ESSAYS ON ECONOMICS OF HIGHER EDUCATION

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by

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The undersigned, appointed by the dean of the Graduate School, have examined the dissertation entitled

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and hereby certify that, in their opinion, it is worthy of acceptance.

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ESSAYS ON ECONOMICS OF HIGHER EDUCATION

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ABSTRACT

This dissertation consists of three chapters. In Chapter 1, We use rich administrative microdata from Missouri to examine the potential to expand and diversify the production of STEM degrees at universities by tapping into the population of community college students. We find that the scope for expansion is modest, even at an upper bound, because most community college students have academic qualifications that make them unlikely to succeed in a STEM field at a university. We also find there is almost no scope for community college students to improve the racial/ethnic diversity of four-year STEM degree recipients. We conclude that it will be challenging to expand and diversify STEM degree production at universities with interventions targeted toward community college students.

In Chapter 2, I extend the analysis of Qian and Koedel (2020) using the Educational Longitudinal Survey of 2002 from the National Center of Education Statistics. Using these data, I examine the potential to expand and diversify the production of STEM degrees at universities by tapping into the population of two-year college students. Consistent with Qian and Koedel (2020), I find that although the number of two-year college students is

large, only a small fraction of these students possess academic qualifications that suggest they would succeed in a STEM field at a four-year university. Therefore, policies targeting two-year college students can only increase STEM-degree production by a small amount. I also find no evidence that such policies can improve female or minority representation in STEM fields, with the possible exception of Hispanic students, for whom my estimates cannot rule out a modest positive effect on representation.

In Chapter 3, I use a recent data panel spanning the years 2001-2017 to study the effect of local-area unemployment on postsecondary enrollment and degree completion. My analysis extends the literature in several ways, most notably by (a) incorporating data well into the recent economic recovery from the Great Recession, (b) using improved (more accurate) measures of postsecondary enrollment, and (c) accounting for the attenuating effect of measurement error in calculated unemployment rates. Like in previous research, I find that postsecondary enrollment is countercyclical. I further show that the countercyclical enrollment pattern is concentrated among students in two-year and sub-two-year degree programs. There is suggestive evidence that men are more elastic than women in their enrollment response to unemployment, and unemployment rates have effect on degree completion, but my estimates are too imprecise to draw strong inference.

Chapter 1

The Potential for Community College Students to Expand and Diversify University Degree Production in STEM Fields I: Evidence from Missouri Administrative Data

1. Introduction

We examine the potential for expanding and diversifying the production of university degrees in science, technology, engineering, and mathematics (STEM) fields by tapping into the pool of students who attend community colleges. Our analysis is based on a thought experiment in which we nudge—either in the traditional sense of the word (Thaler and Sunstein, 2008) or by meaningfully altering student incentives—academically-qualified community college students to enroll in universities.

Our focus on STEM fields is motivated by concerns that the United States is falling behind globally in the production of STEM human capital and this will adversely impact long-term economic prosperity (Atkinson and Ezell, 2012; National Academy of Sciences, National Academy of Engineering, & Institute of Medicine of the National Academies, 2007). Improving and expanding STEM education has been a consistent policy priority at the highest levels of government in the U.S. (National Science & Technology Council, 2018; White House, 2016) and an area of active scholarship (e.g., see Coleman, Smith and Miller, 2019). Underlying reasons for the focus on STEM education are its perceived importance for innovation and the potential for positive spillover effects of STEM-trained workers (Shambough, Nunn, and Portman, 2017; Winters, 2014).

Diversifying the STEM workforce is also an explicit policy objective (e.g., White House, 2016). Participation in STEM fields is low among women and underrepresented minorities (URMs; i.e., Black and Hispanic workers) relative to White and Asian men. An implication is that the size of the STEM workforce can be expanded by increasing participation among these groups (Anderson and Kim, 2006; Committee on Science, Engineering, and Public Policy, 2011). STEM graduates also earn significantly more than graduates in other fields, on average, which suggests that diversifying the STEM workforce can reduce earnings inequality (Altonji, Blom, and Meghir, 2012; Fayer, Lacey, and Watson, 2017; Kinsler and Pavan, 2015). Numerous government programs operate with the goal of improving STEM diversity.[1]

Our focus on community college students is motivated by several factors. First, in efforts to expand and diversify degree production in STEM fields, the most natural alternative to community college students is students who already attend universities and either (a) tried and failed in a STEM field, or (b) made no attempt to pursue a STEM degree. While these students are an appealing group to consider in some respects—most notably, many have strong academic qualifications—a significant drawback is that they have actively decided against pursuing STEM degrees. This is important because Kirkeboen, Leuven, and Mogstad (2016) show that students choose their fields of study based on comparative advantage, which implies that altering these decisions may be undesirable. In contrast, nudging academically-qualified community college students up to the four-year-

[1] Examples include the USDA's Women and Minorities in Science, Technology, Engineering and Mathematics Fields Program (WAMS) and the US Department of Education's Developing Hispanic Serving Institutions STEM and Articulation Program.

university level, then letting them self-select into STEM fields, ensures that students' inherent field-selection processes are preserved.[2]

The community college population is an also appealing group because it is diverse demographically and socioeconomically. Students who attend community colleges are more likely to come from groups that are traditionally underrepresented at universities along the dimensions of race/ethnicity and income (Deming, Goldin and Katz, 2012; Provasnik and Planty, 2008; Wang, 2013). They also have a revealed preference for the pursuit of higher education and most indicate university aspirations.[3] The potential benefits of infusing the STEM pipeline with community college students have been discussed recently in Bahr et al. (2017), Evans, Chen, and Hudes (2020), Hagedorn and Purnamasari (2012), and Terenzini et al. (2014). Bahr et al. (2017) claim "the potential role of community colleges in the production of STEM degrees and professionals is undeniably important, as is the role of community colleges in providing access to STEM pathways for historically disadvantaged groups" (p. 433). Similarly, Evans, Chen and Hudes (2020) argue that community colleges "can act as a bridge between local high schools, 4-year institutions, and the STEM workforce" (p. 247).

Noting these appealing aspects of the community college population, there are challenges associated with intervening with these students. First, success rates of transfer students from community colleges in STEM are low (Wang, 2015) and policies that ease transfer requirements have been shown to have little effect on outcomes at universities

[2] Empirically, it is uncommon for students who start in non-STEM fields to switch to STEM fields during college (Stinebrickner and Stinebrickner, 2014) and Kerr et al. (2020) show that getting students to change majors is generally difficult.

[3] Using data from the Beginning Postsecondary Students Longitudinal Study (BPS), Deming, Goldin and Katz (2012) and Horn and Skomsvold (2012) report that about 80 percent of first-time community college students self-report their education goals as a bachelor's degree or higher.

(Baker, 2016; Gross and Goldhaber, 2009; Roksa and Keith, 2008).[4] This suggests that boosting the STEM pipeline through transfer policies is unlikely to be successful. For this reason, the hypothetical policy we consider aims to circumvent community colleges entirely by re-routing academically-qualified students directly to universities.[5]

Another consideration is whether enough community college students have the academic preparation necessary to succeed in university STEM programs. Hoxby and Avery (2013) find that there are many academically-qualified, low-income students who are undermatched to postsecondary institutions, including community colleges. These students would be prime candidates to move up to more rigorous academic programs at universities in STEM fields. However, Chetty et al. (2020) find that the Hoxby and Avery numbers are likely inflated and there are many fewer of these students. Our study is an indirect, empirical test of sorts for these competing numbers—undergirding our analysis is the question of whether there are enough academically-qualified students who attend community colleges to meaningfully boost the production of STEM degrees at universities, if we could shift their enrollment.

Our research design is an exercise in predictive modeling. We use rich administrative data provided by the Missouri Department of Higher Education and Workforce Development (DHEWD) to design and evaluate a hypothetical policy that can be thought of as a perfectly-effective "nudge" intervention; i.e., in which all nudged individuals respond as intended. The nudge shifts initial community college enrollees to

[4] We are not aware of any studies that have looked at the effect of transfer policies on STEM outcomes specifically, but the general lack of efficacy evidence for these types of policies suggests that any STEM specific policies in a similar vein will face similar challenges.

[5] Although we do not find any policies that specifically try to re-route academically-qualified students directly to universities, evidence from Carrell and Sacerdote (2013) and Hyman (2020) suggest that some interventions aiming to increase college attendings have more impact on enrolling in four-year colleges.

attend universities instead. We start by identifying the subpopulation of community college students likely to succeed in a university STEM program based on observable information. To do this, we use a flexible logistic regression to estimate the likelihood of STEM degree attainment among university students, then apply the parameters out of sample to community college students. We label a community college student with academic qualifications that imply a (relatively) high likelihood of STEM degree completion at a university as "STEM qualified." Assuming we could nudge all of these students to attend universities, we predict their individual likelihoods of completing STEM degrees and produce summary predictions of total STEM degrees produced. We also examine the diversity of students who we predict would earn degrees.

Our predictions assume that initial entrants into community colleges are just as likely to succeed at universities as their observationally-similar peers who start at universities, which is unlikely. As noted above, we also assume that every student we "nudge" changes his or her behavior, which is well outside of the bounds of what can be expected from a plausible real-world intervention (e.g., see Bird et al., 2019; Castleman and Page, 2016; DellaVigna and Linos, 2020; Oreopoulos and Petronijevic, 2019).[6] Primarily for these two reasons, our estimates reflect upper bounds—probably high upper bounds—on the potential for similarly-spirited, real-world policies to affect the production of STEM degrees at universities. We also perform analyses to get more realistic estimates by (1) parameterizing and removing bias from unobserved selection into two-year and four-year colleges, and (2) parameterizing more realistic behavioral changes in response to our hypothetical intervention.

[6] This is the true whether with respect to a textbook nudge as traditionally defined, or a more substantial and costly intervention.

The most closely-related literature to our work includes studies of policy changes and other interventions targeted toward community college students that encourage them to either transfer to or directly enroll in a university (Castleman and Page, 2016; Marx and Turner, 2019; Hyman, 2020; Gurantz et al., forthcoming). We note two major differences between our work and these previous studies. The first difference is that we focus specifically on how community college students can impact *STEM* degree production at universities, motivated by policy interest in the STEM workforce. The second difference is that we do not study a specific policy intervention, but rather focus on the question of whether there is the *potential* for the community college population to be tapped in this way. We view our contribution as a predecessor to policy-intervention studies. We ask whether there is the scope for such policies to be effective, and if so, at what scale, which can inform future efforts to expand and diversify the STEM pipeline.

Our upper-bound results suggest modest potential to expand the production of four-year STEM degrees by tapping into the pool of community college students. The expansion effect is modest, even at an upper bound, because the vast majority of community college students are not academically prepared to succeed in a STEM program at a university. While it is not surprising that few community college students are academically prepared for STEM programs at universities, we show that the magnitude of the drop-off is substantial from the full population of community college students to those who have an appropriate level of academic preparation. When we add more realistic features to our analysis to get away from the upper bound, the expansion potential of policies targeted toward students who attend community colleges shrinks rapidly.

We also find that there is no scope for the community college pipeline to improve the racial/ethnic diversity of four-year STEM degree recipients. Although the community college population on the whole is more diverse than the university population, our analysis reveals that most URM students attending community colleges are not academically prepared to succeed in STEM at universities. Thus, while at a cursory glance the community college population seems like an appealing source to diversify the university-trained STEM workforce, our analysis suggests efforts in this regard will likely fall flat.

A broad takeaway from our study is that it will be challenging to expand and diversify the pool of university-trained STEM workers with interventions targeted toward students who would otherwise plan to attend community college. This finding contributes to what is emerging as a common theme of research examining the determinants of postsecondary outcomes: interventions at the postsecondary level are too late (Cameron and Heckman, 2001; Arcidiacono and Koedel, 2014; Stinebrickner and Stinebrickner, 2014).

2. Data

We use administrative microdata from the Missouri DHEWD for our analysis. The data contain student background characteristics (race, gender, age, high school attended, etc.), pre-entry academic qualifications (high school class percentile rank, ACT test scores), and in-college outcomes (majors, credits, GPA, and graduation). We restrict our analytic sample to first-time, full-time, state-resident students who entered the public college system—which includes 13 universities and 14 community colleges—between 2006 and 2010 as college freshman. We track students for up to six years after initial entry into the system to determine whether they graduate with a four-year degree from any public college

in Missouri.[7]

Table 1.1 shows summary statistics for all university and community college students, and for various restricted subsamples that lead to our primary analytic sample. We review our data restrictions briefly here and examine the sensitivity of our findings to relaxing them below. The first restriction, imposed going from columns (1)-(2) and (4)-(5) in Table 1.1, focuses the analysis on in-state students only. This is due to data limitations for out-of-state students, especially at two-year colleges.[8] The more substantive restrictions occur moving from columns (2)-(3) and (5)-(6), where we drop students from the sample who are (a) older than 20 upon entry as a first-time college student, (b) enrolled part-time at entry, which we define as attempting fewer than 12 credits, (c) missing math or English ACT scores, and/or (d) are from a very small high school (i.e., that sent five or fewer students to a public university during the period covered by our data panel) or the high school attended is missing.

The age restriction is to focus our thought experiment on the population most likely to be susceptible to an intervention that shifts the sector of enrollment. The ACT and full-time-enrollment restrictions are because we treat the steps of taking the ACT prior to college enrollment, and enrolling full time, as indicators of stronger interest and ability to

7 Our data are comprehensive for public colleges and universities statewide but do not cover private or out-of-state institutions. This limits the generalizability of our analysis—most directly in that our results cannot speak to the potential for expanding the STEM pipeline by redirecting community college students to private universities or to universities outside of the state. We view this as a modest limitation in the context of our thought experiment because if we were to nudge community college students to universities at scale, existing transfer patterns suggest that they would be more likely to gravitate toward public in-state universities (Shapiro et al., 2017).

8 The key data issue is that especially for students who attend two-year colleges, out-of-state students often have missing ACT scores even when they took the test. Some ACT scores are reported directly by institutions, but we also have access to scores for all ACT test takers in Missouri. We also use information about students' individual high schools to predict their success in college and out-of-state students attend high schools that are typically sparsely attended by Missouri college-goers, which creates analytic problems in our empirical models.

pursue a higher-level degree among community college students (virtually all four-year college entrants have ACT scores and enroll full time). We drop students from small high schools because we use the high school attended in our prediction models and small schools are problematic empirically.[9] Additional information about data construction, including more information about how the sample changes and summary statistics as each data restriction is enforced, is in Appendix Tables A1-A3. Again, we examine the substantive implications for our findings to relaxing these data restrictions below.

Students' class percentile ranks are important predictors of STEM success and Table 1.1 shows that class percentile ranks are particularly likely to be missing among community college students. This is because of inconsistent institution-level reporting in the DHEWD data, which derives partly from the fact that community colleges' open enrollment policies do not require them to collect data on academic qualifications.[10] For students with missing class percentile ranks, we use linear regression to impute their ranks based on their demographic information, math and English ACT scores, and high schools of attendance. A feature of imputing via linear regression is that the imputed values are shrunken toward the mean, which is problematic because it is students in the upper tail of the distribution of academic qualifications among community college students who are most likely to succeed in STEM fields at universities. To address this issue, we inflate the variance of the imputed values *ex post* at each college level (two-year and four-year) to match the variance of observed percentile ranks at the same level. We also examine the sensitivity of our findings to the variance inflation procedure in the robustness section.

[9] This restriction has no substantive implications for our analysis because it affects very few students.
[10] In contrast, we have data on ACT scores for all students who took the test in Missouri, as noted above.

The racial/ethnic diversity information in Table 1.1 previews our finding regarding the potential for community college students to impact the racial/ethnic diversity in STEM fields at universities. First, while the full community college population has a higher proportion of Black students than the university population (given Missouri demographics, the proportion Black is the most relevant consideration for diversity)—0.14 versus 0.12—the gap in Black representation is not large. Moreover, the Black share among community college students falls to just 0.10 after we restrict the sample to in-state, full-time students (with the latter restriction being most impactful), and then to just 0.08 in the final sample after we add the additional restrictions for age and taking the ACT. In the analytic sample, the Black share among community college students is below the Black share among university students.

We use the Classification of Instructional Programs (CIP) to identify majors in STEM fields, then divide university students into groups of STEM and non-STEM entrants based on their majors at entry.[11] Following Darolia et al. (2020), we use the NSF definition of STEM fields, which includes majors in mathematics, natural sciences, engineering, computer and information sciences, and selected technical subfields within the social and behavioral sciences.

Table 1.2 shows the summary statistics for STEM and non-STEM university entrants. We also include summary statistics for STEM completers. Compared to non-STEM entrants, STEM entrants have higher ACT math scores (25.58 vs 22.16), higher ACT English scores (25.19 vs 23.27), and higher percentile ranks in high school (0.77 vs

[11] Non-STEM entrants include students who do not declare any specific majors at entry.

0.69). Compared to STEM entrants, students who successfully earn a STEM degree possess even stronger academic qualifications.

Consistent with previous research, there is a significantly lower percentage of female students in STEM fields (Chen, 2009) and a slightly lower percentage of URM students (Hill, 2017). But whereas the percentage of female students is the same among STEM completers and STEM entrants, there is a significant drop in the percentage of Black students in STEM fields from entrants to completers (also see Arcidiacono, Aucejo, and Spenner, 2012). Unsurprisingly, both STEM entrants and non-STEM entrants at universities possess substantially stronger academic qualifications than their community college counterparts.

The bottom row of Table 1.2 shows that 44 percent of STEM entrants graduate with a STEM degree in 6 years, but just 4 percent non-STEM entrants transfer to STEM fields and graduate with a STEM degree. This highlights the importance of the initially-declared major in determining the production of four-year STEM degrees.

3. Methodology

Our goal is to determine the potential for the STEM pipeline at universities to be expanded and diversified by tapping into the population of students who attend community colleges. We situate our investigation in the context of a behavioral intervention that shifts student enrollment from community colleges to universities. For most of our analysis we assume that all community college students who we choose to intervene with respond as intended by enrolling in a university. Based on the existing nudge literature, this is well outside the bounds of what is plausible. It is also implausible for an intervention with

forceful incentives—i.e., an intervention that is more than a nudge. This feature of our study contributes to the interpretation of our estimates as giving upper bounds.

The first step in our process is to identify who to target among the community college population for our hypothetical intervention. If the objective function is simply to maximize the number of STEM degrees produced, then the optimal policy would be to target all community college students. However, it would be costly and undesirable to shift students to universities who are underprepared or uninterested in STEM fields given their low likelihoods of success. Therefore, we focus only on students whose observable characteristics suggest they are reasonably likely to succeed in a STEM field. We refer to these students as "STEM qualified," noting that this term broadly reflects academic preparation and interest in STEM fields.

We propose a simple data-driven framework to identify "STEM-qualified" community college students. We begin by estimating the following empirical model to predict STEM degree completion among four-year college students within six years of initial enrollment, which we specify as a logistic regression:

$$Y_{ijt}^{*} = \boldsymbol{X_i \beta_1} + \gamma_j + \delta_t + \varepsilon_{ijt} \tag{1.1}$$

In equation (1.1), Y_{ijt}^{*} is the latent utility of completing a STEM degree within six years, versus not completing a STEM degree, for student *i* from high school *j* who first enrolled in one of Missouri's 13 four-year public universities in year *t*. Students who complete a STEM degree within six years—i.e., $Y_{ijt} = 1$—have latent utility above zero. $\boldsymbol{X_i}$ is a vector of control variables including student' ACT math and English scores, high school percentile ranks, racial/ethnic and gender designations, and interaction terms between race/

gender and ACT scores/ rank. γ_j is a fixed effect for high school j and δ_t is a fixed effect for year t. ϵ_{ijt} is the error term.

The fitted values from equation (1.1), $\hat{P}_{ijt} = Pr(Y_{ijt} = 1|\boldsymbol{X_i}, \gamma_j, \delta_t)$, indicate the predicted likelihood of completing a STEM degree conditional on pre-entry student characteristics and qualifications among four-year-university entrants. The next step in our process is to apply the parameter estimates from equation (1.1) to the profiles of community college students. This generates predicted values $\hat{P}_{ijt}^{cc}$, where the superscript *cc* denotes that the value is an out-of-sample prediction for community college student *i*. $\hat{P}_{ijt}^{cc}$ is the likelihood that student *i* would complete a four-year degree in a STEM field if the student initially enrolled in a university instead of a community college, and if there were no unobserved differences between observationally similar students who differ by initial enrollment sector.

We identify the subpopulation of community college students who are "STEM qualified" based on the distribution of $\hat{P}_{ijt}$ among university STEM entrants. Specifically, in our preferred set up, we identify student *i* as "STEM qualified" if $\hat{P}_{ijt}^{cc} > \tilde{\hat{P}}$, where $\tilde{\hat{P}}$ is the median predicted likelihood of STEM success among initial STEM entrants in the university sample. Again, although we refer students with $\hat{P}_{ijt}^{cc} > \tilde{\hat{P}}$ as "STEM qualified," the predicted values are more-precisely described as embodying two factors that determine STEM success: academic preparation and interest in pursuing a STEM degree. In our data, $\tilde{\hat{P}} \approx 0.17$.

Whether the median value $\tilde{\hat{P}}$ is an appropriate threshold for identifying STEM-qualified students is a normative question. On the one hand, we want to choose a threshold

that is high enough that affected students would have a reasonable likelihood of success in STEM fields. On the other hand, total STEM degree production is at least weakly increasing as our threshold for STEM-qualified declines. We use the median success rate among observed four-year college STEM entrants as our primary threshold because it is an intuitive, data-driven anchor for this value. We consider the sensitivity of our estimates to modifications of the threshold below.

The policies we have in mind are of the sort that alter behavior—either by a textbook nudge or a nudge combined with stronger incentives—such that the community college students we identify as "STEM qualified" instead choose to enroll in universities. The most-commonly studied interventions of this type in recent research are nudges that encourage students to make different college and major choices (Bird et al., 2019; Castleman and Page, 2016). Although the literature is clear that the efficacy of these types of behavioral interventions is limited, we assume that all students who we intervene with will change their enrollment behavior. Under this assumption, and continuing to assume these students would succeed in STEM at the same rates as their observationally similar counterparts who initially enrolled in universities, the total number of predicted four-year STEM degrees produced among STEM-qualified community college students is given by:

$$\theta_{STEM}^{cc} = \sum_{i=1}^{N_s^{cc}} \hat{P}_i^{cc} \qquad (1.2)$$

Finally, noting the interpretive caveats given above, it is also straightforward to modify equation (1.2) to get numbers for specific demographic groups to inform the diversity question (i.e., we can redefine the summations to be over targeted groups only). To obtain error bands for our estimates of STEM degrees produced that account for error throughout the process described in this section, we bootstrap the entire procedure 500

times and report 95-percent empirical confidence intervals based on the bootstrapped values.

4. Primary Results

Table 1.3 shows the raw logit coefficients and bootstrapped 95% confidence intervals for equation (1.1), estimated on the university sample and using our preferred specification. The results are intuitive and consistent with past research showing that students who succeed in STEM fields are positively selected (Arcidiacono and Koedel, 2014; Arcidiacono et al., 2016). Among the pre-entry academic qualifications in our data, the class percentile rank is by far the strongest predictor of STEM success. The ACT math score is also a significant predictor of STEM success.[12] In terms of demographics, the familiar gender difference in STEM success is clearly present in our data (also see Kahn and Ginther, 2017). Similarly, the well-documented lack of differences in success by race/ethnicity after conditioning on pre-entry academic qualifications, with the exception of Asian students, is also present (Griffith, 2010; Sass, 2015).

For all students in both the two-year and four-year samples, we use the median predicted likelihood of STEM success among initial STEM entrants as the threshold to identify "STEM qualified" students. Note that non-STEM entrants at universities can also be STEM-qualified by this definition, even if they prefer a non-STEM major. Summary statistics for these groups are shown in Table 1.4. Overall, about a fourth of all university entrants have academic qualifications that align with our definition of STEM qualified. These students have stronger academic qualifications along all dimensions than their non-

[12] The coefficient on the ACT English score is negative, which is perhaps unintuitive, but this reflects the conditional relationship only—if the ACT English score is included without any other controls, the coefficient is positive (results omitted for brevity).

STEM-qualified counterparts. The magnitudes of the differences in core qualifications (ACT scores and percentile ranks) are large, ranging from 0.8 – 1.4 standard deviations.

STEM-qualified students are also more likely to be male and more likely to be white or Asian than non-qualified students, but for different reasons. The gender gap reflects the strong negative coefficient for female students in Table 1.3. In contrast, the low representation of Black and Hispanic students is not driven by conditional differences in the likelihood of succeeding in STEM by student group—this can be seen by the insignificant coefficients on the racial/ethnic indicators for these groups in Table 1.3. Instead, the racial/ethnic differences emerge due to differences in pre-entry academic qualifications, which are much lower on average for Black students in particular (this result is also consistent with previous research—e.g., see Arcidiacono and Koedel, 2014; Arcidiacono et al., 2016; Bahr et al., 2017).

Unsurprisingly, the fraction of community college students whose pre-entry characteristics and qualifications are sufficient to meet our definition of STEM-qualified is much smaller than the fraction of four-year students, at around 7.4 percent. This can be seen in the bottom row of Table 1.4. Moreover, among the STEM-qualified group in community colleges, their academic qualifications are clearly below those of their STEM-qualified peers at four-year institutions. This reflects the fact that the distribution of academic readiness at community colleges, as measured by the observable information we have, is to the left of the distribution of academic readiness among four-year college students. The implication is that among students above the STEM-qualified threshold, those at community colleges are closer to the threshold value, on average, than their four-year-college counterparts. A notable result in Table 1.4 is that the STEM-qualified

population of community college students does not include a larger proportion of Black students than the STEM-qualified population at universities.

In the first column of Table 1.5 we show our estimates for STEM degree production among the community college sample from equation (1.2). We report the total number of four-year STEM degrees produced among STEM-qualified community college students and the characteristics of completers. Recall that we bootstrap our entire procedure 500 times and the results in Table 1.5 are the average outcome values across the 500 bootstrap replications, with 95-percent confidence intervals reported in parentheses. The second column replicates descriptive statistics for STEM completers among university entrants from Table 1.2 for comparison.

We focus first on our findings regarding the potential to expand the STEM pipeline, then turn to diversity. In total, recall that in Table 1.4 we move 3,209 STEM-qualified community college students to universities. Of these, our model based on observables predicts that 869 would complete a STEM degree within 6 years (with an empirical 95 percent confidence interval of 778-965 students). The number of STEM degrees produced among four-year entrants over our sample period was 9060 students (column 2), meaning that our estimate of 869 degrees corresponds to an increase in production of 9.6 percent.

This is a non-negligible increase, although a reasonable interpretation is that it is actually quite small given the upper-bound assumptions built into our analysis. Moreover, the total number of STEM degrees conferred among the nudged sample overstates the number of new STEM degrees produced because in the absence of our hypothetical intervention, some STEM-qualified community college students would still complete university STEM degrees (i.e., via transfer). Among the 3,209 students we would

hypothetically intervene with, we observe 289 actually transferred to a university and obtained a STEM degree within 6 years. Thus, our upper-bound estimate of the net increase in degrees produced is 580 (869-289), or about 6.4 percent relative to observed STEM degree production.

Turning to the characteristics of the new STEM completers, column (1) shows that their academic qualifications on average are similar to but slightly below their counterparts who start at universities: their average ACT math scores are 25.76, versus 26.63 for university entrants, and their average high school percentile ranks are 81 versus 82. There is no diversity improvement by race-ethnicity among STEM degree recipients in the community college sample relative to the four-year sample. In fact, the fraction of nudged community college students who complete a STEM degree and are Black (0.01) is substantially lower than the fraction of university students who complete a STEM degree and are Black (0.04). The result is driven by the low share of Black students at community colleges who meet our definition of STEM qualified (Table 1.4 shows that just 2 percent of STEM-qualified community college students are Black and the 95% confidence interval is just 1-2 percent).

The new STEM graduates from community colleges are also even more male-dominated than their four-year counterparts. This result derives from the fact that female community college students do not outperform their male peers academically to the same degree that female university students outperform their male peers. Put another way, female community college students are more negatively selected in terms of academic qualifications, in their gender-specific distribution, than their male counterparts.

5. Robustness

In this section we explore the basic robustness of our findings. First, to get a better understanding of the plausibility of our out-of-sample predictions for community college students under the maintained assumption of selection on observables, we document the predictive validity of our models in and out of sample among four-year college students (for whom true outcomes are observed, which is required to test predictive validity). To facilitate in-sample and out-of-sample comparisons, we use 80% of the data to comprise the "training dataset" and the remaining 20% to test predictive validity. We follow the same procedure described in the methodology section with this new data split: we use the training dataset to estimate equation (1.1), then apply the estimated parameters to the prediction dataset, which in this case is the 20% holdout sample of four-year entrants.

In-sample and out-of-sample prediction accuracy are shown in Appendix Table A5. Columns (1)-(2) show the in-sample comparison of true outcomes versus predicted values, and Columns (3)-(4) give the out-of-sample comparison. For the in-sample comparison, unsurprisingly, we see no difference between predicted and true-outcome values. For the out-of-sample comparison the actual and predicted values are also nearly identical. This basic test confirms that the prediction model is effective when applied to university students.

Next, we examine the robustness of our findings to modifying our procedure for imputing missing high school percentile ranks. As noted above, we inflate the variance of the imputed percentile rank values in order to offset the shrinkage inherent to the imputation process in our primary analysis. In Table 1.6, we explore the implications of using the shrunken class-rank values directly without the variance inflation. Following on

the discussion above, a straightforward prediction is that using the shrunken values directly will reduce the number of community college students identified as STEM-qualified by reducing the prevalence of students with imputed values in the tails of the class-rank distribution. The bottom row of Table 1.6 shows that this is indeed the case—we predict a gross increase in STEM degree recipients of just 789 students in Table 1.6 versus 869 students in Table 1.5.

6. Minor Extensions

Next we extend our analysis along several minor dimensions. We describe the extensions in this section as "minor" because their substantive implications for our key findings regarding STEM expansion and diversification are modest.

6.1 Demographic Predictors of STEM Success

In our preferred specification of equation (1.1) we include race/ethnicity and gender indicators, and interactions between these indicators and ACT scores and percentile ranks, to improve the accuracy of our predictions. To test the implications of this for our diversity findings, in Table 1.7 we replicate our entire procedure after (a) dropping the race/ethnicity and gender interactions with ACT scores and percentile ranks, but keeping the race/ethnicity and gender indicators themselves, and (b) dropping all racial/ethnic and gender information from the models. The results from these scenarios are shown in columns (3)-(6) of Table 1.7; columns (1)-(2) replicate the results from our preferred specification for comparison.

Substantively, there are two main takeaways from Table 1.7. First, the racial/ethnic composition of STEM completers in the community college sample does not depend on whether we rely on racial/ethnic data in the prediction model, with the exception of Asian

students, whose predicted STEM completion rate declines slightly when race-ethnicity information is omitted. This result is consistent with the pattern of estimates in Table 1.3, which shows that outside of Asian students, the racial/ethnic designations are not important predictors of STEM completion conditional on measured academic preparation.[13] Second, in terms of gender diversity, the female shares jump markedly in columns (5) and (6) if we remove all information about gender from the predictive model. This is because female students have generally strong academic qualifications (much stronger than for male students, on average). If we ignore their preference for non-STEM fields in our prediction models, we predict many more female students would pursue and complete STEM degrees. Again, this result is not surprising based on the estimates in Table 1.3, and only informative if one believes that female students who attend two-year colleges have fundamentally different preferences for STEM education than their four-year counterparts, which we view as unlikely.

6.2 Modifications to the Sample Restrictions

In Section 2 we describe a number of restrictions that we impose on our preferred estimation sample. In this section we examine the substantive implications of these restrictions for our findings.

First, in Table 1.8 we relax the credit-hour and age-based restrictions, which are set to 12 first-semester credits and age ≤ 20 in the main analysis. We consider reductions of the credit-hour constraint to 9 and then 6 credit hours to accommodate part-time students, and raise the maximum entry age to 22 and then 24. These changes results in small increases in the numbers of students who are identified as STEM qualified, and

[13] That said, the evidence in Table 1.7 is more comprehensive because Table 1.3 does not show all of the interaction coefficients.

correspondingly, small increases in the number of new STEM degrees produced. For example, relaxing the credit-hours restriction from 12 all the way to 6 credit hours results in a total increase in STEM degree production of just 73 degrees; raising the entry age threshold from ≤ 20 to ≤ 24 adds just 14 degrees. The changes are small because relatively few part-time or older community college students are STEM qualified based on their academic profiles. And if anything, part-time and older community college students are even more likely to have unobservable characteristics that make them less likely to complete a STEM degree at a university than their full-time, younger peers.[14]

Next we revisit to our decision to drop students who did not take the ACT prior to college enrollment. Again, the rationale for this decision is that we view the act of taking the ACT as an observable indicator of interest in and/or aptitude for postsecondary education. However, it is possible that some well-prepared community college students elect not to take the ACT for other reasons and to the extent this is true, our exclusion restriction with respect to ACT scores would lead to an understatement of the potential STEM pipeline in community colleges.

To test this, we bring community college students with missing ACT scores back into the analysis by imputing their ACT scores using their first-semester completed credit hours and GPAs. The imputation coefficients are obtained from a regression of each ACT score (math and English) on first-semester completed credit hours and GPAs among community college students with all available information. With these students added back

[14] Note that some or perhaps all of these unobservables will be unrelated to competency, but rather derived from circumstances. As just one example, older and part-time students are more likely to have more non-schooling commitments that would make it harder for them to attend universities, which might require moving and will typically have less flexible degree programs.

into the sample, we replicate our entire predictive procedure. Table 1.9 shows the results compared to the results using our main settings.

Incorporating students with missing ACT scores into the community college sample leads to a substantial increase in its size—the sample increases from 43,214 students (Table 1.1) to 57,382 students, a 33 percent increase. However, Table 1.9 shows that this translates into only a very small increase in the sample of students identified as STEM qualified and who complete STEM degrees. Indeed, the number of STEM degrees produced increases by just 115 degrees relative to baseline. The reason for this results is that most community college students have academic credentials that put them below the threshold for STEM qualified as we've defined it, and those without ACT scores are strongly negatively selected (i.e., when we impute their ACT scores, the average imputed values are low). Consistent with the spirit of our initial exclusion of students who do not take the ACT, adding these students back into our sample has a negligible effect on our findings.

6.3 Sensitivity to the Nudge Threshold

Next we revisit our decision to set the intervention threshold at the median predicted likelihood of success among initial STEM entrants in the university sample. This is a reasonable but arbitrary threshold. As we lower the threshold we will identify more students as "STEM qualified," but the STEM success rate will fall because the marginally-induced students will be less academically prepared. Alternatively, as we raise the threshold the likelihood of obtaining a STEM degree conditional on being identified as "STEM qualified" will rise, but fewer students will be identified so total degree production will fall.

In Table 1.10 we show the sensitivity of our estimates to moving the nudge threshold between the 40th and 60th percentiles of the distribution, at 5-unit intervals. When we decrease the intervention threshold substantially—i.e., down to the 40th percentile—we increase the number of identified students by 57 percent to just over 5000. We also increase the number of STEM degrees produced, although not commensurately because the average student is less prepared—the number of degrees produced increases by 31 percent, or 273 degrees. If we move the threshold up to the 60th percentile we produce 245 fewer degrees in total (624 versus 869), but also nudge 1265 fewer students (1,944 versus 3,209). The conversion rate of enrollment to STEM degrees increases from 27.1 percent (869/3,209) in the baseline case to 32.1 percent (624/1,944) using the 60th-percentile threshold, but at the expense of reduced total production.

Table 1.10 also shows modest diversity implication of modifications to the intervention threshold. Although the implied diversity effects are substantively similar at all thresholds, at lower thresholds the population of students identified as STEM-qualified, and the population predicted to earn STEM degrees, is slightly more likely to be female and Black (relative to male and White).

Overall, Table 1.10 illustrates the tradeoffs as the intervention threshold varies in terms of total STEM degrees produced, the degree-conversion rate, and to a lesser extent the diversity of students who receive STEM degrees. Determining the appropriate threshold requires a normative judgement about the value of degrees produced and the cost of failed interventions (i.e., students who do not complete a STEM degree) and we do not make this judgment here. That said, in assessing the cost of failures, an important distinction exists between failure in STEM and failure to complete any degree at a

university. As a point of information, supplementary predictive models—for which the results are reported in Appendix Table A6—indicate that among students who we intervene with but who fail to complete a STEM degree, just under half (49 percent) would be expected to complete a non-STEM university degree based on their observable characteristics and qualifications, and the other half would fail to complete a degree within 6 years. Among these students, available evidence from Goodman, Hurwitz, and Smith (2017) and Mountjoy (2019) suggests that bachelor's degree receipt would be much higher than in the absence of our intervention; moreover, Mountjoy (2019) shows that the diversion of marginal students toward four-year colleges (or similarly, away from two-year colleges) leads to an increase in earnings.

6.4 Excluding Biology

Biology is one of the largest STEM majors in Missouri and nationally (Snyder, de Brey and Dillow, 2019). However, the field of biology differs from other STEM fields in that it is less mathematically oriented and biology degrees have lower earnings returns.[15] The lower labor-market returns imply that compared to other STEM majors, market demand for biology degrees is low. Therefore, it may be appropriate for policies designed to increase STEM degree production to focus on fields outside of biology.

In Table 1.11 we modify our analysis to examine STEM degree production outside of majors in biology; specifically, outside of majors under the 2-digit CIP code classification for biology.[16] We exclude these majors from the definition of STEM fields,

[15] For example, Webber (2016) shows that the earnings returns to biology degrees are more closely aligned with the returns to degrees in arts and humanities fields than they are with earnings in other STEM disciplines.

[16] We also use different scope of biology majors to determine non-biology STEM fields. For example, keep Biochemistry, Biophysics, and Biomathematics, etc., and results are very similar.

and replicate our entire analytic procedure. In the university sample for the cohorts we study, majors under the biology heading account for 29 percent of all STEM majors, or 2,619 degrees.

Table 1.11 shows that excluding biology, our hypothetical intervention is predicted to generate 705 STEM degrees, versus 869 STEM degrees in the analysis inclusive of biology. The percent increase in non-biology STEM degrees at universities is similar to, but somewhat higher than, in the base case, at 10.9 percent (705/6441). The results for racial/ethnic and gender diversity are substantively similar to what we find in the base case in that diversity conditions worsen in the community college sample. An especially sharp decline is apparent in the female share of STEM degrees produced—with biology included, 14 percent of STEM degrees predicted to result from our intervention are female (Table 1.5), versus just 7 percent when we exclude biology (Table 1.11). This is partly because female students are less likely to enroll in and complete STEM degrees in non-biology fields, which can be seen by comparing column (3) in Table 1.11 to column (2) in Table 1.5, but this does not explain the full decline.

7. Major Extensions

In this section we assess the implications of relaxing the two major assumptions that drive the upper-bound interpretation of our findings thus far: (1) selection-on-observables into universities, and (2) the perfect efficacy of our enrollment intervention.

7.1 Selection on Unobservables

Thus far we have maintained the unrealistic assumption of selection on observables into college sector. This assumption is imbedded in our predictions because we assume that students who choose to initially enroll in community colleges, if they were shifted to start

at a university instead, would perform just as well as observationally similar students who choose to enroll in universities on their own. However, the fact that the community college students did not choose a university on their own suggests that this assumption is unlikely to hold. To the extent that it is violated the expected direction of bias in our estimates of STEM degree production will be positive—i.e., we will overstate the likelihood of STEM success among our hypothetically nudged students.

Although we expect unobserved selection to be non-zero, we are not aware of any research that we can draw on to parameterize a precise value for its magnitude in our context. Absent this, we perform a bounding exercise to assess how varying degrees of unobserved selection would impact our findings. Our procedure follows the logic of Rosenbaum (2002). We begin by estimating the magnitude of *selection on observables* between two-year and four-year students who we identify as STEM qualified. Although we use a fixed threshold to identify STEM-qualified students, Table 1.4 shows that on average the community college sample is negatively selected based on observables. Again, this is because the distribution of academic qualifications at community colleges is shifted to the left of the four-year distribution. The difference in observed selection between students in the two-year and four-year samples can be summarized by the average difference in the likelihood of STEM success between the groups, represented by $(\bar{\hat{p}}_i^{cc} - \bar{\hat{p}}_i)$, where $\bar{p}_i^{cc}$ and $\bar{p}_i$ are the average predicted STEM degree completion rates for two-year and four-year students, respectively, obtained using the parameters from equation (1.1). This calculation indicates that STEM-qualified community college students are 4 percentage points less likely to complete a STEM degree than their STEM-qualified peers who start at universities.

Next, we assume that selection into college sector on unobservables is in the same direction as observed selection and consider magnitudes of unobserved selection ranging from 50 to 300 percent as large as observed selection. To give a sense of the meaning of these values, at the high-end scenario with unobserved selection that is 300% as large as observed selection, we parameterize outcomes such that nudged community college students are 12 percentage points less likely to complete a STEM degree from a university than is implied by their observable student profiles alone. For this exercise we do not allow unobserved factors to affect who is nudged—a realistic policy could act only on observable information—but they will affect the degree production rate among the nudged sample.

Table 1.12 shows the results from the various selection-on-unobservables scenarios. The first row replicates our baseline condition with no unobserved selection. We nudge 3,209 students and 27 percent of these students are predicted to complete a STEM degree. As unobserved selection becomes more severe the conversion rate, and total degree production, decline. Under the assumption that selection on unobservables is of the same magnitude as observed selection (the 100% scenario in row 3 of Table 1.12), the number of STEM degrees produced in our nudged sample declines by 141 degrees, to 738. In the largest selection-on-unobservables condition we consider (at 300% of observed selection) degree production is reduced 45 percent, to just 481 STEM degrees.

We do not have the means to assess the true magnitude of unobserved selection in our data, so we can only provide the range of estimates in Table 1.12 to provide insight into how unobserved selection may impact our findings. If unobserved selection is assumed to be small the implications are modest; if unobserved selection is large, which cannot be

ruled out given our context, it would imply a substantially reduced potential to expand four-year degree production in STEM by shifting community college enrollment.

7.2 A Less than Perfect Nudge

The other important assumption imbedded in our analysis up to this point is that all of the students who we intervene with respond as intended—that is, we can shift enrollment of community college students to universities with perfect efficacy. This is a useful assumption for thinking about the potential upper-bound effect of tapping into the community college population to increase four-year degree production in STEM, but it is not realistic.

If our intervention were a textbook nudge—which Thaler and Sunstein (2008) define as an intervention that is cheap and easy to avoid—evidence suggests that the behavioral response would likely be very small, in the range of 0-5 percentage points (Barr and Turner, 2018; Castleman and Page, 2015; Gurantz et al, 2020; Oreopoulos and Petronijevic, 2019). The response rate could in principle be increased if the nudge became more expensive and costly to avoid, such as if tuition subsidies or stipends were offered, but even then available research suggests a modest behavioral response. For example, Deming and Walters (2017) find very modest impacts of tuition changes on enrollment, whereas they find much larger impacts of institutional spending changes. Marx and Turner (2019) find community college students who have more access to borrowing are around 4 percentage points more likely to transfer to a four-year college.

Based on these studies, in Table 1.13 we consider scenarios where we rescale the intervention effect to be more realistic, but we believe still optimistic, in terms of affecting community college students' enrollment decisions: a 5-percent effect and a 10-percent

effect. The idea is that we would still nudge the same baseline pool of STEM-qualified students (i.e., the 3,209 students from Table 1.4), but only 5 or 10 percent would change their behavior and enroll in a university. For each scenario considered in Table 1.13, we show results for two cases: one where the students who change behavior are a random sample of the STEM-qualified group and one where the students who change are those most likely to succeed in a four-year STEM program.

Unsurprisingly, the results in Table 1.13 imply large reductions in degrees produced and no positive changes in diversity (moreover, gender diversity declines when the students who are most likely to succeed respond to the nudge, but this is tautological because our prediction model embodies the fact that female students are much less likely to succeed in STEM). The extensive-margin effects in these more realistic nudge scenarios are small in absolute terms and as a percentage of STEM degrees produced at universities.

8. Conclusion

We assess the prospects for expanding and diversifying STEM degree production in universities by tapping into the population of academically-qualified community college students. Our work complements a large and growing literature that examines specific policy interventions by providing upper-bound estimates of the types of changes to the STEM pipeline that would be possible via interventions targeted toward community college students.

We find that the number of STEM degrees produced by universities can be modestly expanded by tapping into the community college student population. The exact magnitude of the change depends on assumptions and policy-design details along a variety of dimensions. In our baseline evaluation scenario, we estimate that the academically-

qualified community college students who we would nudge with our hypothetical intervention would generate a gross increase of 869 four-year STEM degrees, or an increase of 9.6 percent on the number STEM degrees already produced by universities. The net increase is smaller—about 6.4 percent—because some of the students who we would hypothetically nudge go on to earn a STEM degree from a university regardless (i.e., by transferring to a university and completing a STEM degree).

Our extensive-margin estimate can be made somewhat larger by modifying aspects of our hypothetical intervention, most notably by lowering the threshold we use to identify the "STEM qualified" students who are nudged, but even a fairly large reduction in the threshold results in tempered gains in STEM degrees. More likely, though, is that our estimates are far above what could be feasibly achieved through a real policy because of the variety of upper-bound conditions we impose on the analysis. The two most significant upper-bound conditions are assumptions: our baseline estimates assume (a) no selection into college sector on unobservables and (b) that we could implement our enrollment intervention with perfect compliance. Relaxing these assumptions quickly degrades the magnitude of gains in STEM degrees we could hope to produce.

Our findings for diversifying STEM degree production are even less promising. Community college students are more racial/ethnically diverse than their four-year counterparts overall. However, the fraction of non-White students who are academically qualified to succeed in four-year STEM degree programs among community colleges is lower than among four-year college students. The end result is that the diversity of individuals who are predicted to earn STEM degrees among our nudged sample of community college students is less than among university students already earning STEM

degrees. We also find no scope for increasing the gender diversity of STEM degree production by tapping into the community college population. This result is partly tautological because we assume that female aversion to STEM, conditional on observable academic qualifications, is similar among two-year and four-year college students. However, the gender gap among community college students is also exacerbated because within their gender-specific distributions of academic qualifications, female students who attend community colleges are more negatively selected than their male peers. This compounds the gender gap in predicted STEM attainment among community college students.

The broad takeaway from our analysis is that policies and interventions targeted toward community college students are unlikely to alter macro-level features of STEM degree production at universities. This does not mean that interventions cannot be effective at the micro-level in terms of improving outcomes for individually-impacted students, and indeed there is evidence that at least for students at the margin of having appropriate academic qualifications, shifts in enrollment from two-year to four-year colleges are beneficial (Goodman, Hurwitz, and Smith, 2017; Mountjoy, 2019). However, our analysis shows that the potential for intervening with community college students in a way that meaningfully impacts overall STEM degree production at universities is limited.

Table 1.1: Summary statistics for two-year and four-year college entrants overall and for key subsamples.

	Four Year University			Community College		
	(1) All	(2) In state	(3) Analytic Sample	(4) All	(5) In state	(6) Analytic Sample
ACT math	22.84	22.7	22.89	18.84	18.84	19.04
	(4.88)	(4.87)	(4.78)	(3.85)	(3.85)	(3.78)
ACT English	23.62	23.49	23.68	18.83	18.83	18.99
	(5.45)	(5.46)	(5.34)	(4.95)	(4.95)	(4.78)
High school percentile rank/100	0.69	0.69	0.71	0.49	0.49	0.56
	(0.23)	(0.23)	(0.22)	(0.25)	(0.25)	(0.23)
High school percentile rank missing indicator	0.16	0.16	0.12	0.55	0.55	0.35
	(0.37)	(0.36)	(0.32)	(0.5)	(0.5)	(0.48)
Female	0.55	0.55	0.55	0.54	0.54	0.54
	(0.50)	(0.5)	(0.50)	(0.50)	(0.5)	(0.50)
White	0.77	0.79	0.81	0.71	0.71	0.79
	(0.42)	(0.41)	(0.39)	(0.45)	(0.45)	(0.41)
Black	0.12	0.12	0.10	0.14	0.13	0.08
	(0.32)	(0.32)	(0.30)	(0.34)	(0.34)	(0.27)
Hispanic	0.02	0.02	0.02	0.03	0.03	0.02
	(0.15)	(0.15)	(0.14)	(0.17)	(0.16)	(0.15)
Asian	0.02	0.02	0.02	0.01	0.01	0.01
	(0.14)	(0.14)	(0.14)	(0.12)	(0.12)	(0.11)
Other Race	0.03	0.02	0.01	0.03	0.03	0.02
	(0.17)	(0.14)	(0.11)	(0.16)	(0.16)	(0.14)
Race missing unknown	0.04	0.04	0.04	0.09	0.09	0.07
	(0.2)	(0.19)	(0.19)	(0.28)	(0.28)	(0.26)
Number of observations	97749	83263	70737	110695	108198	43214

Notes: Table shows means and standard deviations (in parenthesis) for university students and community college students. See the text and Appendix Table A1 for details about the construction of the analytic sample.

Table 1.2: Summary statistics for four-year college entrants in the analytic sample by STEM entry and exit conditions.

	Four Year University			
	(1)	(2)	(3)	(4)
	Analytic Sample	STEM entrants	non-STEM entrants	STEM completers
ACT math	22.89	25.58	22.16	26.63
	(4.78)	(4.76)	(4.52)	(4.54)
ACT English	23.68	25.19	23.27	26.05
	(5.34)	(5.13)	(5.32)	(5.01)
High school percentile rank/100	0.71	0.77	0.69	0.82
	(0.22)	(0.2)	(0.23)	(0.17)
High school percentile rank missing indicator	0.12	0.10	0.12	0.12
	(0.32)	(0.30)	(0.32)	(0.32)
Female	0.55	0.36	0.60	0.36
	(0.5)	(0.48)	(0.49)	(0.48)
White	0.81	0.82	0.80	0.86
	(0.39)	(0.39)	(0.4)	(0.35)
Black	0.10	0.08	0.11	0.04
	(0.3)	(0.27)	(0.31)	(0.20)
Hispanic	0.02	0.02	0.02	0.02
	(0.14)	(0.15)	(0.14)	(0.13)
Asian	0.02	0.03	0.02	0.03
	(0.14)	(0.17)	(0.13)	(0.18)
Other Race	0.01	0.02	0.01	0.01
	(0.11)	(0.12)	(0.11)	(0.11)
Race missing unknown	0.04	0.04	0.04	0.04
	(0.19)	(0.19)	(0.19)	(0.19)
Graduate with STEM in 6 years	0.13	0.44	0.04	1.0
	(0.33)	(0.5)	(0.2)	(0)
Number of observations	70737	15125	55612	9060

Notes: Table shows means and standard deviations (in parenthesis).

Table 1.3: Results from predictive logistic regression of STEM degree completion among four-year college entrants.

	Graduate with STEM
ACT math	0.163***
	[0.153,0.173]
ACT English	-0.041***
	[-0.05,-0.032]
Female	-1.253***
	[-1.599,-0.900]
Asian	2.467***
	[1.546,3.331]
Black	-0.356
	[-1.191,0.335]
Hispanic	0.609
	[-0.709,1.661]
Other Race	1.071
	[-0.426,2.488]
Race Missing Unknown	0.667
	[-0.211,1.438]
High school percentile rank (/100)	2.783***
	[2.589,2.995]
High school percentile rank missing indicator	0.174**
	[0.035,0.318]
Number of observations	68798
	[68432,69114]
High School FE	X
Cohort FE	X
ACT*Race/Ethnicity interactions	X
ACT*Gender interactions	X
Percentile Rank*Race/Ethnicity	X

Notes: The regression output corresponds to equation (1.1) in the main text. Bootstrapped mean estimates and 95 percent confidence intervals are reported. ***p<0.01, **p<0.05, *p<0.10.

Table 1.4: Summary statistics by STEM qualified status at two-year and four-year colleges.

	Four Year University		Community College	
	(1) STEM qualified	(2) Not STEM qualified	(1) STEM qualified	(2) Not STEM qualified
ACT math	28.01	21.08	25.26	18.55
	[27.9,28.11]	[21.02,21.13]	[24.98,25.54]	[18.5,18.59]
ACT English	26.95	22.52	22.42	18.71
	[26.82,27.09]	[22.45,22.58]	[22.11,22.74]	[18.66,18.77]
High school percentile rank (/100)	0.86	0.66	0.79	0.53
	[0.86,0.87]	[0.65,0.66]	[0.77,0.8]	[0.52,0.53]
Female	0.28	0.64	0.16	0.57
	[0.27,0.3]	[0.64,0.65]	[0.13,0.19]	[0.57,0.58]
White	0.87	0.79	0.81	0.79
	[0.86,0.87]	[0.78,0.79]	[0.77,0.85]	[0.78,0.79]
Black	0.03	0.13	0.02	0.09
	[0.02,0.03]	[0.13,0.13]	[0.01,0.02]	[0.08,0.09]
Hispanic	0.02	0.02	0.02	0.02
	[0.01,0.02]	[0.02,0.02]	[0.01,0.03]	[0.02,0.02]
Asian	0.04	0.01	0.04	0.01
	[0.04,0.05]	[0.01,0.01]	[0.02,0.05]	[0.01,0.01]
Other Race	0.01	0.01	0.02	0.02
	[0.01,0.02]	[0.01,0.02]	[0.01,0.04]	[0.02,0.02]
Race missing unknown	0.04	0.04	0.09	0.07
	[0.03,0.04]	[0.03,0.04]	[0.06,0.13]	[0.07,0.08]
Number of students	18509	52228	3209	40005
	[18088,18975]	[51762,52649]	[2907,3520]	[39694,40307]

Notes: Table shows means and 95 percent bootstrapped confidence intervals (500 repetitions) for university students and community college students by STEM qualified status.

Table 1.5: Summary statistics for community college students who are predicted to complete STEM degrees at four-year colleges compared to observed STEM completers at four-year colleges.

	(1) Graduate with STEM	(2) STEM completers at universities (from Table 1.2)
Avg ACT math	25.76	26.63
	[25.45,26.07]	[26.53,26.71]
Avg ACT English	22.64	26.05
	[22.3,22.96]	[25.95,26.14]
Avg HS percentile rank (/100)	0.81	0.82
	[0.79,0.82]	[0.82,0.83]
Share Female	0.14	0.36
	[0.11,0.17]	[0.35,0.37]
Share White	0.81	0.86
	[0.77,0.85]	[0.85,0.86]
Share Black	0.01	0.04
	[0.01,0.02]	[0.04,0.05]
Share Hispanic	0.02	0.02
	[0.01,0.03]	[0.02,0.02]
Share Asian	0.04	0.03
	[0.02,0.05]	[0.03,0.04]
Share Other Race	0.02	0.01
	[0.01,0.04]	[0.01,0.02]
Share Race missing unknown	0.1	0.04
	[0.06,0.14]	[0.03,0.04]
Number of STEM degrees (gross)	869	9060
	[778,965]	[8882,9212]
Number of STEM degrees (net)	580	--
	[498,668]	

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions for nudged community college students. Column (2) reports means and standard deviations for actual STEM completers among initial university entrants. The net number of STEM degrees equals subtracting the number of STEM-qualified community college students observed transferring into university and obtaining a STEM degree within 6 years directly in the data from the number of gross STEM degrees.

Table 1.6: Robustness of findings to selecting STEM-qualified community college students without the variance inflation adjustment to the imputed high-school class percentile ranks.

	Main Settings		Without imputed-HS Rank variance inflation	
	(1)	(2)	(3)	(4)
	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM
Avg ACT math	25.26	25.76	25.46	25.94
	[24.98,25.54]	[25.45,26.07]	[25.2,25.73]	[25.64,26.24]
Avg ACT English	22.42	22.64	22.52	22.7
	[22.11,22.74]	[22.3,22.95]	[22.26,22.79]	[22.41,22.99]
Avg HS percentile rank (/100)	0.79	0.81	0.76	0.78
	[0.77,0.8]	[0.79,0.82]	[0.75,0.78]	[0.76,0.79]
Share Female	0.16	0.14	0.15	0.13
	[0.13,0.19]	[0.11,0.17]	[0.13,0.18]	[0.11,0.16]
Share White	0.81	0.81	0.81	0.81
	[0.77,0.85]	[0.77,0.85]	[0.77,0.85]	[0.77,0.85]
Share Black	0.02	0.01	0.01	0.01
	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]
Share Hispanic	0.02	0.02	0.02	0.02
	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]
Share Asian	0.04	0.04	0.04	0.04
	[0.02,0.05]	[0.02,0.05]	[0.03,0.05]	[0.03,0.05]
Share Other Race	0.02	0.02	0.02	0.03
	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]
Share Race missing unknown	0.09	0.1	0.09	0.1
	[0.06,0.13]	[0.06,0.14]	[0.06,0.13]	[0.06,0.14]
Number of students or degrees (gross)	3209	869	2940	789
	[2907,3520]	[778,965]	[2670,3223]	[712,869]

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions for STEM-qualified community college students when we inflate the variance of imputed high school class percentile ranks in column (1) and (2), and do not inflate the variance of imputed high school percentile ranks in column (3) and (4).

Table 1.7: Robustness of findings to dropping race-gender indicators and/or race-gender indicator interactions in the model that predicts STEM four-year degree completion.

	Main Settings		No Race-Gender Interaction Terms		No Race-Gender Indicators or Interactions	
	(1)	(2)	(3)	(4)	(5)	(6)
	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM
Avg ACT math	25.26	25.76	25.46	25.98	25.86	26.37
	[24.98,25.54]	[25.45,26.07]	[25.2,25.7]	[25.71,26.23]	[25.59,26.11]	[26.07,26.63]
Avg ACT English	22.42	22.64	22.74	22.99	22.94	23.16
	[22.11,22.74]	[22.3,22.95]	[22.52,22.98]	[22.75,23.23]	[22.71,23.17]	[22.92,23.41]
Avg HS percentile rank (/100)	0.79	0.81	0.80	0.82	0.81	0.83
	[0.77,0.8]	[0.79,0.82]	[0.79,0.81]	[0.81,0.83]	[0.8,0.82]	[0.81,0.84]
Share Female	0.16	0.14	0.16	0.14	0.41	0.39
	[0.13,0.19]	[0.11,0.17]	[0.14,0.19]	[0.12,0.17]	[0.39,0.42]	[0.37,0.41]
Share White	0.81	0.81	0.85	0.84	0.87	0.87
	[0.77,0.85]	[0.77,0.85]	[0.82,0.87]	[0.82,0.86]	[0.86,0.88]	[0.86,0.88]
Share Black	0.02	0.01	0.01	0.01	0.01	0.01
	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.01]
Share Hispanic	0.02	0.02	0.02	0.01	0.01	0.01
	[0.01,0.03]	[0.01,0.03]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]
Share Asian	0.04	0.04	0.03	0.03	0.02	0.02
	[0.02,0.05]	[0.02,0.05]	[0.02,0.03]	[0.02,0.04]	[0.02,0.02]	[0.02,0.02]
Share Other Race	0.02	0.02	0.02	0.02	0.02	0.02
	[0.01,0.04]	[0.01,0.04]	[0.01,0.03]	[0.01,0.03]	[0.02,0.02]	[0.02,0.02]
Share Race missing unknown	0.09	0.1	0.08	0.08	0.07	0.07
	[0.06,0.13]	[0.06,0.14]	[0.06,0.1]	[0.06,0.1]	[0.06,0.07]	[0.06,0.07]
Number of students or degrees(gross)	3209	869	3080	827	3105	777
	[2907,3520]	[778,965]	[2814,3361]	[754,908]	[2844,3432]	[713,859]

Notes: Table reports averages and 95 percent confidence intervals of 500 bootstrap predictions

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Table 1.8: Robustness of findings to using more inclusive pools of two-year college students by relaxing the full-time student and age restrictions.

	Initial Credit Hours >=9		Initial Credit Hours >=6		Age<=22		Age<=24	
	(1) STEM qualified	(2) Graduate with STEM	(3) STEM qualified	(4) Graduate with STEM	(5) STEM qualified	(6) Graduate with STEM	(7) STEM qualified	(8) Graduate with STEM
Avg ACT math	25.24	25.74	25.22	25.74	25.27	25.77	25.28	25.79
	[24.92,25.57]	[25.39,26.08]	[24.93,25.51]	[25.4,26.04]	[24.96,25.55]	[25.47,26.08]	[24.99,25.56]	[25.48,26.09]
Avg ACT English	22.42	22.64	22.43	22.66	22.41	22.61	22.43	22.63
	[22.1,22.72]	[22.3,22.96]	[22.13,22.75]	[22.35,22.99]	[22.11,22.73]	[22.31,22.97]	[22.13,22.74]	[22.3,22.96]
Avg HS percentile rank (/100)	0.79	0.8	0.79	0.8	0.79	0.81	0.79	0.81
	[0.77,0.8]	[0.79,0.82]	[0.77,0.8]	[0.79,0.82]	[0.77,0.8]	[0.79,0.82]	[0.77,0.8]	[0.79,0.82]
Share Female	0.16	0.14	0.16	0.14	0.15	0.13	0.15	0.14
	[0.13,0.18]	[0.11,0.17]	[0.13,0.19]	[0.12,0.17]	[0.12,0.18]	[0.11,0.16]	[0.13,0.18]	[0.11,0.16]
Share White	0.81	0.81	0.81	0.81	0.81	0.81	0.81	0.81
	[0.77,0.85]	[0.77,0.85]	[0.76,0.84]	[0.75,0.84]	[0.77,0.85]	[0.77,0.85]	[0.77,0.85]	[0.77,0.85]
Share Black	0.02	0.01	0.02	0.01	0.02	0.02	0.02	0.02
	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]
Share Hispanic	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]
Share Asian	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04
	[0.02,0.06]	[0.02,0.06]	[0.02,0.06]	[0.02,0.06]	[0.02,0.05]	[0.02,0.05]	[0.02,0.05]	[0.02,0.05]
Share Other Race	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]
Share Race missing unknown	0.09	0.1	0.1	0.1	0.09	0.1	0.09	0.1
	[0.06,0.13]	[0.06,0.14]	[0.06,0.13]	[0.06,0.14]	[0.06,0.13]	[0.06,0.14]	[0.06,0.13]	[0.06,0.13]
Number of students or degrees (gross)	3378	907	3509	942	3261	882	3271	883
	[3041,3697]	[807,1007]	[3164,3892]	[840,1049]	[2940,3559]	[793,973]	[2961,3578]	[798,973]
Initial population considered	46493		48841		44001		44335	

Notes: Table reports averages and 95 percent confidence intervals of 500 bootstrap repetitions under different under different sample restrictions: Minimum registered credit hours >=9, Minimum credit hours >=6, Maximum age in freshman year <=22, Maximum age in freshman year <=24.

Table 1.9: Robustness of findings to removing the ACT-taking requirement among two-year-college students to be nudged.

	Main Settings		Recover Missing ACT Scores	
	(1)	(2)	(3)	(4)
	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM
Avg ACT math	25.26	25.76	25.06	25.53
	[24.98,25.54]	[25.45,26.07]	[24.75,25.37]	[25.19,25.85]
Avg ACT English	22.42	22.64	21.6	21.79
	[22.11,22.74]	[22.3,22.95]	[21.24,21.93]	[21.39,22.2]
Avg HS percentile rank (/100)	0.79	0.81	0.77	0.79
	[0.77,0.8]	[0.79,0.82]	[0.75,0.79]	[0.77,0.8]
Share Female	0.16	0.14	0.14	0.13
	[0.13,0.19]	[0.11,0.17]	[0.12,0.17]	[0.1,0.16]
Share White	0.81	0.81	0.8	0.8
	[0.77,0.85]	[0.77,0.85]	[0.75,0.84]	[0.74,0.84]
Share Black	0.02	0.01	0.02	0.01
	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]
Share Hispanic	0.02	0.02	0.02	0.02
	[0.01,0.03]	[0.01,0.03]	[0.01,0.04]	[0.01,0.04]
Share Asian	0.04	0.04	0.04	0.04
	[0.02,0.05]	[0.02,0.05]	[0.02,0.06]	[0.02,0.06]
Share Other Race	0.02	0.02	0.03	0.03
	[0.01,0.04]	[0.01,0.04]	[0.01,0.05]	[0.01,0.06]
Share Race missing unknown	0.09	0.1	0.1	0.1
	[0.06,0.13]	[0.06,0.14]	[0.07,0.15]	[0.07,0.16]
Number of students or degrees (gross)	3209	869	3680	984
	[2907,3520]	[778,965]	[3263,4077]	[870,1097]
Initial population considered	43214		57382	

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions after we recover missing ACT test scores for community college students. We use students' completed credit hours and GPAs during the first semester of community college to impute missing ACT test scores.

Table 1.10: Findings using different nudge thresholds for identifying STEM-qualified two-year college students based on the percentile of the distribution among four-year STEM entrants (the baseline case is at the 50th percentile).

	40th Percentile		45th Percentile		55th Percentile		60th Percentile	
	(1) STEM qualified	(2) Graduate with STEM	(3) STEM qualified	(4) Graduate with STEM	(5) STEM qualified	(6) Graduate with STEM	(7) STEM qualified	(8) Graduate with STEM
Avg ACT math	24.53	25.16	24.9	25.46	25.63	26.08	26	26.4
	[24.27,24.78]	[24.89,25.42]	[24.61,25.16]	[25.17,25.74]	[25.3,25.94]	[25.73,26.4]	[25.64,26.34]	[26.01,26.76]
Avg ACT English	22.1	22.38	22.27	22.51	22.58	22.77	22.74	22.91
	[21.83,22.35]	[22.09,22.63]	[21.98,22.54]	[22.2,22.78]	[22.24,22.91]	[22.41,23.13]	[22.38,23.09]	[22.54,23.27]
Avg HS percentile rank (/100)	0.76	0.79	0.78	0.8	0.8	0.82	0.82	0.83
	[0.75,0.78]	[0.77,0.8]	[0.76,0.79]	[0.78,0.81]	[0.79,0.82]	[0.8,0.83]	[0.8,0.83]	[0.81,0.84]
Share Female	0.2	0.17	0.17	0.15	0.14	0.12	0.12	0.11
	[0.17,0.22]	[0.14,0.2]	[0.15,0.21]	[0.13,0.18]	[0.11,0.17]	[0.1,0.15]	[0.09,0.15]	[0.08,0.14]
Share White	0.81	0.81	0.81	0.81	0.81	0.81	0.81	0.81
	[0.78,0.85]	[0.77,0.85]	[0.77,0.85]	[0.77,0.85]	[0.77,0.85]	[0.76,0.85]	[0.77,0.86]	[0.76,0.86]
Share Black	0.02	0.02	0.02	0.02	0.01	0.01	0.01	0.01
	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]	[0.01,0.02]
Share Hispanic	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.03]	[0.01,0.04]	[0.01,0.04]
Share Asian	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04
	[0.02,0.05]	[0.02,0.05]	[0.02,0.05]	[0.02,0.05]	[0.02,0.06]	[0.02,0.06]	[0.02,0.06]	[0.02,0.06]
Share Other Race	0.02	0.02	0.02	0.02	0.02	0.03	0.02	0.03
	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.04]	[0.01,0.05]
Share Race missing unknown	0.09	0.09	0.09	0.1	0.09	0.1	0.1	0.1
	[0.07,0.12]	[0.06,0.13]	[0.06,0.13]	[0.06,0.13]	[0.06,0.13]	[0.06,0.14]	[0.06,0.14]	[0.06,0.15]
Number of students or degrees (gross)	5024	1142	4037	1003	2515	742	1944	624
	[4645,5427]	[1039,1248]	[3696,4407]	[905,1105]	[2246,2769]	[656,827]	[1721,2163]	[546,702]

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions under different nudge threshold: nudge threshold= 40th percentile, nudge threshold= 45th percentile, nudge threshold= 55th percentile, nudge threshold= 60th percentile.

Table 1.11: Summary statistics for STEM-qualified community college students after dropping Biology majors

	(1) STEM qualified	(2) Graduate with STEM	(3) STEM completers at universities
Avg ACT math	25.17	25.73	26.87
	[24.84,25.51]	[25.37,26.07]	[26.75,26.97]
Avg ACT English	21.86	22.07	25.79
	[21.51,22.18]	[21.7,22.42]	[25.67,25.91]
Avg HS percentile rank (/100)	0.75	0.77	0.81
	[0.74,0.77]	[0.76,0.79]	[0.81,0.82]
Share Female	0.08	0.07	0.28
	[0.06,0.1]	[0.05,0.09]	[0.27,0.29]
Share White	0.84	0.84	0.87
	[0.79,0.88]	[0.79,0.88]	[0.86,0.88]
Share Black	0.01	0.01	0.04
	[0.01,0.02]	[0.01,0.02]	[0.03,0.04]
Share Hispanic	0.01	0.01	0.02
	[0,0.03]	[0,0.02]	[0.01,0.02]
Share Asian	0.03	0.03	0.03
	[0.02,0.05]	[0.02,0.05]	[0.02,0.03]
Share Other Race	0.02	0.03	0.01
	[0.01,0.04]	[0.01,0.05]	[0.01,0.02]
Share Race missing unknown	0.08	0.08	0.04
	[0.05,0.12]	[0.05,0.12]	[0.03,0.04]
Number of students or degrees (gross)	2995	705	6441
	[2721,3339]	[636,787]	[6307,6584]

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions when we exclude biology from STEM majors.

Table 1.12: Summary statistics for STEM qualified community college students: different levels of selection on unobservables

(1) # of Nudged Community College Students	(2) Average STEM Completion Likelihood among four-year STEM-Qualified Entrants	(3) two-year Student Selection on Observables	(4) Selection on Unobservables	(5) Average Likelihood (community college)	(6) # of STEM Degrees Produced via Nudge (gross)
3209	0.31	-0.04	0	0.27	869
3209	0.31	-0.04	-0.02 (50%)	0.25	802
3209	0.31	-0.04	-0.04 (100%)	0.23	738
3209	0.31	-0.04	-0.08 (200%)	0.19	610
3209	0.31	-0.04	-0.12 (300%)	0.15	481

Notes: Table describes the number of STEM degrees produced with different levels of selection on unobservables: 0%, 50%, 100%, 200% and 300% times selection on observables. Selection on observables value are calculated from the average likelihoods of graduating in STEM corresponding to equation (1.1). Column (5)= column (2)- column (3)- column (4). Column (6)= column (1) * column (5).

Table 1.13: Summary statistics for STEM-qualified community college students with 5/10-percent nudge compliance rate.

	Randomly Selected 5 percent		Top 5 Percent		Randomly Selected 10 percent		Top 10 Percent	
	(1) STEM qualified	(2) Graduate with STEM	(3) STEM qualified	(4) Graduate with STEM	(5) STEM qualified	(6) Graduate with STEM	(7) STEM qualified	(8) Graduate with STEM
Avg ACT math	25.26	25.76	29.29	29.39	25.27	25.76	28.45	28.61
	[24.63,25.83]	[25.07,26.46]	[28.12,30.24]	[28.16,30.34]	[24.83,25.71]	[25.26,26.27]	[27.69,29.11]	[27.72,29.34]
Avg ACT English	22.46	22.68	24.37	24.43	22.47	22.68	23.8	23.91
	[21.76,23.22]	[21.92,23.53]	[23.35,25.36]	[23.33,25.46]	[21.9,23.1]	[22.06,23.36]	[23.05,24.54]	[23.15,24.63]
Avg HS percentile rank (/100)	0.79	0.81	0.92	0.93	0.79	0.81	0.9	0.9
	[0.76,0.82]	[0.77,0.84]	[0.88,0.96]	[0.89,0.96]	[0.77,0.81]	[0.78,0.83]	[0.87,0.92]	[0.87,0.93]
Share Female	0.16	0.14	0.06	0.06	0.15	0.14	0.07	0.06
	[0.09,0.22]	[0.08,0.2]	[0.02,0.12]	[0.02,0.11]	[0.11,0.2]	[0.1,0.18]	[0.03,0.11]	[0.03,0.11]
Share White	0.81	0.81	0.78	0.78	0.81	0.81	0.79	0.78
	[0.73,0.88]	[0.73,0.88]	[0.66,0.88]	[0.65,0.88]	[0.75,0.86]	[0.75,0.87]	[0.69,0.86]	[0.69,0.87]
Share Black	0.02	0.01	0.01	0.01	0.02	0.01	0.01	0.01
	[0,0.04]	[0,0.04]	[0,0.04]	[0,0.04]	[0,0.03]	[0,0.03]	[0,0.03]	[0,0.03]
Share Hispanic	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.01
	[0,0.05]	[0,0.05]	[0,0.05]	[0,0.05]	[0,0.04]	[0,0.04]	[0,0.04]	[0,0.04]
Share Asian	0.04	0.04	0.04	0.03	0.04	0.04	0.04	0.04
	[0.01,0.07]	[0.01,0.08]	[0.01,0.08]	[0.01,0.08]	[0.02,0.07]	[0.01,0.07]	[0.01,0.07]	[0.01,0.07]
Share Other Race	0.02	0.02	0.04	0.05	0.02	0.02	0.03	0.04
	[0,0.05]	[0,0.06]	[0.01,0.09]	[0.01,0.1]	[0.01,0.05]	[0.01,0.05]	[0.01,0.07]	[0.01,0.08]
Share Race missing unknown	0.09	0.1	0.11	0.11	0.09	0.1	0.11	0.11
	[0.05,0.15]	[0.05,0.16]	[0.03,0.21]	[0.03,0.22]	[0.05,0.14]	[0.05,0.14]	[0.05,0.21]	[0.04,0.21]
Number of students or degrees (gross)	159	43	159	90	318	86	318	159
	[145,175]	[38,49]	[145,175]	[80,103]	[288,349]	[77,96]	[288,349]	[142,180]
Number of degrees (net)	29		54		58		95	
	[21,37]		[40,69]		[45,70]		[71,120]	

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions when 5/10 percent STEM-qualified community college students actually choose to enroll at universities. Randomly selected 5/10 percent in column (1) and (2)/ (5) and (6); top 5/10 percent in terms of prediction likelihoods in column (3) and (4)/(7) and (8).

Chapter 2

The Potential for Community College Students to Expand and Diversify University Degree Production in STEM Fields II: Evidence from National Data

1. Introduction

Policymakers, including those in the highest level of government, show continued interest in increasing and diversifying the university-trained STEM worker workforce (Coleman, Smith and Miller, 2019; National Science & Technology Council, 2018; Shambough, Nunn, and Portman, 2017; White House, 2016; Winters, 2014). The underlying logic is to increase innovation (Shambough, Nunn, and Portman, 2017; Winters, 2014) and reduce racial earnings inequality (Altonji, Blom, and Meghir, 2012; Fayer, Lacey, and Watson, 2017; Kinsler and Pavan, 2015).

Two-year college students have attracted considerable attention as a potential supply pipeline into university STEM programs. There are three main advantages of efforts to tap into the two-year college population for this reason. First, compared to high school graduates, two-year college students have revealed a preference for pursing higher education (Deming Goldin and Katz, 2012; Horn and Skomsvold, 2012). Policies aiming to reallocate students from two-year college to four-year college should be easier than ones

targeting high school graduates.[17] Second, the most natural other alternatives, four-year university STEM dropouts or non-STEM entrants, are not ideal because these students have actively opted out of STEM, which research shows is at least partly driven by their comparative advantages (Kirkeboen, Leuven, and Mogstad, 2016). Nudging two-year college students into four-year university and letting them choose majors on their own will preserve their preferences and allow students to respond to their own relative strengths and weaknesses. Third, two-year college students are more demographically and socioeconomically diverse than four-year college students (Deming, Goldin and Katz, 2012; Provasnik and Planty, 2008; Wang, 2013). This appeal of the two-year college population has received considerable attention in past research and in policy documents (Bahr et al. , 2017; Evans, Chen, and Hudes, 2020; Hagedorn and Purnamasari, 2012; Terenzini et al., 2014). However, despite the advantages, there are reasons to think that efforts to expand the university STEM pipeline via two-year colleges will fail. Most notably, transfer rates of community college students to four-year universities in STEM fields are low (Wang, 2015), and policies that ease transfer requirements have been shown to have little effect on outcomes at universities (Baker, 2016; Gross and Goldhaber, 2009; Roksa and Keith, 2008). It is also unclear whether there are enough well-qualified community college students who could succeed in a university STEM program if their education were redirected (Hoxby and Avery, 2013; Chetty et al., 2020).

In a recent paper, Qian and Koedel (2020) use administrative microdata covering all Missouri public colleges to examine the potential to expand and diversify the production

[17] Using data from the Beginning Postsecondary Students Longitudinal Study (BPS), Deming, Goldin and Katz (2012) and Horn and Skomsvold (2012) report that about 80 percent of first-time community college students self-report their education goals as a bachelor's degree or higher.

of STEM degrees at universities by tapping into the population of community college students. In their thought experiment, they nudge community college students who possess relatively high academic qualifications to enroll in four-year universities directly. They find that nudging community college students can only increase STEM degrees production by a small amount, and that there is no scope for increasing racial/ethnic diversity of four-year STEM degree recipients via community college students.

The findings of Qian and Koedel (2020) are discouraging about the potential for improving the STEM pipeline using community college students. They dampen enthusiasm expressed in a large research literature and among the policy community. Given the significance of their findings, in this paper I replicate and extend their analysis using national data, with the goal of both (a) assessing the general robustness of their findings, and (b) exploring areas of extension that they could not explore due to data constraints.

One concern with their study is that it uses data from just one state: Missouri. A basic question is whether their findings also replicate nationally. One reason this is important to test is that the nation a whole is more diverse than Missouri. In Missouri, around 79 percent population is Non-Hispanic White, about 12 percent is Black, and just 4 percent is Hispanic. In contrast, nationally, 60, 13, and 19 percent of the US population is Non-Hispanic White, Black, and Hispanic (U.S. Census Bureau., 2019). The null findings from Qian and Koedel (2020) regarding diversity may not hold in a more-diverse national sample.

A second limitation of Qian and Koedel (2020) is that they only look at students who attend public postsecondary institutions, but nationally, many students attend private colleges: 31 percent four-year college students and 3 percent two-year college students

enroll in private colleges (De Brey et. al., 2021). It may be that a more comprehensive analysis of college students, inclusive of non-public institutions, can yield new insights into the potential STEM pipeline.

Finally, a third limitation of Qian and Koedel (2020) is with respect to their data. Although the number of observations in their data are large enough to provide precise estimate, the control variables available are sparse: they have access to student demographics, ACT score, and high-school class ranks only.

Following the general framework of Qian and Koedel (2020), I use the restricted-use version of the Educational Longitudinal Study of 2002 (ELS:2002) dataset provided by the National Center for Education Statistics (NCES) to further investigate the potential of expanding and diversifying the STEM pipeline by nudging two-year college students into four-year universities. I use multiple methods, including machine learning techniques, to increase the predictive power of my models and test the robustness of my findings. Consistent with Qian and Koedel (2020), I find that although the number of two-year college students is large, there is only a small fraction that possess enough academic qualifications to succeed in STEM fields and policies targeted toward two-year college students can only increase STEM-degree production by a small amount. In terms of increasing the racial-ethnic and gender diversity of STEM degree recipients, on average, my findings again support Qian and Koedel (2020) and show no scope for improving female and minority representation. However, because of the small sample size, I cannot rule out the possibility there might be some room to increase Hispanic representation in STEM fields.

The rest of the paper is organized as follows. Section 2 discusses the ELS: 2002

dataset, especially its advantages and disadvantages compared to dataset in Qian and Koedel (2020), and basic descriptive statistics. Section 3 is methodology part, describing the procedure of this thought experiment. I show primary results in section 4. Robustness tests and extensions are examined in section 5. I conclude in section 6.

2. Data

The ELS:2002 is a survey dataset is produced by the National Center for Education Statistics (NCES). NCES collected data from a national representative sample of tenth graders (base year) and followed them throughout their secondary and postsecondary years. I include all students who either enter a four-year university or two-year college after high school in my sample. I track them for up to six years after initial entry into a postsecondary institution to observe whether they graduate with a four-year degree and whether it is in a STEM field.

Compared to the administrative microdata from the Missouri DHEWD used in Qian and Koedel (2020), there are some unique advantages of ELS data. First, the geographic scope is much broader and allows me to assess the generalizability of the findings from Qian and Koedel (2020) in Missouri. I can also examine the STEM pipeline more broadly, inclusive of private four-year and two-year institutions and of students who move across state lines. Missouri demographics also differ from the U.S., most notably with respect to Missouri's small Hispanic population share (about 2 percent of the college-going population compared to about 12 percent in the ELS). Using the nationally-representative ELS data allows me to expand on Qian and Koedel's original analysis of diversity.

The final advantage of the ELS dataset is the richness of variables. The data contain student background information (race, gender, etc.), family background (mother's

education, father's education, family income, etc.), academic qualifications and course-taking behaviors (math test scores, reading test scores, high school GPA, STEM course-taking, etc.), and high school information (high school enrollment, free or reduced meal percentage, etc.). In contrast, the control variables available to Qian and Koedel (2020) are sparse by comparison—they have access to student demographics, ACT score, and high-school class ranks only.

The ELS data also have two limitations compared to Qian and Koedel (2020). First, the ELS sample size is much smaller than Missouri administrative microdata. There are only 10,840 students in analytic sample, of which 6,820 initially enroll in four-year universities and 4,020 enroll in two-year colleges.[18] The small sample size will make the findings more sensitive to model specification issues and make my estimates generally less precise than in Qian and Koedel (2020). I also cannot estimate high school fixed effects using the ELS data, which are feasible in Qian and Koedel (2020) because of their much larger sample and the concentrated set of high schools that students attend (i.e., high schools in Missouri). I offset this latter limitation of the ELS data by using rich high-school level characteristics as control variables. The second disadvantage of the ELS is that there are not credible information about students' intended majors at college entry. Qian and Koedel (2020) construct an index of "STEM qualification" for community college students in their data based on a model that predicts STEM degree completion, conditional on STEM entry, among four-year university students. In the ELS, without access to information about STEM entry, I take a similar approach of constructing the index based on STEM completion outcome among four-year college entrants, but without conditioning

[18] In this paper, all sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.

on initial STEM entry—as I describe below, I align my approach with Qian and Koedel (2020) such that the fraction of two-year college students who are nudged in both studies is similar.

Table 2.1 shows summary statistics of selected variables for four-year university entrants, four-year university STEM completers, and two-year college entrants in the ELS data.[19] Following Darolia et al. (2020), I use the NSF definition of STEM fields based on the Classification of Instructional Programs (CIP), which includes majors in mathematics, natural sciences, engineering, computer and information sciences, and also some technical subfields within the social and behavioral sciences majors. Overall, there are 6,820 four-year university entrants in my analytic sample and 820 graduate with a STEM degree within six years. There are 4,020 two-year college students in the ELS sample. When comparing column (1) and column (3), we see similar female representation, but a higher percentage of minority students in two-year colleges, mostly driven by a gap in the Hispanic student share (0.09 vs 0.19 in four-year and two-year colleges, respectively). The difference in the Black share between four-year and two-year colleges is small by comparison (0.11 vs 0.13). As mentioned above, the demographic distribution is quite different in Qian and Koedel (2020) with respect to Hispanic representation—specifically, in the Missouri sample there is no difference in the Hispanic share between four-year and two-year colleges (both are 0.02).

Four-year university students have higher standardized math and reading test scores, higher GPAs, and higher ACT scores than their two-year college counterparts. Moreover, among four-year university students, STEM completers are additionally positively selected

[19] For brevity only selected variables are shown in Table 2.1. See Appendix Table C1 for all variables.

along these dimensions. When we compare demographics between four-year university entrants and STEM completers, there is a sharp decrease from the female share of four-year university students to the share who obtain a STEM degree. There is also a significant increase in Asian representation, and significant decreases in Black and Hispanic representation, among STEM completers.

3. Methodology

The first step in my thought experiment is to identify two-year college students whose academic qualifications and characteristics suggest they can succeed in STEM fields at four-year universities. I find these students, hypothetically nudge them from two-year to four-year colleges, and predict how many would graduate with a STEM degree. I also calculate the characteristics of the predicted STEM completers.

There are two basic assumptions underlying my hypothetical intervention. First, all two-year college students who are selected to be nudged respond as intended by enrolling in a four-year university; i.e., I assume a perfect nudge success rate. Second, I assume selection on observables into college sector; i.e., if their enrollment behavior were modified, two-year college students would perform equally to observationally similar four-year university students in terms of STEM success. Admittedly, these two assumptions are unrealistic, but they are useful for recovering upper-bound estimates of the potential impact of my hypothetical intervention.

I begin by using the sample of four-year university entrants to estimate a model predicting degree completion in a STEM field. Next, I apply the parameters from the prediction model out of sample to two-year college students to predict their likelihoods of completing a STEM degree at a four-year university. The fundamental logic is to identify

a target group of "STEM qualified" two-year college students. Because it would be costly and undesirable to shift two-year college students to universities who are underprepared or uninterested in STEM fields, I focus only on students whose observable characteristics suggest they are reasonably likely to succeed in a STEM field. The term "STEM qualified" broadly reflects academic preparation and interest in STEM fields.

Specifically, I use a logistic regression to predict STEM degree completion within six year after enrollment into a four-year university as follows:

$$Y_i^* = \boldsymbol{X_i \beta_1} + \varepsilon_i \qquad (2.1)$$

Here Y_i^* is the latent utility of completing a STEM degree within six years, versus not completing a STEM degree, for student *i* who first enrolled in a four-year university. If Y_i^* >0, student *i* would complete a STEM degree within six years, denoted by $Y_i = 1$. $\boldsymbol{X_i}$ is a vector of control variables, which I divide into four conceptual groups. The first set of control variables includes student demographic characteristics and basic academic indicators during the base year of the ELS survey and includes race-ethnicity and gender indicators, 10th grade standardized math and reading test scores, and the 9th grade high school GPA. The second set of controls focuses on each student's family background information, including mother's highest level of education, father's highest level of education, and family income. Third set of controls includes information about each student's high school, including enrollment, number of FTE teachers, the share of students eligible for free and reduced-price lunch percent, the minority student share, and high school urbanicity. The last group of control variables includes other pre-entry academic qualifications of the student based on data from the late high school years, including standardized math test scores in the 12th grade, math course-taking in high school, ACT

math test scores, and ACT composite scores. ϵ_i is the error term. I include these four sets of control variables sequentially into the model. My preferred specification includes all of the controls, and best captures each student's preparation for STEM coursework in college at the end of high school, which is the hypothetical intervention point.

After I estimate the model, I use the fitted values $\hat{P}_i = Pr(Y_i = 1|\boldsymbol{X_i})$ as the predicted likelihoods of completing a STEM degree conditional on pre-entry student characteristics and qualifications among four-year-university entrants.

Next, I apply the parameter estimates from equation (2.1) to the profiles of two-year college students. The predicted value $\hat{P}_i^{2yr}$ is the likelihood that two-year college student *i* would complete a STEM degree at a four-year university if she was nudged from the two-year to four-year sector at entry, under the maintained assumptions (namely selection on observables into college sector).

With the predicted values $\hat{P}_i^{2yr}$ for each two-year college students, I then identify a threshold value, $\tilde{\hat{P}}$, above which students are nudged into four-year universities. Qian and Koedel (2020) use the median predicted likelihood of STEM degree completion among all initial four-year university STEM entrants as the threshold to identify the target group. In the Missouri sample at this threshold, about 7.4 percent community college students are identified as "STEM qualified" and nudged into the four-year sector. Because the ELS data do not provide credible information on the intended major, I cannot exactly replicate their approach. Instead, I use the 60th percentile of the predicted likelihood of STEM success among all four-year university entrants as the threshold (without conditioning on the intended major). This threshold also results in 7.4 percent of two-year college students

being identified as "STEM qualified," aligning my work in this respect with Qian and Koedel (2020).

To summarize, I identify a two-year college student as "STEM qualified" if her predicted likelihood of STEM degree completion, $\hat{P}_i^{2yr}$, is greater than $\tilde{\hat{P}}$, where $\tilde{\hat{P}}$ is at the 60th percentile of the predicted likelihood of STEM degree completion among university entrants in the ELS data ($\tilde{\hat{P}} \approx 0.10$). Like in Qian and Koedel (2020), I consider the sensitivity of my findings to modifications of the threshold below.

Under the assumptions of selection on observables and that all students who I intervene with will change their enrollment behavior, the total number of predicted four-year STEM degrees among STEM-qualified two-year college students is given by:

$$\theta_{STEM}^{2yr} = \sum_{i=1}^{N_s^{2yr}} \hat{P}_i^{2yr} \qquad (2.2)$$

where N_s^{2yr} is the number of STEM-qualified two year college students, and also the number of students nudged into four-year colleges. Redefining this basic summation over specific demographic groups, I can also calculate the numbers of STEM degrees produced for different types of students to inform the diversity question. I bootstrap my entire procedure 500 times to obtain error bands for my estimates; the bootstrapping procedure accounts for error throughout the process.

4. Primary Results

Table 2.2 shows the raw logit coefficients and empirical bootstrapped 95% confidence intervals for equation (2.1) estimated on the four-year university sample using different control variables.[20] In column (1), I only control for student characteristics and

[20] For brevity, I only show results for key variables. See Appendix Table C2 for complete results.

academic indicators during the base survey year. This is the most basic specification. As expected, when I include more control variables the explanatory power rises as indicated by the pseudo R-squared values over columns (1) to (4). The model with full control variables in column (4) is the preferred specification because it includes the most comprehensive information about students and their academic skills and choices leading up to the hypothetical intervention point (i.e., after high school). My discussion focuses primarily on the findings from this model.

Overall, the results are intuitive and consistent with past research showing that students who succeed in STEM fields are positively selected (Arcidiacono and Koedel, 2014; Arcidiacono et al., 2016). In column (1) – (3), the 10th grade math test score and 9th grade GPA are very strong predictor of STEM success. When I control additional test scores and math course-taking behavior in column (4), the coefficients on the 10th grade math test score and 9th grade GPA are attenuated because more updated information about academic performance is included (which is highly correlated with the early-high-school controls).[21] In terms of demographics, the familiar gender difference in STEM success is clearly present in our data (also see Kahn and Ginther, 2017; Qian and Koedel, 2020). Similarly, it is well-documented that after conditioning on pre-entry academic qualifications, underrepresented minority students are no less likely, or even more likely, than White students to complete a STEM a degree (Griffith, 2010; Qian and Koedel, 2020; Sass, 2015). In the ELS data, Black students in particular are conditionally more likely to complete STEM degrees.

[21] The coefficients for other math test scores are positive and significant. See appendix Table C2.

Table 2.3 shows summary statistics for two-year and four-year college students by STEM-qualified status. Because I set the STEM-qualified threshold at the 60th percentile of the distribution among four-year students, it is by construction that 40 percent of these students are STEM qualified. Reflecting their much weaker academic qualifications, just 7.4 percent of two-year college students are STEM qualified. Following from Table 2.2, in both college sectors STEM-qualified students possess much higher academic qualifications than their non-STEM-qualified counterparts.

Demographically, STEM-qualified students are much more likely to be male and less likely to be Black or Hispanic than non-qualified students. The gender gap between columns (1) and (2), and columns (3) and (4), reflects the large negative coefficient for female students in Table 2.2 and is consistent with the well-documented female preference for non-STEM fields. However, the racial/ethnic differences emerge due to differences in pre-entry academic qualifications, which are much lower on average for Black and Hispanic students (recall that the coefficients on the Black and Hispanic student indicators in Table 2.2 are 0.700 and 0.075, respectively, in the preferred specification). Although there are more underrepresented minority students in two-year colleges, many do not possess academic qualifications that suggest they would be likely to complete a STEM degree at a four-year university.

Figure 2.1 summarizes the academic qualifications of two-year and four-year students in the ELS data to illustrate the substantial differences across sectors.

Also note that even conditional on crossing the STEM-qualified threshold, four-year university students have stronger academic qualifications. This can be seen by comparing columns (1) and (3) in Table 2.3—e.g., see the 10th grade test scores and GPAs.

This reflects the fact that the distribution of academic readiness at two-year colleges, as measured by the observable academic indicators, is to the left of the distribution of academic readiness among four-year college students. The implication is that among students above the STEM-qualified threshold, those at community colleges are closer to the threshold value, on average, than their four-year-college counterparts. Figure 2.2 shows the distribution of predicted likelihood of STEM success in four-year college for the STEM-qualified samples in each college sector. Although the difference between two groups is much smaller than in Figure 2.1, it is still substantial.

Table 2.4 shows the number of STEM degrees produced and the characteristics of STEM completers among STEM-qualified two-year college students as calculated by equation (2.2). For comparison, I show the descriptive statistics for observed STEM completers among four-year university students in column (2). Regarding the potential to expand the STEM pipeline, the number of STEM degree produced among the nudged sample of two-year college students is 58, with an empirical 95 percent confidence interval of 47 to 69 students. Noting that 820 STEM degrees were actually produced among four-year entrants, this number corresponds to an increase in STEM degree production of 7.1 percent. However, these estimates are gross—some of the students I would hypothetically nudge would end up getting STEM degrees anyway. Specifically, in the ELS data, among the 300 STEM-qualified two-year college students hypothetically nudged, I observe 40 students actually complete a STEM degree within six years via transfer to a four-year university. Removing these 40 students to arrive at an estimate of "net degrees produced," I conclude that the nudge of STEM-qualified community college students would generate

just 18 new STEM degrees, or a 2-percent increase over the production level at four-year universities already. Again, these estimates are under my strong upper-bound assumptions.

My estimates for the gross and net increases of STEM degrees produced of 7.1 and 2.0 percent, respectively, are lower than in Qian and Koedel (2020), who estimate analogous increases of about 9.6 and 6.4 percent in Missouri. A key reason for the difference is that my models include much richer information about student skills and interests in high school. In their analysis, these variables are unobserved and thus materialize in the form of omitted variables bias. They show that under parameterized assumptions of the degree on selection into college sector on unobservables, their estimates can fall dramatically. This insight is supported by my analysis, which reveals some of the previously unobserved information via the rich ELS data and shows that the boost to STEM degree production that is possible is very small.

Table 2.4 also documents the characteristics of STEM completers from each sector. Column (1) of Table 2.4 shows that the academic qualifications of STEM completers from the nudged sample of two-year college students, on average, are similar to but below their counterparts who start at universities: their average 10th grade math test scores are 0.41, versus 0.51 for university entrants, and their average 9th grade GPAs are 3.38 vs 3.49. Regarding the diversifying objective, there is no obvious diversity improvement by race-ethnicity among STEM degree recipients in the two-year college sample. The share of Black students is 0.07 among two-year college students who are nudged, slightly below the 0.08 at four-year colleges; the analogous shares of Hispanic students are 0.08 and 0.06, respectively. The new STEM graduates from two-year colleges are also even more male-dominated than their four-year counterparts.

My findings with respect to diversity are broadly similar to the findings of Qian and Koedel (2020). Because the ELS sample size is relatively small, the confidence intervals on the shares of minority students are quite large, which prevents me from ruling out the possibility that there might be increases of the diversity among STEM degree recipients based on the ELS data. But noting the Missouri findings are also similar, and with much tighter error bands, it is reasonable to conclude that there is no evidence to suggest that the STEM pipeline at four-year universities can be meaningfully diversified by tapping into the community college population.

5. Robustness and Extensions

5.1 Demographic Predictors of STEM Success

First, I test the sensitivity of the diversity findings to excluding race/ethnicity and gender indicators in equation (2.1). Table 2.5 shows results. I also repeat the results from the preferred specification in column (1) and (2) for comparison. Overall, the number of STEM-qualified two-year college students and STEM degrees produced are very stable regardless of whether I include the race/ethnicity and gender indicators (see the bottom row of the table). The share of female students increases significantly in column (3) and (4), showing that without accounting for the general female preference for non-STEM majors, many more female students are predicted to complete STEM degrees. This result only makes sense if one believes that female students who attend two-year colleges have fundamentally different preferences for STEM education than their four-year counterparts, but I view this as unlikely. When looking at race/ethnicity, there is a marked decrease in the share of Black students and nominal but insignificant increase in the share of Hispanic students—these findings are consistent with expectations given the predictive coefficients

in Table 2.2.

5.2 Sensitivity to Different Control Variables

The second robustness test is using different sets of control variables leading up to the full specification: (1) students' basic characteristics and academic qualifications from the base year of the ELS survey only, (2) these variables, plus family information, (3) these variables, plus add high school information. Table 2.6 shows the results when I keep the 60th percentile nudge threshold.

There are no (meaningful) diversity implications of using different control-variable sets, but when I include fewer control variables, I identify more two-year college students as STEM-qualified. Figure 2.3 visualizes the differences. With fewer predictors, the distribution of predicted likelihood among two-year college students is closer to the distribution among four-year students and more STEM-qualified students corresponds to more STEM degrees produced. That said, even using the sparsest models in columns (1) and (2) of Table 2.2, and still under the maintained upper-bound assumptions, the number of STEM degrees produced is only 82. This corresponds to an increase in STEM degree production of 10 percent of the production level at four-year universities, which is similar to the 9.6 percent estimate in Qian and Koedel (2020). This exercise helps to explain the lower effects of the hypothetical intervention on the STEM pipeline in the ELS data—the Missouri data used by Qian and Koedel (2020) are much more limited in terms of the richness of the control variables.

5.3 Sensitivity to the Nudge Threshold

Next, I adjust the intervention threshold that determines STEM-qualified status. As mentioned above, thus far I have used the 60th percentile of the predicted likelihood of

completing a STEM degree within six years among four-year students as the threshold. This allows me to match the 7.4 percent STEM-qualified rate among the two-year sample used by Qian and Koedel (2020). Table 2.7 shows the sensitivity of my estimates to moving the nudge threshold between the 50th and 70th percentiles of the distribution, in 5-unit intervals. Not surprisingly, the lower the nudge threshold, the more two-year college students are identified as STEM-qualified, but the rate at which nudged students complete STEM degrees also declines. For example, when I move the nudge threshold from the 50th percentile to 70th percentile of the four-year distribution, the number of STEM-qualified two-year college students decreases from 450 to 180 (a 60 percent decrease), the number of STEM degrees produced decreases from 70 to 44 (a 37 percent decrease), while the degree conversion rate increases from about 16 percent (70/450) to 24 percent (44/180). Increasing the nudge threshold also decreases the share of female students, Black students and Hispanic students. As discussed by Qian and Koedel (2020), determining the appropriate nudge threshold is a normative question. It requires a judgement about the value of degrees produced, the diversity of STEM degree recipients, and the cost of failed interventions (i.e., students who enroll in a four-year university but fail to obtain a degree).

5.4 Machine Learning Methods

The many variables available in the ELS data suggests that utilizing machine learning techniques may improve my predictions. In this section I briefly implement three alternative methods to replace the logistic regression model: neural network, random forest, and lasso. Parameters of these models are tuned based on five-fold cross validation. The procedure is the same as in the main analysis: first estimate the model; then apply the model to two-year college students and use the 60th-percentile threshold among four-year college

students to decide which two-year students are nudged. Table 2.8 shows the number of STEM degrees produced and degree-recipient characteristics using each approach. Overall, the results are qualitatively similar to main findings, suggesting that main results using logistic regressions are broadly robust. A modest exception is in column (3) and (4)—when using the random forest method, the share of female students is higher and the share of Black students is lower than in the main findings or either of the other two machine learning techniques. This is because the random forest prediction algorithm does not put much weight on gender and race-ethnicity indicators in the predictions. Also note that the Lasso method identifies the fewest students as STEM-qualified and indicates an even lower number of STEM degrees produced than the other methods.

5.5 Selection on Unobservables

A maintained assumption throughout my analysis is that two-year college students would perform as well as observationally similar university students if they enrolled in a university initially. In other words, I assume selection on observables into college level. This assumption is unlikely to hold and almost certainly leads to an overstatement of the predicted success likelihoods of two-year college students. In turn, this implies that my estimates of STEM degree produced by my hypothetical intervention are overstated.

The differences in my estimates across specifications in Table 2.6 gives a sense of the likely implications of omitted variables. When I use basic specification in column (1) in Table 2.2, the additional controls are the unobservables. By comparing the estimated numbers of STEM degrees produced in Table 2.6 with the estimates in Table 2.4, one can clearly see that including more control variables reduces the upward bias caused by unobservables.

Even in my full specification, there is reason to be concerned there is remaining unobserved selection. To assess its potential impact, I perform a bounding exercise to quantitively estimate how varying degrees of unobserved selection would impact the results (Oster, 2019; Rosenbaum, 2002). First, I define the magnitude of selection on *observables* between two-year and four-year STEM qualified students as the average difference in the likelihood of STEM degree completion between the two groups. Column (3) in Table 2.9 shows that this difference, $\bar{\hat{p}}_i^{2yr} - \bar{\hat{p}}_i^{4yr}$, is -0.04, indicating that two-year STEM-qualified students are four percentage points less likely to complete a STEM degree than four-year STEM-qualified students.[22]

If there is no selection on unobservables, as in the main settings, the number of STEM degrees produced would be 58. If I assume the selection on unobservables in the same direction as observed selection, which is intuitive and common in the literature, I can parameterize its potential impact on my results. Specifically, in Table 2.9 I consider scenarios where the range of unobserved selection is 50 to 300 percent as large as observed selection. Across these scenarios, the number of STEM degrees produced among the two-year college sample ranges from 24-53. Thus, if unobserved selection is assumed to be small the implications are modest; if unobserved selection is large, the effect is substantial. My access to the rich covariates in the ELS data gives reason for optimism that selection on remaining unobservables is modes in my data, although I cannot rule out the possibility that unobserved selection is larger, in which case my already small estimated impacts on the STEM pipeline would be even smaller.

[22] This degree of observed selection is same as in Qian and Koedel (2020)

5.6 A Less than Perfect Nudge

In main analysis I assume that all nudged students will respond as intended; i.e., all of them choose to enroll in four-year universities. This is a useful assumption for generating upper-bound estimates, but not realistic. From the previous literature, a low cost and easily implemented nudge—e.g., a text-message or email based intervention—can be expected to generate an average response rate in the range of 0-5 percent (Barr and Turner, 2018; Castleman and Page, 2015; Gurantz et al, 2020; Oreopoulos and Petronijevic, 2019). More substantive inventions that attach valuable incentives may generate larger responses, but even then, a 100-percent response rate is too high.

In this section I relax the "perfect nudge" assumption by instead assuming that just 5 or 10 percent of students respond as intended. I consider two different scenarios for each response rate. In the first, responding students are a random sample of the STEM-qualified group and in the second, they are the students who are the most likely to succeed in a four-year STEM program (i.e., they are positively-selected among the STEM-qualified group). Intuitively, and regardless of whether the nudged students are selected at random or positively selected, when I reduce the nudge success rate it causes a dramatic decline in the number of STEM degrees produced.[23]

6. Conclusion

I use the nationally-representative ELS data from NCES to examine the potential for expanding and diversifying STEM degree production in four-year universities by tapping into the population of academically-qualified two-year college students. Consistent with findings from Missouri in Qian and Koedel (2020), I find that redirecting two-year

[23] In terms of average characteristics among completers, because there are so few completers in these scenarios my estimates are too imprecise to draw meaningful inference.

college students into four-year universities can only modestly increase STEM degree production. Using the preferred specification, and under assumptions that surely result in my estimates being high upper bounds, I find that re-directing STEM-qualified students from two-year colleges to universities could increase the number of university STEM degrees produced by around 2 percent, on net.

I also find no evidence that tapping into the two-year college population can improve the diversity of STEM degrees produced at four-year universities. The Hispanic and Black student shares in two-year colleges exceed those in four-year colleges, but (like other two-year college students) most do not possess academic qualifications indicating they could succeed in a university STEM program. I find no scope for increasing gender diversity of STEM degree production by tapping into two-year colleges. As explained in Qian and Koedel (2020), this result is partly tautological because of the assumption that female aversion to STEM, conditional on observable academic qualifications, is similar among two-year and four-year college students. But it is also true that there is a larger gap in academic qualifications between students who attend two-year and four-year colleges among women than men, which also contributes to this result.

My analysis supports the argument by Qian and Koedel (2020) that interventions targeted toward two-year college students are unlikely to alter macro-level features of STEM degree production at universities.

Table 2.1: Summary statistics for two-year and four-year college entrants overall and by STEM exit conditions.

	Four-year University		Two-year College
	(1)	(2)	(3)
	Analytic Sample	STEM Completers	Analytic Sample
Female	0.55	0.40	0.54
	(0.50)	(0.49)	(0.50)
Asian	0.12	0.21	0.10
	(0.32)	(0.41)	(0.29)
Black	0.11	0.08	0.13
	(0.32)	(0.27)	(0.34)
Hispanic	0.09	0.06	0.19
	(0.29)	(0.24)	(0.39)
White	0.63	0.61	0.53
	(0.48)	(0.49)	(0.50)
Other Race	0.05	0.04	0.05
	(0.22)	(0.20)	(0.22)
10th Grade Math Test Score	0.06	0.51	-0.53
	(0.71)	(0.63)	(0.69)
10th Grade Read Test Score	-0.05	0.19	-0.62
	(0.71)	(0.65)	(0.70)
9th Grade GPA	3.15	3.49	2.54
	(0.65)	(0.47)	(0.70)
N	6820	820	4020

Notes: Table shows means and standard deviations (in parenthesis) for four-year university students and community college students. For brevity, here only shows key variables. See appendix Table C1 for the complete version. All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Table 2.2: Results from predictive logistic regression of STEM degree completion among four-year college entrants.

	(1) Graduate With STEM	(2) Graduate With STEM	(3) Graduate With STEM	(4) Graduate With STEM
Female	-0.727***	-0.724***	-0.726***	-0.667***
	[-0.888,-0.561]	[-0.875,-0.568]	[-0.905,-0.565]	[-0.84,-0.508]
Asian	0.623***	0.561***	0.566***	0.350***
	[0.411,0.823]	[0.342,0.768]	[0.307,0.782]	[0.062,0.6]
Black	0.578***	0.562***	0.606***	0.700***
	[0.273,0.856]	[0.25,0.853]	[0.252,0.931]	[0.331,1.093]
Hispanic	0.053	0.056	0.035	0.075
	[-0.289,0.35]	[-0.272,0.376]	[-0.303,0.352]	[-0.243,0.431]
Other Race	0.037	0.008	0.015	0.014
	[-0.380,0.393]	[-0.441,0.357]	[-0.367,0.388]	[-0.416,0.374]
10th Grade Math Test Score	0.916***	0.891***	0.888***	0.113
	[0.747,1.103]	[0.718,1.066]	[0.733,1.067]	[-0.134,0.334]
10th Grade Read Test Score	-0.207***	-0.221***	-0.238***	-0.287***
	[-0.363,-0.039]	[-0.379,-0.072]	[-0.388,-0.068]	[-0.467,-0.102]
9th Grade GPA	1.065***	1.067***	1.096***	-0.112
	[0.880,1.248]	[0.871,1.261]	[0.904,1.29]	[-0.491,0.279]
Basic Information	X	X	X	X
Family Background		X	X	X
High School information			X	X
Math Course Taking and ACT Tests				X
N	6820	6820	6820	6820
Pseudo R-squared	0.131	0.137	0.141	0.184
	[0.114,0.15]	[0.122,0.155]	[0.124,0.159]	[0.163,0.205]

Notes: The regression output corresponds to equation (2.1) in the main text. Bootstrapped mean estimates and 95 percent confidence intervals are reported. ***p<0.01, **p<0.05, *p<0.10. For brevity, here only shows key variables. See appendix Table C2 for the complete version. All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Table 2.3: Summary statistics by STEM qualified status at two-year and four-year colleges.

	Four Year University		Community College	
	(1)	(2)	(3)	(4)
	STEM qualified	Not STEM qualified	STEM qualified	Not STEM qualified
Female	0.41	0.64	0.32	0.56
	[0.37,0.45]	[0.61,0.66]	[0.24,0.4]	[0.54,0.57]
Asian	0.20	0.06	0.26	0.08
	[0.18,0.22]	[0.05,0.07]	[0.2,0.33]	[0.07,0.09]
Black	0.08	0.14	0.07	0.14
	[0.05,0.1]	[0.12,0.15]	[0.03,0.12]	[0.12,0.15]
Hispanic	0.06	0.11	0.08	0.20
	[0.04,0.08]	[0.1,0.13]	[0.04,0.15]	[0.19,0.21]
White	0.63	0.64	0.56	0.53
	[0.59,0.66]	[0.61,0.66]	[0.48,0.64]	[0.51,0.55]
Other Race	0.04	0.05	0.03	0.05
	[0.03,0.05]	[0.04,0.07]	[0.0,0.07]	[0.05,0.06]
10th Grade Math Test Score	0.52	-0.25	0.29	-0.59
	[0.49,0.56]	[-0.27,-0.22]	[0.18,0.39]	[-0.61,-0.57]
10th Grade Read Test Score	0.23	-0.23	-0.20	-0.65
	[0.19,0.27]	[-0.27,-0.2]	[-0.32,-0.09]	[-0.67,-0.63]
9th Grade GPA	3.53	2.90	3.33	2.48
	[3.50,3.56]	[2.88,2.93]	[3.22,3.44]	[2.46,2.50]
N	2730	4090	300	3720
	[2730,2730]	[4090,4090]	[250,350]	[3670,3770]

Notes: Table shows means and 95 percent bootstrapped confidence intervals (500 repetitions) for university students and two-year college students by STEM qualified status. All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Table 2.4: Summary statistics for two-year college students who are predicted to complete STEM degrees at four-year colleges compared to observed STEM completers at four-year colleges.

	(1) Graduate with STEM	(2) STEM completers at four-year universities (from Table 2.2)
Share Female	0.30	0.39
	[0.21,0.38]	[0.36,0.42]
Share Asian	0.31	0.21
	[0.23,0.39]	[0.19,0.24]
Share Black	0.07	0.08
	[0.03,0.11]	[0.06,0.1]
Share Hispanic	0.07	0.06
	[0.03,0.12]	[0.05,0.08]
Share White	0.54	0.61
	[0.45,0.63]	[0.57,0.64]
Share Other Race	0.02	0.04
	[0.00,0.05]	[0.03,0.05]
Avg. 10th Grade Math Test Score	0.41	0.51
	[0.27,0.55]	[0.46,0.55]
Avg. 10th Grade Read Test Score	-0.19	0.19
	[-0.34,-0.06]	[0.15,0.24]
Avg. 9th Grade GPA	3.38	3.49
	[3.26,3.49]	[3.46,3.53]
Number of STEM degrees (gross)	58	820
	[47,69]	[770,870]
Number of STEM degrees (net)	17	-
	[2,31]	

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions for nudged two-year college students. Column (2) reports means and standard deviations for actual STEM completers among initial university entrants. The net number of STEM degrees equals subtracting the number of STEM-qualified two-year college students observed transferring into university and obtaining a STEM degree within 6 years directly in the data from the number of gross STEM degrees. All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Table 2.5: Robustness of findings to dropping race-gender indicators in the model that predicts STEM four-year degree completion.

	Main Settings		No Race-Gender Indicators	
	(1)	(2)	(3)	(4)
	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM
Share Female	0.32	0.30	0.51	0.50
	[0.24,0.4]	[0.21,0.38]	[0.44,0.57]	[0.42,0.58]
Share Asian	0.26	0.31	0.23	0.28
	[0.2,0.33]	[0.23,0.39]	[0.18,0.28]	[0.21,0.35]
Share Black	0.07	0.07	0.03	0.03
	[0.03,0.12]	[0.03,0.11]	[0.01,0.05]	[0.01,0.05]
Share Hispanic	0.08	0.07	0.10	0.09
	[0.04,0.15]	[0.03,0.12]	[0.06,0.15]	[0.05,0.13]
Share White	0.56	0.54	0.61	0.58
	[0.48,0.64]	[0.45,0.63]	[0.53,0.67]	[0.5,0.67]
Share Other Race	0.03	0.02	0.03	0.03
	[0.0,0.07]	[0.0,0.05]	[0.02,0.06]	[0.01,0.05]
Avg. 10th Grade Math Test Score	0.29	0.41	0.33	0.45
	[0.18,0.39]	[0.27,0.55]	[0.23,0.43]	[0.31,0.59]
Avg. 10th Grade Read Test Score	-0.20	-0.19	-0.17	-0.18
	[-0.32,-0.09]	[-0.34,-0.06]	[-0.29,-0.07]	[-0.32,-0.05]
Avg. 9th Grade GPA	3.33	3.38	3.37	3.41
	[3.22,3.44]	[3.26,3.49]	[3.25,3.47]	[3.28,3.53]
Number of students or degrees (gross)	300	58	300	57
	[250,350]	[47,69]	[250,350]	[47,70]

Notes: Table reports averages and 95 percent confidence intervals of 500 bootstrap predictions. All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Table 2.6: Findings using different model specifications

	Basic Control		Family Background		High School Info	
	(1) STEM qualified	(2) Graduate with STEM	(3) STEM qualified	(4) Graduate with STEM	(5) STEM qualified	(6) Graduate with STEM
Share Female	0.23	0.20	0.25	0.22	0.25	0.22
	[0.15,0.33]	[0.13,0.3]	[0.17,0.33]	[0.14,0.29]	[0.16,0.33]	[0.14,0.3]
Share Asian	0.31	0.36	0.30	0.36	0.31	0.36
	[0.23,0.39]	[0.27,0.46]	[0.23,0.38]	[0.26,0.45]	[0.24,0.4]	[0.28,0.46]
Share Black	0.05	0.04	0.05	0.04	0.05	0.04
	[0.01,0.09]	[0.01,0.08]	[0.02,0.10]	[0.01,0.09]	[0.01,0.09]	[0.01,0.08]
Share Hispanic	0.07	0.06	0.08	0.06	0.08	0.07
	[0.02,0.15]	[0.02,0.13]	[0.02,0.13]	[0.02,0.12]	[0.03,0.14]	[0.02,0.13]
Share White	0.56	0.52	0.55	0.52	0.54	0.51
	[0.45,0.65]	[0.42,0.62]	[0.46,0.64]	[0.43,0.61]	[0.45,0.63]	[0.42,0.61]
Share Other Race	0.02	0.02	0.02	0.02	0.02	0.02
	[0.0,0.06]	[0.0,0.05]	[0.0,0.06]	[0.0,0.05]	[0.0,0.06]	[0.0,0.05]
Avg. 10th Grade Math Test Score	0.46	0.55	0.38	0.47	0.37	0.46
	[0.35,0.56]	[0.44,0.68]	[0.28,0.47]	[0.35,0.59]	[0.26,0.47]	[0.34,0.59]
Avg. 10th Grade Reading Test Score	-0.12	-0.11	-0.16	-0.15	-0.18	-0.17
	[-0.26,0.03]	[-0.25,0.06]	[-0.26,-0.06]	[-0.27,-0.03]	[-0.29,-0.06]	[-0.30,-0.03]
Avg. 9th Grade GPA	3.48	3.52	3.43	3.48	3.43	3.47
	[3.38,3.59]	[3.43,3.62]	[3.32,3.53]	[3.37,3.58]	[3.32,3.53]	[3.38,3.58]
Number of students or degrees (gross)	450	82	420	77	400	75
	[390,500]	[71,95]	[360,480]	[65,91]	[340,470]	[62,88]

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions using different control variables. All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Table 2.7: Findings using different nudge thresholds for identifying STEM-qualified two-year college students based on the percentile of the distribution among four-year entrants (the baseline case is at the 60th percentile).

	50th Percentile		55th Percentile		65th Percentile		70th Percentile	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM
Share Female	0.34	0.31	0.33	0.31	0.31	0.29	0.29	0.27
	[0.27,0.41]	[0.24,0.39]	[0.26,0.41]	[0.23,0.38]	[0.22,0.39]	[0.20,0.37]	[0.20,0.38]	[0.19,0.37]
Share Asian	0.22	0.28	0.24	0.29	0.28	0.32	0.30	0.34
	[0.18,0.28]	[0.21,0.35]	[0.19,0.3]	[0.23,0.37]	[0.20,0.35]	[0.24,0.41]	[0.22,0.38]	[0.25,0.44]
Share Black	0.08	0.07	0.07	0.07	0.06	0.06	0.06	0.06
	[0.04,0.13]	[0.04,0.12]	[0.04,0.12]	[0.03,0.12]	[0.02,0.11]	[0.02,0.11]	[0.02,0.11]	[0.02,0.11]
Share Hispanic	0.10	0.08	0.10	0.08	0.07	0.06	0.06	0.05
	[0.05,0.16]	[0.04,0.13]	[0.04,0.16]	[0.03,0.13]	[0.03,0.14]	[0.02,0.12]	[0.02,0.13]	[0.02,0.11]
Share White	0.55	0.54	0.56	0.54	0.56	0.54	0.56	0.53
	[0.48,0.63]	[0.45,0.62]	[0.48,0.63]	[0.45,0.63]	[0.48,0.65]	[0.44,0.63]	[0.46,0.65]	[0.42,0.64]
Share Other Race	0.03	0.02	0.03	0.02	0.02	0.01	0.01	0.01
	[0.01,0.06]	[0.0,0.05]	[0.0,0.07]	[0.0,0.05]	[0.0,0.06]	[0.0,0.05]	[0.0,0.06]	[0.0,0.04]
Avg. 10th Grade Math Test Score	0.18	0.33	0.24	0.37	0.34	0.46	0.40	0.50
	[0.09,0.28]	[0.2,0.46]	[0.14,0.34]	[0.24,0.51]	[0.23,0.46]	[0.3,0.6]	[0.25,0.53]	[0.33,0.67]
Avg. 10th Grade Read Test Score	-0.24	-0.21	-0.22	-0.20	-0.18	-0.18	-0.17	-0.17
	[-0.35,-0.14]	[-0.34,-0.1]	[-0.33,-0.11]	[-0.34,-0.08]	[-0.32,-0.05]	[-0.33,-0.04]	[-0.31,-0.04]	[-0.34,-0.02]
Avg. 9th Grade GPA	3.26	3.33	3.29	3.36	3.36	3.4	3.39	3.42
	[3.16,3.35]	[3.22,3.43]	[3.2,3.39]	[3.24,3.46]	[3.25,3.47]	[3.28,3.51]	[3.27,3.5]	[3.29,3.55]
Number of students or degrees (gross)	450	70	370	64	240	51	180	44
	[390,530]	[59,84]	[310,430]	[53,77]	[190,280]	[41,62]	[150,230]	[36,54]

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions under different nudge threshold: nudge threshold= 50th percentile, nudge threshold= 55th percentile, nudge threshold= 65th percentile, nudge threshold= 70th percentile. All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Table 2.8: Findings using machine learning techniques.

	Neural Network		Random Forest		Lasso	
	(1)	(2)	(3)	(4)	(5)	(6)
	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM
Share Female	0.30	0.29	0.46	0.44	0.32	0.30
	[0.23,0.37]	[0.21,0.36]	[0.39,0.52]	[0.37,0.51]	[0.24,0.4]	[0.22,0.39]
Share Asian	0.29	0.32	0.23	0.25	0.27	0.31
	[0.21,0.38]	[0.24,0.43]	[0.18,0.29]	[0.2,0.32]	[0.2,0.34]	[0.23,0.41]
Share Black	0.07	0.06	0.02	0.02	0.06	0.06
	[0.03,0.13]	[0.03,0.12]	[0.01,0.04]	[0.0,0.03]	[0.02,0.11]	[0.02,0.11]
Share Hispanic	0.09	0.07	0.09	0.08	0.08	0.06
	[0.04,0.15]	[0.03,0.13]	[0.05,0.13]	[0.05,0.12]	[0.03,0.14]	[0.03,0.11]
Share White	0.52	0.51	0.62	0.62	0.57	0.55
	[0.43,0.6]	[0.42,0.6]	[0.54,0.69]	[0.54,0.68]	[0.49,0.64]	[0.46,0.63]
Share Other Race	0.03	0.02	0.04	0.04	0.02	0.02
	[0.00,0.07]	[0.0,0.06]	[0.02,0.07]	[0.02,0.06]	[0.0,0.06]	[0.0,0.04]
Avg. 10th Grade Math Test Score	0.30	0.40	0.38	0.48	0.32	0.45
	[0.19,0.4]	[0.27,0.51]	[0.28,0.47]	[0.38,0.58]	[0.22,0.43]	[0.32,0.58]
Avg. 10th Grade Read Test Score	-0.21	-0.19	0.00	0.03	-0.17	-0.15
	[-0.34,-0.09]	[-0.32,-0.06]	[-0.10,0.10]	[-0.08,0.12]	[-0.28,-0.05]	[-0.29,-0.02]
Avg. 9th Grade GPA	3.33	3.38	3.39	3.44	3.37	3.43
	[3.21,3.43]	[3.25,3.48]	[3.30,3.49]	[3.35,3.53]	[3.27,3.46]	[3.32,3.53]
Number of students or degrees (gross)	300	60	350	59	280	39
	[250,360]	[48,74]	[300,410]	[51,68]	[230,330]	[26,53]

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions using machine learning techniques. All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Table 2.9: Summary statistics for STEM qualified two-year college students: different levels of selection on unobservables

(1) # of Nudged Community College Students	(2) Average STEM Completion Likelihood among four-year STEM-Qualified Entrants	(3) two-year Student Selection on Observables	(4) Selection on Unobservables	(5) Average Likelihood (community college)	(6) # of STEM Degrees Produced via Nudge (gross)
300	0.24	-0.04	0	0.20	58
300	0.24	-0.04	-0.02 (50%)	0.18	53
300	0.24	-0.04	-0.04 (100%)	0.16	48
300	0.24	-0.04	-0.08 (200%)	0.12	36
300	0.24	-0.04	-0.12 (300%)	0.08	24

Notes: Table describes the number of STEM degrees produced with different levels of selection on unobservables: 0%, 50%, 100%, 200% and 300% times selection on observables. Selection on observables value are calculated from the average likelihoods of graduating in STEM corresponding to equation (2.1). Column (5)= column (2)- column (3)- column (4). Column (6)= column (1) * column (5). All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.

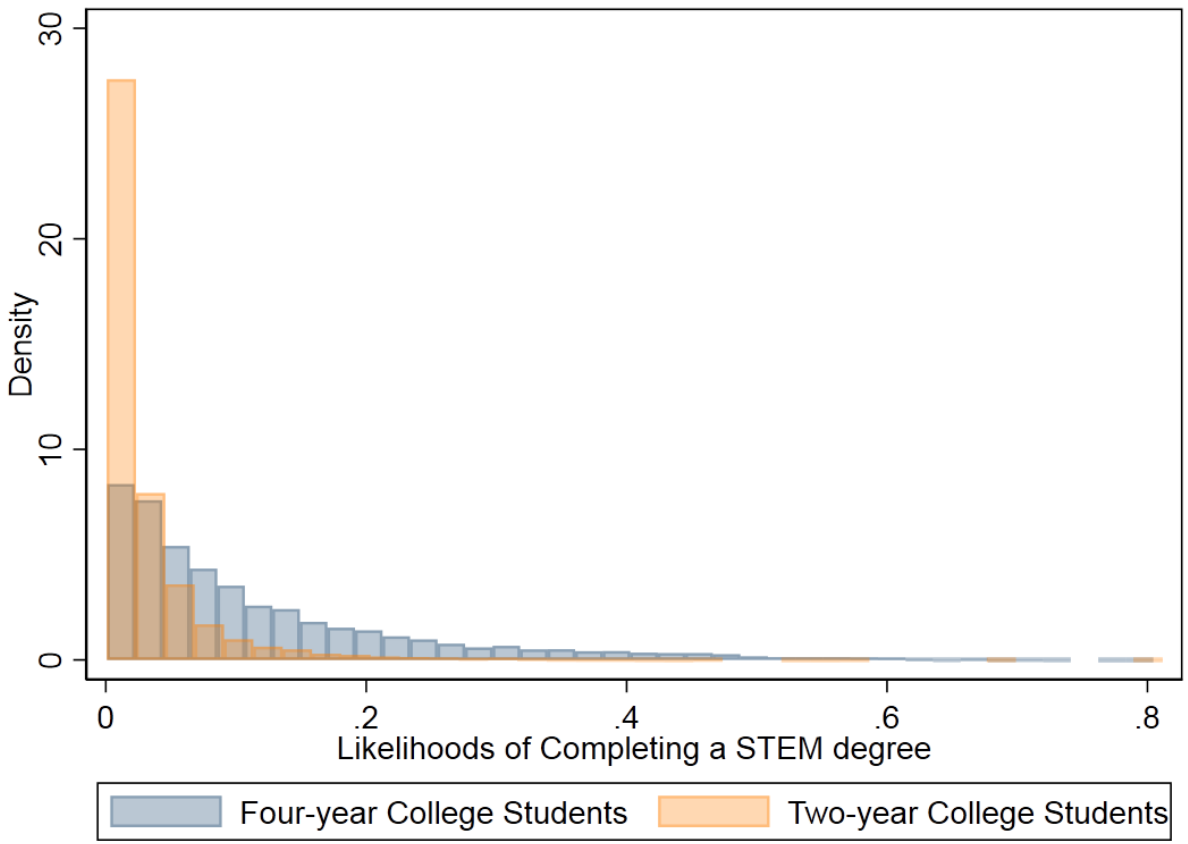
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Table 2.10: Summary statistics for STEM-qualified two-year college students with 5/10-percent nudge compliance rate.

	Randomly Selected 5 percent		Top 5 Percent		Randomly Selected 10 percent		Top 10 Percent	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM	STEM qualified	Graduate with STEM
Share Female	0.32	0.30	0.24	0.24	0.32	0.30	0.23	0.24
	[0.08,0.59]	[0.06,0.61]	[0.0,0.5]	[0.0,0.54]	[0.14,0.52]	[0.12,0.51]	[0.07,0.43]	[0.06,0.44]
Share Asian	0.26	0.31	0.53	0.57	0.26	0.31	0.45	0.49
	[0.07,0.5]	[0.05,0.59]	[0.23,0.82]	[0.25,0.87]	[0.1,0.44]	[0.11,0.53]	[0.22,0.67]	[0.26,0.7]
Share Black	0.07	0.06	0.08	0.07	0.07	0.06	0.07	0.07
	[0.0,0.21]	[0.0,0.24]	[0.0,0.28]	[0.0,0.24]	[0.0,0.18]	[0.0,0.19]	[0.0,0.18]	[0.0,0.18]
Share Hispanic	0.09	0.07	0	0	0.09	0.07	0.02	0.01
	[0.0,0.27]	[0.0,0.25]	[0.0,0.07]	[0.0,0.06]	[0.0,0.21]	[0.0,0.18]	[0.0,0.11]	[0.0,0.1]
Share White	0.56	0.53	0.38	0.35	0.56	0.54	0.46	0.43
	[0.29,0.81]	[0.24,0.82]	[0.08,0.69]	[0.07,0.67]	[0.36,0.74]	[0.33,0.76]	[0.25,0.70]	[0.22,0.68]
Share Other Race	0.03	0.02	0	0	0.03	0.02	0	0
	[0.0,0.14]	[0.0,0.11]	[0.0,0.0]	[0.0,0.0]	[0.0,0.12]	[0.0,0.08]	[0.0,0.0]	[0.0,0.0]
Avg. 10th Grade Math Test Score	0.28	0.4	1.03	1.09	0.29	0.41	0.79	0.88
	[-0.10,0.63]	[-0.14,0.87]	[0.45,1.46]	[0.40,1.55]	[0.05,0.53]	[0.07,0.76]	[0.39,1.12]	[0.38,1.24]
Avg. 10th Grade Read Test Score	-0.19	-0.18	-0.24	-0.27	-0.19	-0.19	-0.16	-0.19
	[-0.54,0.12]	[-0.68,0.2]	[-0.79,0.24]	[-0.83,0.23]	[-0.45,0.05]	[-0.55,0.12]	[-0.58,0.20]	[-0.59,0.18]
Avg. 9th Grade GPA	3.33	3.38	3.52	3.53	3.33	3.38	3.53	3.53
	[3.05,3.60]	[3.07,3.68]	[3.11,3.82]	[2.96,3.83]	[3.11,3.53]	[3.14,3.60]	[3.28,3.71]	[3.18,3.73]
Number of students or degrees (gross)	10	3	10	8	30	6	30	13
	[10,20]	[2,4]	[10,20]	[6,10]	[20,40]	[4,7]	[20,40]	[10,17]

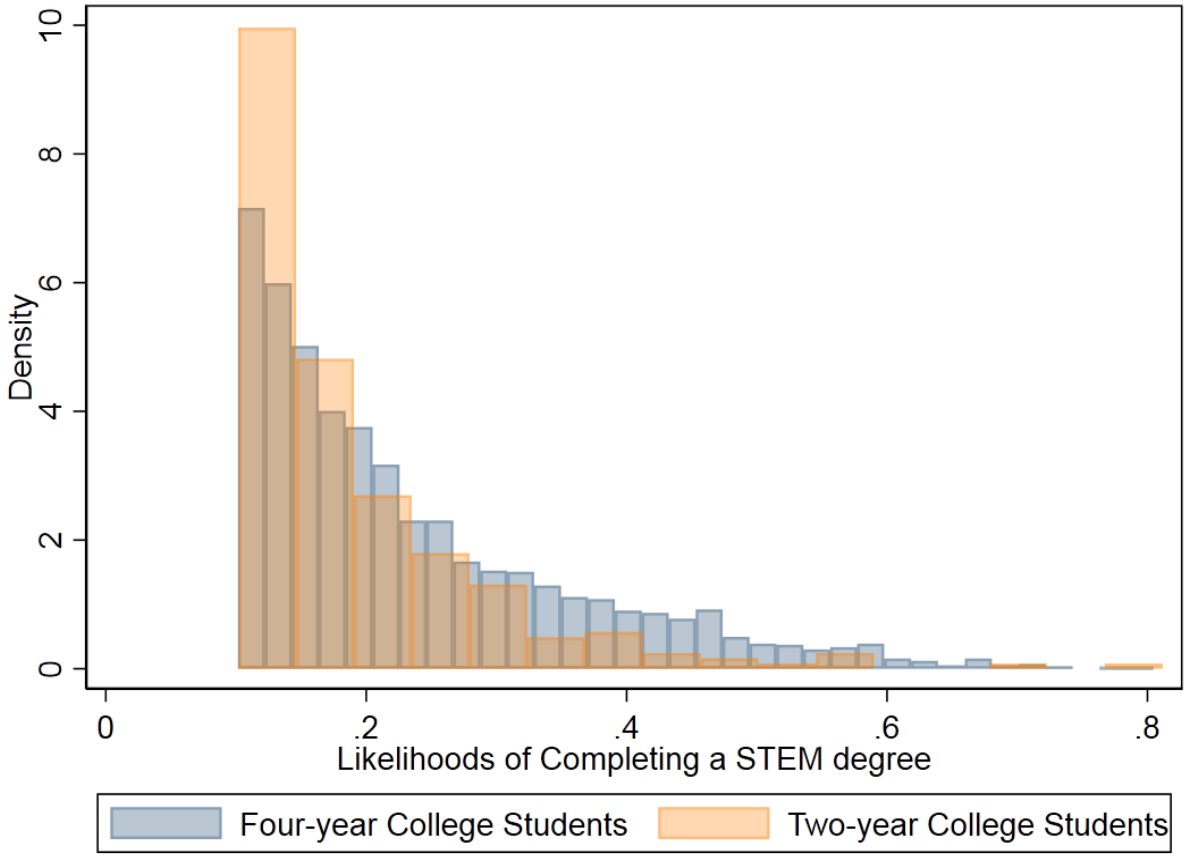
Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions when 5/10 percent STEM-qualified two-year college students actually choose to enroll at universities. Randomly selected 5/10 percent in column (1) and (2)/ (5) and (6); top 5/10 percent in terms of prediction likelihoods in column (3) and (4)/(7) and (8). All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Figure 2.1: Distributions of predicted likelihoods of completing a STEM degree for four-year and two-year college students



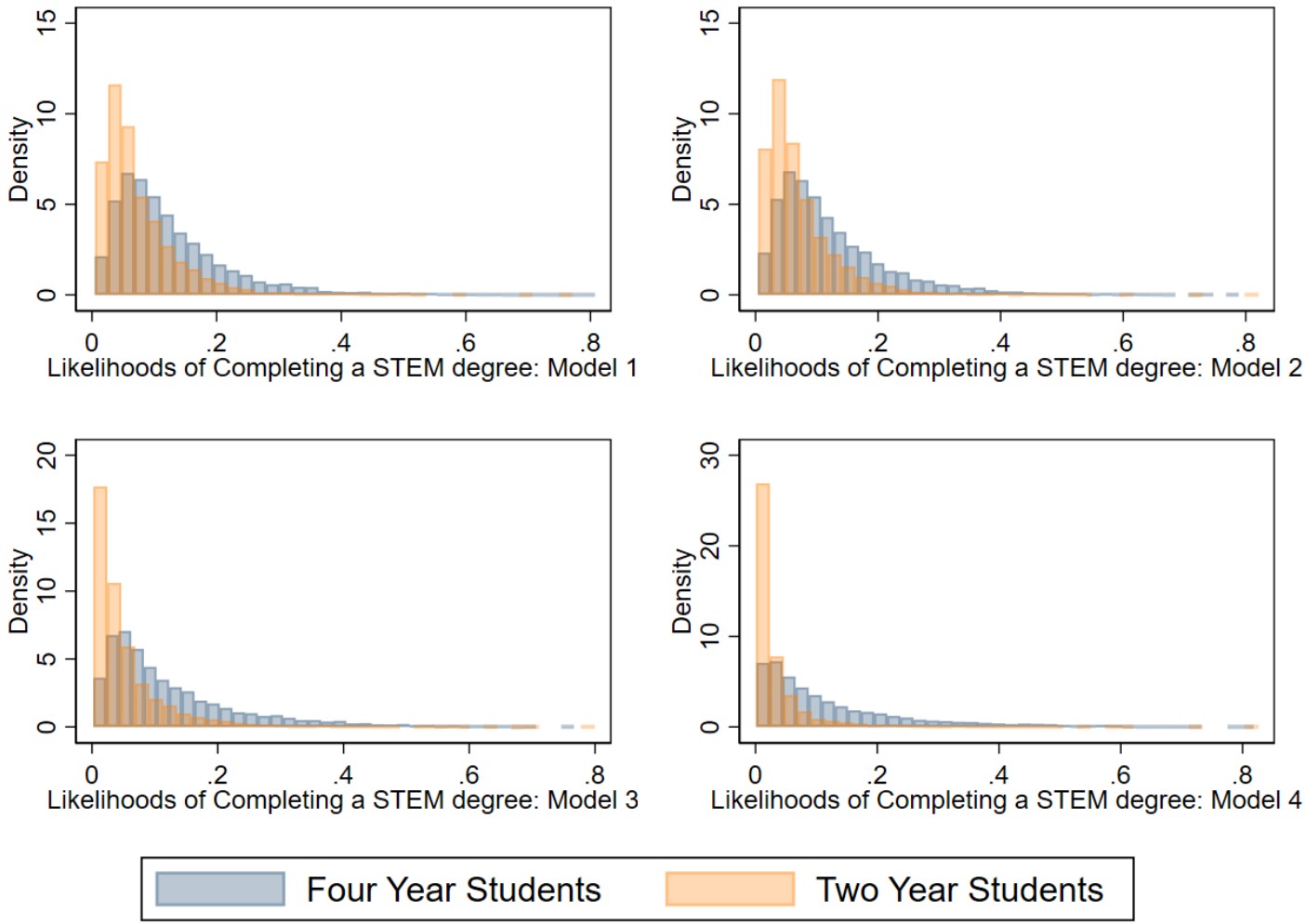
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Figure 2.2: Distributions of predicted likelihoods of completing a STEM degree for four-year and two-year college STEM qualified students



SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Figure 2.3: Distributions of predicted likelihoods of completing a STEM degree for four-year and two-year college students using different control variables



SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Chapter 3

The effect of local labor market conditions on postsecondary enrollment and degree completion

1. Introduction

According to classic human capital theory, when the labor market experiences downturns, the reduced likelihood of finding a job and lower wage expectations decrease the opportunity cost of attending college (Becker, 1964). As a result, people become more likely to pursue postsecondary education (Betts and McFarland, 1995; Clark, 2011). Consistent with theoretical predictions, during the great recession the unemployment rate reached a thirty-year high (Barr and Turner, 2015) and college enrollment increased significantly (Dunbar, et. al., 2011). After the great recession, from 2010 to 2017, undergraduate enrollment decreased by 7 percent during the economic recovery period (McFarland et. al., 2019).

More broadly, there is vast literature examining the effect of local labor market conditions on postsecondary enrollment at four-year universities and two-year community colleges. The results are consistent: most research finds that the effect of the unemployment rate on college enrollment is positive (Betts and McFarland, 1995; Hillman and Orians, 2013; Barrow and Davis, 2012; Johnson, 2013). Johnson (2013) finds differences by gender—specifically that female enrollment is countercyclical while male enrollment is procyclical. In a recent paper, Acton (2020) examines the effect of local mass layoffs on

enrollment by program type within the Michigan community college system. She finds declines in local employment induce students to choose vocationally oriented programs.

I contribute to the literature by studying the college-enrollment response to labor-market conditions in a recent data panel covering the years 2001-2017. I create a data panel of Metropolitan Statistical Areas (MSAs) containing enrollment and degree-completion data taken from the U.S. Department of Education Integrated Postsecondary Education Data Systems (IPEDS). I merge the postsecondary data with labor market data from two sources: Local Area Unemployment Statistics (LAUS) and the basic monthly Current Population Survey (CPS) (Ruggles et. al., 2020). The sample period is of sufficient length to capture the relationships between local labor market conditions and college enrollment before, during and after the Great Recession.

I leverage variation in unemployment rates within MSAs over time via two-way fixed effects models (with fixed effects for MSAs and years) to estimate the effect of the local unemployment rate on postsecondary education outcomes. I use fall semester full-time degree seeking and part-time degree seeking enrollment for three types of degree programs: four-year degrees (i.e., bachelor's degrees), two-year degrees (i.e. associate's degrees), and sub-two-year degree (i.e., certificates). I also estimate the effect of the local unemployment rate on the attainment of these degrees. In addition, I break down total enrollment and degree attainment by gender to test whether men and women respond to the labor market differently.

From 2000 to 2011, the number of associate's degrees and certificates conferred increased by 77 and 87 percent, respectively. During the same time period, the number of bachelor's degrees increased by only around 40 percent. Previous research focuses on one

specific level of postsecondary education or degree (e.g., Betts and McFarland, 1995; Hillman and Orians, 2013), or aggregates all enrollment and degrees together (e.g., Card and Lemieux,2000; Rivkin, 1995). However, at least in more recent data, the shift in the total postsecondary enrollment and attainment shares toward 2-year and sub-2-year colleges suggests that separately examining various college pathways is valuable. To the best of my knowledge, my study is the first to distinctively and simultaneously evaluate the effect of the unemployment rate on enrollment and attainment of bachelor's degrees, associate's degrees, and certificates.

I also improve on the previous literature by constructing more accurate measures of postsecondary enrollment by pathway. Common practice in the literature is to use the categorical designations from IPEDS to determine the enrollment for each type of degree program and education level (Foote and Grosz, 2019; Hillman and Orians, 2013). IPEDS designates a postsecondary institution as one of the following, based on the highest-level degree program that the institution offers: four-year college, two-year college, or less-than-two-year college. For example, if the highest degree program an institution provides is an associate's degree program, it is a two-year college. However, categorizing all enrollment based on the IPEDS designation is problematic. For instance, if a two-year community college introduces a single four-year degree program, its institution type in IPEDS switches from a two-year college to a four-year college. Using IPEDS' categorical designation would imply that all students who attend this institution shift from two-year enrollment to four-year enrollment despite the fact that most students would still be pursuing two-year degrees.

I develop a new degree-program-based method to measure enrollment that improves on the traditional method. My new method allows me to produce more informative enrollment measures for each institution based on institution-level information about recently conferred degrees. I show that using my measures reduces the number of unrealistic sample composition shifts in the data at the institution level, and generally decreases measurement error in the postsecondary data.

Previous studies investigating the relationship between labor market conditions and college enrollment mostly use state-level, or even national-level data. Using data at such a high level of aggregation could introduce aggregation bias and miss important variation in economic conditions locally. Following Hillman and Orians (2013), I use LAUS data from Bureau of Labor Statistics (BLS) to calculate unemployment rates at MSA level. In addition, there is an emerging literature studying how the gender-specific unemployment rate affects decision making (Qian, 2008; Lindo et. al., 2018). I allow for the possibility that men and women react to gender-specific local labor market conditions differently (Clark, 2011) by creating gender specific unemployment rates for MSAs from the CPS data. I then extend the literature by identifying gender specific response elasticities to changing economic conditions and testing for evidence of gender segregation of labor markets.

Corroborating most of the existing literature, I find that postsecondary enrollment is counter-cyclical. I further show that two-year degree enrollment and sub-two-year degree enrollment are more responsive to local employment conditions than four-year degree enrollment. There is suggestive evidence that an increase in the unemployment rate also affects degree completions, although statistical imprecision prevents me from drawing

strong inference in this regard. In terms of associate's degree enrollment, men are more elastic than women in their response to the unemployment rate, but I find no evidence of gender heterogeneity in responsiveness for other types of enrollment. There is little evidence to reject the null hypothesis of no gender segregation in the labor market.

The remainder of the paper is organized as follows. Section 2 presents the construction of the dataset and basic descriptive statistics. Methodology and estimation results are shown in section 3 and section 4, and I conclude in section 5.

2. Data

I construct my analytic data panel using two datasets: an education dataset and a labor-market dataset. The education dataset contains enrollment and degree attainment information from IPEDS. IPEDS provides detailed information about all postsecondary institutions that receive federal financial assistance, including enrollment, degree completion, address information, etc. The local labor market dataset consists of unemployment rates and gender specific unemployment rates from the BLS and CPS.

I measure full-time and part-time degree seeking enrollment and degrees awarded for all postsecondary institutions in the US and further break down enrollment and degree receipt by program type. I define three types of programs: (1) four-year degree programs, i.e., for bachelor's degree seekers, (2) two-year degree programs, i.e., for associate's degree seekers, and (3) sub-two-year degree programs, i.e., for seekers of sub-2-year certificates. In addition, I break down the enrollment and degree attainment by gender. With MSA codes provided by IPEDS for each institution, I aggregate enrollment and degree attainment numbers to the MSA level.

As noted above, past studies of postsecondary enrollment (not limited to studies investigating the effect of the unemployment rate), have primarily relied on a categorical "institution based" definition of enrollment from IPEDS. With this approach, researchers identify each postsecondary institution by type first—i.e., four-year universities, community colleges, etc.—then extract the enrollment information and assign all of the enrollment at the level of the institutional categorization. This approach can be problematic because IPEDS uses coarse categories for institutions—returning to the example from above, if an institution provides any bachelor's degree programs, IPEDS designate it as a four-year university even if very few students are enrolled in four-year degree programs.

An observable consequence of using the IPEDS categorizations in this way is that when an institution adds a new degree program (or drops a program), the shift in status can result in large and inaccurate shifts in student enrollment. For example, consider the simple task of capturing state or MSA-level enrollment in bachelor's degree programs. The rise in community colleges offering at least some 4-year degree programs over time introduces excess variation in 4-year enrollment that is an artifact of the IPEDS coding structure only. This is an important problem in the data: during my data panel from 2001-2017, 1057 out of 7676 postsecondary institutions changed their institution type at least once.

One of the major goals of this paper is to evaluate different postsecondary sectors at the degree-program level, thus the traditional "institution based" method for measuring the type of enrollment is a poor fit. Therefore, I develop a new degree-program-based method that calculates enrollment continuously for each institution based on information about degrees conferred. Specifically, I combine present year degree completion data with data from the previous two years to calculate three-year average shares of all three types

of degrees at each institution. By multiplying prior years' degree shares by current enrollment of full-time and part-time degree seeking students, I can calculate the number of students in each degree program more accurately.[24]

While my degree-program-based measure is an improvement over the commonly used categorical measures from IPEDS, it is not perfect. One way to interpret my measure is that it is a (significantly) smoothed version of the traditional measure. The biggest concern is that my measure misses true, immediate institutional changes in degree composition. Although such changes at scale are likely rare, they could happen. In addition, slower-moving enrollment changes that surely do happen are missed. For example, if a community college introduces a new bachelor's degree program, my approach will take four years to capture the enrollment change.[25] While my method is an improvement over recoding all students at the community college as enrolling in bachelor's degree programs from the perspective of total measurement error, there is still error.

Noting this caveat, Figure 3.1 illustrates the benefits of my measure. It shows enrollment at Fashion Institute of Design & Merchandising (FIDM), a two-year college, from 2001 to 2017. In 2005, IPEDS begins designating FIDM as a four-year university because it introduced at least one four-year degree program. Applying the traditional "institution based" method of measuring enrollment, all students enrolled at FIDM would be coded as four-year university students beginning in 2005. However, if I use my new degree-program-based method, students at FIDM would still be counted as seeking two-

[24] I also use other time windows of different length to calculate enrollment and the results are similar. See the Appendix E for details.
[25] Another approach is to use degree completion data in later years to approximate enrollment. This might be more accurate, but it is also undesirable because it uses information after the treatment to infer treatment effects.

year degrees in 2005, 2006 and 2007; then, after 2007, the share of four-year degrees awarded starts to increase and my method would assign part of total enrollment in each year to both types.

The value of my approach is illustrated by the real trend in 4-year degree receipt from FIDM. Notice that the 4-year degree share increases very slowly, and even by 2017, 12 years after the type-change in IPEDS, four-year degrees account for less than 15 percent of total degrees awarded. Even though my method understates 4-year degree enrollment in the early years after FIDM makes the change, the degree of understatement is far less than the overstatement of using the traditional measure (i.e., coding all FIDM students as bachelor's enrollment in 2005 and later).

The labor market dataset contains information from two sources: unemployment rates at the MSA-level are from the Local Area Unemployment Statistics (LAUS) published by the Bureau of Labor Statistics (BLS), and gender-specific unemployment rates that I calculate from the basic monthly Current Population Survey (CPS) (Ruggles et. al., 2019). LAUS contains monthly unemployment rate data for workers aged 16 and above at the county level, but does not provide gender specific unemployment rates information at the county level or MSA level, which is why I construct my own measures using the CPS data. Because the enrollment of each postsecondary institution in IPEDS is reported in October, I only use the LAUS and CPS data from January to October to calculate unemployment rates to avoid time misalignment between treatments and outcomes (i.e., using treatment information measured after outcomes are captured).[26]

[26] I also supplement the data panel with gender and racial composition information from the Surveillance, Epidemiology, and End Results (SEER) program of the National Cancer Institute.

Table 3.1 shows the descriptive statistics for the main variables aggregated to the MSA level over the 2001 to 2017 period. The unit of observation is MSA by year. Columns (1), (2), and (3) are for the full sample, women, and men, respectively. The total sample size is 6527 MSA-years. The mean of full-time four-year degree enrollment (14911), which is much higher than part-time four-year degree enrollment (2983). However, for two-year and sub-two-year degree programs there is no significant difference between full-time and part-time enrollment. The share of female students enrolled in postsecondary institutions and who receive degrees is higher than the share of male students among all program types, consistent with the modern empirical regularity that women are more represented and more successful in higher education (McFarland et. al., 2019). The mean unemployment rate across MSAs during the sample period, calculated based on LAUS, is 6.5%, and the standard deviation is 3%.[27] After disaggregating the unemployment rate by gender, the mean unemployment rate among men is higher than the mean unemployment rate among women.

Figure 3.2 shows the different types of postsecondary enrollment in relation to the unemployment rate over time. Before the great recession, all types of enrollment experienced rapid growth, although the rates of increase of different degree programs vary year to year. After 2010, most types of degree program enrollment, except for full-time four-year degree enrollment, show a noticeable decline. Notably, full-time two-year and sub-two-year degree enrollment move almost simultaneously with the unemployment rate.

[27] Similar calculations using the CPS data produce very similar results.

3. Methodology

I leverage within-MSA and cross-time variation to estimate the effect of the local unemployment rate on postsecondary education outcomes using the following model:

$$y_{it} = \alpha + \beta UR_{it} + \Gamma X_{it} + \eta_i + \delta_t + \epsilon_{it} \quad (3.1)$$

In equation (3.1), y_{it} is a logged outcome variable for MSA *i* in year *t*. Per above, I focus on two outcomes: fall enrollment, disaggregating by full-time and part-time enrollment status, and degree attainment, using the three types of degree programs described in the previous section. I estimate models of total enrollment and also models that separate enrolment by gender. UR_{it} is the MSA level total unemployment rate and also the main variable of interest. X_{it} is a vector of control variables measuring time-varying MSA level demographics, including the share of population that is white and the share of the population that is female. η_i is an MSA fixed effects and δ_t is a year fixed effect. ϵ_{it} is the error term. In some specifications I further divide the full sample into two sub-samples, men and women, to test for effect heterogeneity by gender, i.e., whether men and women would response differently to the local labor market change. Regressions are weighted by MSA total population from the 2010 United States Census. To account for correlated errors within MSA over years, I cluster standard errors at the MSA level throughout (Bertrand, Duflo and Mullainathan, 2004).

A causal interpretation of the unemployment-rate coefficient requires the assumption that after controlling MSA fixed effects and year fixed effects, there are no dynamic unobservables correlated with both the local unemployment rate and education outcomes. A possible threat to this identifying assumption is reverse causality—in particular, a concern is that reduced enrollment could increase the measured unemployment

rate by adding some individuals to the workforce. However, given that the average ratio of college enrollment to working-age population is relatively small, at about 0.05 across MSAs, it is reasonable to believe enrollment shifts will not cause a significant change to the size of the local labor force. Moreover, in a robustness test I reduce the potential influence of this type of mechanical confounding by measuring unemployment only for workers aged 35 and above, who are unlikely to enroll in college, and show that the results are qualitatively similar to those using the full working-age population.

Next I expand the model to assess the effect of gender-specific unemployment rates and test whether the local labor market is gender segregated. The expanded version of the model is as follows:

$$y_{it} = \alpha + \beta_1 UR_{women_{it}} + \beta_2 UR_{men_{it}} + \eta_i + \delta_t + \epsilon_{it} \qquad (3.2)$$

In equation (3.2), the recurring variables follow the same definition as in equation (3.1). The new variables in equation (3.2) allow me to account for female and male unemployment rates separately. The identifying assumption is the same as in equation (3.1). I test the gender segregation of the labor market by examining the gender-specific coefficients. The null hypothesis is that the labor market is not gender segregated.

4. Primary Results

4.1 Enrollment

Table 3.2 shows the effect of the local unemployment rate on postsecondary enrollment corresponding to equation (3.1). Each column shows results for a different type of enrollment. Panel A shows the effect on total enrollment, panel B shows the effect on female enrollment, and panel C shows the effect on male enrollment. Each coefficient is

from a separate regression and shows the effect of a one-percentage-point increase in the local area unemployment rate as measured for the working-age population.

Overall, there is strong evidence that postsecondary enrollment is counter-cyclical. Starting with panel A, the consistently positive coefficients indicate that a higher unemployment rate increases college enrollment. The coefficient in column (1) indicates that a one percentage point increase in the unemployment rate increases full time four-year enrollment by 1.65 percent (significant at the 10 percent level). The coefficient in column (1) is nominally larger than the coefficient for part-time four-year enrollment in column (2), although the bottom row in panel A shows they cannot be distinguished statistically. Column (3) and (4) shows the results for two-year enrollment. The coefficient for full-time two-year enrollment, 2.962, is large and significant at the 1 percent level. This estimate is close to the findings in previous research (Betts and Mcfarland, 1995; Hillman and Orians, 2013) and implies that a one percentage point increase in the local unemployment rate would increase 2-year college enroll by about 3 percent. This estimate, in turn, implies a shift of about 0.22 percent of the student-aged population into a two-year degree program.[28] The coefficient for part-time two-year enrollment is much smaller and not significant at conventional levels. The coefficients for both types of sub-two-year enrollment are large and significant, indicating that sub-two-year enrollment is more responsive to local labor market conditions than other types of enrollment.[29]

[28] To arrive at this estimate, note that average full-time enrollment in two-year colleges at the MSA level is 6,274, and the average MSA population is 877,708. Data from the U.S. Census indicates that the 18-24 age is 9.6 percent of the total population. Therefore, my estimate of 2.96 can be converted to the percent of the student-aged population shifting to a two-year program by the following calculation: (6,274*0.0296/(877,708*0.096)) = 0.22.

[29] In Appendix Table D1 I show that these findings are robust to using different lags of the unemployment rate in the models.

Panels B and C show that except for two-year enrollment, the magnitudes and significance levels of the coefficients are close to corresponding results in the top panel. Some heterogeneity exists in the model of enrollment in two-year degree programs. In column (3), for full-time two-year degree programs, the coefficient for male students is 3.461, which is about 1 percentage point higher than the coefficient for female students. In terms of part-time two-year degree enrollment in column (4), the estimate for female students is close to 0, though the large standard error clouds inference. The estimate for male students is 2.106 and significant at the 5 percent level. The p-values from statistical tests of equality between the effect of the unemployment rate on female and male enrollment in column (3) and column (4) are all less than 0.01. These results suggest that men are more elastic than women in their two-year degree enrollment response to unemployment. For other types of enrollment, there is no evidence suggesting that there is any significant difference between male and female students.

In summary, Table 3.2 shows that two-year enrollment and sub-two-year enrollment are more responsive to the local unemployment rate than four-year enrollment. This finding is consistent with previous research (Dellas and Sakellaris, 2003). Within each type of enrollment, I do not find clear evidence that full-time enrollment is more responsive to the local unemployment rate than part-time enrollment, although there is suggestive evidence at all levels that this is the case. Male enrollment is more responsive than female enrollment to the unemployment rate, driven most clearly by differences in enrollment in two-year degree programs.

4.2 Degree Completions

Table 3.3 presents estimates of the effect of lagged local area unemployment rates on degree completions. In columns (1), (2), and (3), I test the effect of four-year lagged (year t-4) MSA level unemployment rates on the numbers of four-year degrees awarded to total students, female students, and male students, respectively. In Columns (4), (5), and (6), I test the effect of two-year lagged (year t-2) MSA level unemployment rates on the numbers of two-year degree awarded to total students, female students, and male students, respectively. In Column (7), (8), and (9), I test the effect of one-year lagged (year t-1) MSA level unemployment rates on the number of sub-two-year degree awarded to total students, female students, and male students, respectively.

All of the coefficients in Table 3.3 are positive, but few are significant at conventional levels. The exception is for sub-two-year degrees, where there is some indication of effect driven by female students. My estimates indicates that a one percentage-point increase in the unemployment rate results in a 2.475 percent increase in total sub-two-year degrees awarded one year later.[30] Overall, these results suggest some "slippage" between unemployment-induced postsecondary enrollment and degrees conferred, which is not surprising if marginally induced students are less interested in and attached to postsecondary education, but are generally only suggestive of impacts of unemployment rates on postsecondary attainment.

5. Robustness

[30] In Appendix Table D4, I show the results with other model specifications, which include more lagged years. When including all four lagged years from *t-1* to *t-4*, the unemployment rate in year t-2 has moderate effects on degree awards, but standard errors for other coefficients are still too large to give useful inference. Overall, there is suggestive evidence that local unemployment rates have positive effects on future degree completions, especially for short term degrees. Because of the large standard errors, I cannot rule out large degree effects in any sectors.

In this section I consider the robustness of my findings to several modifications to the sample and key measures. I focus on the enrollment models from the previous section for brevity.

5.1 Sensitivity to Outliers

I first test the sensitivity of my findings to dropping the three largest MSAs in terms of population: New York metropolitan area, Los Angeles metropolitan area, and Chicago metropolitan area. These three MSAs have total populations in the 2010 Census of 19.6 million, 12.8 million and 9.5 million, which are significantly higher than the average of other MSAs, which is just 0.6 million. This has the potential to significantly skew my results due to the MSA-population weighting. Table 3.4 shows the results after dropping these three MSAs. The magnitudes and significant levels are very close to estimates in Table 3.2, except for part-time enrollment in two-year and sub-two-year degree programs. The main findings regarding full-time enrollment still hold, suggesting that my results are robust to this sample composition change.

5.2 Measuring Unemployment for ages 35+

As noted above, reverse causality could violate the identifying assumption of my models. In this section I use the unemployment rate among workers aged 35-plus to remove the potential cofounding effect of flows between the labor force and postsecondary education among younger workers. Because the LAUS does not provide measures of the unemployment rate by age at the MSA level, I use the monthly CPS to calculate older workers' unemployment rates, pooling the data for each year from January to October. Using survey data raises the concern that additional measurement error may be introduced, especially at a low level of aggregation such as MSAs (Betts & McFarland, 1995). I take

two steps to assess and improve the accuracy of my measures, reduce the attenuation bias, and ensure the estimation results using two different measures are comparable.

First, I compare the total unemployment rates I calculate using the CPS data to the rates provided by LAUS. The unemployment rates from LAUS are calculated based on information from multiple sources, including CPS, the Current Employment Statistics (CES), and the Quarterly Census of Employment and Wages (QCEW). Figure 3.3 shows the histogram of differences between the two measures (in percentage points), which are generally small—e.g., for just over half of the MSA observations, the difference is less than one percentage point, or roughly one third of a standard deviation of the unemployment rate. Although my CPS-based measures are close to the LAUS measures on average and for most MSAs, there are still some extreme differences that may reflect non-negligible measurement error. To reduce the influence of measurement error and make the results more comparable, I only keep the MSAs in which the difference between the two measures is no larger than three percentage points, which I view as an indicator of higher-quality data in the MSA. Second, I bootstrap the CPS data to directly calculate the variance of the sampling error, and fit errors-in-variables (EIV) regressions to reduce attenuation bias.[31]

To test the efficacy of my approach, Appendix Tables D2 and D3 compare results corresponding to equation (3.1) using the LAUS and CPS measures, with the sample restricted to MSAs for which the LAUS- and CPS-based total unemployment rate estimates are within three percentage points, and using the EIV regressions for the CPS-based models. The results are similar using both datasets. Taking this baseline similarity as a point of

[31] The Appendix E describes the bootstrap procedure and errors-in-variables regressions in detail.

departure, Table 3.5 shows results using unemployment rates among aged 35-plus workers calculated based on CPS data. The coefficients for four-year enrollment increase slightly, while the coefficients for two-year part-time enrollment and sub-two-year part-time enrollment decrease slightly. But on the whole the results are similar, which suggests that any bias in my main specifications due to reverse causality is small.

6. Extensions

6.1 Comparison of Postsecondary Enrollment Measures

To investigate the practical significance of the measurement error issue when using the traditional "institution based" IPEDS designations to measure college enrollment, I compare my findings to findings using the traditional approach. If the measurement error is independent—which is a reasonable assumption but challenging to test empirically—it should not cause bias in my estimates because it is contained in the dependent variable, but it will increase the variance and reduce model efficiency.

In Table 3.6 I estimate the same regressions as in Table 3.2, replacing the dependent variables constructed using my procedure with the traditional, categorical measures based on IPEDS institutional designations. Compared to results in Table 3.2, the coefficients for full-time four-year degree enrollment and sub-two-year degree enrollment are larger, while the coefficients for two-year degree enrollment are lower, and even negative for part-time enrollment. More telling is that the standard errors explode in Table 3.6 compared to Table 3.2. Outside of the full-time, four-year degree results in column (1), all of the standard errors in Table 3.5 are at least twice as large as the corresponding standard errors in Table 3.2. For part-time two-year enrollment and part-time sub-two-year enrollment, the standard errors are almost three times larger. The high standard errors are a clear indication that

constructing enrollment measures using IPEDS' institutional categorizations results in very noisy measurement and substantially reduced model efficiency.

6.2 Excluding institutions with high online enrollment

The underlying premise of my analysis is that locations of institutions can be linked to local-area economic conditions. However, the geographic locations of institutions as given in IPEDS will be misleading indicators of local enrollment for colleges with high online enrollment. To test the robustness of my findings to this concern, I re-estimate my models after excluding institutions with a major online presence.[32] Table 3.7 shows the results, which are qualitatively similar to the main findings. This indicates that the geographic misalignment of enrollment due to online enrollment is not large enough to significantly bias my primary estimates.[33]

6.3 First-time Students vs Continuing Students

Given the above evidence that postsecondary enrollment is counter-cyclical, an interesting question one might ask is that how much of this is from new enrollment, and how much of this is from increased retention of already enrolled students? A natural way to investigate this question is to divide the enrollment of each degree program type into two components: first-time students and other students. Table 3.8-1 and 8-2 show estimation results of regressions of first-time enrollment and non-first-time enrollment on the unemployment rate. Overall, the results are similar in these two tables, suggesting that both types of enrollment contribute to my findings. One exception is that coefficients for

[32] Institutions online enrollment information is from IPEDS. Institutions with average share of online enrollment in year 2012-2014 greater than 20 percent are dropped from the sample. About 10 percent institutions are excluded because of this restriction. I also test other thresholds and the results are similar.
[33] Because geographic locations of for-profit colleges are less accurate than public institutions. I also test results when excluding all for-profit colleges. Results are qualitatively similar and omitted for brevity.

four-year, part-time, first-time enrollment are significantly negative in column (2) in Table 3.8-1. However, given this is a relatively small enrollment category and I am conducting a large number of statistical tests (which increases the likelihood of some spurious results), I do not interpret this as strong evidence that enrollment effects are different between first-time students and non-first-time students.

6.4 Testing for Gender Segregation in the Labor Market

Thus far I have estimated the effect of the total unemployment rate on total postsecondary enrollment and enrollment by gender. The total unemployment rate, to which I allow for differential gender responsiveness for both genders, is fixed, so that the differences I observe can be attributed to differences in responsiveness between genders to the same conditions. In this section I expand the model as described by equation (3.2) to test the hypothesis that men and women are responding to different, gender-specific local labor markets—i.e., I test for gender segregation in the labor market.

For this analysis I again rely on the monthly CPS data, this time to calculate gender-specific unemployment rates. Per equation (3.2), the gender-specific rates are included simultaneously to look for evidence of differential responsiveness. Table 3.9 shows the results. Overall, the estimates are smaller in magnitude than the estimates for total unemployment rates in Table 3.2, although some estimates are especially noisy. I test the gender segregation hypothesis by testing for equality between the coefficients on the female and male unemployment rates. If the local labor market is gender segregated, then we should observe that the effect of the gender-specific unemployment rates differ significantly, but most p-values from the statistical tests are very high, except some tests for four-year enrollment. Moreover, even these estimates are not consistent with gender-

segregated markets, but seem to suggest that the male unemployment rate is a stronger driver of behavior for both genders. The generally insignificant results, their direction, and the possibility of type I errors among multiple statistical tests lead to the summary conclusion that there is no evidence of gender segregation in the labor market as measured by my models.

7. Conclusion

I investigate the link between local labor market conditions and postsecondary education outcomes at the MSA level. My sample covers the years 2001 to 2017 and spans the Great Recession. I find that postsecondary enrollment is counter-cyclical: enrollment increases when the unemployment rate rises. My results are most pronounced for two-year degree and sub-two-year degree enrollment, which appear more responsive to local employment conditions than four-year degree enrollment. Within each type of enrollment, I do not find strong evidence that full-time enrollment is more responsive to unemployment rates than part-time enrollment, but there is suggestive evidence that this is the case. I also track degree outcomes corresponding to lagged unemployment rates and show suggestive evidence of positive impacts, although it is not conclusive and even an optimistic interpretation of those results suggests significant slippage between the enrollment effect and degree-production effect of unemployment fluctuations. Finally, I show that men are more elastic than women in their two-year degree enrollment response to the unemployment rate, and find no evidence to suggest this is caused by gendered segregation in the labor market.

My analysis is facilitated by a new "degree program based" measure of postsecondary enrollment that I construct for MSAs using IPEDS data. My measures

produce program-specific enrollment estimates for each institution in year-*t* using the (recent) historical composition of degrees conferred at that institution and year-*t* enrollment. Relative to the standard approach of categorizing all enrollment in an institution based on the IPEDS degree-level category, my measure is less error prone and results in more efficient estimation.

Table 3.1: Summary Statistics

	Total	Women	Men
Full time 4-year enrollment	14911.32	8173.74	6743.02
	(29224.83)	(16212.40)	(13085.76)
Part time 4-year enrollment	2983.43	1716.50	1264.47
	(6884.67)	(4025.54)	(2925.85)
Full time 2-year enrollment	5096.99	2902.07	2201.48
	(13474.29)	(7735.88)	(5814.89)
Part time 2-year enrollment	5286.29	3286.29	2025.93
	(12851.93)	(7676.69)	(5263.37)
Full time sub-2-year enrollment	3101.45	1803.62	1285.82
	(6983.30)	(4003.24)	(3009.40)
Part time sub-2-year enrollment	2575.78	1445.31	1107.00
	(7062.00)	(3744.23)	(3356.30)
4-year degree conferred	7339.13	4213.66	3125.47
	(15012.64)	(8788.83)	(6260.98)
2-year degree conferred	3775.27	2290.22	1485.05
	(8670.51)	(5316.58)	(3429.65)
Sub-2-year degree conferred	3788.69	2332.20	1456.49
	(8716.16)	(5379.98)	(3420.46)
Fraction of white	0.51	0.51	0.51
	(0.01)	(0.01)	(0.01)
Fraction of Women	0.84	0.84	0.84
	(0.12)	(0.12)	(0.12)
Unemployment rate	0.07	0.06	0.07
	(0.03)	(0.03)	(0.04)
Number of Observations	6527	6527	6527

Notes: Unit of Observation is Metropolitan Statistical Area by year. Means and standard deviations displayed. Total unemployment rate measure is calculated from LAUS data, gender-specific unemployment rate measure is calculated from CPS data.

Table 3.2: Effect of the unemployment rate on college enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate: β	1.648*	1.068	2.962***	1.184	4.192***	2.938*
	(0.855)	(1.245)	(0.970)	(0.948)	(0.846)	(1.579)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.981	0.973	0.974	0.973	0.970	0.957
P-value for H_0: $\beta_{\text{Full-time}} = \beta_{\text{Part-time}}$	0.534		0.098		0.294	
Panel B: Women						
Unemployment Rate: β	1.481*	0.767	2.486**	0.515	3.971***	2.847**
	(0.834)	(1.335)	(1.003)	(0.960)	(0.798)	(1.420)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.982	0.971	0.972	0.972	0.970	0.958
P-value for H_0: $\beta_{\text{Full-time}} = \beta_{\text{Part-time}}$	0.511		0.056		0.325	
Panel C: Men						
Unemployment Rate: β	1.635**	1.130	3.461***	2.106**	4.037***	3.314*
	(0.789)	(1.028)	(0.856)	(0.990)	(1.081)	(1.827)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.984	0.978	0.976	0.974	0.968	0.957
P-value for H_0: $\beta_{\text{Full-time}} = \beta_{\text{Part-time}}$	0.501		0.206		0.592	
P-value for H_0: $\beta_{\text{Women}} = \beta_{Men}$	0.526	0.468	0.006	0.001	0.923	0.630
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. H_0: $\beta_{\text{Full-time}} = \beta_{\text{Part-time}}$ is in reference for equation (3.1), the null hypothesis is that the effect of unemployment rate on full-time enrollment and part-time enrollment are equal. H_0: $\beta_{\text{Women}} = \beta_{\text{Men}}$ is in reference for equation (3.1), the null hypothesis is that the effect of unemployment rate on female enrollment and male enrollment are equal. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Table 3.3: Effect of the unemployment rate on degree completions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	4-year degree			2-year degree			sub-2-year degree		
	Total	Women	Men	Total	Women	Men	Total	Women	Men
Unemployment Rate in year t-1							2.475*	2.940**	1.339
							(1.346)	(1.360)	(1.432)
Unemployment Rate in year t-2				1.419	0.796	2.201			
				(1.333)	(1.293)	(1.376)			
Unemployment Rate in year t-4	1.472	1.374	1.859						
	(1.363)	(1.344)	(1.238)						
Observations	4768	4768	4768	5511	5511	5511	5883	5883	5883
R-squared	0.982	0.982	0.986	0.979	0.979	0.979	0.967	0.967	0.960
MSA FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. 4-year degree includes bachelor's degree, 2-year degree includes associate's degree, and sub-2-year degree includes all kinds of certificates. Regressions are weighted by 2010 MSA population. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.4: Effect of the unemployment rate on college enrollment, dropping three largest MSAs

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate	1.907**	1.422	3.141***	0.541	3.794***	1.743
	(0.931)	(1.322)	(1.003)	(0.890)	(0.703)	(1.315)
Observations	6206	6206	6206	6206	6206	6206
R-squared	0.976	0.966	0.966	0.967	0.967	0.952
Panel B: Women						
Unemployment Rate	1.709*	1.166	2.629**	-0.045	3.670***	1.924
	(0.908)	(1.404)	(1.031)	(0.926)	(0.735)	(1.261)
Observations	6206	6206	6206	6206	6206	6206
R-squared	0.977	0.963	0.962	0.966	0.966	0.951
Panel C: Men						
Unemployment Rate	1.893**	1.373	3.667***	1.394	3.556***	1.798
	(0.857)	(1.102)	(0.883)	(0.927)	(0.869)	(1.455)
Observations	6206	6206	6206	6206	6206	6206
R-squared	0.980	0.972	0.969	0.969	0.965	0.953
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. Three largest MSAs are dropped: New York metropolitan area, Los Angeles metropolitan area, and Chicago metropolitan area. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Table 3.5: Effect of the unemployment rate among senior workers on college enrollment restricting MSAs

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate	0.881*	0.346	2.797***	2.224*	2.901***	3.243***
	(0.511)	(0.808)	(0.907)	(1.275)	(0.628)	(1.171)
Observations	3613	3613	3613	3613	3613	3613
R-squared	0.985	0.973	0.982	0.977	0.983	0.965
Panel B: Women						
Unemployment Rate	0.760	0.133	2.488**	1.703	2.992***	3.274***
	(0.504)	(0.896)	(0.973)	(1.276)	(0.61)	(1.091)
Observations	3613	3613	3613	3613	3613	3613
R-squared	0.986	0.971	0.978	0.975	0.982	0.964
Panel C: Men						
Unemployment Rate	0.998**	0.533	3.207***	2.769**	2.783***	3.293**
	(0.481)	(0.693)	(0.832)	(1.212)	(0.789)	(1.338)
Observations	3613	3613	3613	3613	3613	3613
R-squared	0.988	0.978	0.983	0.977	0.977	0.961
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. Unemployment rate are calculated among senior workers whose ages are greater than 35, based on CPS data. MSAs in which the difference between two unemployment rates measured by CPS data and LAUS data are no larger than three percentage points are kept. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Table 3.6: Effect of the unemployment rate on college enrollment, using measures from IPEDS designation

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate	2.108*	1.002	1.300	-3.322	6.929***	6.946
	(1.124)	(2.111)	(1.989)	(3.930)	(2.121)	(5.726)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.962	0.945	0.906	0.892	0.926	0.894
Panel B: Women						
Unemployment Rate	2.054*	0.553	0.809	-3.612	6.692***	6.807
	(1.108)	(2.158)	(1.953)	(3.857)	(2.053)	(5.206)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.963	0.943	0.908	0.892	0.924	0.894
Panel C: Men						
Unemployment Rate	2.057*	1.400	1.990	-2.971	6.962***	10.60
	(1.048)	(1.970)	(1.898)	(3.784)	(2.615)	(7.486)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.968	0.952	0.918	0.901	0.935	0.872
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Enrollment is measured based on IPEDS designation. Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Table 3.7: Effect of the unemployment rate on college enrollment, excluding online colleges

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate	1.800**	-0.0908	2.747***	0.969	4.070***	2.727*
	(0.806)	(0.948)	(0.994)	(1.034)	(0.834)	(1.602)
Observations	6192	6192	6192	6192	6192	6192
R-squared	0.980	0.976	0.977	0.975	0.970	0.958
Panel B: Women						
Unemployment Rate	1.633**	-0.347	2.271**	0.304	3.776***	2.760*
	(0.756)	(1.034)	(1.032)	(1.065)	(0.778)	(1.456)
Observations	6192	6192	6192	6192	6192	6192
R-squared	0.982	0.976	0.976	0.974	0.970	0.958
Panel C: Men						
Unemployment Rate	1.669**	-0.0627	3.228***	1.874*	4.171***	3.134*
	(0.718)	(0.775)	(0.869)	(1.048)	(1.109)	(1.816)
Observations	6192	6192	6192	6192	6192	6192
R-squared	0.984	0.982	0.978	0.976	0.967	0.959
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. Online enrollment information is from 2012-2014 IPEDS. Online colleges are institutions whose average share of online enrollment is more than 20 percent in 2012-2014. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Table 3.8-1: Effect of the unemployment rate on college first-time enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate	0.898	-4.320**	1.955**	2.756**	4.003***	3.877*
	(0.706)	(1.959)	(0.963)	(1.287)	(0.814)	(2.132)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.984	0.939	0.973	0.965	0.965	0.942
Panel B: Women						
Unemployment Rate	0.789	-4.476**	1.275	2.042	3.608***	3.625*
	(0.685)	(2.053)	(1.017)	(1.312)	(0.913)	(2.065)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.985	0.931	0.971	0.964	0.963	0.942
Panel C: Men						
Unemployment Rate	0.929	-4.162**	2.684***	3.523***	4.092***	4.298*
	(0.665)	(1.845)	(0.882)	(1.258)	(0.959)	(2.253)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.986	0.944	0.973	0.965	0.963	0.940
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Table 3.8-2: Effect of the unemployment rate on college non-first-time enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate	1.759**	1.384	3.165***	0.660	4.502***	3.113**
	(0.878)	(1.239)	(1.079)	(1.141)	(1.094)	(1.558)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.981	0.973	0.967	0.966	0.959	0.954
Panel B: Women						
Unemployment Rate	1.586*	1.078	2.728**	0.0548	4.275***	3.162**
	(0.867)	(1.328)	(1.105)	(1.141)	(1.021)	(1.406)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.981	0.971	0.965	0.966	0.958	0.954
Panel C: Men						
Unemployment Rate	1.735**	1.440	3.496***	1.456	4.348***	3.196*
	(0.802)	(1.023)	(0.953)	(1.147)	(1.277)	(1.804)
Observations	6257	6257	6257	6257	6257	6257
R-squared	0.984	0.978	0.971	0.969	0.959	0.955
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

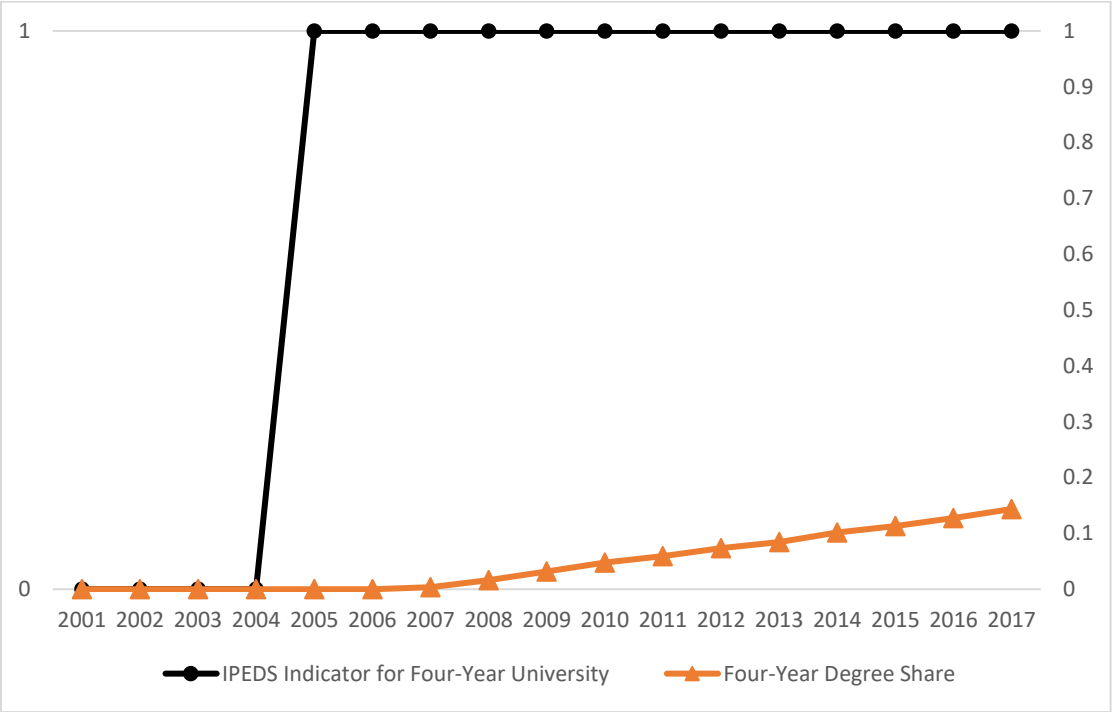
Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Table 3.9: Effect of by gender unemployment rate on college enrollment

	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Women						
Unemployment Rate among Women: β_1	-0.200	-1.204*	0.610	0.443	1.106***	0.610
	(0.375)	(0.687)	(0.554)	(0.601)	(0.401)	(0.581)
Unemployment Rate among Men: β_2	0.636*	1.008*	1.128*	0.955	1.057**	2.115***
	(0.362)	(0.565)	(0.602)	(0.923)	(0.451)	(0.774)
Observations	3,545	3,545	3,545	3,545	3,545	3,545
R-squared	0.979	0.959	0.963	0.965	0.972	0.952
P-value for H_0: $\beta_1 = \beta_2$	0.150	0.029	0.532	0.536	0.936	0.157
Panel B: Men						
Unemployment Rate among Women: β_1	-0.141	-0.743	1.259***	1.113*	1.422***	0.337
	(0.326	(0.542)	(0.449)	(0.649)	(0.472)	(0.721)
Unemployment Rate among Men: β_2	0.845**	0.917*	1.556***	1.281	1.097**	2.078**
	(0.352)	(0.515)	(0.527)	(0.878)	(0.514)	(0.946)
Observations	3,545	3,545	3,545	3,545	3,545	3,545
R-squared	0.983	0.97	0.969	0.967	0.965	0.948
P-value for H_0: $\beta_1 = \beta_2$	0.086	0.065	0.666	0.848	0.662	0.186
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. H_0: $\beta_1 = \beta_2$ is in reference for equation (3.2), the null hypothesis is that coefficients on female unemployment rates and male unemployment rates are equal. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Figure 3.1: Example of difference between IPEDS designations and "degree program based" method

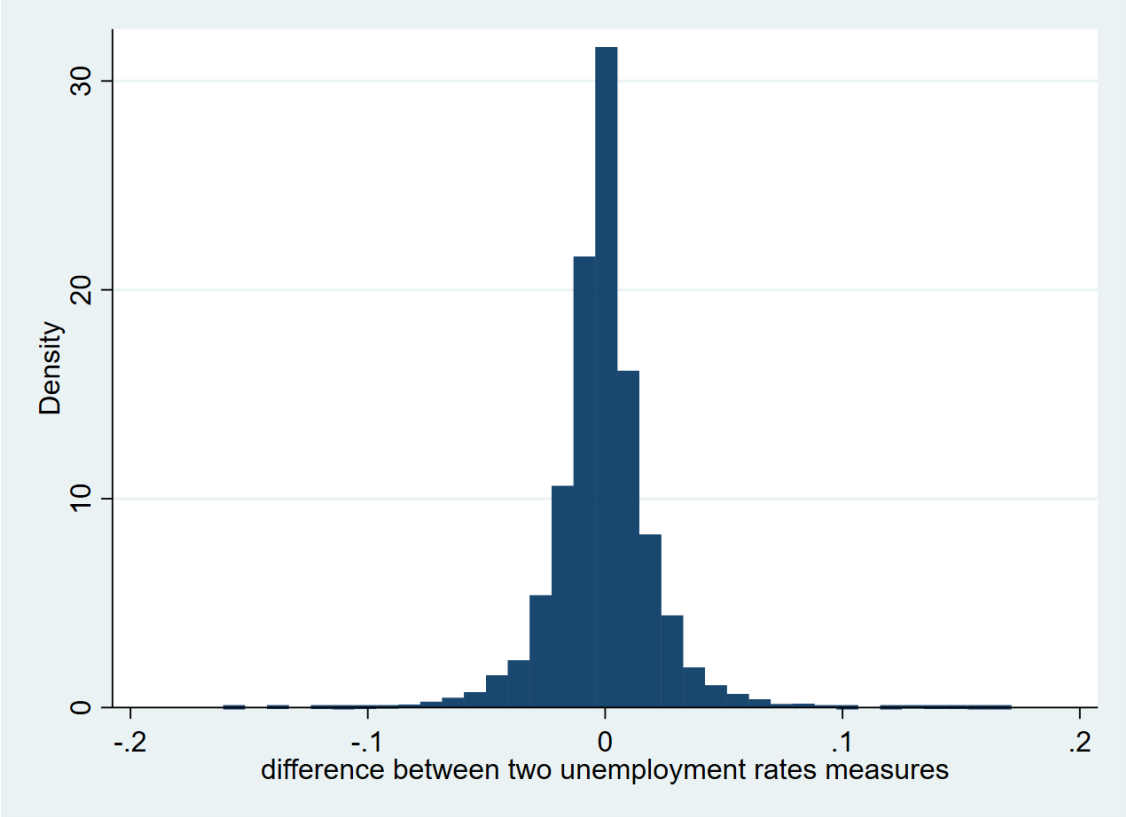


Notes: Figure compares two methods using an example of Fashion Institute of Design & Merchandising: IPEDS designations (black line) and "degree program based" method (orange line).

Figure 3.2: College enrollment by degree program type and unemployment rate

Notes: Figure shows total enrollment by degree program type (blue solid line) and unemployment rate (red dashed line) in US from 2001 to 2017.

Figure 3.3: Histogram of the difference between unemployment rates calculated by LAUS data and CPS data



Notes: Figure shows the histogram of the difference between unemployment rates calculated by LAUS data and CPS data.

Appendix A

Supplementary Tables for Chapter 1

Appendix Table A1: Construction of the analytic sample

first time entrants at four year universities from 2006 to 2010		97749	first time entrants at community colleges from 2006 to 2010		110695
	record lost	Remaining sample		record lost	remaining sample
Out of state/ Foreign student	14486	83263	Out of state/ Foreign student	2497	108198
Not full time	3320	79943	Not full time	32647	75551
Older than 20	2656	77287	Older than 20	12228	63323
Missing high school code	6053	71234	Missing high school code, or out of state high school	5529	57794
Missing ACT Math score or ACT English Score	398	70836	Missing ACT Math score or ACT English Score	14537	43257
Missing high school rank*	8199	70836	Missing high school rank*	28299	43257
Drop extremely small high schools **	99	70737	Drop extremely small high schools **	43	43214

* We do not drop those students whose high school percentile ranks are missing, instead we impute their ranks based on other covariates
** We drop high schools that sent five or fewer students to a public college during the period covered by our data panel

Appendix Table A2: Summary statistics for the university sample:

	(1) Raw	(2) In state	(3) In state full time	(4) In state full time <=20	(5) In state full time <=20, in state hs	(6) In state full time <=20, in state hs, nomissing ACT	(7) Analytic sample
ACT math	22.84	22.7	22.83	22.89	22.88	22.89	22.89
	(4.88)	(4.87)	(4.82)	(4.81)	(4.78)	(4.78)	(4.78)
ACT English	23.62	23.49	23.64	23.7	23.68	23.68	23.68
	(5.45)	(5.46)	(5.39)	(5.37)	(5.34)	(5.34)	(5.34)
HS percentile rank	0.69	0.69	0.7	0.7	0.7	0.71	0.71
	(0.23)	(0.23)	(0.23)	(0.22)	(0.22)	(0.22)	(0.22)
HS percentile rank missing indicator	0.16	0.16	0.15	0.14	0.12	0.12	0.12
	(0.37)	(0.36)	(0.35)	(0.34)	(0.32)	(0.32)	(0.32)
Female	0.55	0.55	0.55	0.55	0.55	0.55	0.55
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
White	0.77	0.79	0.79	0.79	0.8	0.81	0.81
	(0.42)	(0.41)	(0.41)	(0.4)	(0.4)	(0.39)	(0.39)
Black	0.12	0.12	0.11	0.11	0.11	0.1	0.1
	(0.32)	(0.32)	(0.31)	(0.31)	(0.31)	(0.3)	(0.3)
Hispanic	0.02	0.02	0.02	0.02	0.02	0.02	0.02
	(0.15)	(0.15)	(0.15)	(0.15)	(0.14)	(0.14)	(0.14)
Asian	0.02	0.02	0.02	0.02	0.02	0.02	0.02
	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
Other Race	0.03	0.02	0.02	0.02	0.01	0.01	0.01
	(0.17)	(0.14)	(0.13)	(0.13)	(0.11)	(0.11)	(0.11)
Race missing unknown	0.04	0.04	0.04	0.04	0.04	0.04	0.04
	(0.2)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)
Number of observations	97749	83263	79943	77287	71234	70836	70737

Appendix Table A3 Summary statistics for the community college sample:

	(1) Raw	(2) In state	(3) In state full time	(4) In state full time <=20	(5) In state full time <=20, in state hs	(6) In state full time <=20, in state hs, nomissing ACT	(7) Analytic sample
ACT math	18.84	18.84	19.01	19.05	19.04	19.04	19.04
	(3.85)	(3.85)	(3.8)	(3.79)	(3.78)	(3.78)	(3.78)
ACT English	18.83	18.83	19.02	19.04	18.99	18.99	18.99
	(4.95)	(4.95)	(4.82)	(4.81)	(4.78)	(4.78)	(4.78)
HS percentile rank	0.48	0.49	0.51	0.52	0.52	0.56	0.56
	(0.25)	(0.25)	(0.24)	(0.24)	(0.24)	(0.23)	(0.23)
HS percentile rank missing indicator	0.55	0.55	0.46	0.41	0.37	0.35	0.35
	(0.5)	(0.5)	(0.5)	(0.49)	(0.48)	(0.48)	(0.48)
Female	0.54	0.54	0.53	0.52	0.52	0.54	0.54
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
White	0.71	0.71	0.76	0.77	0.78	0.79	0.79
	(0.45)	(0.45)	(0.43)	(0.42)	(0.41)	(0.41)	(0.41)
Black	0.14	0.13	0.1	0.09	0.09	0.08	0.08
	(0.34)	(0.34)	(0.3)	(0.29)	(0.28)	(0.27)	(0.27)
Hispanic	0.03	0.03	0.03	0.03	0.02	0.02	0.02
	(0.17)	(0.16)	(0.16)	(0.16)	(0.15)	(0.15)	(0.15)
Asian	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(0.12)	(0.12)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Other Race	0.03	0.03	0.02	0.02	0.02	0.02	0.02
	(0.16)	(0.16)	(0.15)	(0.15)	(0.14)	(0.14)	(0.14)
Race missing unknown	0.09	0.09	0.08	0.08	0.08	0.07	0.07
	(0.28)	(0.28)	(0.27)	(0.27)	(0.26)	(0.26)	(0.26)
Number of observations	110695	108198	75551	63323	57794	43257	43214

Appendix Table A4: Summary statistics for university students

	STEM-entrants		non-STEM entrants	
	STEM qualified	Not STEM qualified	STEM qualified	Not STEM qualified
ACT math	28.7	22.45	27.53	20.84
	[28.59,28.8]	[22.34,22.56]	[27.42,27.65]	[20.79,20.9]
ACT English	27.01	23.36	26.9	22.38
	[26.87,27.14]	[23.23,23.5]	[26.76,27.06]	[22.3,22.45]
HS percentile rank	0.86	0.68	0.86	0.65
	[0.86,0.87]	[0.67,0.69]	[0.86,0.87]	[0.65,0.65]
HS percentile rank missing indicator	0.1	0.11	0.12	0.12
	[0.09,0.11]	[0.1,0.11]	[0.11,0.13]	[0.11,0.12]
Female	0.21	0.5	0.33	0.67
	[0.2,0.23]	[0.49,0.51]	[0.31,0.35]	[0.66,0.67]
White	0.86	0.77	0.87	0.79
	[0.85,0.87]	[0.76,0.78]	[0.86,0.88]	[0.78,0.79]
Black	0.03	0.13	0.02	0.13
	[0.02,0.04]	[0.12,0.14]	[0.02,0.03]	[0.13,0.13]
Hispanic	0.02	0.03	0.02	0.02
	[0.01,0.02]	[0.02,0.03]	[0.01,0.02]	[0.02,0.02]
Asian	0.04	0.01	0.04	0.01
	[0.04,0.05]	[0.01,0.02]	[0.04,0.05]	[0.01,0.01]
Other Race	0.01	0.02	0.01	0.01
	[0.01,0.02]	[0.01,0.02]	[0.01,0.01]	[0.01,0.01]
Race missing unknown	0.03	0.04	0.04	0.04
	[0.03,0.04]	[0.03,0.04]	[0.03,0.04]	[0.03,0.04]
Number of students	7569	7570	10940	44658
	[7466,7665]	[7466,7668]	[10549,11391]	[44163,45118]

Appendix Table A5: In-sample and out-of-sample predictive validity among the university sample, equation (1.1).

	(A) In-sample		(B) Out-of-sample	
	(1) Actual Observed	(2) Predicted Value	(3) Actual Observed	(4) Predicted Value
Avg ACT math	26.64	26.64	26.62	26.60
	[26.57,26.72]	[26.57,26.72]	[26.47,26.77]	[26.5,26.71]
Avg ACT English	26.07	26.07	26.02	26.02
	[25.98,26.14]	[25.98,26.14]	[25.82,26.21]	[25.92,26.14]
Avg HS percentile rank	0.82	0.82	0.82	0.82
	[0.82,0.82]	[0.82,0.82]	[0.82,0.83]	[0.82,0.83]
Share Female	0.36	0.36	0.36	0.36
	[0.35,0.37]	[0.35,0.37]	[0.34,0.4]	[0.35,0.38]
Share White	0.86	0.86	0.86	0.85
	[0.85,0.86]	[0.85,0.86]	[0.84,0.87]	[0.85,0.86]
Share Black	0.04	0.04	0.04	0.04
	[0.04,0.05]	[0.04,0.05]	[0.03,0.05]	[0.04,0.05]
Share Hispanic	0.02	0.02	0.02	0.02
	[0.02,0.02]	[0.02,0.02]	[0.01,0.02]	[0.02,0.02]
Share Asian	0.03	0.03	0.03	0.03
	[0.03,0.04]	[0.03,0.04]	[0.03,0.04]	[0.03,0.04]
Share Other Race	0.01	0.01	0.01	0.01
	[0.01,0.01]	[0.01,0.01]	[0.01,0.02]	[0.01,0.02]
Share Race missing unknown	0.04	0.04	0.04	0.04
	[0.03,0.04]	[0.03,0.04]	[0.03,0.05]	[0.03,0.04]
Number of students	7158	7159	1820	1839
	[6965,7302]	[6965,7302]	[1749,1890]	[1777,1887]

Notes: Table shows the in-sample and out-of-sample comparison of predicted values versus true outcomes using equation (1.1) and the corresponding sample of initial STEM entrants. We use 80% of the data for the "training dataset" and the remaining 20% to test out-of-sample predictive validity. Averages and 95 percent confidence intervals over 500 bootstrap repetitions are provided.

Appendix Table A6: Summary statistics for STEM-qualified community college students in supplementary predictive models

	(1) Graduate with Non-STEM	(2) Drop-out
Avg ACT math	25.08	25.09
	[24.84,25.34]	[24.76,25.43]
Avg ACT English	22.72	22
	[22.45,23.01]	[21.65,22.35]
Avg HS percentile rank	0.83	0.74
	[0.82,0.84]	[0.72,0.75]
Share Female	0.23	0.1
	[0.19,0.26]	[0.07,0.12]
Share White	0.84	0.78
	[0.81,0.87]	[0.74,0.82]
Share Black	0.01	0.02
	[0.01,0.02]	[0.01,0.03]
Share Hispanic	0.02	0.02
	[0.01,0.03]	[0.01,0.04]
Share Asian	0.03	0.04
	[0.02,0.05]	[0.03,0.07]
Share Other Race	0.02	0.03
	[0.01,0.03]	[0.01,0.05]
Share Race missing unknown	0.08	0.11
	[0.05,0.11]	[0.07,0.14]
Number of non-STEM degrees or dropouts	1145	1180
	[1036,1269]	[1065,1312]

Notes: Table reports averages and 95 percent confidence intervals over 500 bootstrap repetitions for nudged community college students: graduate with a non-STEM degree in column (1) and fail to graduate with any bachelor's degrees in column (2).

Appendix B

Supplementary Predictive Models for Chapter 1

In addition to the main model predicting STEM attainment, we also estimate two separate, supplementary models to predict the likelihood of graduating with a non-STEM degree and the likelihood of failing to earn any four-year degree (within six years). These models are of the same structure as equation (1.1):

$$P_{ijt}^{*} = \boldsymbol{X_i \beta_1} + \gamma_{1j} + \delta_{1t} + \varepsilon_{1ijt}$$

(A1)

In equation (A1), P_{ijt}^{*} is either the latent utility of completing a non-STEM degree within six years, or failing to complete a bachelor's degree within six years.

We estimate the models for these outcomes independently, but note that in conjunction with the main model estimated in the text for STEM degrees, the model can be further modified to account for outcome-dependence. That is, we can specify a single multinomial outcome and model the outcomes jointly. We did not do this here because we view this as an add-on to the main analysis and do not wish to overwrite the main model. That said, in unreported results we have confirmed that inference from the main analysis, and Appendix Table A6, is very similar if we use a multinomial model that accounts for outcome-dependence in the data to generate the predictions for the three categorical outcomes we consider in this brief extension: STEM degree attainment, non-STEM degree attainment, and dropout.

Appendix C

Supplementary Tables for Chapter 2

Appendix Table C1: Summary statistics for two-year and four-year college entrants overall and by STEM exit conditions.

	Four-year University		Two-year College
	(1) Analytic Sample	(2) STEM Completers	(3) Analytic Sample
Female	0.547	0.538	0.395
	(0.498)	(0.499)	(0.489)
Asian	0.116	0.096	0.213
	(0.320)	(0.294)	(0.410)
Black	0.111	0.130	0.078
	(0.315)	(0.337)	(0.268)
Hispanic	0.090	0.192	0.061
	(0.287)	(0.394)	(0.239)
White	0.633	0.531	0.607
	(0.482)	(0.499)	(0.489)
Other Race	0.049	0.051	0.040
	(0.216)	(0.221)	(0.197)
Mother's Education less than HS	0.249	0.417	0.201
	(0.432)	(0.493)	(0.401)
Mother's Education less than College	0.315	0.357	0.254
	(0.465)	(0.479)	(0.435)
Mother's Education equals or greater than College	0.390	0.177	0.482
	(0.488)	(0.382)	(0.500)
Mother's Education Missing	0.046	0.049	0.063
	(0.209)	(0.215)	(0.244)
Father's Education less than HS	0.259	0.443	0.165
	(0.438)	(0.497)	(0.371)
Father's Education less than College	0.246	0.294	0.201
	(0.431)	(0.456)	(0.401)
Father's Education equals or greater than College	0.449	0.213	0.571
	(0.497)	(0.410)	(0.495)
Family income <35K	0.211	0.369	0.180
	(0.408)	(0.483)	(0.385)
Family income 35-50K	0.160	0.205	0.140

	(0.366)	(0.404)	(0.347)
Family income 50-75K	0.219	0.218	0.205
	(0.414)	(0.413)	(0.404)
Family income >75K	0.410	0.208	0.474
	(0.492)	(0.406)	(0.500)
10th Grade Math Test Score	0.062	-0.527	0.506
	(0.707)	(0.689)	(0.635)
10th Grade Test Score Missing	0.010	0.013	0.015
	(0.099)	(0.113)	(0.120)
10th Grade Read Test Score	-0.050	-0.619	0.192
	(0.706)	(0.702)	(0.648)
10th Grade Read Score Missing	0.010	0.013	0.015
	(0.099)	(0.113)	(0.120)
9th Grade GPA	3.074	2.459	3.419
	(0.690)	(0.746)	(0.569)
9th Grade GPA missing	0.097	0.104	0.090
	(0.296)	(0.305)	(0.287)
12th Grade Math Test Score	1.194	0.042	1.950
	(1.012)	(1.037)	(0.900)
12th Grade Test Score Missing	0.064	0.159	0.027
	(0.245)	(0.365)	(0.162)
High School enrollment	1216.949	1317.492	1279.661
	(791.724)	(863.505)	(822.808)
High School enrollment Missing	0.012	0.012	0.013
	(0.110)	(0.108)	(0.115)
High School FRL Pct	16.357	24.116	14.501
	(14.227)	(16.941)	(12.583)
High School FRL Pct Missing	0.398	0.235	0.416
	(0.490)	(0.424)	(0.493)
High School FTE	73.263	74.599	77.040
	(41.069)	(43.699)	(42.023)
High School FTE Missing	0.033	0.036	0.033
	(0.178)	(0.187)	(0.179)
High School Minority Pct	28.389	35.468	27.267
	(29.316)	(31.774)	(28.328)
High School Minority Pct Missing	0.023	0.028	0.022
	(0.149)	(0.164)	(0.147)
High School in Rural	0.172	0.223	0.173
	(0.378)	(0.416)	(0.379)
High School in Urban Town	0.467	0.472	0.463
	(0.499)	(0.499)	(0.499)

High School in City	0.350	0.295	0.350
	(0.477)	(0.456)	(0.477)
High School Location Missing	0.011	0.010	0.013
	(0.106)	(0.100)	(0.115)
High School GPA	3.102	2.497	3.445
	(0.607)	(0.662)	(0.475)
High School GPA Missing	0.074	0.078	0.063
	(0.262)	(0.268)	(0.244)
Math Course Taking Mid lvl 1	0.083	0.359	0.017
	(0.277)	(0.480)	(0.130)
Math Course Taking Mid lvl 2	0.174	0.286	0.049
	(0.379)	(0.452)	(0.216)
Math Course Taking Advanced lvl 1	0.187	0.140	0.104
	(0.390)	(0.347)	(0.305)
Math Course Taking Advanced lvl 2	0.245	0.096	0.228
	(0.430)	(0.295)	(0.420)
Math Course Taking Advanced lvl 3	0.237	0.041	0.540
	(0.426)	(0.199)	(0.499)
Math Course Taking Missing	0.073	0.077	0.062
	(0.261)	(0.267)	(0.242)
ACT Composite Score	22.716	12.359	25.738
	(4.603)	(7.258)	(4.581)
ACT Composite Score Missing	0.121	0.478	0.057
	(0.326)	(0.500)	(0.233)
ACT Math Score	22.471	12.111	26.261
	(4.916)	(7.265)	(5.027)
ACT Math Score Missing	0.123	0.481	0.060
	(0.329)	(0.500)	(0.237)
N	6820	4020	820

Notes: All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Appendix Table C2: Results from predictive logistic regression of STEM degree completion among four-year college entrants.

	(1) Graduate With STEM	(2) Graduate With STEM	(3) Graduate With STEM	(4) Graduate With STEM
Female	-0.727***	-0.724***	-0.726***	-0.667***
	[-0.888,-0.561]	[-0.875,-0.568]	[-0.905,-0.565]	[-0.84,-0.508]
Asian	0.623***	0.561***	0.566***	0.35***
	[0.411,0.823]	[0.342,0.768]	[0.307,0.782]	[0.062,0.6]
Black	0.578***	0.562***	0.606***	0.7***
	[0.273,0.856]	[0.25,0.853]	[0.252,0.931]	[0.331,1.093]
Hispanic	0.053	0.056	0.035	0.075
	[-0.289,0.35]	[-0.272,0.376]	[-0.303,0.352]	[-0.243,0.431]
Other Race	0.037	0.008	0.015	0.014
	[-0.38,0.393]	[-0.441,0.357]	[-0.367,0.388]	[-0.416,0.374]
10th Grade Math Test Score	0.916***	0.891***	0.888***	0.113
	[0.747,1.103]	[0.718,1.066]	[0.733,1.067]	[-0.134,0.334]
10th Grade Test Score Missing	0.643*	0.215	0.356	0.468
	[-0.049,1.271]	[-0.457,0.85]	[-0.43,1.238]	[-0.865,1.822]
10th Grade Reading Test Score	-0.207***	-0.221***	-0.238***	-0.287***
	[-0.363,-0.039]	[-0.379,-0.072]	[-0.388,-0.068]	[-0.467,-0.102]
9th Grade GPA	1.065***	1.067***	1.096***	-0.111
	[0.88,1.248]	[0.871,1.261]	[0.904,1.29]	[-0.491,0.279]
9th Grade GPA Missing	0.14	0.133	0.095	0.197
	[-0.137,0.397]	[-0.168,0.401]	[-0.17,0.369]	[-0.402,0.69]
Mother's Education less than College		-0.123	-0.134	-0.132
		[-0.348,0.13]	[-0.372,0.108]	[-0.397,0.131]
Mother's Education equals or greater than College		-0.04	-0.049	-0.102
		[-0.258,0.2]	[-0.281,0.185]	[-0.304,0.152]
Mother's Education Missing		0.712***	0.677***	0.625***
		[0.293,1.127]	[0.27,1.051]	[0.226,1.068]
Father's Education less than College		0.239*	0.239*	0.271**
		[-0.025,0.467]	[-0.01,0.519]	[0.026,0.521]
Father's Education equals or greater than College		0.438***	0.421***	0.404***
		[0.198,0.669]	[0.203,0.662]	[0.149,0.671]
Family income 35-50K		-0.207	-0.214	-0.29**
		[-0.48,0.042]	[-0.473,0.079]	[-0.589,-0.009]

Family income 50-75K	-0.212*	-0.24*	-0.316**
	[-0.498,0.03]	[-0.509,0.018]	[-0.606,-0.024]
Family income >75K	-0.162	-0.201*	-0.334**
	[-0.42,0.092]	[-0.486,0.055]	[-0.57,-0.023]
High School enrollment		0	0
		[-0.0,0.0]	[-0.0,0.0]
High School enrollment Missing		0.318	0.131
		[-0.887,1.725]	[-1.413,1.574]
High School FRL Pct		-0.002	0
		[-0.009,0.006]	[-0.008,0.007]
High School FRL Pct Missing		0.142	0.068
		[-0.06,0.345]	[-0.128,0.256]
High School FTE		0.003	0.002
		[-0.001,0.007]	[-0.003,0.006]
High School FTE Missing		0.121	0.095
		[-0.736,0.818]	[-0.865,0.846]
High School Minority Pct		-0.001	-0.001
		[-0.005,0.004]	[-0.005,0.004]
High School Minority Pct Missing		-0.585	-0.536
		[-1.887,0.744]	[-1.872,0.899]
High School in Rural		0.117	0.211*
		[-0.125,0.35]	[-0.003,0.437]
High School in City		0.093	0.154
		[-0.095,0.274]	[-0.03,0.355]
12th Grade Math Test Score			0.442***
			[0.254,0.62]
12th Grade Test Score Missing			-0.337
			[-0.811,0.089]
High School GPA			1.118***
			[0.686,1.57]
High School GPA Missing			4.645
			[-12.973,3.349]
Math Course Taking Mid lvl 2			-0.143
			[-0.855,0.508]
Math Course Taking Advanced lvl 1			0.223
			[-0.38,0.862]
Math Course Taking Advanced lvl 2			0.438
			[-0.196,1.057]
Math Course Taking Advanced lvl 3			0.79***

				[0.175,1.471]
Math Course Taking Missing				-4.334
				[-2.983,13.457]
ACT Composite Score				-0.027
				[-0.073,0.015]
ACT Composite Score Missing				0.645
				[-1.711,13.299]
ACT Math Score				0.066***
				[0.025,0.108]
ACT Math Score Missing				-0.801
				[-13.267,1.535]
N	6820	6820	6820	6820
Pseudo R-squared	0.131	0.137	0.141	0.184
	[0.114,0.15]	[0.122,0.155]	[0.124,0.159]	[0.163,0.205]

Notes: The regression output corresponds to equation (2.1) in the main text. Bootstrapped mean estimates and 95 percent confidence intervals are reported. ***p<0.01, **p<0.05, *p<0.10. All sample sizes based on actual ELS data are rounded to the nearest ten. All unrounded sample sizes are generated sample sizes.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), Base Year, First Follow-up, Second Follow-up, Third Follow-up, and Postsecondary Transcripts Restricted-Use File.

Appendix D

Supplementary Tables for Chapter 3

Appendix Table D1: Effect of the lagged unemployment rate on college enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate in year t-1	1.545	0.978	3.553***	1.849	4.452***	3.272*
	(0.967)	(1.247)	(1.020)	(1.228)	(1.432)	(1.922)
Observations	5883	5883	5883	5883	5883	5883
R-squared	0.983	0.975	0.975	0.974	0.971	0.959
Panel B: Total						
Unemployment Rate in year t-2	1.614	0.765	3.376**	1.780	3.987*	2.574
	(1.088)	(1.175)	(1.536)	(1.685)	(2.180)	(2.266)
Observations	5511	5511	5511	5511	5511	5511
R-squared	0.985	0.977	0.976	0.975	0.972	0.961
Panel C: Total						
Unemployment Rate in year t-3	1.531	0.463	2.325	1.162	2.724	1.555
	(1.279)	(1.131)	(2.001)	(1.924)	(2.604)	(2.352)
Observations	5139	5139	5139	5139	5139	5139
R-squared	0.986	0.979	0.977	0.977	0.972	0.962
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Appendix Table D2: Effect of the unemployment rate (LAUS measures) on college enrollment restricting MSAs

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate	0.909	1.046	2.413**	1.825	3.867***	3.799**
	(0.641)	(1.172)	(1.014)	(1.269)	(0.689)	(1.56)
Observations	3613	3613	3613	3613	3613	3613
R-squared	0.985	0.973	0.982	0.977	0.983	0.965
Panel B: Women						
Unemployment Rate	0.812	0.816	1.99*	1.073	3.823***	3.802***
	(0.668)	(1.329)	(1.083)	(1.293)	(0.698)	(1.42)
Observations	3613	3613	3613	3613	3613	3613
R-squared	0.986	0.971	0.978	0.975	0.982	0.964
Panel C: Men						
Unemployment Rate	0.952	1.213	2.954***	2.584**	3.507***	3.869**
	(0.617)	(0.962)	(0.956)	(1.209)	(0.933)	(1.873)
Observations	3613	3613	3613	3613	3613	3613
R-squared	0.988	0.978	0.983	0.977	0.977	0.961
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. Unemployment rate are calculated based on LAUS data. MSAs in which the difference between two unemployment rates measured by CPS data and LAUS data are no larger than three percentage points are kept. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Appendix Table D3: Effect of the unemployment rate (CPS measures) on college enrollment restricting MSAs

	(1)	(2)	(3)	(4)	(5)	(6)
	4-year degree enrollment		2-year degree enrollment		sub-2-year degree enrollment	
	full-time	part-time	full-time	part-time	full-time	part-time
Panel A: Total						
Unemployment Rate	0.623	-0.157	2.694***	2.793*	2.99***	4.056***
	(0.522)	(0.803)	(0.962)	(1.664)	(0.734)	(1.387)
Observations	3613	3613	3613	3613	3613	3613
R-squared	0.985	0.973	0.982	0.977	0.983	0.965
Panel B: Women						
Unemployment Rate	0.465	-0.443	2.102**	2.116	2.793***	4.088***
	(0.562)	(0.935)	(1.012)	(1.64)	(0.739)	(1.264)
Observations	3613	3613	3613	3613	3613	3613
R-squared	0.986	0.971	0.978	0.975	0.982	0.965
Panel C: Men						
Unemployment Rate	0.834*	0.261	3.426***	3.546**	3.189***	4.131**
	(0.472)	(0.679)	(0.887)	(1.59)	(0.884)	(1.645)
Observations	3613	3613	3613	3613	3613	3613
R-squared	0.988	0.978	0.983	0.977	0.977	0.961
MSA FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. Unemployment rate are calculated based on CPS data. MSAs in which the difference between two unemployment rates measured by CPS data and LAUS data are no larger than three percentage points are kept. Regressions are weighted by 2010 MSA population.*** p<0.01, ** p<0.05, * p<0.1.

Appendix Table D4: Effect of the unemployment rate on degree completions

	4-year degree			2-year degree			sub-2-year degree		
	Total	Women	Men	Total	Women	Men	Total	Women	Men
Unemployment Rate in year t-1	0.354	0.248	0.226	-0.352	-0.728	0.141	2.475*	2.940**	1.339
	(0.873)	(0.849)	(0.765)	(1.570)	(1.585)	(1.495)	(1.346)	(1.360)	(1.432)
Unemployment Rate in year t-2	1.197**	1.071*	1.549***	1.689	1.355	2.092			
	(0.606)	(0.584)	(0.588)	(2.202)	(2.139)	(2.282)			
Unemployment Rate in year t-3	0.029	0.025	-0.189						
	(0.423)	(0.427)	(0.423)						
Unemployment Rate in year t-4	0.964	0.926	1.386						
	(1.283)	(1.250)	(1.166)						
Observations	4768	4768	4768	5511	5511	5511	5883	5883	5883
R-squared	0.982	0.982	0.986	0.978	0.978	0.978	0.967	0.967	0.960
MSA FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Control Variables	X	X	X	X	X	X	X	X	X
Weighted by Pop.	X	X	X	X	X	X	X	X	X

Notes: Robust standard errors clustered at MSA level in parentheses, control variables include share of population whites, and sex ratio. 4-year degree includes bachelor's degree, 2-year degree includes associate's degree, and sub-2-year degree includes all kinds of certificates. Regressions are weighted by 2010 MSA population. *** p<0.01, ** p<0.05, * p<0.1.

Appendix E

Supplementary Materials for Chapter 3

Measuring Enrollment by Program Level

I combine present-year degree completion data with the same data from the previous two years to calculate three-year-average shares of three types of degrees awarded (bachelor's degrees, associate's degrees and certificates) at each institution. I then multiply these shares by the enrollment of full-time and part-time degree seeking students to calculate the number of students in each degree program in each year.

If an institution does not have any degrees conferred in past three years, I use the nearest year's information available to impute the value. If IPEDS does not report any degree conferred information about an institution in any year, I use the institution type designated by IPEDS in the first year of my data sample to impute the values—this matches the standard approach (e.g., for 4-year colleges as categorized by IPEDS, I assume all students are seeking a bachelor's degree).

I also use other time windows of different length to calculate the shares of types of degrees awarded. For example, I use only the present degree completion data to increase the responsiveness of the approximation to immediate changes. The results are very similar to my main findings if I use alternative measures.

CPS Data Bootstrapping Procedure

The bootstrap sampling procedure is as follows: first, I randomly draw with replacement within each cell of MSA by year from original CPS data to get new sample. Then I use the new sample to calculate unemployment rates for each MSA in each year. Call this value UR_{ist}, where *i* denotes the MSA, *s* denotes bootstrap sample number, and *t* denotes the year. I repeat this 300 times, so that for each MSA in each year, I have 300

sample unemployment rates. I then calculate the variance of the 300 unemployment rates for each MSA in each year, $Var\_UR_{it} = \sum_{s=1}^{300}(UR_{ist} - \overline{UR_{it}})^2/300$. The average over all MSA-by-year variances Var_UR_{it} is the variance of sampling error. $variance\ of\ noise = \overline{Var\_UR_{it}} = \sum_i \sum_t Var\_UR_{it}$ /N, where N is the number of observations of MSA-by-year units.

After getting the variance of noise, I can calculate the reliability ratio of the unemployment rate: $r = 1 - \frac{variance\ of\ noise}{variance\ of\ unemployment\ rate}$. When fitting the error-in-variable regressions, instead of using $(X'X)^{-1}(X'y)$, now I use $(X'X - S)^{-1}(X'y)$, where S is a diagonal matrix with elements $N(1 - r_i){s_i}^2$, where N is the number of observations, r_i is the reliability ration of ith independent variable, and s_i^2 is the variance of ith independent variable.

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