992 observations on U.S. citizens who graduated college under age 25 and within three to 6 years of graduating from high school, forgraduation years 2002–2011.

** $p < 0.01$, $p < 0.05$, $p < 0.1$.

APPENDIX C: Additional robustness checks

	Panel A: SMART-eligible STEM majors			*Panel B: STEM occupations*		
	Control: Continuing generation w/ Grant only	**With group-specific time trends**	**Linear probability model**	**STEM-H occupations with breakout by FirstGen**	**STEM Occs expanded, removing IT support**	**Control: Continuing generation w/ Grant only**
First-generation	0.0632**	0.1228**	0.0688**	−0.0706*	−0.0974**	−0.0813*
* Grant * SMART program in effect	(0.0269)	(0.0418)	(0.0282)	(0.0424)	(0.0400)	(0.0418)
First-generation	−0.0082	0.0397	−0.0056	0.1097**	−0.0001	0.0508
* Grant * SMART Program in 4th year only	(0.0510)	(0.0388)	(0.0486)	(0.0467)	(0.0468)	(0.0514)
First-generation: parent w/some college	0.0924***	0.1385***	0.0808***	−0.0439	−0.0958**	−0.0526
* Grant * SMART program in full effect	(0.0284)	(0.0467)	(0.0229)	(0.0569)	(0.0472)	(0.0480)
First-generation: no parent attended college	0.0094	0.0496	0.0059	−0.0930	−0.0933	−0.1282**
* Grant * SMART program in full effect	(0.0408)	0.0635	(0.0325)	(0.0606)	(0.0674)	(0.0611)
First-generation: parent w/some college	−0.0200	0.0162	−0.0063	0.0695	0.0656	0.0986**
* Grant * SMART program in 4th year only	(0.0390)	(0.0387)	(0.0301)	(0.0525)	(0.0429)	(0.0456)

	Panel A: SMART-eligible STEM majors			*Panel B: STEM occupations*		
	Control: Continuing generation w/ Grant only	**With group-specific time trends**	**Linear probability model**	**STEM-H occupations with breakout by FirstGen**	**STEM Occs expanded, removing IT support**	**Control: Continuing generation w/ Grant only**
First-generation: no parent attended college	−0.0064	0.0285	−0.0061	0.1157**	−0.0659	−0.0094
* Grant * SMART program in 4th year only	(0.0777)	(0.0620)	(0.0586)	(0.0532)	(0.0731)	(0.0725)
Observations	13,125	15,992	15,992	7365	7911	6279

Note: Panel A repeats models estimated in Table 3, with the following changes: column (1) excludes first-generation non-grant recipients, using only continuing-generation grant recipients as the control group; column (2) allows different time trends for each First Generation X Grant Recipient group; and column (3) uses a linear probability model instead of probit estimation. Panel B column (1) breaks out results from the model shown in Table 4 column (6) into separate effects for first-to-attend and first-to-complete. Panel B column (2) re-estimates the model shown in Table 4 column (3) with an expanded definition of STEM occupations including quantitative social scientists, secondary school teachers, and other STEM graduates who indicate their job requires bachelor's or higher STEM technical expertise, including individuals who are not currently working but whose last job was a STEM occupation, but excluding allied health professions. Panel B column (3) re-estimates the model shown in Table 4 column (5) excluding first-generation non-grant recipients and using only continuing-generation grant recipients as the control.

***$p < 0.01$, ** $p < 0.05$, *$p < 0.1$.

APPENDIX D: Relative risk ratios for STEM degree field

	Computer Science and Engineering	Life sciences	Math and Physical Sciences
First-generation: parent w/some college	1.549*	2.208***	1.547
* Grant * SMART program in effect	(0.411)	(0.596)	(0.831)
First-generation: first to attend college	0.4917	1.055	1.479
* Grant * SMART program in effect	(0.221)	(0.437)	(0.603)
First-generation: parent w/some college	2.806**	0.862	1.014
	(1.254)	(0.356)	(0.447)
First-generation: first to attend college	1.812**	0.647	0.873
	(0.525)	(0.195)	(0.267)
Received grant support	5.440***	1.361	2.359**
	(1.960)	(0.473)	(0.916)

Note: Results from a single multinomial logistic regression predicting broad field of degree, with non-STEM fields as the reference category. Models also include indicators (not shown) for: female gender, race/ethnicity, and their interactions; cohort (SMART program in effect, 4th year only) and its interaction with the grant support indicator; community college attendance; 9 geographic regions; 8 institution categories (Carnegie classification and public versus private control); graduation year; and the 4th-year-only difference-in-difference interaction terms.

***$p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

APPENDIX E: Differences across major fields in probability of a STEM occupation among STEM graduates

	Computer Science and Engineering	Life sciences	Math and Physical Sciences
First-generation	−0.0327	−0.2237***	−0.0157
* Grant * SMART program in effect	(0.0591)	(0.0840)	(0.1267)
First-generation	0.1905***	−0.0795	0.1500
* Grant * SMART program in 4th year only	(0.0666)	(0.1433)	(0.1226)
First-generation	0.0128	0.1288**	−0.0573
	(0.0344)	(0.0583)	(0.0932)
Received grant support	0.0374	0.0449	0.0130
	(0.0535)	(0.0575)	(0.0920)
Female	−0.0929***	−0.1011***	−0.1106***
	(0.0276)	(0.0305)	(0.0313)

Note: Average marginal effects from a single probit model predicting STEM occupation, using the same analytic sample and including the same covariates as used in Table 4 column 4.
***$p < 0.01$, ** $p < 0.05$, * $p < 0.1$.